BRITISH ASSOCIATION
FOR THE ADVANCEMENT
OF SCIENCE

REPORT

OF THE

ANNUAL MEETING, 1938
(108TH YEAR)

CAMBRIDGE
AUGUST 17-24

LONDON

OFFICE OF THE BRITISH ASSOCIATION
BURLINGTON HOUSE, LONDON, W.1

1938

CONTENTS.

APPENDIX.

BRITISH ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE.

OFFICERS & COUNCIL, 1938-39.

PATRON:
HIS MAJESTY THE KING.

PRESIDENT, 1938:
Rt. Hon. Lord Rayleigh, D.Sc., LL.D., F.R.S.

PRESIDENT, 1939:
Sir Albert Seward, F.R.S., Sc.D., LL.D.

VICE-PRESIDENTS FOR THE CAMBRIDGE MEETING.

The Chancellor of the University (Rt. Hon. the Earl Baldwin of Bewdley, P.C., F.R.S.).
The Vice-Chancellor of the University (Professor H. R. Dean, M.D., Master of Trinity Hall).
The Lord-Lieutenant of Cambridgeshire (C. R. W. Adeane, C.B., J.P.).
The High Sheriff for Cambridgeshire and Huntingdonshire (T. Peake, J.P.).
The Mayor of Cambridge (E. Saville Peck, M.A.).
Alderman W. L. Briggs, J.P.
The Chairman of the Cambridgeshire County Council (Councillor A. R. Fordham, J.P.).
The Rt. Rev. the Lord Bishop of Ely (Rt. Rev. B. O. F. Heywood, D.D.).
The Rt. Hon. Lord Fairhaven, D.L.
Alderman H. Franklin.
W. A. H. Harding, M.A.
W. W. Pemberton, M.B., B.Ch., J.P.
Sir J. J. Thomson, O.M., F.R.S.
Prof. Sir F. Gowland Hopkins, O.M., F.R.S.
Sir Albert Seward, F.R.S.
The Very Rev. the Dean of Ely (Very Rev. Lionel E. Blackburne, M.A.).
Rev. Prof. C. E. Raven, D.D.

VICE-PRESIDENTS ELECT FOR THE DUNDEE MEETING.

(To be appointed.)

GENERAL TREASURER.

Prof. P. G. H. Boswell, O.B.E., D.Sc., F.R.S.

GENERAL SECRETARIES.

Prof. F. T. Brooks, M.A., F.R.S.
Prof. Allan Ferguson, D.Sc.

SECRETARY.

O. J. R. Howarth, O.B.E., Ph.D.

ASSISTANT SECRETARY.

D. N. Lowe, M.A., B.Sc.

ORDINARY MEMBERS OF THE COUNCIL.

R. W. Allen, C.B.E.
Dr. F. W. Aston, F.R.S.
Prof. F. Aveling.
Prof. F. Balfour-Browne.
Sir T. Hudson Beare.
Rt. Hon. Viscount Bledisloe, P.C., G.C.M.G., G.B.E.
Dr. W. T. Calman, C.B., F.R.S.
Prof. F. Debenham, O.B.E.
Dr. C. R. Fay.
Prof. W. G. Fearnsides, F.R.S.
Prof. H. J. Fleure, F.R.S.
Prof. F. E. Fritsch, F.R.S.
Sir Richard Gregory, F.R.S.
Prof. A. V. Hill, O.B.E., Sec.R.S.
Prof. T. G. Hill.
Prof. T. S. Moore.
Prof. J. C. Philip, O.B.E., F.R.S.
Prof. J. G. Smith.
Lt.-Col. W. Campbell Smith.
Prof. C. Spearman, F.R.S.
Dr. C. Tierney.
Dr. J. A. Venn.
Prof. Sir Gilbert Walker, C.S.I., F.R.S.
R. S. Whipple.
J. S. Wilson.

EX-OFFICIO MEMBERS OF THE COUNCIL.

Past Presidents of the Association, the President for the year, the President and Vice-Presidents for the ensuing Annual Meeting, past and present General Treasurers and General Secretaries, and the Local Treasurers and Local Secretaries for the Annual Meetings immediately past and ensuing.

PAST PRESIDENTS OF THE ASSOCIATION.

Sir J. J. Thomson, O.M., F.R.S. (1909).
Sir Oliver Lodge, F.R.S. (1913).
Sir Arthur Evans, F.R.S. (1916–18).
Prof. Sir C. S. Sherrington, O.M., G.B.E., F.R.S. (1922).
H.R.H. The Prince of Wales, K.G., D.C.L., F.R.S. (1926).
Prof. Sir Arthur Keith, F.R.S. (1927).
Prof. Sir William H. Bragg, O.M., K.B.E., Pres.R.S. (1928).
Sir Thomas H. Holland, K.C.I.E., K.C.S.I., F.R.S. (1929).
Prof. F. O. Bower, F.R.S. (1930).
Gen. The Rt. Hon. J. C. Smuts, P.C., C.H., F.R.S. (1931).
Sir F. Gowland Hopkins, O.M., F.R.S. (1933).
Sir James H. Jeans, F.R.S. (1934).
Prof. W. W. Watts, LL.D., Sc.D., F.R.S. (1935).
Rt. Hon. Lord Stamp, G.C.B., G.B.E., D.Sc. (1936).
Prof. Sir Edward Poulton, F.R.S. (1937).

PAST GENERAL OFFICERS OF THE ASSOCIATION.

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Prof. F. J. M. Stratton, D.S.O., O.B.E., M.A.
Sir Frank Smith, K.C.B., C.B.E., Sec.R.S.

HON. AUDITORS.

Dr. Ezer Griffiths, F.R.S. | R. S. Whipple.

HON. CURATOR OF DOWN HOUSE.

Sir Buckston Browne, F.R.C.S.

LOCAL OFFICERS FOR THE CAMBRIDGE MEETING.

LOCAL HON. SECRETARIES.

C. H. Kemp, Town Clerk of Cambridge.
A. Tabrum, O.B.E., M.A., LL.M., Clerk to the Cambridgeshire County Council.
F. P. White, M.A., St. John's College, Cambridge.
E. N. Willmer, M.A., Physiological Laboratory, Cambridge.

LOCAL HON. TREASURERS.

R. Ede, M.A., School of Agriculture, Cambridge.
R. H. Parker, M.A., D.L., M.C., Barclays Bank, Cambridge.

LOCAL OFFICERS FOR THE DUNDEE MEETING.

(August 30 to September 6, 1939.)

CHAIRMAN OF THE LOCAL GENERAL COMMITTEE.

The Lord Provost of Dundee
John Phin, LL.D.

LOCAL HON. SECRETARIES.

W. A. R. Allardice, Lord Provost's Secretary, Dundee.
Prof. E. T. Copson, University College, Dundee.
David Latto, Town Clerk, Dundee.

LOCAL HON. TREASURERS.

Wm. Aitken, City Chamberlain, Dundee.
Quintin B. Grant, Royal Bank of Scotland, Dundee.

SECTIONAL OFFICERS.

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Recorder.—Dr. Ezer Griffiths, F.R.S.
Secretaries.—J. H. Awbery, Prof. W. H. McCrea, Dr. D. M. Wrinch.
Local Secretaries.—Dr. N. Feather, Dr. J. Wishart.

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Secretaries.—Dr. H. J. T. Ellingham, T. W. J. Taylor.
Local Secretary.—Dr. A. E. Moelwyn-Hughes.

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Vice-Presidents.—Dr. H. von Eckermann, Dr. A. Harker, F.R.S., Prof. O. T. Jones, F.R.S., Sir Albert Seward, F.R.S., Prof. C. E. Tilley, F.R.S., Prof. L. J. Wills.
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Secretaries.—Dr. O. M. B. Bulman, W. H. Wilcockson.
Local Secretary.—M. Black.

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Local Secretary.—Dr. F. S. J. Hollick.

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Recorder.—J. N. L. Baker.
Secretaries.—Dr. R. O. Buchanan, D. L. Linton.
Local Secretary.—J. A. Steers.

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President.—R. F. Harrod.
Vice-Presidents.—Sir W. Beveridge, K.C.B., Dr. C. R. Fay, Prof. P. Sargant Florence, Dr. J. N. Keynes, Mrs. M. Marshall.
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Secretaries.—S. R. Dennison, E. D. McCallum.
Local Secretary.—D. G. Champernowne.

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Vice-Presidents.—O. Borer, Sir Alexander Gibb, G.B.E., C.B., F.R.S., Prof. C. E. Inglis, O.B.E., F.R.S., C. C. Mason.
Recorder.—Wing-Commander T. R. Cave-Browne-Cave, C.B.E.
Secretaries.—H. M. Clarke, Prof. W. J. John.
Local Secretary.—Dr. R. D. Davies.

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President.—Prof. V. Gordon Childe.
Vice-Presidents.—L. C. G. Clarke, Dr. A. C. Haddon, F.R.S., Prof. J. H. Hutton, C.I.E., Prof. E. H. Minns, F.B.A.
Recorder.—R. U. Sayce.
Secretaries.—Miss C. Fell, K. H. Jackson.
Local Secretary.—Dr. G. E. Daniel.

J.—PSYCHOLOGY.

President.—Dr. R. H. Thouless.
Vice-Presidents.—Prof. F. C. Bartlett, F.R.S., R. J. Bartlett, Dr. Mary Collins, E. Farmer, Prof. H. S. Langfeld, Prof. A. Michotte.
Recorder.—Dr. S. J. F. Philpott.
Secretaries.—Dr. Hilda Oldham, Dr. P. E. Vernon.
Local Secretary.—Miss M. D. Vernon.

K.—BOTANY.

President.—Prof. W. Stiles, F.R.S.
Vice-Presidents.—G. E. Briggs, F.R.S., Prof. F. T. Brooks, F.R.S., Sir Roy Robinson (Chairman, Dept. of Forestry, K*), Prof. E. J. Salisbury, F.R.S., Sir Albert Seward, F.R.S., W. L. Taylor, Dr. H. H. Thomas, F.R.S.
Recorder.—Dr. B. Barnes.
Secretaries.—Prof. T. M. Harris, C. H. Thompson, T. Thomson, Dr. S. Williams.
Local Secretary.—G. C. Evans.

L.—EDUCATIONAL SCIENCE.

President.—J. Sargent.
Vice-Presidents.—Prof. E. Barker, A. R. Fordham, G. F. Hickson, Prof. G. R. Owst, H. G. Wells, D.Litt.
Recorder.—A. Gray Jones.
Secretaries.—S. R. Humby, N. F. Sheppard.
Local Secretary.—J. O. Roach.

M.—AGRICULTURE.

President.—Prof. R. G. Stapledon, C.B.E.
Vice-Presidents.—J. M. Caie, J. S. Chivers, Sir Wm. Dampier, F.R.S., Prof. F. L. Engledow.
Recorder.—W. Godden.
Secretary.—G. V. Jacks.
Local Secretary.—F. Hanley.

CONFERENCE OF DELEGATES OF CORRESPONDING SOCIETIES.

President.—Rt. Hon. the Earl of Onslow, C.B.E., P.C., F.S.A.
Secretary.—Dr. C. Tierney.

TABLE OF

Date of Meeting	Where held	Presidents	Old Life Members	New Life Members
1831, Sept. 27	York	Viscount Milton, D.C.L., F.R.S.	—	—
1832, June 19	Oxford	The Rev. W. Buckland, F.R.S.	—	—
1833, June 25	Cambridge	The Rev. A. Sedgwick, F.R.S.	—	—
1834, Sept. 8	Edinburgh	Sir T. M. Brisbane, D.C.L., F.R.S.	—	—
1835, Aug. 10	Dublin	The Rev. Provost Lloyd, LL.D., F.R.S.	—	—
1836, Aug. 22	Bristol	The Marquis of Lansdowne, F.R.S.	—	—
1837, Sept. 11	Liverpool	The Earl of Burlington, F.R.S.	—	—
1838, Aug. 10	Newcastle-on-Tyne	The Duke of Northumberland, F.R.S.	—	—
1839, Aug. 26	Birmingham	The Rev. W. Vernon Harcourt, F.R.S.	—	—
1840, Sept. 17	Glasgow	The Marquis of Breadalbane, F.R.S.	—	—
1841, July 20	Plymouth	The Rev. W. Whewell, F.R.S.	169	65
1842, June 23	Manchester	The Lord Francis Egerton, F.G.S.	303	169
1843, Aug. 17	Cork	The Earl of Rosse, F.R.S.	109	28
1844, Sept. 26	York	The Rev. G. Peacock, D.D., F.R.S.	226	150
1845, June 19	Cambridge	Sir John F. W. Herschel, Bart., F.R.S.	313	36
1846, Sept. 10	Southampton	Sir Roderick I. Murchison, Bart., F.R.S.	241	10
1847, June 23	Oxford	Sir Robert H. Inglis, Bart., F.R.S.	314	18
1848, Aug. 9	Swansea	The Marquis of Northampton, Pres. R.S.	149	3
1849, Sept. 12	Birmingham	The Rev. T. R. Robinson, D.D., F.R.S.	227	12
1850, July 21	Edinburgh	Sir David Brewster, K.H., F.R.S.	235	9
1851, July 2	Ipswich	G. B. Airy, Astronomer Royal, F.R.S.	172	8
1852, Sept. 1	Belfast	Lieut.-General Sabine, F.R.S.	164	10
1853, Sept. 3	Hull	William Hopkins, F.R.S.	141	13
1854, Sept. 20	Liverpool	The Earl of Harrowby, F.R.S.	238	23
1855, Sept. 12	Glasgow	The Duke of Argyll, F.R.S.	194	33
1856, Aug. 6	Cheltenham	Prof. C. G. B. Daubeney, M.D., F.R.S.	182	14
1857, Aug. 26	Dublin	The Rev. H. Lloyd, D.D., F.R.S.	236	15
1858, Sept. 22	Leeds	Richard Owen, M.D., D.C.L., F.R.S.	222	42
1859, Sept. 14	Aberdeen	H.R.H. The Prince Consort	184	27
1860, June 27	Oxford	The Lord Wrottesley, M.A., F.R.S.	286	21
1861, Sept. 4	Manchester	William Fairbairn, LL.D., F.R.S.	321	113
1862, Oct. 1	Cambridge	The Rev. Professor Willis, M.A., F.R.S.	239	15
1863, Aug. 26	Newcastle-on-Tyne	Sir William G. Armstrong, C.B., F.R.S.	203	36
1864, Sept. 13	Bath	Sir Charles Lyell, Bart., M.A., F.R.S.	287	40
1865, Sept. 6	Birmingham	Prof. J. Phillips, M.A., LL.D., F.R.S.	292	44
1866, Aug. 22	Nottingham	William R. Grove, Q.C., F.R.S.	207	31
1867, Sept. 4	Dundee	The Duke of Buccleuch, K.C.B., F.R.S.	167	25
1868, Aug. 19	Norwich	Dr. Joseph D. Hooker, F.R.S.	196	18
1869, Aug. 18	Exeter	Prof. G. G. Stokes, D.C.L., F.R.S.	204	21
1870, Sept. 14	Liverpool	Prof. T. H. Huxley, LL.D., F.R.S.	314	39
1871, Aug. 2	Edinburgh	Prof. Sir W. Thomson, LL.D., F.R.S.	246	28
1872, Aug. 14	Brighton	Dr. W. B. Carpenter, F.R.S.	245	36
1873, Sept. 17	Bradford	Prof. A. W. Williamson, F.R.S.	212	27
1874, Aug. 19	Belfast	Prof. J. Tyndall, LL.D., F.R.S.	162	13
1875, Aug. 25	Bristol	Sir John Hawkshaw, F.R.S.	239	36
1876, Sept. 6	Glasgow	Prof. T. Andrews, M.D., F.R.S.	221	35
1877, Aug. 15	Plymouth	Prof. A. Thomson, M.D., F.R.S.	173	19
1878, Aug. 14	Dublin	W. Spottiswoode, M.A., F.R.S.	201	18
1879, Aug. 20	Sheffield	Prof. G. J. Allman, M.D., F.R.S.	184	16
1880, Aug. 25	Swansea	A. C. Ramsay, LL.D., F.R.S.	144	11
1881, Aug. 31	York	Sir John Lubbock, Bart., F.R.S.	272	28
1882, Aug. 23	Southampton	Dr. C. W. Siemens, F.R.S.	178	17
1883, Sept. 19	Southport	Prof. A. Cayley, D.C.L., F.R.S.	203	60
1884, Aug. 27	Montreal	Prof. Lord Rayleigh, F.R.S.	235	20
1885, Sept. 9	Aberdeen	Sir Lyon Playfair, K.C.B., F.R.S.	225	18
1886, Sept. 1	Birmingham	Sir J. W. Dawson, C.M.G., F.R.S.	314	25
1887, Aug. 31	Manchester	Sir H. E. Roscoe, D.C.L., F.R.S.	428	86
1888, Sept. 5	Bath	Sir F. J. Bramwell, F.R.S.	266	36
1889, Sept. 11	Newcastle-on-Tyne	Prof. W. H. Flower, C.B., F.R.S.	277	20
1890, Sept. 3	Leeds	Sir F. A. Abel, C.B., F.R.S.	259	21
1891, Aug. 19	Cardiff	Dr. W. Huggins, F.R.S.	189	24
1892, Aug. 3	Edinburgh	Sir A. Geikie, LL.D., F.R.S.	280	14
1893, Sept. 13	Nottingham	Prof. J. S. Burdon Sanderson, F.R.S.	201	17
1894, Aug. 8	Oxford	The Marquis of Salisbury, K.G., F.R.S.	327	21
1895, Sept. 11	Ipswich	Sir Douglas Galton, K.C.B., F.R.S.	214	13
1896, Sept. 16	Liverpool	Sir Joseph Lister, Bart., Pres. R.S.	330	31
1897, Aug. 18	Toronto	Sir John Evans, K.C.B., F.R.S.	120	8
1898, Sept. 7	Bristol	Sir W. Crookes, F.R.S.	281	19
1899, Sept. 13	Dover	Sir Michael Foster, K.C.B., Sec. R.S.	296	20

* Ladies were not admitted by purchased tickets until 1843. † Tickets of Admission to Sections only.

[*Continued on p.* xii.

ANNUAL MEETINGS.

Old Annual Members	New Annual Members	Associates	Ladies	Foreigners	Total	Amount received for Tickets	Sums paid on account of Grants for Scientific Purposes	Year
—	—	—	—	—	353	—	—	1831
—	—	—	—	—	—	—	—	1832
—	—	—	—	—	900	—	—	1833
—	—	—	—	—	1298	—	£20 0 0	1834
—	—	—	—	—	—	—	167 0 0	1835
—	—	—	—	—	1350	—	435 0 0	1836
—	—	—	—	—	1840	—	922 12 6	1837
—	—	—	1110*	—	2400	—	932 2 2	1838
—	—	—	—	34	1438	—	1595 11 0	1839
—	—	—	—	40	1353	—	1546 16 4	1840
46	317	—	60*	—	891	—	1235 10 11	1841
75	376	33†	331*	28	1315	—	1449 17 8	1842
71	185	—	160	—	—	—	1565 10 2	1843
45	190	9†	260	—	—	—	981 12 8	1844
94	22	407	172	35	1079	—	831 9 9	1845
65	39	270	196	36	857	—	685 16 0	1846
197	40	495	203	53	1320	—	208 5 4	1847
54	25	376	197	15	819	£707 0 0	275 1 8	1848
93	33	447	237	22	1071	963 0 0	159 19 6	1849
128	42	510	273	44	1241	1085 0 0	345 18 0	1850
61	47	244	141	37	710	620 0 0	391 9 7	1851
63	60	510	292	9	1108	1085 0 0	304 6 7	1852
56	57	367	236	6	876	903 0 0	205 0 0	1853
121	121	765	524	10	1802	1882 0 0	380 19 7	1854
142	101	1094	543	26	2133	2311 0 0	480 16 4	1855
104	48	412	346	9	1115	1098 0 0	734 13 9	1856
156	120	900	569	26	2022	2015 0 0	507 15 4	1857
111	91	710	509	13	1698	1931 0 0	618 18 2	1858
125	179	1206	821	22	2564	2782 0 0	684 11 1	1859
177	59	636	463	47	1689	1604 0 0	766 19 6	1860
184	125	1589	791	15	3138	3944 0 0	1111 5 10	1861
150	57	433	242	25	1161	1089 0 0	1293 16 6	1862
154	209	1704	1004	25	3335	3640 0 0	1608 3 10	1863
182	103	1119	1058	13	2802	2965 0 0	1289 15 8	1864
215	149	766	508	23	1997	2227 0 0	1591 7 10	1865
218	105	960	771	11	2303	2469 0 0	1750 13 4	1866
193	118	1163	771	7	2444	2613 0 0	1739 4 0	1867
226	117	720	682	45‡	2004	2042 0 0	1940 0 0	1868
229	107	678	600	17	1856	1931 0 0	1622 0 0	1869
303	195	1103	910	14	2878	3096 0 0	1572 0 0	1870
311	127	976	754	21	2463	2575 0 0	1472 2 6	1871
280	80	937	912	43	2533	2649 0 0	1285 0 0	1872
237	99	796	601	11	1983	2120 0 0	1685 0 0	1873
232	85	817	630	12	1951	1979 0 0	1151 16 0	1874
307	93	884	672	17	2248	2397 0 0	960 0 0	1875
331	185	1265	712	25	2774	3023 0 0	1092 4 2	1876
238	59	446	283	11	1229	1268 0 0	1128 9 7	1877
290	93	1285	674	17	2578	2615 0 0	725 16 6	1878
239	74	529	349	13	1404	1425 0 0	1080 11 11	1879
171	41	389	147	12	915	899 0 0	731 7 7	1880
313	176	1230	514	24	2557	2689 0 0	476 8 1	1881
253	79	516	189	21	1253	1286 0 0	1126 1 11	1882
330	323	952	841	5	2714	3369 0 0	1083 3 3	1883
317	219	826	74	26 & 60 H.§	1777	1855 0 0	1173 4 0	1884
332	122	1053	447	6	2203	2256 0 0	1385 0 0	1885
428	179	1067	429	11	2453	2532 0 0	995 0 6	1886
510	244	1985	493	92	3838	4336 0 0	1186 18 0	1887
399	100	639	509	12	1984	2107 0 0	1511 0 5	1888
412	113	1024	579	21	2437	2441 0 0	1417 0 11	1889
368	92	680	334	12	1775	1776 0 0	789 16 8	1890
341	152	672	107	35	1497	1664 0 0	1029 10 0	1891
413	141	733	439	50	2070	2007 0 0	864 10 0	1892
328	57	773	268	17	1661	1653 0 0	907 15 6	1893
435	69	941	451	77	2321	2175 0 0	583 15 6	1894
290	31	493	261	22	1324	1236 0 0	977 15 5	1895
383	139	1384	873	41	3181	3228 0 0	1104 6 1	1896
286	125	682	100	41	1362	1398 0 0	1059 10 8	1897
327	96	1051	639	33	2446	2399 0 0	1212 0 0	1898
324	68	548	120	27	1403	1328 0 0	1430 14 2	1899

‡ Including Ladies. § Fellows of the American Association were admitted as Hon. Members for this Meeting.

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Table of

Date of Meeting	Where held	Presidents	Old Life Members	New Life Members
1900, Sept. 5	Bradford	Sir William Turner, D.C.L., F.R.S.	267	13
1901, Sept. 11	Glasgow	Prof. A. W. Rücker, D.Sc., Sec. R.S.	310	37
1902, Sept. 10	Belfast	Prof. J. Dewar, LL.D., F.R.S.	243	21
1903, Sept. 9	Southport	Sir Norman Lockyer, K.C.B., F.R.S.	250	21
1904, Aug. 17	Cambridge	Rt. Hon. A. J. Balfour, M.P., F.R.S.	419	32
1905, Aug. 15	South Africa	Prof. G. H. Darwin, LL.D., F.R.S.	115	40
1906, Aug. 1	York	Prof. E. Ray Lankester, LL.D., F.R.S.	322	10
1907, July 31	Leicester	Sir David Gill, K.C.B., F.R.S.	276	19
1908, Sept. 2	Dublin	Dr. Francis Darwin, F.R.S.	294	24
1909, Aug. 25	Winnipeg	Prof. Sir J. J. Thomson, F.R.S.	117	13
1910, Aug. 31	Sheffield	Rev. Prof. T. G. Bonney, F.R.S.	293	26
1911, Aug. 30	Portsmouth	Prof. Sir W. Ramsay, K.C.B., F.R.S.	284	21
1912, Sept. 4	Dundee	Prof. E. A. Schäfer, F.R.S.	288	14
1913, Sept. 10	Birmingham	Sir Oliver J. Lodge, F.R.S.	376	40
1914, July–Sept.	Australia	Prof. W. Bateson, F.R.S.	172	13
1915, Sept. 7	Manchester	Prof. A. Schuster, F.R.S.	242	19
1916, Sept. 5	Newcastle-on-Tyne		164	12
1917	(No Meeting)	Sir Arthur Evans, F.R.S.	—	—
1918	(No Meeting)		—	—
1919, Sept. 9	Bournemouth	Hon. Sir C. Parsons, K.C.B., F.R.S.	235	47
1920, Aug. 24	Cardiff	Prof. W. A. Herdman, C.B.E., F.R.S.	288	11
1921, Sept. 7	Edinburgh	Sir T. E. Thorpe, C.B., F.R.S.	336	9
1922, Sept. 6	Hull	Sir C. S. Sherrington, G.B.E., Pres. R.S.	228	13
1923, Sept. 12	Liverpool	Sir Ernest Rutherford, F.R.S.	326	12
1924, Aug. 6	Toronto	Sir David Bruce, K.C.B., F.R.S.	119	7
1925, Aug. 26	Southampton	Prof. Horace Lamb, F.R.S.	280	8
1926, Aug. 4	Oxford	H.R.H. The Prince of Wales, K.G., F.R.S.	358	9
1927, Aug. 31	Leeds	Sir Arthur Keith, F.R.S.	249	9
1928, Sept. 5	Glasgow	Sir William Bragg, K.B.E., F.R.S.	260	10
1929, July 22	South Africa	Sir Thomas Holland, K.C.S.I., K.C.I.E., F.R.S.	81	1
1930, Sept. 3	Bristol	Prof. F. O. Bower, F.R.S.	221	5
1931, Sept. 23	London	Gen. the Rt. Hon. J. C. Smuts, P.C., C.H., F.R.S.	487	14
1932, Aug. 31	York	Sir Alfred Ewing, K.C.B., F.R.S.	206	1
1933, Sept. 6	Leicester	Sir F. Gowland Hopkins, Pres. R.S.	185	37
1934, Sept. 5	Aberdeen	Sir James H. Jeans, F.R.S.[14]	199	21
1935, Sept. 4	Norwich	Prof. W. W. Watts, F.R.S.	191	11
1936, Sept. 9	Blackpool	Sir Josiah Stamp, G.C.B., G.B.E.	188	10
1937, Sept. 1	Nottingham	Sir Edward B. Poulton, F.R.S.	213	6
1938, Aug. 17	Cambridge	Rt. Hon. Lord Rayleigh, F.R.S.	263	25[15]

[1] Including 848 Members of the South African Association.
[2] Including 137 Members of the American Association.
[3] Special arrangements were made for Members and Associates joining locally in Australia, see Report, 1914, p. 686. The numbers include 80 Members who joined in order to attend the Meeting of L'Association Française at Le Havre.
[4] Including Students' Tickets, 10s.
[5] Including Exhibitioners granted tickets without charge.
[6] Including grants from the Caird Fund in this and subsequent years.
[7] Including Foreign Guests, Exhibitioners, and others.

Annual Meetings—(continued).

Old Annual Members	New Annual Members	Associates	Ladies	Foreigners	Total	Amount received for Tickets	Sums paid on account of Grants for Scientific Purposes	Year
297	45	801	482	9	1915	£1801 0 0	£1072 10 0	1900
374	131	794	246	20	1912	2046 0 0	920 9 11	1901
314	86	647	305	6	1620	1644 0 0	947 0 0	1902
319	90	688	365	21	1754	1762 0 0	845 13 2	1903
449	113	1338	317	121	2789	2650 0 0	887 18 11	1904
937[1]	411	430	181	16	2130	2422 0 0	928 2 2	1905
356	93	817	352	22	1972	1811 0 0	882 0 9	1906
339	61	659	251	42	1647	1561 0 0	757 12 10	1907
465	112	1166	222	14	2297	2317 0 0	1157 18 8	1908
290[2]	162	789	90	7	1468	1623 0 0	1014 9 9	1909
379	57	563	123	8	1449	1439 0 0	963 17 0	1910
349	61	414	81	31	1241	1176 0 0	922 0 0	1911
368	95	1292	359	88	2504	2349 0 0	845 7 6	1912
480	149	1287	291	20	2643	2756 0 0	978 17 1	1913
139	4160[3]	539[3]	—	21	5044[3]	4873 0 0	1861 16 4[5]	1914
287	116	628[4]	141	8	1441	1406 0 0	1569 2 8	1915
250	76	251[4]	73	—	826	821 0 0	985 18 10	1916
—	—	—	—	—	—	—	677 17 2	1917
—	—	—	—	—	—	—	326 13 3	1918
254	102	688[4]	153	3	1482	1736 0 0	410 0 0	1919

Old Annual Regular Members	Annual Members: Meeting and Report	Annual Members: Meeting only	Transferable Tickets	Students' Tickets	Foreigners / Complimentary[7]	Total	Amount received for Tickets	Sums paid on account of Grants for Scientific Purposes	Year
136	192	571	42	120	20	1380	1272 10 0	1251 13 0[8]	1920
133	410	1394	121	343	22	2768	2599 15 0	518 1 10	1921
90	294	757	89	235[6]	24	1730	1699 5 0	722 0 7	1922
					Complimentary[7]				
123	380	1434	163	550	308	3296	2735 15 0	777 18 6[9]	1923
37	520	1866	41	89	139	2818	3165 19 0[10]	1197 5 9	1924
97	264	878	62	119	74	1782	1630 5 0	1231 0 0	1925
101	453	2338	169	225	69	3722	3542 0 0	917 1 6	1926
84	334	1487	82	264	161	2670	2414 5 0	761 10 0	1927
76	554	1835	64	201	74	3074	3072 10 0	1259 10 0	1928
24	177	1227[11]	—	161	83	1754	1477 15 0	2193 2 1	1929
68	310	1617	97	267	54	2639	2481 15 0	631 1 9	1930
78	656	2994	157	454	449	5702[12]	4792 10 0	1319 9 6	1931
44	226	1163	45	214	125	2024	1724 5 0	1218 13 11	1932
39	236	1468	82	147	74	2268	2428 2 0	562 19 11[13]	1933
30	273	1884	181	280	70	2938	2900 13 6	1423 4 9	1934
29	237	1444	142	197	70	2321	2218 14 6	1649 2 4	1935
29	257	1184	128	178	93	2067	2006 14 0	1098 1 1	1936
28	290	1096	102	200	92	2027	1883 12 0	720 15 1	1937
32	355	1932	53	209	114	2983	3072 19 0	1066 6 8	1938

[8] The Bournemouth Fund for Research, initiated by Sir C. Parsons, enabled grants on account of scientific purposes to be maintained.

[9] Including grants from the Caird Gift for research in radioactivity in this and subsequent years to 1926.

[10] Subscriptions paid in Canada were $5 for Meeting only and others pro rata; there was some gain on exchange.

[11] Including 450 Members of the South African Association.

[12] Including 413 tickets for certain meetings, issued at 5*s.* to London County Council school-teachers.

[13] For nine months ending March 31, 1933.

[14] Sir William B. Hardy, F.R.S., who became President on January 1, 1934, died on January 23.

[15] Including 8 representatives of Corporation Members.

NARRATIVE OF THE CAMBRIDGE MEETING.

On Wednesday, August 17, at 8.30 P.M., the Inaugural General Meeting was held in the Regal Cinema, when the Vice-Chancellor of the University of Cambridge (Prof. H. R. Dean, M.D.) and His Worship the Mayor of Cambridge (Councillor E. Saville Peck, M.A.) welcomed the Association to Cambridge. The President of the Association, the Rt. Hon. Lord Rayleigh, F.R.S., delivered an address entitled: (Part I) *Vision in Nature and Vision aided by Science*, (Part II) *Science and Warfare*, for which see p. 1. A vote of thanks to the President was proposed by Sir Joseph J. Thomson, O.M., F.R.S., Master of Trinity College, and seconded by Dr. G. D. Birkhoff, Past President of the American Association for the Advancement of Science.

Evening Discourses were delivered to the members as follows:

(1) Friday, August 19, in the Arts Theatre, Peas Hill, Dr. H. Godwin: *The History of the Fens.* (*See* p. 535.)

(2) Monday, August 22, in the same theatre, Prof. M. L. Oliphant, F.R.S.: *The Contribution of the Electrical Engineer to Modern Physics.* (See p. 536.)

On Tuesday, August 23, at 8.30 P.M., in the Reception Room (Examination School), members of the Scientific Delegation in India, 1937–38, spoke of the experiences of the Delegation. Sir James Jeans, F.R.S., General President of the Indian Science Congress Association for its Jubilee meeting, was in the chair, and the other speakers were Dr. C. G. Darwin, F.R.S., Dr. J. A. Venn, Prof. Winifred Cullis, C.B.E., Prof. J. H. Fleure, F.R.S., and Prof. W. W. Tattersall. A series of lantern slides from photographs by the late Dr. A. E. H. Tutton, F.R.S., was shown. Photographs by delegates were on exhibition in the Reception room throughout the Cambridge Meeting. For a report on the proceedings of the Delegation, see p. xxvi.

A summary of Sectional Transactions on August 18–24 will be found on pp. 381 and following.

On Thursday evening, August 18, a Reception was given by the Vice-Chancellor on behalf of the University of Cambridge, in the Senate House and Old Schools. By kind permission of the Master and Fellows of Gonville and Caius College there was dancing in the hall of that college.

On Tuesday afternoon, August 23, the Mayor and Mayoress of Cambridge (Councillor and Mrs. E. Saville Peck) entertained members at a Sherry Party, held in Emmanuel College by kind permission of the Master and Fellows.

Garden Parties were given at the following Colleges: Downing and Sidney Sussex (August 19), Christ's and Queens' (August 22); and informal evening conversaziones were held at Trinity College (August 19) and St. John's College (August 22).

On Saturday, August 20, general excursions were arranged as follows, with the co-operation of institutions and individuals whose premises were visited:

(1) King's Lynn, Castle Rising, and Sandringham (by gracious permission of H.M. The King).

(2) Hengrave Hall, Bury St. Edmund's, Lavenham, Long Melford.

(3) Tring Museum, London Gliding Club, and works of Stonehenge Bricks, Ltd. An additional visit was arranged to Whipsnade Zoological Park through the courtesy of the Zoological Society of London.

(4) Ely, Sutton, and Earith.

(5) Audley End, Saffron Walden, and Thaxted.

Visits were arranged to colleges and to many other points of interest in Cambridge, and to the works of the British Portland Cement Manufacturers, Ltd., the Cambridge Instrument Co., Ltd., Messrs. Chivers & Sons, Ltd., Messrs. Pye Radios, Ltd., and Messrs. Towgood and Sons and Dufay-Chromax, Ltd. Other excursions and visits devoted to the interests of particular sections are mentioned among the Sectional Transactions in later pages.

The official sermon was preached by the Rt. Rev. the Lord Bishop of Winchester in Great St. Mary's Church on Sunday morning, August 21.

An exhibition of paintings and other objects of art by members of the Association was on view throughout the period of the Meeting.

At the final meeting of the General Committee on Wednesday, August 24, it was resolved:

That the British Association places upon record its deep gratitude for the reception afforded to it by the University, the Borough, and the County of Cambridge. The Association wishes to convey its most cordial thanks to the departments and colleges of the University which have so generously provided accommodation for its meetings and hospitality for its members. Its thanks are due also to the Corporation of the Borough and the County authorities, as well as to the many commercial and industrial institutions in Cambridge and the neighbourhood, for co-operation in the arrangements for the meeting, for generous entertainment, and for the facilities which have been provided for excursions and visits. Finally, the congratulations as well as the gratitude of the Association are offered to the local officers and their efficient helpers, to whose unsparing efforts the brilliant success of the meeting has been due.

REPORT OF THE COUNCIL, 1937–38.

Obituary.

The Council have had to deplore the loss by death of the following office-bearers and supporters :—

Prof. W. A. Bone, F.R.S.	Mr. Hugh Ramage
Dr. G. A. Boulenger, F.R.S.	Dr. A. B. Rendle, F.R.S.[1]
Prof. E. W. Brown, F.R.S.	Lady Robertson
Prof. H. B. Fantham	Prof. the Rt. Hon. Lord Rutherford of Nelson, O.M., F.R.S.[1]
Prof. L. N. G. Filon, F.R.S.	Sir John Snell, G.B.E.
Prof. A. Hutchinson, F.R.S.	Miss Edith Stoney
Prof. A. Lodge	Dr. A. E. H. Tutton, F.R.S.
Prof. Magnus Maclean	Dr. W. W. Vaughan, M.V.O.[1]
Prof. G. H. F. Nuttall, F.R.S.	

The Association was represented at Lord Rutherford's funeral by the President, Sir Edward Poulton, F.R.S. (a pall-bearer), Sir James Jeans, F.R.S., and Prof. A. Ferguson, General Secretary, and at the memorial service in Trinity College Chapel, Cambridge, by Prof. F. T. Brooks, F.R.S., General Secretary, and Prof. F. J. M. Stratton.

Dr. W. T. Calman, F.R.S., represented the Association at the funeral of Dr. A. B. Rendle, F.R.S.

The President, the Rt. Hon. Lord Rayleigh, F.R.S., and the Secretary, Dr. O. J. R. Howarth, on behalf of the General Officers, represented the Association at the memorial service for Dr. W. W. Vaughan, M.V.O.

Representation.

Representatives of the Association have been appointed as follows :—

Meeting held at the House of Lords to discuss the desirability of nature reserves in National Parks (by invitation of the Society for the Promotion of Nature Reserves)	Dr. J. S. Huxley, F.R.S., and Prof. E. J. Salisbury, F.R.S.
Sub-committee of the International Seismological Association, dealing with the Seismological Summary . . .	Dr. H. Jeffreys, F.R.S.
International Congress of Anthropology and Ethnology, Copenhagen . .	Mr. H. J. E. Peake.
International Union of Chemistry, Rome .	Dr. F. W. Aston, F.R.S.

[1] See narrative of the Scientific Delegation in India, annexed to this report.

Resolutions and Recommendations.

Resolutions and recommendations, referred by the General Committee to the Council for consideration, and, if desirable, for action, were dealt with as follows. The resolutions will be found in the *Report* for 1937, p. xlviii.

(*a*) On a resolution from Section A (Mathematical and Physical Sciences), the Council laid before the Corporation of the City of Nottingham a report on the bad condition of the grave of George Green in Sneinton churchyard in that city, and were gratified to learn that the Corporation had undertaken to restore the grave.

(*b*) On a resolution from Section D (Zoology), the Council communicated to the Trustees of the British Museum an expression of their hope that the custody of the late Lord Rothschild's museum at Tring would be undertaken by the Trustees.

(*c*) The Council were informed by the Ministry of Agriculture and Fisheries (i) that the resumption of the publication of one-inch Ordnance Survey maps in the ' relief ' style will be considered when the publication of the present Fifth Edition in the ordinary style is nearing completion ; (ii) that with regard to maps showing physical features only the Minister is prepared to arrange for an edition, showing water and contours only, when the ordinary edition of each forthcoming sheet is printed. (Resolution of Section E, Geography.)

(*d*) The General Secretaries were authorised to consult the Secretary of the Institution of Civil Engineers on the subject of a resolution from Section G (Engineering) on the desirability of improving the co-ordination of arrangements for publishing and indexing new engineering knowledge and the results of engineering research.

(*e*) The Council received unofficial information from a representative of the India Office to the effect that, while it was admitted that a knowledge of Anthropology would be of advantage to civil servants, the present syllabus, as reviewed recently by an authoritative committee set up by the Secretary of State, would not allow of the introduction of an additional compulsory subject. The Council therefore decided not to transmit the resolution of Section H (Anthropology) officially to the India Office.

(*f*) In reply to the resolution of Section L (Educational Science) on adult education, the officers of the Board of Education have undertaken to consider the resolution when reports are presented of a survey, at present in operation, of existing provisions for adult education in England and Wales.

(*g*) A resolution for the Conference of Delegates of Corresponding Societies, supported by Section D (Zoology), on the necessity for an inquiry into methods of dealing with rodents and other wild mammals which affect agriculture, was communicated to the Ministry of Agriculture and Fisheries and to the Department of Agriculture for Scotland. Both departments replied to the effect that experiments on the control of rabbits were already in progress.

(*h*) The Council approved a resolution from the Conference of Delegates of Corresponding Societies on the desirability of establishing through the Corresponding Societies Committee a close liaison with the Association for the Study of Systematics in Relation to General Biology, with a view to the Corresponding Societies undertaking work bearing upon systematic problems.

Finance.

The Council have received reports from the General Treasurer throughout the year. His account has been audited and is presented to the General Committee.

The Council made the following grants from funds under their control:—

From the Caird Fund.

	£
Seismological investigations	100
Mathematical tables	200
Critical geological sections	25 (contingent)
Reduction of noise	10 (contingent)
Perseveration and its testing	10 (contingent)
Kent's Cavern, Torquay	5

From the Bernard Hobson Fund.

Critical geological sections	25
Oölite of Stow-on-the-Wold	25

From the Leicester and Leicestershire Fund.

Archæology of the Fens	25
Transplant experiments	5
Organisation of research in Education	5
Gaps in the informative content of Education	10

From the Norwich Fund.

It was reported that a grant of £40, made last year from the Norwich Fund to the Norfolk Research Committee for the investigation of the post-glacial deposits of East Norfolk, would not be required, with the exception of a sum of £2 13*s*. 9*d*., the payment of which was authorised.

The balance of the fund was granted as follows:—

	£	*s*.	*d*.
(*a*) To Mr. J. E. Sainty, to continue investigations on the Long Barrow at West Rudham, Norfolk	33	2	0
(*b*) To Dr. A. S. Watt, to continue work on rhythmic phenomena of Breckland plants	21	0	0

Corporation Membership.—Messrs. Metropolitan-Vickers Electrical Company, Ltd., and the Educational Institute of Scotland have been admitted to corporation membership of the Association.

President (1939), General Officers, General Committee, and Council.

President (1939).—The Council's nomination to the Presidency of the Association for the year 1939 (Dundee Meeting) is Sir Albert Seward, F.R.S.

The General Officers have been nominated by the Council as follows :—

General Treasurer, Prof. P. G. H. Boswell, F.R.S.

General Secretaries, Prof. F. T. Brooks, F.R.S., Prof. Allan Ferguson.

General Committee.—The following have been admitted as members of the General Committee, mainly on the nomination of Organising Sectional Committees under Regulation 1 :—

Dr. H. B. Cott
Mr. H. R. Hewer
Dr. F. S. J. Hollick
Dr. R. G. S. Hudson
Dr. M. M. Lewis
Mr. J. A. McMillan
Miss A. E. Miller
Mr. F. Rayns, O.B.E.
Mr. A. Rodger
Dr. B. Semeonoff
Mr. W. J. H. Sprott
Dr. W. Stephenson
Dr. E. C. Stoner, F.R.S.
Mr. S. H. Straw
Mr. F. C. Thomas

Council.—The retiring Ordinary Members of the Council are : Prof. R. N. Rudmose Brown, Mr. H. M. Hallsworth, C.B.E., Prof. G. W. O. Howe, and Prof. F. E. Weiss, F.R.S., and a further vacancy is created by the death of Dr. W. W. Vaughan, M.V.O.

The Council have nominated as new members Mr. R. W. Allen, C.B.E., Prof. F. E. Fritsch, F.R.S., and Sir Richard Gregory, Bt., F.R.S. ; leaving two vacancies to be filled by the General Committee without nomination by the Council.

The full list of Ordinary Members nominated is as follows :—

R. W. Allen, C.B.E.
Dr. F. W. Aston, F.R.S.
Prof. F. Aveling
Prof. F. Balfour-Browne
Sir T. Hudson Beare
Rt. Hon. Viscount Bledisloe, P.C., G.C.M.G., G.B.E.
Dr. W. T. Calman, C.B., F.R.S.
Prof. F. Debenham, O.B.E.
Prof. W. G. Fearnsides, F.R.S.
Prof. H. J. Fleure, F.R.S.
Prof. F. E. Fritsch, F.R.S.
Sir Richard Gregory, Bt., F.R.S.
Prof. A. V. Hill, O.B.E., Sec.R.S.
Prof. T. G. Hill
Prof. T. S. Moore
Prof. J. C. Philip, O.B.E., F.R.S.
Prof. J. G. Smith
Lt.-Col. W. Campbell Smith
Dr. C. Tierney
Dr. J. A. Venn
Prof. Sir Gilbert Walker, C.S.I., F.R.S.
R. S. Whipple
J. S. Wilson

Future Meetings.

Dundee has been already determined by the General Committee as the place of meeting in 1939. The dates now proposed for the Dundee Meeting are August 30 to September 6.

There have been received invitations for the Association to meet in

Newcastle-upon-Tyne in 1940, in Belfast in 1941 or any year nearly following, in 1942 in Birmingham. As previously reported, there is also an invitation to meet in Swansea in any convenient year. In view of informal discussion as to the possibility of an invitation from Australia for the year 1942, the General Committee should be made aware that such an invitation will not be forthcoming.

Miscellanea.

Scientific Delegation in India.—A narrative report of the activities of the Scientific Delegation in India is annexed to this Report of the Council.

Proposed Overseas Delegation Fund.—The General Committee last year granted a sum not exceeding £1,000 from the general funds of the Association towards the expenses of the Scientific Delegation in India. In the event, it was necessary to use only £217 of this sum. The Council, recognising the great success of the Indian visit, and believing that similar opportunities may arise to send delegations elsewhere, and that, if arising, advantage should by all means be taken of them, now recommend to the General Committee that the unexpended balance of the above grant should be held as the nucleus of a fund from which to assist expenses of such delegations.

British Science Guild Lectures.—Prof. H. L. Hawkins was appointed to deliver the Alexander Pedler Lecture for 1939 at the Worthing Congress of the South-Eastern Union of Scientific Societies, and did so on June 24.

Dr. H. Spencer Jones, F.R.S., was appointed to deliver the Norman Lockyer Lecture on December 6.

British Science Guild : South Australian Handbooks.—Following upon the incorporation of the Guild into the Association, the important work of the Handbooks Committee of the South Australian branch of the Guild was brought to the notice of the Council, and it was resolved that, while no financial aid could be offered to assist in the production of forthcoming books, an expression of the Council's ' appreciation of the great value of the handbooks of the flora and fauna of South Australia ' should be recorded and conveyed to the Committee.

Geology in Schools.—During the past year the Association's two reports on the teaching of geology in schools have been distributed to appropriate educational authorities, together with an expression of the Council's hope that careful consideration would be given to the question of introducing geology into the school curriculum, either by inclusion in a course of general elementary science or as a separate subject.

Discussion on Planning the Land of Britain.—Reprints of this discussion, which took place at the Nottingham Meeting last year and appears in the Report of that Meeting, have been widely circulated to planning authorities and organisations interested in this subject.

Scientific Advisory Committee of the Trades Union Congress.—The Trades Union Congress asked for the co-operation of the Association in proposing names of scientific workers who might be invited by the Congress to serve on a Scientific Advisory Committee. The General

Officers were authorised to advise the representatives of the Congress informally in this matter.

Reports on a Division for Social and International Relations of Science, and a Publication.—The Council have approved in principle, and recommend, the establishment of a Division of the Association to deal with the social and international relations of science. A Committee was appointed to formulate a scheme for the working of this Division. The same Committee was instructed to consider and report upon present methods of publication by the Association, and to suggest alternative methods if thought desirable. The reports of this Committee are appended hereto.

Down House.

The following report for the year 1937–38 has been received from the Down House Committee :—

The number of visitors to Down House during the year ending June 6, 1938, has been 7,185, compared with 6,148 in 1936–37.

A number of valued gifts have been added to the collection during the past year. Sir Buckston Browne acquired and presented a portrait of Darwin in oils, by E. Pailthorp, apparently made from a photograph already in the collection, as Darwin is believed to have sat only to Ouless and Collier. Sir Buckston Browne also collected photographs of members of Darwin's family, which have been framed together. He received from Mr. Sidney Spokes, M.R.C.S., a copy of the second edition of Lyell's *Elements of Geology*, on a flyleaf of which there appears in Lyell's hand the note : ' Darwin recommends a short chap. on metallic veins, giving the present state of our knowledge. He denies seeing a beginning to each crop of species. Jan. 26, 1842.' With this volume is now shown one of Lyell's geological hammers, presented by Miss D. Pertz. Darwin's aneroid barometer now hangs again in the old study, through the generosity of Miss Hooker. Prof. F. W. Oliver, F.R.S., has presented an important series of letters, which his father, Prof. Daniel Oliver, F.R.S., had from Darwin. Mr. T. M. Ragg gave a reproduction of a portrait of Fitzroy, commander of H.M.S. *Beagle*. Sir Josiah Stamp presented a reproduction of the armorial bearings of the Association in stained glass. The statuette of Darwin, mentioned in last year's report as by an unknown artist, has been recognised as a studio model by Horace Montford : no statue appears to have been executed from it.

A new edition of the Catalogue has been prepared and will be brought into circulation shortly.

Rainfall is now read regularly from the standard gauge. The total precipitation last year (1937) was 39·12 in., but as the standard gauge was not in use in the first half of the year, no return was made to the Meteorological Office. By way of contrast, it may be mentioned that the rainfall in January to March, 1937, was over 15 in. ; in January to March, 1938, it was 5 in., of which 3·54 in. fell in January, 0·895 in February, and 0·565 in March.

The Committee were glad to hear of the visit of a party to the House on May 27 in connection with the celebration of the 150th anniversary of the foundation of the Linnean Society.

The following financial statement shows income and expenditure on account of Down House for the years ending March 31, 1937 and 1938 :—

Income	£	s.	d.	*Corresponding figures*, 1936–37 £	s.	d.
By Rents receivable	139	5	0	*141*	*0*	*0*
,, Income Tax recovered	177	8	0	*168*	*1*	*6*
,, Interest and Dividends	807	15	0	*817*	*2*	*0*
,, Donations	3	8	9	*3*	*17*	*1*
,, Sale of Catalogues, Postcards and Photographs	26	8	4	*23*	*8*	*4*
,, Pilgrim Trust Grant	150	0	0	*150*	*0*	*0*
,, Instalment of Grant from Herbert Spencer Bequest	366	9	0	*132*	*3*	*4*
,, Balance, being excess of expenditure over income for the year, transferred to Suspense Account	50	1	2	—		
	£1,720	15	3	*£1,435*	*12*	*3*

Expenditure	£	s.	d.	*Corresponding figures*, 1936–37 £	s.	d.
To Wages of Staff	822	8	10	*803*	*19*	*7*
,, Rates, Insurance, etc.	71	9	3	*69*	*5*	*6*
,, Heating, etc.	175	0	7	*138*	*14*	*8*
,, Lighting and Drainage (including oil and petrol)	91	13	7	*79*	*4*	*11*
,, Water	14	10	2	*15*	*7*	*1*
,, Repairs and Renewals	428	3	1	*159*	*2*	*4*
,, Garden and Land: Materials and Maintenance	61	8	5	*45*	*19*	*8*
,, Donations to Village Institutions	5	5	0	*5*	*5*	*0*
,, Household Requisites, etc.	12	5	11	*15*	*18*	*10*
,, Transport and Carriage	3	14	10	*1*	*16*	*6*
,, Printing, Postages, Telephone and Stationery	34	15	7	*35*	*7*	*5*
,, Sundries (non-recurrent)	—			*9*	*15*	*8*
,, Balance, being excess of income over expenditure for the year, transferred to Suspense Account	—			*55*	*15*	*1*
	£1,720	15	3	*£1,435*	*12*	*3*

Thanks to the grant of £500 by the Council from the Spencer Bequest, it has been possible to carry out important repairs and renovations during the past two years without drawing upon the general funds. This sum has now been expended.

As the Council are already aware, the Pilgrim Trustees have made a final grant of £150, payable as to £100 and £50 in the two ensuing financial years respectively.

The Council desire to commend, and to bring to the notice of Members of the General Committee and others, a proposal which has received the approval of the Down House Committee that steps should be taken to form a collection of biographies of Darwin and of contemporary literature bearing upon his work, for addition to the library now at Down House.

PROPOSAL FOR THE ESTABLISHMENT OF A DIVISION TO DEAL WITH THE SOCIAL AND INTERNATIONAL RELATIONS OF SCIENCE.

The following report, and proposals contained therein, were adopted by the General Committee at its Meeting on August 17, 1938.

At the present time a strong feeling exists that the social relations of science demand close and objective study. The question has been dealt with recently in the press and elsewhere. At an informal meeting of persons specially interested, it was stated that there is nothing in the constitution of the British Association to prevent the establishment of machinery within that organisation for the purpose desired. A resolution was thereupon addressed from this meeting to the Council of the Association, inviting the Association to establish a special department which would consider the social and international relations of science, by means of enquiry, publication, and the holding of meetings not necessarily confined to the annual meetings of the Association.

International relations were specified in this resolution primarily because of the deep interest of the American Association for the Advancement of Science in the subject. Discussion is expected to take place between officers of the two Associations, during the present summer, on the best means for international co-operation.

The Council supported the proposal to establish an organisation for these purposes within the Association. They appointed a Committee to formulate a scheme for the working of such an organisation, to be presented to the General Committee at the Cambridge Meeting. It is thought that the organisation should work on lines in some respects different from those of a Section, and should not bear that title. The term DIVISION is therefore recommended.

The purpose of the Division would be to further the objective study of the social relations of science. The problems with which it would deal would be concerned with the effects of advances in science on the well-being of the community, and, reciprocally, the effects of social conditions upon advances in science.

The Division would be worked by a Committee, nominated annually by the Council and appointed by the General Committee. The Council should have power to appoint additional members of the Committee during the year.

The Committee should embody the existing British Science Guild Committee of the Association, inasmuch as the Norman Lockyer, Alexander Pedler, and Radford Mather Lectures, now administered by that Committee, would appropriately come within the purview of the Division.

The President of the Association and the General Officers should be *ex-officio* members of the Committee. A chairman of the Committee should be appointed for a fixed period of office. A fixed proportion of the ordinary members of the Committee should retire annually (as in the case of the Council) and should not be eligible for immediate re-election.

The functions of the Committee would be:

(*a*) To arrange meetings of the Division both at the time and place of the Annual Meetings of the Association, and elsewhere at other times, as invited or otherwise arranged; to appoint speakers, and to accept or reject communications offered to the Division.

(*b*) To furnish material for the information of the public.

(*c*) To co-ordinate work dealing with the social relations of science, both at home and abroad.

(*d*) To be prepared to act in a consultative capacity and to supply information, and to that end to establish relations with organisations and persons engaged in practical administration.

(For the furtherance of the above objects, the Committee, immediately upon the establishment of the Division, should issue an announcement thereof, together with a reasoned statement of its aims, to institutions and other organisations and individuals known or likely to be interested in its work.)

(*e*) To set up sub-committees for executive purposes, or for research, enquiry, or co-ordination. If any such sub-committee should require a grant of money for its work, the Committee should be empowered to apply for such grant to the General Committee or the Council in accordance with the usual procedure relating to research committees.

(*f*) To maintain close relations with the Sections of the Association and their Organising Committees. In particular, there may be imagined subjects which two or more Sections might be disposed to recommend to the Division for discussion, in lieu of arranging joint meetings of the Sections. The Committee of the Division, on its part, should be enabled to invite the advice of the sectional organisations on all appropriate questions. The Organising Sectional Committees should be kept regularly informed of the activities of the Division.

The Committee should meet regularly throughout the year, at intervals determined by itself, and in particular it should hold a meeting at or near the time of the joint meetings of Organising Sectional Committees in January, in order to assure the relations with the Sections referred to above.

The Committee should report to the Council as and when necessary, and annually through the Council to the General Committee.

PROPOSAL FOR A QUARTERLY REPORT

IN PLACE OF THE ANNUAL VOLUME PUBLISHED BY THE ASSOCIATION.

The following report was adopted by the General Committee at its Meeting on August 24, 1938, *excepting the portion enclosed in* [*brackets*] *and dealing with the Journal, which was amended so as to admit of the retention of abstracts, more strictly limited as to length, for use at the Annual Meeting and subsequently for record if necessary.*

In November 1937 the Council directed the General Officers to consider and report upon the format and printing of the Report of the Association. Subsequently, the Committee which was appointed to formulate a scheme for the new Division referred to above was instructed also to consider and report upon the whole question of publication by the Association.

The Committee, after considering various schemes in detail, recommend that as from the year 1939–40 the Annual Volume should be superseded by a Quarterly Report. The annual volume following the Cambridge Meeting would thus be the last of its series.

The principal considerations which have led the Committee to make this recommendation are as follows :

Quarterly publication should go far to overcome the widespread belief that the British Association is inactive except during its annual meeting. The fact that it now administers the Norman Lockyer, Alexander Pedler, and Radford Mather lectures (which are given at times and places other than those of the annual meetings) points to the desirability of publication at less than annual intervals ; and the establishment of the new Division on the lines recommended would strongly reinforce this argument.

Quarterly publication would provide the means of keeping members and the public informed as to the activities of the Association, as an annual volume cannot. Quarterly publication should achieve a wider circulation than the annual volume does for individual communications which call for a wider publicity than they receive by inclusion in an annual volume.

It is recommended that the Quarterly should appear in October, January, April and July. The size proposed is royal octavo (approximately $10 \times 6\frac{1}{2}$ in.). It is suggested that the title *The Advancement of Science* should be transferred to the Quarterly from the present publication which bears that name and contains the presidential addresses given at the annual meeting. In substitution for the publication of all these addresses together, it is proposed to issue individual addresses separately, at the time of the meeting.

The bulk of the material made available from the annual meeting would appear in the October and January numbers. There should, however, be the fullest possible measure of elasticity. This consideration might be expected to apply especially to the reports of research committees, for which delayed publication is sometimes found desirable ; or on the other hand publication in advance of the meeting at which a particular research is to be discussed might be allowed at the discretion of the appropriate Organising Sectional Committee.

[It is considered that the Journal of Sectional Transactions, as at present issued at the annual meeting and subsequently incorporated in the Annual Report, is of little value as a permanent record. It is proposed that the present Programme and Timetable should include the programme of each Section separately (as the Journal does now), with abstracts of the briefest possible nature, or none where titles of communications would suffice alone. The transactions of the Sections should be reported in the Quarterly in narrative form, and] so far as finance would allow there should be additional opportunity for publication *in extenso* or full abstract, and for the reporting of discussions.

No changes in the terms of membership subscription are recommended ; life members and annual members now entitled to receive the

Annual Report would receive the Quarterly. The price of 3*s.* 6*d.* per part is recommended for non-subscribers.

The Quarterly should be marketed by arrangement with a publishing firm.

The division into quarterly parts would in itself cost little more than the annual volume, even allowing for improvement of the format. Additional matter for publication, however, would be expected from the new Division and from more effective reporting of the work of the Sections. The establishment of the new Division would increase clerical work in the office. On these considerations it has been estimated that the proposals here made might involve the Association in an additional annual expenditure of £400–£500 in a few years' time; and in this event a temporary draft upon capital would be necessary.

It is hoped, however, that such additional expenditure would be offset by increased sales of the Quarterly and reports of Presidential Addresses, as against those of the Annual Volume and the present *Advancement of Science*, and also by receipts from advertisements in the Quarterly. Moreover, the establishment of the new Division and the publication of a Quarterly are both measures which should help to increase the membership of the Association.

THE SCIENTIFIC DELEGATION IN INDIA, 1937–38.

At the Norwich Meeting of the British Association in 1935, the General Committee of the British Association received Professor J. N. Mukherjee, one of the General Secretaries of the Indian Science Congress Association, who announced that that body would celebrate its jubilee at a meeting in Calcutta in the winter of 1937–38. The Indian Association was founded in 1912–13, and the first meeting took place at Calcutta in the following year. Professor Mukherjee, with Professor S. P. Agharkar, had been appointed to negotiate with the British Association for the purpose of securing the organisation of a representative scientific delegation to participate in the jubilee meeting. The proposal was new in the sense that the British Association had never before received a definite invitation to co-operate in this manner with a kindred association overseas—that is to say apart from, and in addition to, its own overseas meetings. The General Committee recognised the far-reaching importance of the proposal, and directed the Council to carry on negotiations with the Indian Science Congress Association. This was done, and the Council were able to report in the following year that the formal invitation of the Indian Association had been received and accepted. The Calcutta Congress was appointed to be held from January 3 to 9, 1938.

The Indian Association appointed Professor the Rt. Hon. Lord Rutherford, O.M., F.R.S., to be its President for the jubilee year. He died on October 19, 1937; he had intended to leave England for India on November 26. His loss, deplored by the whole scientific world, was very specially grievous to the delegation and to the Congress. The Indian Association, through the British Association, invited Sir James

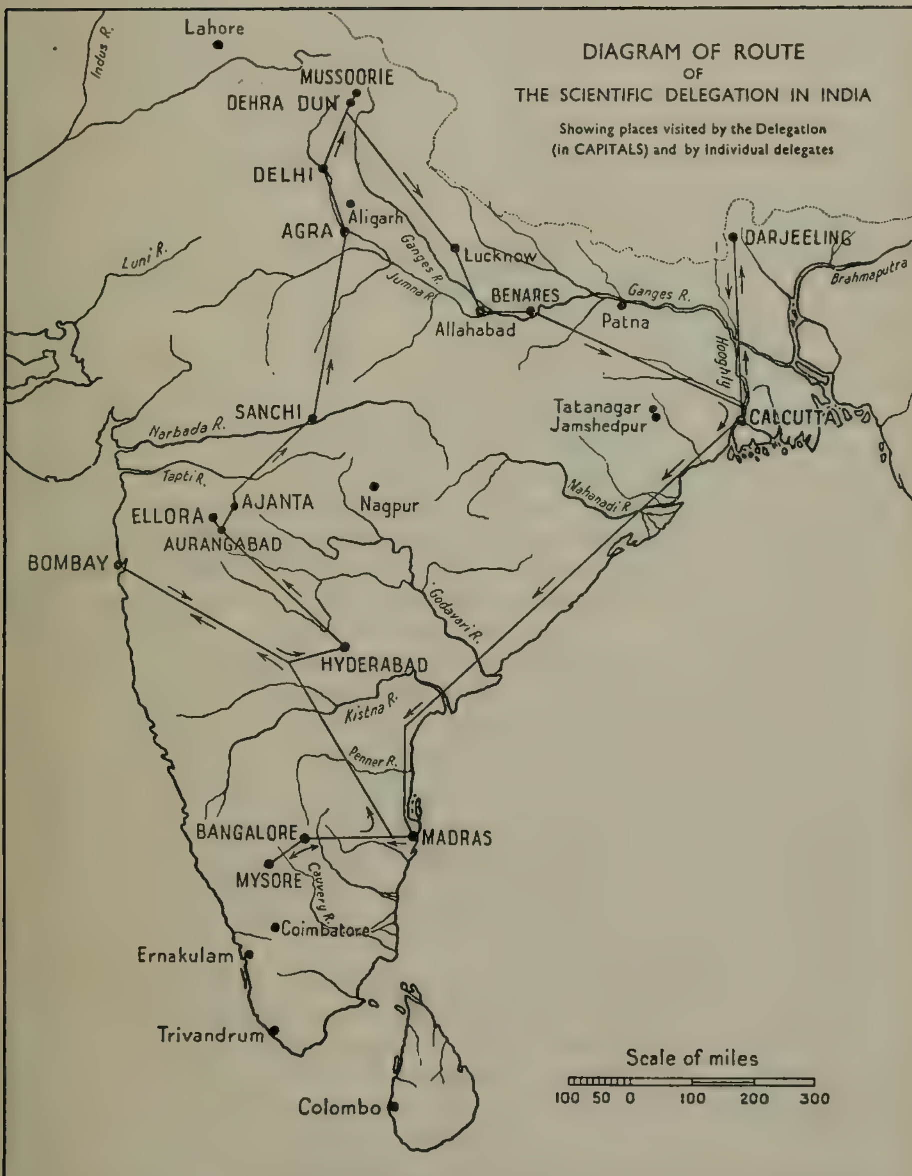


DIAGRAM OF ROUTE
OF
THE SCIENTIFIC DELEGATION IN INDIA
Showing places visited by the Delegation
(in CAPITALS) and by individual delegates
Lahore
Indus R.
MUSSOORIE
DEHRA DUN
DELHI
Aligarh
AGRA
Luni R.
Ganges R.
Jumna R.
Lucknow
BENARES
Allahabad
Patna
Ganges R.
DARJEELING
Brahmaputra
Hooghly
CALCUTTA
Tatanagar
Jamshedpur
SANCHI
Narbada R.
Tapti R.
AJANTA
ELLORA
AURANGABAD
Nagpur
Mahanadi R.
BOMBAY
Godavari R.
HYDERABAD
Kistna R.
Penner R.
BANGALORE
MADRAS
MYSORE
Cauvery R.
Coimbatore
Ernakulam
Trivandrum
Colombo
Scale of miles
100 50 0 100 200 300

Jeans, F.R.S., to take Lord Rutherford's place in the chair, and fortunately for both bodies he was able to do so at short notice.

The Indian Association presented to the British Association lists of scientific representatives whose presence was specially desired. The Council of the British Association appointed a Committee to supervise invitations and arrangements generally, and under the direction of that Committee the General Secretaries issued invitations to persons named as above by the Indian Association, to members of the British Association who had occupied sectional chairs or other high offices, and to certain others whose attendance was desirable in order to assure proper representation of departments of Science specially appropriate to India. The Indian Association itself issued direct a limited number of invitations, principally to representatives from European countries. The number of invited delegates who accepted invitations was 65, and with the addition of relatives of some of these and certain other members the total number of the visiting party was 101.

The Indian Science Congress Association placed at the disposal of the British Association a sum of £3,125, including a grant from the Government of India and contributions from other sources. The British Association collected from institutions, firms, and individuals in Great Britain a sum of £1,356 11*s.*, and made a contribution from its own funds. Grants in aid of travelling expenses were made to invited delegates (with some few exceptions), amounting in total to £4,590. Particulars of the Delegation Account are included in the General Treasurer's Report for 1937–38. The President travelled as the guest of the Indian Science Congress Association. The same Association appointed at its own charge a tour manager for the official journey of the visiting party in India.

Of the party of 101 members, all except eleven either left England by the P. & O. Company's steamer *Cathay* on November 26, 1937, or joined her at Marseilles after leaving England at the beginning of December and travelling overland. They reached Bombay early on December 17. Here one of the delegates, Dr. A. B. Rendle, F.R.S., who had been in poor health, was advised not to continue the journey. He remained in hospital at Bombay for a short time, and then returned to England, but died shortly after reaching home (Jan. 12), to the deep regret of his colleagues in the delegation.

On landing at Bombay the party was received on the lawn adjacent to Ballard Pier by Mr. V. N. Chandavarkar, Vice-Chancellor of the University, Rao Bahadur T. S. Venkataraman, retiring President of the Indian Science Congress Association, Professor J. N. Mukherjee and other representatives. The local reception committee entertained the party to dinner on December 17 at the Willingdon Club, and to luncheon on December 18 at the Taj Mahal Hotel. Opportunities were afforded for visiting departments of the University, St. Xavier's College, the Royal Insititute of Science, the Grant Medical College, the Haffkine Institute, and other institutions, and also for seeing something of the many points of interest in the city and its neighbourhood. Lectures or short addresses were given by Sir James Jeans, F.R.S., Prof. F. A. E. Crew (two), Dr. F. W. Aston, F.R.S., Dr. C. S. Myers, C.B.E., F.R.S. (two), Dr. C. G. Darwin, F.R.S., Prof. R. A. Fisher, F.R.S., Mr. H. J. E. Peake, and Prof. H. J.

Fleure, F.R.S.; and Mr. H. M. Hallsworth, C.B.E., had a discussion with advanced students in the Department of Economics in the University. Prof. Winifred Cullis, C.B.E., addressed Bombay University Women at the Cama Hospital. It may be stated here, and taken as applying at all points throughout India where general or public lectures were given by delegates, that the numbers and enthusiasm of the audiences were such as to gratify and even astonish the visitors. Broadcasts were given from the Bombay station of All-India Radio by Prof. Winifred Cullis, C.B.E., and Prof. F. A. E. Crew.

The party left Bombay in the afternoon of December 18, in the special train which was to be their headquarters during the tour through northern India until January 2, and again for those of them who joined the southern tour after the Congress in Calcutta. The train consisted of the Punjab Limited rolling-stock of the Great Indian Peninsula Railway, and for the outward tour included seven corridor coaches with compartments affording very comfortable living and sleeping accommodation for two persons each, two dining cars, a brake, a servants' car, and a commissariat car. The travel arrangements were made by the Indian Science Congress Association in collaboration with the railway companies concerned and with Messrs. Thos. Cook & Son as agents. Mr. W. D. West, one of the General Secretaries of the Indian Association, had principally dealt with the details of organisation of the tours in advance, and Prof. J. N. Mukherjee, the other General Secretary, accompanied the tour preceding the Congress, and dealt with all the arrangements therefor excepting those at Agra and Dehra Dun and those of the geologists' visit to Dhanbad, etc.

In the morning of December 19 the party reached Hyderabad, the capital of the Deccan State of that name, and during their sojourn within its frontiers they were guests of the State in respect not only of entertainment, but also of accommodation and travel. They visited the site of the Osmania University, which was established in 1918, and were shown the buildings and departments already erected and in operation. The medical college and Osmania hospital, the museum, the Nizamiah Observatory, and the Cottage Industries Institute were seen by individual members. Sir James Jeans, F.R.S., addressed the University staff and students, and various members of the party were enabled to meet professors and students in the departments in which they were specially interested, and to discuss their work. The whole party was entertained to lunch in the University hostel. Afterwards Golconda, an immense hill-fort and the capital of the Kutb Shahi Kingdom of the sixteenth and seventeenth centuries, was visited, and also tombs of the kings of this dynasty. Sir Arthur Eddington, F.R.S., gave a lecture in the Town Hall, and a banquet was held in the Address Hall of the University. The party left at night for Aurangabad in a narrow-gauge train provided by the State, and next day (December 20) visited the rock-hewn temples at Ellora, which range in dates from the third to the ninth century A.D., and in which the Buddhist, Brahmanic, and Jain religions are represented. The hill-fortress of Daulatabad, founded probably in the twelfth century, and other historic sites were also seen. On December 21 the party was taken by road to Ajanta, the site of another great series of rock-temples, where the architecture, sculpture, and painting 'represent every stage of Buddhist art from the first century B.C. to the middle of the seventh century A.D.' [1] From Ajanta the party proceeded by road to Jalgaon, where the broad-gauge train was rejoined at night.

In the morning of December 22 a halt was made at Sanchi, in Bhopal State, where the Buddhist stupas and other remains, dating from the third century B.C. to the twelfth century A.D., were visited. In the evening the train arrived at Agra. Some of the delegates visited the Taj Mahal the same night, and it was here that Dr. W. W. Vaughan met with the lamentable accident which resulted in his death. In the darkness he fell from a terrace which is unprotected by any parapet. One of his legs was broken, and after a long illness he died in the Thomason Hospital at Agra on February 4, 1938, to the keen distress of all his colleagues in the delegation.

On December 23 the Fort and the Taj Mahal, superb monuments of the Mogul Emperors Akbar, Shah Jahan, and Aurangzeb (1556–1707), were visited, and some of the party were able to see the fort of Fatehpur Sikri and also the Latitude Variation Observatory of the Survey of India, and the Upper Air Observatory of the Meteorological Department. Sir James Jeans, F.R.S., gave an address to students at the University. The main party left Agra in the evening of December 23, and arrived at Delhi in the morning of December 24. Some members, however, diverged in order to visit Aligarh, where, at the University, short addresses were given by Prof. Ernest Barker, Sir Arthur Eddington, F.R.S., Prof. W. T. Gordon, Dr. W. G. Ogg, and Dr. Dudley Stamp.

At Delhi on Christmas Eve and Christmas Day the great modern group of Government buildings—the Viceroy's House, the Secretariat, and the Council House—and the new Imperial Institute of Agricultural Research were visited, as well as many historical monuments, such as the fort, the palace, and Juma Masjid (mosque) of Shah Jahan (*c.* 1640), the ruined old fort of the fifteenth century, the mosque of Sher Shah, and the twelfth-century Tower of Victory known as the Kutb Minar, with its adjacent Jain and Hindu temples and mosque and the famous iron pillar to which is assigned an age of fifteen centuries or more. The Government of India entertained the party to luncheon on Christmas Eve, and on Christmas Day most generous entertainment was extended to individual members by many residents, Indian and British, in New Delhi. Broadcasts were given on Christmas Eve by Sir James Jeans, F.R.S., and Dr. O. J. R. Howarth.

Leaving Delhi on Christmas night, the party reached Dehra Dun in the morning of December 26. Here members visited the Forest Research Institute and the Geodetic Branch of the Survey of India. The Forestry Research Institute, established in 1906, occupies an estate of 1,400 acres, and its fine buildings, besides administrative and residential quarters, include a chemical branch, insectary, saw mill, pulp and paper plant, wood workshops, and timber testing, seasoning and preservation laboratories, while there are also an arboretum and botanical and experimental

[1] This quotation, and much of the information throughout this report, are taken with grateful acknowledgment from the guide-book specially prepared for the delegation by the Indian Science Congress Association.

gardens. The work of the Geodetic Branch includes, among other activities, precise levelling for the determination of heights, tidal predictions and the publication of tide tables for ports between Suez and Singapore, the magnetic survey, astronomical observations for the determination of latitude, longitude, and time, seismographical and meteorological observations, and topographical survey and map reproductions. Most of the party found time to drive up to Mussoorie (6,500 feet), from which the view of Himalayan snow-mountains is restricted, but that over the foothills and the plains to the south is of impressive extent.

The party left Dehra Dun late on December 26, and reached Benares in the afternoon of December 27. Sir Arthur Eddington, F.R.S., visited Allahabad, and presided over a colloquium on astrophysics. On arrival at Benares the party was conveyed to Sarnath, where, about five centuries before Christ, Buddha first preached after his enlightenment, and where Asoka set up the great Dhamekh stupa in the third century B.C., and a column of which broken remains are seen on the ground, while the richly sculptured capital is in the adjacent museum. On December 28 the party viewed from boats the famous river-frontage of Benares with its temples, ghats, and steps. Afterwards members were entertained in the Benares Hindu University, and attended its twentieth Convocation, at which, among others, the following delegates received honorary degrees: Sir James Jeans, F.R.S., Sir Arthur Eddington, F.R.S., Dr. F. W. Aston, F.R.S., Prof. E. C. C. Baly, C.B.E., F.R.S., Prof. V. H. Blackman, F.R.S., Prof. C. G. Jung, and Prof. F. A. E. Crew. Sir James Jeans addressed the Convocation, and lectures or short addresses to students were subsequently given by Dr. F. W. Aston, F.R.S., Prof. E. C. C. Baly, C.B.E., F.R.S., Prof. Ernest Barker, Prof. V. H. Blackman, F.R.S., Prof. F. A. E. Crew, Sir Arthur Eddington, F.R.S., and Prof. C. G. Jung.

From Benares, which was quitted on the night of December 28, the special train proceeded to Calcutta, which was reached in the afternoon of December 29. It crossed the great Chinsurah bridge over the Hooghly above the city, in order to enter Sealdah station, where it remained for little more than an hour, and then proceeded through the night to Siliguri, taking the great majority of the members for a visit to Darjeeling.

It will be apparent from the preceding narrative that much of the railway-travelling was done at night, but sufficient took place in daylight to afford, together with the long road-journeys in Hyderabad and shorter drives elsewhere, at least a cursory view of the main geographical regions of central and northern India which were traversed. After the departure from Bombay in the late afternoon, there remained just sufficient light to reveal the transition from the flat lowland of the Konkan country to the flat-topped hills of the Western Ghats with their isolated pinnacles and bold escarpments of basaltic lavas, deeply eroded. The plateau of peninsular India, wherever it was traversed, whether in Hyderabad or during the tour after the Congress, farther south, was seen in dry conditions; occasionally even a semi-desert type of vegetation was apparent. If the scenery of the plateau left a general sense of monotony, it was at any rate possible to distinguish some of its different physical characteristics. The vast tracts of red laterite soil gave a peculiar impression of aridity, by contrast, especially, with the alternating areas of black cotton soil. Again, during the traverse of Hyderabad State it was possible to observe the distinctions of form between the volcanic region of the Deccan trap and the undulating plains and rounded hills of the Archaean crystalline rocks with their irregularly weathered tors of granite boulders. One of the escarpments of the trap country was finely seen on the descent to the gorge in which the caves of Ajanta are excavated, and here, as well as at Ellora and in the moat and scarp of Daulatabad fort, the manner in which the basalt on the one hand had lent itself to artificial working, and on the other its resistance to the influences of weathering, was wonderful to see. The area of Pre-Cambrian sandstones which 'have furnished a great wealth of building stone to the architects of ancient India and stimulated their art' were crossed in the vicinity of Sanchi, and the rough and rather barren quartzites and metamorphic rocks of the Delhi system offered a further contrast both to the Sanchi country and to the rich alluvial plains of the Gangetic rivers, which were traversed northward towards Dehra Dun and eastward to Calcutta. 'The plains rise in gentle undulations away from the river banks, and for miles there is an unbroken succession of fields, orchards, and mango groves, surrounding clusters of mud villages.' The scenery thus described was uninterrupted during December 29, save where the Rajmahal hills in Bihar rise as outliers of the Chota Nagpur plateaux to the south. This last district was visited by a small geological party, which left the special train at Kodarma on December 29, and proceeding by way of Ranchi, Gua, Jamshedpur, and Dhanbad, arrived in Calcutta early on January 3.

At two points the route of the special train traversed the rich submontane tracts bordering the plains on the north-east, and afforded views of the impressive approaches to the wall-like foothills of the Himalayan mountain-system. The first of these occasions was at Dehra Dun as already indicated; the second at Siliguri on December 30. Here the railway was left for the ascent by road to Darjeeling, where, at a height of some 7,000 feet above sea-level, the party had the extreme good fortune to enjoy two-and-a-half days (December 30–January 1) of perfect weather, in unclouded view of the Himalayan range which culminates in Kangchenjunga (28,146 feet). The party returned to Calcutta in the morning of January 2.

From Calcutta the President, Sir James Jeans, F.R.S., conveyed thanks on behalf of the party to the following gentlemen who had been instrumental in arranging for the hospitality and facilities afforded at the various places visited:—

Bombay: Rao Bahadur V. N. Chandavàrkar, Vice-Chancellor of the University.

Hyderabad: The Rt. Hon. Sir Akbar Hydari, President of the Executive Council and Chancellor of the Osmania University; the Hon. Nawab Mehdi Yar Jung, political and education member and Vice-Chancellor of the University; Prof. Kasi Mohamed Husain, Pro-Vice-Chancellor of the University.

Agra: Mr. Zafar Hasan, Superintendent, Archæological Survey of India, Northern Circle; Mr. G. Chatterjee, Meteorological Office; Prof. K. C. Mehta, Department of Botany, Agra University.

Delhi : Sir Girja Sunkar Bagpai ; the Hon. Sir Shah Sulaiman ; Mr. Lala Sri Ram.

Dehra Dun : Mr. L. Mason, C.I.E., Inspector-General of Forests ; Col. C. M. Thompson, Director of the Geodetic Branch, Survey of India.

Benares : Pundit M. M. Malaviya, Vice-Chancellor of the Benares Hindu University ; Raja Juala Prasad, Pro-Vice-Chancellor of the University.

The Silver Jubilee Session of the Indian Science Congress Association was opened by H.E. the Viceroy of India (the Marquess of Linlithgow), in the University College of Science, Calcutta, on January 3, 1938. Sir James Jeans, F.R.S., after his own short prefatory address, communicated to the Congress the presidential address which had been prepared by Lord Rutherford. The reception room, offices, and section meeting rooms of the Congress were in the Presidency College, the University Buildings, the All-India Institute of Hygiene and Public Health, and the School of Tropical Medicine. The transactions of the Congress were continued daily until January 9, with the exception of January 6, a day devoted to excursions. The transactions are fully reported by the Indian Science Congress Association, but it may be mentioned here that occasion was taken during the week to hold also the annual meetings of the National Institute of Sciences of India, the Indian Chemical Society, the Indian Physical Society, the Indian Section of the Institute of Chemistry of Great Britain and Ireland, the Indian Botanical Society, the Society of Biological Chemists of India, the Indian Psychological Association, the Indian Society of Soil Science, the Physiological Society of India, and the Indian Anthropological Institute.

Diplomas of honorary Silver Jubilee membership were presented to Sir James Jeans, F.R.S., Dr. F. W. Aston, F.R.S., Prof. L. F. de Beaufort, Prof. A. H. R. Buller, F.R.S., Prof. Sir Arthur Eddington, F.R.S., Sir Frederick Hobday, C.M.G., Prof. C. G. Jung, and Prof. J. L. Simonsen, F.R.S., of the delegation, and also to Sir Venkata Raman, F.R.S., Sir Prafulla Ray, Prof. M. N. Saha, F.R.S., and Sir M. Visbesbaraya.

Public lectures were given by Sir James Jeans, F.R.S., and other members of the delegation, including Dr. F. W. Aston, F.R.S., Prof. Ernest Barker (two), Prof. F. A. E. Crew, Dr. C. G. Darwin, F.R.S., Prof. Sir Arthur Eddington, F.R.S., Prof. H. J. Fleure, F.R.S., and Dr. J. A. Venn. Among other lectures given by the delegates to various bodies in Calcutta were the following. The Indian Association for the Cultivation of Science conferred upon Sir James Jeans, F.R.S., and Dr. F. W. Aston, F.R.S., the Joy Kissen Mookerjee Medal for 1937 and 1938 respectively, and each delivered an address to this Association on the occasion of the award of the medals. The same Association heard three lectures by Prof. J. E. Lennard-Jones, F.R.S., as Coochbehar Professor, and three by Sir Arthur Hill, K.C.M.G., F.R.S., as Ripon Professor ; while Dr. A. E. H. Tutton, F.R.S., devoted much time to discussion in the laboratory of the Association. Sir Henry Tizard, K.C.B., F.R.S., and Prof. J. L. Simonsen, F.R.S., addressed the Institute of Chemists ; Sir Arthur Eddington, F.R.S., the Indian Physical Society and also the Rotary Club ; Prof. F. A. E. Crew the local branch of the Indian Medical Association. The Institution of Engineers received lectures by Prof. R. V. Southwell, F.R.S., and Prof. G. W. O. Howe; and Prof. Howe also addressed the Association of Engineers. Four lectures in the University and one to industrialists were given by Dr. C. S. Myers, C.B.E., F.R.S., and two in the College of Science by Prof. C. G. Jung. Prof. Winifred Cullis, C.B.E., and Dr. E. P. Poulton addressed the Physiological Society of India, Dr. Poulton the Indian Medical Association, and Prof. R. Ruggles Gates, F.R.S., the Botanical Society of Bengal and the Bose Institute. Prof. P. G. H. Boswell, F.R.S., addressed University students. Broadcasts were given by Dr. W. G. Ogg, Sir Arthur Hill, K.C.M.G., F.R.S., Prof. P. G. H. Boswell, F.R.S., Dr. C. S. Myers, C.B.E., F.R.S., Sir Arthur Eddington, F.R.S., and Prof. H. J. Fleure. F.R.S. Sir James Jeans, Sir Arthur Hill, and other delegates took part in the celebration of the 150th anniversary of the Botanical Gardens on January 6. A number of the delegates attended a Vice-regal garden party at Belvidere on January 4, and H.E. the Governor of Bengal (Lord Brabourne) and Lady Brabourne gave a garden party for the Congress on January 7, which was followed at Government House by a special Convocation of the University of Calcutta, at which honorary degrees were conferred upon Sir James Jeans, F.R.S., Dr. F. W. Aston, F.R.S., Prof. Ernest Barker, Sir Arthur Eddington, F.R.S., Prof. A. H. R. Buller, F.R.S., Prof. R. A. Fisher, F.R.S., Prof. C. G. Jung, Dr. C. S. Myers, C.B.E., F.R.S., and Prof. W. Straub. The Corporation of the City of Calcutta gave a civic reception on January 4; a Science Congress dinner was held on January 8; the University of Calcutta gave a farewell party in the afternoon of January 9, and the hospitality of other official and non-official bodies and private residents was lavish and extensive. The Indian Science Congress Association, at the conclusion of the Congress, embodied their thanks to all concerned in a series of resolutions, and on behalf of the delegation Sir James Jeans, F.R.S., issued the following message to the Press:

'At the moment of leaving Calcutta, the visiting scientific delegation tender their most sincere thanks to all the kind hosts who have helped to make their stay in Calcutta so enjoyable.

'The Scientific Congress which we have been privileged to attend has impressed us all with its extraordinary vitality, with the widespread and generous attention accorded to our own contributions, and with the keen public interest which the transactions of the meeting have aroused. The huge audiences at the public lectures have been specially gratifying.

'I must reiterate our appreciation of the compliment paid by the Indian Science Congress Association to the British Association for the Advancement of Science in inviting its co-operation in the arrangement of the delegation; that invitation has forged a powerful new bond between Indian and British Science, to the great advantage of both, and we all hope that the effects of that bond may prove wider even than the bounds of science.

'We offer our thanks to the Indian Science Congress Association, to its kindred scientific institutions, to the many organisations which have contributed to the success of the Congress, to the City and University of Calcutta and to the province of Bengal.

'The women of the party owe special gratitude to the ladies, resident in Calcutta, who have afforded them such ample opportunities for learning of the manifold interests of the City.'

On the conclusion of the Congress some of the delegates proceeded to various points in India in pursuance of personal scientific interests and engagements. A party of over fifty of the visitors, however, left Calcutta for the south in the special train on the night of January 9. On the following day they saw something of the picturesque scenery of the maritime plain bordering the Eastern Ghats, and they reached Madras in the forenoon of January 11. Here they were entertained by the University of Madras at a luncheon, visited the museum, the aquarium, and other points of interest, and on the invitation of the Sheriff of Madras attended a garden party arranged by the city in honour of the Viceroy. The thanks of the party were subsequently conveyed by the President to the Vice-Chancellor of the University and to the Sheriff of the city. Lectures were given by Prof. Ernest Barker, by Prof. F. J. M. Stratton at the Christian College, Tambaram, and by Prof. J. L. Simonsen, F.R.S., at the Presidency College Chemical Society.

The special train left Madras at night, and the next morning (January 12) the party changed at Bangalore into a narrow-gauge train for Mysore City. At Bangalore and Mysore, and for the intervening journey, they were the guests of the State of Mysore. At Mysore City they were accommodated in Government House and in a camp (a term of more elaborate connotation in India than at home). Lectures were given by Sir James Jeans, F.R.S., Dr. F. W. Aston, F.R.S., Prof. Ernest Barker, Sir Arthur Eddington, F.R.S., and Prof. C. E. Spearman, F.R.S. The Maharaja's palace, the University, the Technical Institute, the Zoological Gardens, and various institutions were visited by members, and after nightfall they viewed with wonder the illuminated fountains at the great dam on the river Cauvery, and the city, brilliantly lit up, from Chamundi Hill. On the morning of January 13 the fort at Seringapatam and the tombs of Hyder Ali and Tippu Sultan were inspected, and the party entrained for Bangalore. Here again a number of institutions were visited, including the Indian Institute of Science and the College of Science. Sir James Jeans, F.R.S., addressed students at both the college and the institution, and the following also spoke: Dr. F. W. Aston, F.R.S., Prof. Ernest Barker, Prof. P. G. H. Boswell, F.R.S., Prof. F. A. E. Crew, Dr. C. G. Darwin, F.R.S., Prof. W. T. Gordon, and Prof. J. L. Simonsen, F.R.S. The thanks of the members were subsequently conveyed by the President to His Highness the Maharaja of Mysore, to Sir Mirza M. Ismail, Dewan Sahib of Mysore, and to Sir Charles Todhunter, K.C.S.I., private secretary to the Maharaja.

The party entrained at Bangalore on the night of January 13, and travelled direct to Bombay, where on January 15 they embarked on the *S.S. Strathaird* for the voyage home. Before doing so, Prof. Ernest Barker and Dr. R. N. Salaman, F.R.S., gave lectures, and Prof. Winifred Cullis, C.B.E., addressed the Association of British Women Graduates in India.

The members who took both the tours described above, before and after the meeting, travelled close upon six thousand miles in India. The weather was beautiful throughout the visit, except for a storm of short duration at Calcutta in the afternoon of January 9.

Before leaving Bombay, the President, on behalf of the Delegation, issued the following message through the Press :—

'In taking leave of India, we of the Scientific Delegation desire again to express our thanks for the overwhelming kindness with which we have been received in all parts. A month ago we landed here, eagerly expectant of what we were to see and learn. We are now returning home after a journey of more than five thousand miles through the country, during which we have been able to visit many monuments of ancient civilisations, and have admired the care with which the legacies of the past are preserved. But more of our time has been devoted to the present, and we have realised to the full the scientific and cultural developments which are in progress both in the universities and in the field of practical applications throughout the country.

'Nothing has more deeply impressed us than the interest shown in Science by the community at large and the eagerness with which students are following and practising the most recent advances in research. India has achieved self-sufficiency in many directions, but there is an acknowledged need for influences which shall further bind together her varied races. Her achievements in the realm of thought and her progress in the developments of industry lead us to hope that Science, which transcends all national and racial frontiers, may provide such a unifying influence. Long may Science continue to help in maintaining and advancing the position of India in the community of civilised nations.'

The activities of delegates were not confined to Calcutta and to the places visited during the tours. Thus, Prof. R. A. Fisher, F.R.S., following from Bombay an itinerary independently of the main party, lectured at Hyderabad (twice), Lucknow (twice), Aligarh, and Benares, and again at Bombay on his homeward journey, besides giving a course at Calcutta University. Before the Congress at Calcutta, Sir Henry Tizard, K.C.B., F.R.S., visited the Tata Iron & Steel Works at Jamshedpur, and lectured there. Sir Frederick Hobday addressed students in the Indian Veterinary Colleges at Bombay, Calcutta, Lahore, Madras, and Patna. At Madras, at times other than that of the visit described above, Prof. C. B. Fawcett gave lectures in the University and to the Madras Geographical Association ; Prof. F. E. Fritsch, F.R.S., gave four post-graduate lectures, and Lt.-Col. R. B. S. Sewell, C.I.E., F.R.S., three lectures in the University ; and Prof. R. Ruggles Gates, F.R.S., also lectured there. Prof. C. B. Fawcett, Prof. R. Ruggles Gates, F.R.S., Prof. C. G. Jung, and Prof. W. M. Tattersall visited and spoke at the University of Travancore in Trivandrum ; Sir Arthur Hill, K.C.M.G., F.R.S., and Dr. E. M. Crowther, the Agricultural College and Research Institute in Coimbatore ; Prof. J. E. Lennard-Jones, F.R.S., the University of Lahore ; Mr. J. McFarlane that of Patna ; Dr. C. S. Myers, C.B.E., F.R.S., that of Allahabad. Prof. R. Ruggles Gates, in addition to lectures already mentioned, spoke at Bombay (Royal Institute of Science), Bangalore (Central College), Coimbatore (Association of Economic Biologists), and Ernakulam (University College). Prof. H. H. Read lectured to the Indian School of Mines Scientific Society at Dhanbad. Prof. A. G. Ogilvie lectured to the Bombay Geographical Association, and informally addressed students at Wilson College in that city and at Hislop College, Nagpur. Prof. P. A. Buxton, visiting Ceylon, addressed the Colombo branch of the British Medical Association. Dr. W. G. Ogg advised the State authorities in Hyderabad on the Tungabadhra irrigation project, and other delegates were called into conference at many points for advice on matters connected with their special interests.

After the return of the delegation to England, the Council adopted the following resolution :—

The Council of the British Association have learned with gratification of the complete success that attended the visit of the Scientific Delegation to India, the members of which, through the invitation of the Indian Science Congress Association, were enabled to co-operate in its Jubilee Meeting in Calcutta, to visit many places of scientific and historical interest in India, to become acquainted with the work of many universities and other institutions, and to make or renew personal contacts with large numbers of Indian scientific workers and leaders of thought. The Council are glad to hear of the opinion, widely expressed in India, that much good would result from the visit, and this belief the Council heartily reciprocate. The Council desire to endorse the expressions of gratitude which have already been transmitted, on behalf of the Delegation, to the Government of India, to all other participant authorities and individuals, and very specially to the executive of the Indian Science Congress Association.

The Executive Committee of the Indian Science Congress Association at its meeting on September 20, 1938, returned the following reply to the above resolution :—

The Executive Committee of the Indian Science Congress Association have received with sincere pleasure and gratification the resolution of the Council of the British Association stating that the visit of their Scientific Delegation to India has been a complete success and has enabled the Members of the Delegation to make or renew personal contacts with Indian scientific workers and leaders of thought. They share with the Council of the British Association the belief that much good would result from the visit. The Executive Committee very much appreciate the friendly feelings expressed by the Council of the British Association on behalf of the Delegation to the authorities and individuals who contributed towards the success of the Silver Jubilee Session and expressed in particular their appreciation of the reference to the Executive Committee of the Indian Science Congress Association. The Executive Committee convey to the Council of the British Association their warmest appreciation of the manner in which the British Association have responded to their invitation to join the Indian Science Congress Association in joint session to celebrate the Silver Jubilee Session.

Balance Sheet,

Corresponding Figures 31st March, 1937. £ s. d.	LIABILITIES		£ s. d.	£ s. d.
	GENERAL PURPOSES :—			
	Sundry Creditors		785 2 9	
	Hon. Sir Charles Parsons' gift (£10,000) and legacy (£2,000) .		12,000 0 0	
	The late Sir Alfred Ewing's legacy		500 0 0	
	British Science Guild : Capital Fund		3,431 9 1	
	Bequest of Jaakoff Prelooker . .		10 0 0	
	Yarrow Fund			
	As per last Account	£4,744 16 1		
	Less Transferred to Income and Expenditure Account under terms of the gift .	383 9 8		
			4,361 6 5	
	Life and Corporate Compositions			
	As per last Account	3,138 12 2		
	Add Received during year . . .	175 17 0		
		3,314 9 2		
	Less Transferred to Income and Expenditure Account .	55 10 0		
			3,258 19 2	
	Contingency Fund "A"			
	As per last Account	1,940 17 1		
	Add Amount transferred from Income and Expenditure Account . .	59 2 11		
			2,000 0 0	
	Contingency Fund "B"			
	Amount transferred from Income and Expenditure Account .		29 16 3	
	Accumulated Fund			
	As per last Account	16,488 9 0		
	Less Transfer to Indian Science Congress Delegation Fund .	216 15 6		
			16,271 13 6	
42,702 0 11	SPECIAL PURPOSES :—			42,648 7 2
	Caird Fund			
	Balance at 1st April, 1937 . .		9,791 15 10	
	Less Excess of Expenditure over Income for the year . . .		41 1 8	
9,791 15 10				9,750 14 2
	Mathematical Tables Fund			
	Balance at 1st April, 1937 . . .		144 6 3	
	Receipts from Sales . . .		29 2 4	
			173 8 7	
	Less Payment to Cambridge University Press, *re* Vol. VI . .		150 0 0	
144 6 3				23 8 7
	Cunningham Bequest Fund			
	Balance at 1st April, 1937 . .		1,395 9 10	
	Add Excess of Income over Expenditure for the year . . .		12 8 8	
1,395 9 10				1,407 18 6
	Carried forward . .			53,830 8 5

31st March, 1938

Corresponding Figures 31st March, 1937. £ s. d.	ASSETS	£ s. d.	£ s. d.
	GENERAL PURPOSES :—		
	Investments as scheduled with Income and Expenditure Account, No. 1 .	41,979 7 7	
	Sundry debtors and payments in advance	528 10 9	
	Cash at bank	96 9 7	
	Cash in hand	43 19 3	
42,702 0 11			42,648 7 2
	SPECIAL PURPOSES :—		
	Caird Fund Account		
	Investments (see Income and Expenditure Account, No. 2) . .	9,582 16 3	
	Cash at bank	167 17 11	
9,791 15 10			9,750 14 2
	Mathematical Tables Fund Account		
	Cash at bank	23 8 7	
	Sundry debtors	—	
144 6 3			23 8 7
	Cunningham Bequest Fund Account		
	Investments (see Income and Expenditure Account, No. 3) . .	1,305 7 2	
	Cash at bank	102 11 4	
1,395 9 10			1,407 18 6
	Carried forward . .		53,830 8 5

Balance Sheet,

Corresponding Figures 31st March, 1937. £ s. d.	LIABILITIES (*continued*)	£ s. d.	£ s. d.	£ s. d.
	Brought forward . . .			53,830 8 5
	Toronto University Presentation Fund			
	Capital		178 11 4	
	Revenue		4 7 6	
182 18 10				182 18 10
	Bernard Hobson Fund			
	Capital . . .		1,000 0 0	
	Revenue—Balance per last Account . .	58 1 3		
	Less Excess of Expenditure over Income for the year . .	26 13 6		
			31 7 9	
1,058 1 3				1,031 7 9
	Leicester and Leicestershire Fund, 1933			
	Capital		1,000 0 0	
	Revenue—Balance per last Account . .	42 2 6		
	Less Excess of Expenditure over Income for the year . . .	15 10		
			41 6 8	
1,042 2 6				1,041 6 8
	Herbert Spencer Bequest Fund . .		1107 9 0	
	Less Excess of Expenditure over Income for year . . .		384 4 6	
1,107 9 0				723 4 6
	Norwich Fund, 1935			
	Balance per last Account . .		105 0 0	
	Less Expenditure for year . .		50 18 0	
105 0 0				54 2 0
	Radford Mather Lecture Fund			
	Capital		250 0 0	
	Sundry Creditor		39 4 6	
250 0 0				289 4 6
	Down House			
	Endowment Fund . . .		20,000 0 0	
	Sundry Creditors and Credit Balances		12 12 6	
	Suspense Account			
	Balance per last Account	80 13 9		
	Less Excess of Expenditure over Income for the year . . .	50 1 2		
			30 12 7	
20,093 5 5				20,043 5 1
	NOTE.—There are contingent Liabilities in respect of grants voted to Research Committees at Nottingham and by Council in 1937 but not claimed at 31st March, 1938, amounting to £416 17*s.* 9*d.*			
77,872 9 10				£77,195 17 9

I have examined the foregoing Account with the Books and Vouchers and certify
and the Investments, and the Bank have certified to me that they hold the

Approved.

EZER GRIFFITHS
R. S. WHIPPLE } *Auditors.*

31st March, 1938 *(continued)*

Corresponding Figures 31st March, 1937. £ s. d.	ASSETS *(continued)*	£ s. d.	£ s. d.
	Brought forward . . .		53,830 8 5
	Toronto University Presentation Fund Account		
	Investments (see Income and Expenditure Account, No. 4) . .	178 11 4	
	Cash at bank	4 7 6	
182 18 10			182 18 10
	Bernard Hobson Fund Account		
	Investments (see Income and Expenditure Account, No. 5) .	1,000 0 0	
	Cash at bank	31 7 9	
1,058 1 3			1,031 7 9
	Leicester and Leicestershire Fund, 1933 *Account*		
	Investments (see Income and Expenditure Account, No. 6) .	1,000 0 0	
	Cash at bank	41 6 8	
1,042 2 6			1,041 6 8
	Herbert Spencer Bequest Fund Account		
	Investments (see Income and Expenditure Account, No. 7) .	723 4 6	
	Cash at bank	—	
1,107 9 0			723 4 6
	Norwich Fund, 1935 Account		
105 0 0	(Income and Expenditure Account, No. 8)		54 2 0
	Cash at bank	—	
	Radford Mather Lecture Fund Account		
	Investments (see Income and Expenditure Account, No. 9) .	250 0 0	
	Add Excess of Expenditure over Income for the year . . .	39 4 6	
250 0 0			289 4 6
	Down House Account		
	Endowment Fund Investments (see Income and Expenditure Account, No. 10)	20,000 0 0	
	Cash at bank	31 13 4	
	Cash in hand	—	
	Sundry debtors and payments in advance	11 11 9	
20,093 5 5			20,043 5 1
77,872 9 10			£77,195 17 9

the same to be correct. I have also verified the Balance at the Bankers Deeds of Down House.

W. B. Keen, *Chartered Accountant.*
23 Queen Victoria St., London, E.C. 4.
2nd June, 1938.

INCOME AND EXPENDITURE ACCOUNTS FOR THE YEAR ENDED 31ST MARCH, 1938.

No. 1. General Income and Expenditure

Corresponding Figures 31st March, 1937. £ s. d.	*Investments :*		£ s. d.	
	£6,749 12s. 2d.	Consolidated 2½ per cent. Stock, at cost	5,142 3 3	
	£3,600 0s. 0d.	India 3 per cent. Stock, at cost	3,522 2 6	
	£879 14s. 9d.	Great Indian Peninsula Railway 'B' Annuity £43, at cost	827 15 0	
	£52 12s. 7d.	War Stock (Post Office Issue), at cost	54 5 2	
	£1,400 0s. 0d.	3½ per cent. War Loan Bonds, at cost	1,393 16 11	
	£8,062 6s. 4d.	3½ per cent. War Loan Inscribed Stock, at cost	8,111 12 10	
	£12,994 16s. 6d.	4½ per cent. Conversion Stock, at cost	12,844 8 4	
	£6,368 15s. 7d.	3½ per cent. Conversion Stock, at cost	5,304 8 11	
	£94 7s. 0d.	4½ per cent. Conversion Stock (Post Office Issue), at cost	62 15 0	
	£4,184 18s. 2d.	3 per cent. Local Loans, at cost	3,073 12 2	
	£1,000 0s. 0d.	4 per cent. Funding Loan Bonds	1,170 0 0	
	£100 0s. 0d.	5 per cent. Great Western Railway Consolidated Pref. Stock	125 0 0	
	£900 0s. 0d.	Royal Mail Steam Packet Co. Ordinary Stock	*nil*	
	£345 16s. 8d.	3 per cent. London County Consolidated Stock	347 7 6	(Value of Stocks
41,961 12 1	*(Value of Stocks at 31/3/37, £44,804 18s. 10d.)*		£41,979 7 7	at 31/3/38, £44,244 4s. 9d.)

Cash at bank and in hand, £158 5s. 5d.

Corresponding Figures 31st March, 1937.	EXPENDITURE	£ s. d.	£ s. d.	Corresponding Figures 31st March, 1937. £ s. d.	INCOME	£ s. d.
37 10 10	To Heat, Lighting and Power		40 9 0			
75 18 9	,, Stationery		87 18 10			
1 0 0	,, Rent		1 0 0	82 0 0	By Old Annual Regular Members	74 0 0
188 14 10	,, Postages		143 17 10	1,333 5 0	,, Annual members for Meeting only	1,211 4 6
131 8 2	,, Travelling expenses		119 1 11	469 10 0	,, Annual members, with Report	532 12 6
64 13 9	,, Exhibitioners		56 16 10	156 5 0	,, Transferable Tickets	116 15 0
35 14 0	,, Audit and Accountancy		84 0 0	89 10 0	,, Student members	91 5 0

Previous year £ s. d.		£ s. d.	£ s. d.
26 10 0	To *Cost of Fordigraph* . .		– – –
10 10 0	,, Subscription to Parliamentary Science Committee .		10 10 0
28 2 0	,, *Legal charges* re *Pensions* .		– – –
252 11 11	,, General Expenses . .		292 17 0
—	,, Office decorations . .		46 6 0
1,932 9 0	,, Salaries and wages . .		1,967 14 0
148 0 3	,, Pension contributions. .		143 2 0
1,151 10 11	,, Printing, binding, etc. .		1,032 8 6
92 0 0	,, Armorial Bearings: Expenditure during year . .		16 8 3
—	,, Cost of Badges . . .		190 6 3
—	,, Publicity at Nottingham .		37 10 0
	,, Norman Lockyer Lecture Expenses . . .		26 12 10
—	,, Alexander Pedler Lecture Expenses . . .		12 1 0
	To Grants paid to Research Committees by General Committee 1937/1938:		
	Kent's Cavern. . .	5 0 0	
	Brundon Bone-bed . .	3 0 0	
	Naples Committee . .	100 0 0	
	Freshwater Biological Station	75 0 0	
	Blood Groups . . .	8 5 0	
	Human Geography of Inter-Tropical Africa . .	1 0 10	
	Grey Seal Committee .	50 0 0	
	Derbyshire Caves . .	25 0 0	
	British Immigrant Insects	10 0 0	
	Artemia Salina . .	20 0 0	
	Carried forward . .		4,309 0 3

Previous year £ s. d.		£ s. d.
30 0 0	By Life compositions: amount transferred on expiry of membership . . .	55 10 0
526 12 8	,, Sale of Publications	455 13 10
246 2 4	,, Advertisements in B.A. publications .	252 13 2
6 0 0	,, Unexpended balances of grants, returned	1 16 0
13 10 0	,, Liverpool Exhibitioners . . .	18 0 0
92 1 0	,, *Donations towards cost of Armorial Bearings*	– – –
14 1 7	,, Income Tax recovered . . .	21 16 0
1,529 10 6	,, Interest on investments . . .	1,603 8 11
1 3 6	,, Donations	2 0 6
370 10 3	,, Sir Alfred Yarrow's Gift: amount transferred	383 9 8
	,, Grant from Herbert Spencer Bequest towards cost of Office decoration . .	37 19 6
	,, Amount recoverable for Badges at Nottingham	32 3 4
	Carried forward . .	4,890 7 11

No. 1. General Income and Expenditure (*continued*)

Corresponding Figures 31st March, 1937. £ s. d.	EXPENDITURE	£ s. d.	£ s. d.
	Brought forward		4,309 0 3
	To Grants—*continued.*		
	Critical Sections	25 0 0	
	Science in Education	5 0 0	
	Mining Sites in Wales	1 19 4	
	Petrographic Classification	1 6 9	
	Anthropometric Work in Cyprus	25 0 0	
	Adaptation of Animals to Algerian Salt Waters	50 0 0	
	Thermal Conductivities	7 14 3	
	Plymouth Laboratory	50 0 0	
264 6 2			463 6 2
	To Publications—		
75 11 8	Receipts from sales transferred to Mathematical Tables Fund		29 2 4
443 9 7	,, Balance, being excess of Income over Expenditure for the year carried down		88 19 2
4,960 1 10			£4,890 7 11
	To amount transferred to Contingency Fund "A"		59 2 11
	,, amount transferred to Contingency Fund "B"		29 16 3
443 9 7			£88 19 2
	Grants to research authorised but not yet claimed at 31st March, 1938, amount to		
207 5 0			£184 5 9

Corresponding Figures 31st March, 1937. £ s. d.	INCOME	£ s. d.
	Brought forward	4,890 7 11
4,960 1 10		£4,890 7 11
443 9 7	By balance brought down	£88 19 2

No. 2. Caird Fund

The unconditional gift of Sir James Caird, in 1912, administered by the Council in accordance with recommendations adopted by the General Committee in 1913.

Corresponding Figures 31st March, 1937. £ s. d.	*Investments*:		£ s. d.	
	£2,627 0s. 10d.	India 3½ per cent. Stock, at cost	2,400 13 3	
	£2,100 0s. 0d.	London Midland and Scottish Railway Consolidated 4 per cent. Preference Stock, at cost	2,190 4 3	
	£2,500 0s. 0d.	Canada 3½ per cent. Registered Stock, 1930/50, at cost	2,397 1 6	
	£2,000 0s. 0d.	Southern Railway Consolidated 5 per cent. Preference Stock, at cost	2,594 17 3	
9,582 16 3	*(Value at 31/3/37, £8,762 18s. 7d.)*		£9,582 16 3	(Value at 31/3/38, £8,772 9s. 5d.)

Cash at bank, £167 17s. 11d.

Corresponding Figures 31st March, 1937. £ s. d.	EXPENDITURE	£ s. d.	£ s. d.	Corresponding Figures 31st March, 1937. £ s. d.	INCOME	£ s. d.
	To Grants paid—			298 19 0	By Dividends and Interest	295 11 2
	Perseveration Committee	10 0 0		61 1 10	,, Income Tax recovered	64 9 8
	Zoological Record Committee	50 0 0		5 3 7	,, *Part Repayment of grant—Sir J. B. Harrison's Monograph*	—
	Seismology Committee	100 0 0		—	,, Balance, being excess of Expenditure over Income for the year	41 1 8
	Mathematical Tables Committee	241 2 6				
363 15 6			401 2 6			
1 8 11	,, *Balance being excess of Income over Expenditure for the year*		—			
365 4 5			£401 2 6	365 4 5		£401 2 6

Grants to research authorised, but not yet claimed at 31st March, 1938, amount to £126 0 0

No. 3. Cunningham Bequest

A legacy received by the Association in 1929 in trust under the will of Lt.-Col. A. J. C. Cunningham, for the preparation of new mathematical tables in the theory of numbers; administered by the Council.

Corresponding Figures 31st March, 1937. £ s. d.		£ s. d.	
	Investments:		
	£1,187 6s. 10d. Consolidated 2½ per cent. Stock	653 0 9	
	£300 0s. 0d. Port of London 3½ per cent. Stock, 1949/99	216 0 0	
	£862 13s. 3d. Local Loans 3 per cent. Stock, at cost	436 6 5	
1,305 7 2	(*Value at 31/3/37, £1,963 12s. 11d.*)	£1,305 7 2	(Value at 31/3/38, £1,918 18s. 1d.)

Cash at bank, £102 11s. 4d.

Corresponding Figures 31st March, 1937. £ s. d.	EXPENDITURE	£ s. d.	Corresponding Figures 31st March, 1937. £ s. d.	INCOME	£ s. d.
24 16 0	To Grants for the preparation of tables	53 10 0	63 12 10	By Interest	63 10 2
41 4 0	,, Balance, being excess of income over expenditure for the year	12 8 8	2 7 2	,, Income Tax recovered	2 8 6
66 0 0		£65 18 8	66 0 0		£65 18 8

No. 4. Toronto University Presentation Fund

A fund voluntarily subscribed by members present at the Toronto Meeting in 1924. From the income a presentation of two bronze medals each year is made, together with presents of books, to selected students in pure and applied science respectively.

Corresponding Figures 31st March, 1937. £ s. d.			
	Investment:		
178 11 4	£175 3½ per cent. War Stock at cost	£178 11 4	(Value at 31/3/38, £177 12s. 6d.)
	(*Value at 31/3/37, £179 7s. 6d.*)		

Cash at bank, £4 7s. 6d.

Corresponding Figures 31st March, 1937. £ s. d.	EXPENDITURE	£ s. d.	Corresponding Figures 31st March, 1937. £ s. d.	INCOME	£ s. d.
6 2 6	To awards	£6 2 6	6 2 6	By Interest	£6 2 6

No. 5. Bernard Hobson Fund

The bequest of Mr. Bernard Hobson, 1933; the income to be applied to the promotion of geological research; administered by the Council.

Corresponding Figures 31st March, 1937. £ s. d.	*Investments:*		£ s. d.	
	£450 0s. 0d.	4 per cent. Victory (Bearer) Bonds at cost	491 13 6	
	£601 9s. 0d.	3 per cent. Local Loans at cost	508 6 6	
1,000 0 0	*(Value at 31/3/37, £1,021 5s. 7d.)*		£1,000 0 0	(Value at 31/3/38, £1,011 17s. 7d.)
		Cash at bank, £31 7s. 9d.		

Corresponding Figures 31st March, 1937. £ s. d.	EXPENDITURE	£ s. d.	£ s. d.	Corresponding Figures 31st March, 1937. £ s. d.	INCOME	£ s. d.
	To Grants paid to Committees:			31 15 6	By Interest	31 11 0
	Reptile-bearing Öolite	50 0 0		4 1 0	,, Income Tax Recovered	4 5 6
	Critical Sections . .	12 10 0			,, Balance, being excess of Expenditure	
7 17 5			62 10 0	—	over Income for the year . .	26 13 6
27 19 1	,, *Balance, being excess of Income over Expenditure for the year* . . .		—			
35 16 6			£62 10 0	35 16 6		£62 10 0

Grants to research authorised, but not yet claimed at 31st March, 1938, amount to £12 10 0

No. 6. Leicester and Leicestershire Fund, 1933

Corresponding Figures 31st March, 1937. £ s. d.		£
	The unexpended balance of the local fund for the Leicester Meeting in 1933, presented to the Association, the interest to be used in assisting by scholarships or otherwise students working for the advancement of science; administered by the Council.	
	Investments :	
	£487 2*s.* 11*d.* $3\frac{1}{2}$ per cent. Conversion Stock at cost	500
	£490 5*s.* 11*d.* $3\frac{1}{2}$ per cent. War Stock at cost	500
1,000 0 0	(*Value at 31/3/37, £994 11s. 6d.*)	£1,000 (Value at 31/3/38, £987 4*s.* 8*d.*)
	Cash at bank, £41 6*s.* 8*d.*	

Corresponding Figures 31st March, 1937. £ s. d.	EXPENDITURE	£ s. d.	£ s. d.	Corresponding Figures 31st March, 1937. £ s. d.	INCOME	£ s. d.
	To Grants, paid to Committees :			34 4 2	By Interest	34 4 2
	Routine Manual Factor	30 0 0		2 10 0	,, *Unexpended balance of grant returned*	—
	Transplant Experiments	5 0 0			,, Balance, being excess of Expenditure	
60 0 0			35 0 0	23 5 10	over Income for the year	15 10
60 0 0			35 0 0	60 0 0		35 0 0

Grants to research authorised but not yet claimed at 31st March, 1938, amount to £40 0 0

No. 7. Herbert Spencer Bequest Fund

A gift of £1,241 from the late Herbert Spencer received during 1936, to be exhausted within 5 years and to be expended on purchase or enlargement of premises, books, apparatus or collections, or for furniture or repairs or equipment of travellers or donation of instruments of research, but in no way or degree for purposes of endowment.

Corresponding Figures 31st March, 1937. £ s. d.		£ s. d.	
	Investments :		
	£398 4*s*. 3*d*. 3 per cent. London County Consolidated Stock, 1956/61, at cost	400 0 0	
	£303 15*s*. 3*d*. 3½ per cent. War Stock, at cost	323 4 6	
741 0 0	(*Cash at bank, 31/3/37, £366 9s.*)	£723 4 6	(Value at 31/3/38, £692 11*s*. 11*d*.)

Cash at bank, *nil*.

EXPENDITURE	£ s. d.	Corresponding Figures 31st March, 1937. £ s. d.	INCOME	£ s. d.
To Down House—Contribution towards cost of repairs, etc.	366 9 0		By Interest and Dividends	20 4 0
Contribution towards cost of office decoration	37 19 6		By Balance, being excess of Expenditure over Income for the year	384 4 6
		133 11 0		
	£404 8 6	*133 11 0*		£404 8 6

Corresponding Figures 31st March, 1937.
133 11 0

No. 8. Norwich Fund, 1935

A gift of £105 received from the Local Committee of the Norwich Meeting held there in 1935, to be dealt with at the discretion of the Council.

Cash at bank, £54 2*s*. 0*d*.

EXPENDITURE	£ s. d.	£ s. d.	INCOME	£ s. d.
To Grants paid :			By Unexpended Balance of Grant refunded	11 15 9
Breckland vegetation . . .	30 0 0		,, Balance being net Expenditure for year . . .	50 18 0
West Rudham long barrow . .	30 0 0			
Post-glacial deposits in Norfolk .	2 13 9	62 13 9		
		£62 13 9		£62 13 9
Grants to research authorised but not yet claimed at 31st March, 1938, amount to . . .		£54 2 0		

No. 9. Radford Mather Lecture Fund

A gift of £250 received from Mr. G. Radford Mather in 1936 to establish a Fund, the income therefrom to be devoted to meeting the expense of triennial Lectures on Recent Advances in Science and their relation to the Welfare of the Community.

	£ s. d.	
Investment : £248 17*s*. 8*d*. 3 per cent. London County Consolidated Stock, 1956/61, at cost .	250 0 0	(Value at 31/3/38, £240 3*s*. 5*d*.)

EXPENDITURE	£ s. d.	INCOME	£ s. d.
To Lecture Fee	21 0 0	By Interest and Dividends .	5 12 0
,, Expenses of Lecture	23 16 6	,, Balance being excess of Expenditure over Income for the year	39 4 6
	£44 16 6		£44 16 6

No. 10. Down House

In response to an appeal made in 1927 by Sir Arthur Keith, F.R.S., then President of the British Association, Mr. (now Sir) Buckston Browne, F.R.C.S., acquired the property of Down House, formerly the home of Darwin, and transferred it with an endowment to the Association as a gift to be held as a memorial to Darwin in custody for the nation.

Corresponding Figures 31st March, 1937. £ s. d.			£ s. d.	
	Investments :			
		DOWN HOUSE ENDOWMENT FUND		
	£5,500	India 4½ per cent. Stock, 1958/68, at cost	5,001 17 4	
	£2,500	Commonwealth of Australia 5 per cent. Stock, 1945/75, at cost	2,468 19 0	
	£3,000	Fishguard and Rosslare Railway and Harbours 3½ per cent. Guaranteed Preference Stock, at cost	2,139 17 3	
	£2,500	New South Wales 5 per cent. Stock, 1945/65, at cost	2,467 7 9	
	£2,500	Western Australia 5 per cent. Stock, 1945/75, at cost	2,472 1 6	
	£3,340	Great Western Railway 5 per cent. Consolidated Guaranteed Stock, at cost	3,436 7 5	
	£2,500	Birkenhead Railway 4 per cent. Consolidated Stock, at cost	2,013 9 9	
20,000 0 0			£20,000 0 0	(Value of Stocks at 31/3/38, £23,925 16*s.* 0*d.*)
	(*Value of Stocks at 31/3/37, £23,657 18s. od.*)			
		Cash at bank and in hand	£31 13*s.* 4*d.*	

Corresponding Figures 31st March, 1937. £ s. d.	EXPENDITURE	£ s. d.	Corresponding Figures 31st March, 1937. £ s. d.	INCOME	£ s. d.
803 19 7	To Wages of Staff	822 8 10	141 0 0	By Rents Receivable	139 5 0
69 5 6	,, Rates, Insurance, etc.	71 9 3	168 1 6	,, Income Tax recovered	177 8 0
138 14 8	,, Coal, Coke, etc.	175 0 7	817 2 0	,, Interest and Dividends	807 15 0
79 4 11	,, Lighting and Drainage (including oil and petrol)	91 13 7	3 17 1	,, Donations	3 8 9
15 7 1	,, Water	14 10 2	23 8 4	,, Sale of Postcards and Catalogues	26 8 4
1 7 8	,, *Rain-gauge*	—	150 0 0	,, Pilgrim Trust grant	150 0 0
			132 3 4	,, Instalment of Grant from Herbert Spencer Bequest	366 9 0
			—		
	Carried forward	1,175 2 5		Carried forward	1,670 14 1

No. 10. Down House (*continued*)

Corresponding Figures 31st March, 1937. £ s. d.	EXPENDITURE	£ s. d.
	Brought forward	1,175 2 5
159 2 4	To Repairs and Renewals	428 3 1
	,, Garden and Land :	
45 19 8	Materials and Maintenance	61 8 5
5 5 0	,, Donations to Village Institutions	5 5 0
15 18 10	,, Household Requisites, etc.	12 5 11
1 16 6	,, Transport and Carriage	3 14 10
8 8 0	,, *Accountants' Fees*	—
35 7 5	,, Printing, Postages, Telephone, and Stationery	34 15 7
55 15 1	,, *Balance, being excess of Income over Expenditure for the year transferred to Suspense Account*	—
1,435 12 3		£1,720 15 3

Corresponding Figures 31st March, 1937. £ s. d.	INCOME	£ s. d.
	Brought forward	1,670 14 1
—	By Balance being excess of Expenditure over Income for the year transferred to Suspense Account	50 1 2
1,435 12 3		£1,720 15 3

No. 11. India Delegation Fund

£ s. d.	EXPENDITURE	£ s. d.
	To Grants paid	4,590 0 0
	,, Secretary's Travelling Expenses	69 12 3
	,, Administration Expenses	43 19 11
		£4,703 12 2

£ s. d.	INCOME	£ s. d.
	By Balance brought forward, being Donations received prior to 1st April, 1937	278 5 0
	,, Donations during year	1,078 6 0
	,, Indian Science Congress Association : Contribution	3,125 0 0
	,, Bank Deposit Interest	5 5 8
	,, Balance, being excess of Expenditure over Income contributed by Accumulated Fund	216 15 6
		£4,703 12 2

RESEARCH COMMITTEES, Etc.

APPOINTED BY THE GENERAL COMMITTEE, MEETING IN CAMBRIDGE, 1938.

Grants of money, if any, from the Association for expenses connected with researches are indicated in heavy type.

SECTION A.—MATHEMATICAL AND PHYSICAL SCIENCES.

Seismological investigations.—Dr. F. J. W. Whipple (*Chairman*), Mr. J. J. Shaw, C.B.E. (*Secretary*), Miss E. F. Bellamy, Prof. P. G. H. Boswell, O.B.E., F.R.S., Dr. E. C. Bullard, Dr. A. T. J. Dollar, Sir Frank Dyson, K.B.E., F.R.S., Dr. A. E. M. Geddes, O.B.E., Prof. G. R. Goldsbrough, F.R.S., Dr. Wilfred Hall, Mr. J. S. Hughes, Dr. H. Jeffreys, F.R.S., Mr. Cosmo Johns, Dr. A. W. Lee, Prof. E. A. Milne, M.B.E., F.R.S., Prof. H. H. Plaskett, F.R.S., Prof. H. C. Plummer, F.R.S., Prof. J. Proudman, F.R.S., Dr. A. O. Rankine, O.B.E., F.R.S., Rev. C. Rey, S.J., Rev. J. P. Rowland, S.J., Prof. R. A. Sampson, F.R.S., Mr. F. J. Scrase, Capt. H. Shaw, Sir Frank Smith, K.C.B., C.B.E., Sec. R.S., Dr. R. Stoneley, F.R.S., Mr. E. Tillotson, Sir G. T. Walker, C.S.I., F.R.S. **£100** (Caird Fund grant).

Calculation of mathematical tables.—Prof. E. H. Neville (*Chairman*), Dr. J. Wishart (*Secretary*), Dr. A. J. Thompson (*Vice-Chairman*), Dr. W. G. Bickley, Prof. R. A. Fisher, F.R.S., Dr. J. Henderson, Dr. E. L. Ince, Dr. J. O. Irwin, Dr. J. C. P. Miller, Mr. F. Robbins, Mr. D. H. Sadler, Mr. W. L. Stevens, Dr. J. F. Tocher. **£200** (Caird Fund grant).

SECTIONS A, C.—MATHEMATICAL AND PHYSICAL SCIENCES, GEOLOGY.

The direct determination of the thermal conductivities of rocks in mines or borings where the temperature gradient has been, or is likely to be, measured.—Dr. Ezer Griffiths, F.R.S. (*Chairman*), Dr. D. W. Phillip (*Secretary*), Dr. E. C. Bullard, Dr. H. Jeffreys, F.R.S. (*from Section A*); Dr. E. M. Anderson, Prof. W. G. Fearnsides, F.R.S., Prof. G. Hickling, F.R.S., Prof. A. Holmes, Dr. J. H. J. Poole (*from Section C*).

SECTIONS A, J.—MATHEMATICAL AND PHYSICAL SCIENCES, PSYCHOLOGY.

The possibility of quantitative estimates of sensory events.—Prof. A. Ferguson (*Chairman*), Dr. C. S. Myers, C.B.E., F.R.S. (*Vice-Chairman*), Mr. R. J. Bartlett (*Secretary*), Dr. H. Banister, Prof. F. C. Bartlett, F.R.S., Dr. Wm. Brown, Dr. N. R. Campbell, Prof. J. Drever, Mr. J. Guild, Dr. R. A. Houstoun, Dr. J. O. Irwin, Dr. G. W. C. Kaye, Dr. S. J. F. Philpott, Dr. L. F. Richardson, F.R.S., Dr. J. H. Shaxby, Mr. T. Smith, F.R.S., Dr. R. H. Thouless, Dr. W. S. Tucker, O.B.E.

SECTION C.—GEOLOGY.

To excavate critical geological sections in Great Britain.—Prof. W. T. Gordon (*Chairman*), Prof. W. G. Fearnsides, F.R.S. (*Secretary*), Prof. E. B. Bailey, F.R.S., Mr. H. C. Berdinner, Mr. W. S. Bisat, Prof. P. G. H. Boswell, O.B.E., F.R.S., Prof. W. S. Boulton, Prof. A. H. Cox, Miss M. C. Crosfield, Mr. E. E. L. Dixon, Dr. Gertrude Elles, M.B.E., Mr. C. I. Gardiner, Prof. E. J. Garwood, F.R.S., Mr. F. Gossling, Prof. H. L. Hawkins, F.R.S., Prof. G. Hickling, F.R.S., Dr. R. G. S. Hudson, Prof. V. C. Illing, Prof. O. T. Jones, F.R.S., Dr. Murray

Macgregor, Dr. F. J. North, Dr. J. Pringle, Prof. S. H. Reynolds, Sir Franklin Sibly, Dr. W. K. Spencer, F.R.S., Dr. W. E. Swinton, Prof. A. E. Trueman, Dr. F. S. Wallis, Prof. W. W. Watts, F.R.S., Dr. W. F. Whittard, Sir A. Smith Woodward, F.R.S., Dr. S. W. Wooldridge. **£60** (Bernard Hobson Fund, **£40** contingent).

To consider and report upon petrographic classification and nomenclature.—Lt.-Col. W. Campbell Smith (*Chairman and Secretary*), Prof. E. B. Bailey, F.R.S., Dr. R. Campbell, Dr. W. Q. Kennedy, Dr. A. G. MacGregor, Prof. S. J. Shand, Mr. S. J. Tomkeieff, Dr. G. W. Tyrrell, Dr. F. Walker, Dr. A. K. Wells. **£10.**

To consider and report on questions affecting the teaching of geology in schools.—Prof. W. W. Watts, F.R.S. (*Chairman*), Prof. A. E. Trueman (*Secretary*), Prof. P. G. H. Boswell, O.B.E., F.R.S., Mr. C. P. Chatwin, Prof. A. H. Cox, Mr. J. Davies, Miss E. Dix, Miss Gaynor Evans, Prof. W. G. Fearnsides, F.R.S., Prof. A. Gilligan, Prof. G. Hickling, F.R.S., Prof. D. E. Innes, Prof. A. G. Ogilvie, O.B.E., Prof. W. J. Pugh, Mr. J. A. Steers, Prof. H. H. Swinnerton, Dr. A. K. Wells.

The collection, preservation, and systematic registration of photographs of geological interest.—Prof. E. J. Garwood, F.R.S. (*Chairman*), Prof. S. H. Reynolds (*Secretary*), Mr. H. Ashley, Mr. G. Macdonald Davies, Mr. J. F. Jackson, Dr. A. G. MacGregor, Dr. F. J. North, Dr. A. Raistrick, Mr. J. Ranson, Prof. W. W. Watts, F.R.S.

SECTION D.—ZOOLOGY.

To nominate competent naturalists to perform definite pieces of work at the Marine Laboratory, Plymouth.—Dr. W. T. Calman, C.B., F.R.S. (*Chairman and Secretary*), Prof. H. Munro Fox, F.R.S., Dr. J. S. Huxley, F.R.S., Prof. H. G. Jackson, Prof. C. M. Yonge. **£50.**

To co-operate with other sections interested, and with the Zoological Society, for the purpose of obtaining support for the Zoological Record.—Sir Sidney Harmer, K.B.E., F.R.S. (*Chairman*), Dr. W. T. Calman, C.B., F.R.S. (*Secretary*), Prof. E. S. Goodrich, F.R.S., Prof. D. M. S. Watson, F.R.S. **£50.**

To investigate the density of living organisms.—Dr. S. W. Kemp, F.R.S. (*Chairman*), Mr. A. G. Lowndes (*Secretary*), Prof. R. A. Fisher, F.R.S., Dr. C. F. A. Pantin, Dr. F. S. Russell. **£40.**

To investigate sex in salmon.—Prof. F. A. E. Crew (*Chairman*), Prof. J. H. Orton, Prof. J. Ritchie. **£25.**

To study the progressive adaptation to new conditions in *Artemia salina* (Diploid and Octoploid, Parthenogenetic *v.* Bisexual).—Prof. R. A. Fisher, F.R.S. (*Chairman*), Mr. A. C. Fabergé (*Secretary*), Dr. F. Gross, Mr. A. G. Lowndes, Dr. K. Mather, Dr. E. S. Russell, O.B.E., Prof. D. M. S. Watson, F.R.S. **£20.**

To study insular faunas.—Prof. Sir E. B. Poulton, F.R.S. (*Chairman*), Prof. G. D. Hale Carpenter (*Secretary*), Prof. H. G. Jackson, Capt. N. D. Riley. **£10.**

To investigate the adaptations of freshwater animals to waters of very high salinity in Algeria.—Prof. P. A. Buxton (*Chairman*), Mr. L. C. Beadle (*Secretary*), Dr. G. S. Carter, Dr. E. B. Worthington. **£5 4s. 8d.** (unexpended balance).

To investigate the social behaviour of the grey seal.—Prof. J. Ritchie (*Chairman*), Dr. Fraser Darling (*Secretary*), Prof. F. A. E. Crew, Dr. J. S. Huxley, F.R.S., Dr. E. S. Russell.

To consider the position of animal biology in the school curriculum and matters relating thereto.—Prof. R. D. Laurie (*Chairman and Secretary*), Mr. P. Ainslie, Dr. H. W. Cousins, Dr. J. S. Huxley, F.R.S., Mr. Percy Lee, Mr. A. G. Lowndes, Prof. E. W. MacBride, F.R.S., Dr. W. K. Spencer, F.R.S., Prof. W. M. Tattersall, Dr. E. N. Miles Thomas.

SECTIONS D, E, K.—ZOOLOGY, GEOGRAPHY, BOTANY.

To assist in the preservation of Wicken Fen.—Prof. F. T. Brooks, F.R.S. (*Chairman*), Dr. H. Godwin (*Secretary*), Prof. F. Balfour-Browne, Dr. H. C. Darby, Prof. J. Stanley Gardiner, F.R.S., Mr. J. A. Steers, Dr. W. H. Thorpe, Dr. D. Valentine. **£50.**

SECTIONS D, I, K.—ZOOLOGY, PHYSIOLOGY, BOTANY.

To aid competent investigators selected by the Committee to carry on definite pieces of work at the Zoological Station at Naples.—Prof. E. W. MacBride, F.R.S. (*Chairman and Secretary*), Prof. Sir J. Barcroft, C.B.E., F.R.S., Dr. Margery Knight, Dr. J. Z. Young. **£50.**

SECTIONS D, J.—ZOOLOGY, PSYCHOLOGY.

To conduct field experiments on bird behaviour.—Dr. J. S. Huxley, F.R.S. (*Chairman*), Mr. F. B. Kirkman (*Secretary*), Prof. F. Aveling, Dr. C. S. Myers, C.B.E., F.R.S., Dr. E. S. Russell. **£40.**

SECTIONS D, K.—ZOOLOGY, BOTANY.

To aid competent investigators selected by the Committee to carry out definite pieces of work at the Freshwater Biological Station, Wray Castle, Windermere.—Prof. F. E. Fritsch, F.R.S. (*Chairman*), Dr. E. B. Worthington (*Secretary*), Prof. P. A. Buxton, Miss P. M. Jenkin, Dr. C. H. O'Donoghue (*from Section D*) ; Dr. W. H. Pearsall (*from Section K*). **£75.**

Co-ordinating committee for Cytology and Genetics.—Prof. F. T. Brooks, F.R.S. (*Chairman*), Dr. D. G. Catcheside (*Secretary*), Mr. E. B. Ford, Prof. F. A. E. Crew, Dr. C. D. Darlington, Prof. R. A. Fisher, F.R.S., Prof. R. R. Gates, F.R.S., Dr. C. Gordon, Prof. Dame Helen Gwynne Vaughan, G.B.E., Dr. J. Hammond, Dr. J. S. Huxley, F.R.S., Dr. T. J. Jenkin, Mr. W. J. C. Lawrence, Dr. F. W. Sansome, Dr. W. B. Turrill, Dr. C. H. Waddington, Dr. D. M. Wrinch. **£5.**

SECTION E.—GEOGRAPHY.

To prepare a scheme for a projected National Atlas of Great Britain and Northern Ireland.—Prof. E. G. R. Taylor (*Chairman*), Dr. S. W. Wooldridge (*Secretary*), Dr. H. C. Darby, Prof. F. Debenham, Mr. C. Diver, Prof. H. J. Fleure, F.R.S., Mr. D. L. Linton, Brig. M. N. MacLeod, Prof. E. J. Salisbury, F.R.S., Prof. A. G. Tansley, F.R.S. **£10.**

To collect and record information on demography and seasonal activities in relation to environment in Inter-Tropical Africa.—Prof. P. M. Roxby (*Chairman*), Prof. A. G. Ogilvie, O.B.E. (*Secretary*), Mr. S. J. K. Baker, Prof. C. B. Fawcett, Prof. H. J. Fleure, F.R.S., Prof. C. Daryll Forde, Mr. R. H. Kinvig, Mr. J. McFarlane, Prof. J. L. Myres, O.B.E., Mr. R. A. Pelham, Mr. R. U. Sayce. **£2.**

To consider and report upon ambiguities and innovations in geographical terminology.—Prof. E. G. R. Taylor (*Chairman*), Dr. S. W. Wooldridge (*Secretary*), Mr. H. King, Mr. R. H. Kinvig.

To co-operate with bodies concerned with the cartographic representation of population, and in particular with the Ordnance Survey, for the production of population maps.— (*Chairman*), Prof. C. B. Fawcett (*Secretary*), The Director General of the Ordnance Survey, Col. Sir Charles Close, K.B.E., C.B., C.M.G., F.R.S., Prof. H. J. Fleure, F.R.S., Mr. A. C. O'Dell, Mr. A. Stevens, Mr. A. V. Williamson.

SECTION G.—ENGINEERING.

To review the knowledge at present available for the reduction of noise, and the nuisances to the abatement of which this knowledge could best be applied.— (*Chairman*), Wing-Commander T. R.

Cave-Browne-Cave, C.B.E. (*Secretary*), Dr. A. H. Davis, Prof. G. W. O. Howe, Mr. E. S. Shrapnell-Smith, C.B.E. **£10** (Contingent).

Electrical terms and definitions.—Prof. Sir J. B. Henderson (*Chairman*), Prof. F. G. Baily and Prof. G. W. O. Howe (*Secretaries*), Prof. W. Cramp, Prof. W. H. Eccles, F.R.S., Prof. C. L. Fortescue, Prof. A. E. Kennelly, Prof. E. W. Marchant, Prof. J. Proudman, F.R.S., Sir Frank Smith, K.C.B., C.B.E., Sec. R.S., Prof. L. R. Wilberforce.

SECTION H.—ANTHROPOLOGY.

To co-operate with a Committee of the Royal Anthropological Institute in the exploration of caves in the Derbyshire district.—Mr. M. C. Burkitt (*Chairman*), Mr. A. Leslie Armstrong (*Secretary*), Prof. H. J. Fleure, F.R.S., Miss D. A. E. Garrod, Dr. J. Wilfred Jackson, Prof. L. S. Palmer, Mr. H. J. E. Peake. **£25.**

To co-operate with the Committee for the Standardisation of Anthropological Technique (a permanent committee of the International Congress of Anthropological and Ethnological Sciences).—Prof. J. L. Myres, O.B.E. (*Chairman*), Miss M. Tildesley (*Secretary*), Prof. A. Low, Dr. G. M. Morant. **£20.**

To investigate early mining sites in Wales.—Mr. H. J. E. Peake (*Chairman*), Mr. Oliver Davies (*Secretary*), Dr. C. H. Desch, F.R.S., Mr. E. Estyn Evans, Prof. H. J. Fleure, F.R.S., Prof. C. Daryll Forde, Sir Cyril Fox, Dr. Willoughby Gardner, Dr. F. J. North, Mr. V. E. Nash Williams. **£8.**

To investigate blood groups among primitive peoples.—Prof. H. J. Fleure, F.R.S. (*Chairman*), Prof. R. Ruggles Gates, F.R.S. (*Secretary*), Dr. F. W. Lamb, Dr. G. M. Morant. **£7.** (**£2** unexpended balance.)

To co-operate with the Torquay Antiquarian Society in investigating Kent's Cavern.—Sir A. Keith, F.R.S. (*Chairman*), Prof. J. L. Myres, O.B.E. (*Secretary*), Mr. M. C. Burkitt, Miss D. A. E. Garrod, Mr. A. D. Lacaille. **£5.**

To co-operate with a committee of the Royal Anthropological Institute in assisting Miss G. Caton-Thompson to investigate the prehistoric archæology of the Kharga Oasis.—Prof. J. L. Myres, O.B.E. (*Chairman*), Miss G. Caton-Thompson (*Secretary*), Dr. H. S. Harrison, Mr. H. J. E. Peake.

To report on the classification and distribution of rude stone monuments in the British Isles.—Mr. H. J. E. Peake (*Chairman*), Mr. E. Estyn Evans (*Secretary*), Mr. A. L. Armstrong, Mr. H. Balfour, F.R.S., Mrs. E. M. Clifford, Dr. G. E. Daniel, Sir Cyril Fox, Mr. W. F. Grimes, Mr. W. J. Hemp, Mr. A. Keiller, Mr. T. D. Kendrick, Dr. Margaret A. Murray, Prof. J. L. Myres, O.B.E., Mr. C. W. Phillips.

To carry out research among the Ainu of Japan.—Prof. C. G. Seligman, F.R.S. (*Chairman*), Mrs. C. G. Seligman (*Secretary*), Dr. H. S. Harrison, Capt. T. A. Joyce, O.B.E., Rt. Hon. Lord Raglan.

To conduct archæological and ethnological researches in Crete.—Prof. J. L. Myres, O.B.E. (*Chairman*), Dr. G. M. Morant (*Secretary*), Mr. L. Dudley Buxton, Dr. W. L. H. Duckworth.

To report to the Sectional Committee on the question of re-editing 'Notes and Queries in Anthropology.'—Prof. H. J. Fleure, F.R.S. (*Chairman*), Mr. Elwyn Davies (*Secretary*), Prof. J. H. Hutton, C.I.E., Dr. G. M. Morant, Prof. A. R. Radcliffe-Brown, Prof. C. G. Seligman, F.R.S., Mrs. C. G. Seligman.

To report on the composition of ancient metal objects.—Mr. H. J. E. Peake (*Chairman*), Dr. C. H. Desch, F.R.S. (*Secretary*), Mr. H. Balfour, F.R.S., Prof. V. G. Childe, Mr. O. Davies, Prof. H. J. Fleure, F.R.S., Mr. C. Hawkes, Miss W. Lamb, Mr. M. E. L. Mallowan, Mr. H. Maryon, Dr. A. Raistrick, Dr. R. H. Rastall.

SECTION I.—PHYSIOLOGY.

To deal with the use of a stereotactic instrument.—Prof. J. Mellanby, F.R.S. (*Chairman*), Prof. R. J. S. McDowall (*Secretary*).

SECTION J.—PSYCHOLOGY.

To develop tests of the routine manual factor in mechanical ability.—Dr. C. S. Myers, C.B.E., F.R.S. (*Chairman*), Dr. G. H. Miles (*Secretary*), Prof. C. Burt, Dr. F. M. Earle, Dr. Ll. Wynn Jones, Prof. T. H. Pear. **£40.**

The nature of perseveration and its testing.—Prof. F. Aveling (*Chairman*), Dr. W. Stephenson (*Secretary*), Prof. F. C. Bartlett, F.R.S., Dr. Mary Collins, Prof. J. Drever, Mr. E. Farmer, Prof. C. Spearman, F.R.S., Dr. P. E. Vernon. **£5.**

SECTION K.—BOTANY.

Transplant experiments.—Sir Arthur Hill, K.C.M.G., F.R.S. (*Chairman*), Dr. W. B. Turrill (*Secretary*), Prof. F. W. Oliver, F.R.S., Prof. E. J. Salisbury, F.R.S., Prof. A. G. Tansley, F.R.S.

SECTION L.—EDUCATION.

To consider and report on the gaps in the informative content of education, with special reference to the curriculums of schools.—Sir Richard Gregory, Bart., F.R.S. (*Chairman*), Mr. A. E. Henshall (*Secretary*), Prof. C. M. Attlee, Mr. G. D. Dunkerley, Miss L. Higson, Mr. D. Shillan, Dr. F. H. Spencer, Mr. H. G. Wells. **£15** (Leicester and Leicestershire Fund).

To consider and report on the possibilities of organising and developing research in education.—Prof. F. Clarke (*Chairman*), Mr. A. Gray Jones (*Secretary*), Miss D. Bailey, Dr. M. M. Lewis, Sir Richard Livingstone, Mr. W. H. Robinson, Mr. N. F. Sheppard. **£10** (Leicester and Leicestershire Fund).

CORRESPONDING SOCIETIES.

Corresponding Societies Committee.—The President of the Association (*Chairman ex-officio*), Dr. C. Tierney (*Secretary*), the General Secretaries, the General Treasurer, Dr. Vaughan Cornish, Mr. T. S. Dymond, Prof. W. T. Gordon, Mr. N. B. Kinnear, the Rt. Hon. the Earl of Onslow, P.C., Dr. G. F. Herbert Smith.

RESOLUTIONS & RECOMMENDATIONS.

The following resolutions and recommendations were referred, unless otherwise stated, to the Council by the General Committee at the Cambridge Meeting for consideration and, if desirable, for action :

From Section A (Mathematical and Physical Sciences).

That the Committee of Section A request the Council to communicate to the University authorities their satisfaction on learning that the establishment of a Museum of Historic Scientific Instruments is contemplated, and hopes that the scheme will be brought to fruition.

From Section D (Zoology).

That it is in the highest interest of the history of Science that the objects of historic and scientific importance now being shown in the exhibition arranged under the auspices of the Cambridge Philosophical Society should be kept together so far as possible to form the nucleus of a permanent University Museum illustrating the history of Science in Cambridge.

The substance of the two preceding resolutions was ordered to be communicated to the Vice-Chancellor of the University of Cambridge as from the Association.

From Section H (Anthropology).

That the Association views with appreciation the recognition by the Commonwealth Government of Australia of the value of the application of scientific anthropological methods to the solution of the problems associated with the aborigines, and the renewed efforts directed to the preservation of the remaining native tribes.

Inasmuch as it is now generally recognised that a thorough knowledge of the material and spiritual life of a people is the best approach for solving the problems connected with the administration of native affairs and in view of the rapid decline of the Australian aborigines and the great value to Science, both now and in centuries to come, of the information about their languages and cultures that might still be collected by scientific field-work during the next fifteen or twenty years, it is urgently necessary that the research work financed for ten years by the Rockefeller Foundation and subsequently by the Commonwealth Government should be continued.

It is respectfully suggested to the Commonwealth Government that the best method of safeguarding the remaining tribal natives, in particular those of Arnhem Land, who are still in possession of their own culture, would be to segregate them effectively from all alien influences pending the establishment of a settled uniform policy for the treatment of the whole of the natives of Australia.

From Section H (Anthropology).

The Committee of Section H desire to maintain on record their resolution adopted last year to the effect that in view of the importance of anthropology as a means of promoting concord and understanding between men of different traditions, the British Association earnestly recommends to the Secretary of State for India that anthropology should be made a compulsory subject of study in the training of all probationers appointed to proceed to India and Burma.

The Committee understand that it has been found inexpedient to give effect to this resolution during the past year, but they trust that the Council will ask the Government of India to re-consider the matter sympathetically whenever opportunity may arise.

British Association for the Advancement of Science.

CAMBRIDGE: 1938

THE PRESIDENTIAL ADDRESS

PART I

VISION IN NATURE AND VISION AIDED BY SCIENCE

PART II

SCIENCE AND WARFARE

BY

THE RT. HON. LORD RAYLEIGH, Sc.D., LL.D., F.R.S.
PRESIDENT OF THE ASSOCIATION.

I.

VISION, AND ITS ARTIFICIAL AIDS AND SUBSTITUTES.

THE last occasion that the British Association met at Cambridge was in 1904, under the presidency of my revered relative, Lord Balfour, who at the time actually held the position of Prime Minister. That a Prime Minister should find it possible to undertake this additional burthen brings home to us how much the pace has quickened in national activities, and I may add, anxieties, between that time and this.

Lord Balfour in his introductory remarks recalled the large share which Cambridge had had in the development of physics from the time of Newton down to that of J. J. Thomson and the scientific school centred in the Cavendish Laboratory, ' whose physical speculations,' he said, ' bid fair to render the closing year of the old century and the opening ones of the new as notable as the greatest which have preceded them.' It is a great pleasure to me, as I am sure it is to all of you, that my old master is with us here to-night.

as he was on that occasion. I can say in his presence that the lapse of time has not failed to justify Lord Balfour's words. What was then an intelligent anticipation is now an historical fact.

I wish I could proceed on an equally cheerful note. The reputation of the scientific school in the Cavendish Laboratory has been more than sustained in the interval under the leadership of one whose friendly presence we all miss to-night. The death of Ernest Rutherford leaves a blank which we can never hope to see entirely filled in our day. We know that the whole scientific world joins with us in mourning his loss.

Lord Balfour's address was devoted to topics which had long been of profound interest to him. He was one of the first to compare the world picture drawn by science and the world picture drawn by the crude application of the senses, and he emphasised the contrast between them. A quotation from his address will serve as an appropriate text to introduce the point of view which I wish to develop this evening.

'So far,' he said, 'as natural science can tell us, every quality of sense or intellect which does not help us to fight, to eat, and to bring up our children, is but a by-product of the qualities which do. Our organs of sense perception were not given us for purposes of research . . . either because too direct a vision of physical reality was a hindrance, not a help in the struggle for existence . . . or because with so imperfect a material as living tissue no better result could be attained.'

Some of those who learn the results of modern science from a standpoint of general or philosophical interest come away, I believe, with the impression that what the senses tell us about the external world is shown to be altogether misleading. They learn, for example, that the apparent or space-filling quality of the objects called solid or liquid is a delusion, and that the volume of space occupied is held to be very small compared with that which remains vacant in between. This is in such violent contrast with what direct observation seems to show that they believe they are asked to give up the general position that what we learn from our senses must be our main guide in studying the nature of things.

Now this is in complete contrast with the standpoint of the experimental philosopher. He knows very well that in his work he does and must trust in the last resort almost entirely to what can be seen, and that his knowledge of the external world is based upon it: and I do not think that even the metaphysician claims that we can learn much in any other way. It is true that the conclusions of modern science seem at first sight to be very far removed from what our senses tell us. But on the whole the tendency of progress is to bring the more remote conclusions within the province of direct observation, even when at first sight they appeared to be hopelessly beyond it.

For example, at the time of Lord Balfour's address some who were regarded as leaders of scientific thought still urged that the conception of atoms was not to be taken literally. We now count the atoms by direct methods. We see the electrometer needle give a kick and we say, 'There goes an atom.' Or we see the path of an individual atom marked out by a cloud track and we see where it was abruptly bent by a violent collision with another atom.

Again, the theory of radioactive decomposition put forward by Rutherford, however cogent it may have seemed and did seem to those who were well acquainted with the evidence, was originally based on indirect inferences about quantities of matter far too small to be weighed on the most delicate balance. Chemists were naturally inclined to feel some reserve ; but in due course the theory led to a conclusion which could be tested by methods in which they had confidence—the conclusion, namely, that lead contained in old uranium minerals ought to have a lower atomic weight than ordinary lead and in all probability to be lighter, and on trying this out it proved to be so. More recently we have the discovery of heavy hydrogen with twice the density of ordinary hydrogen and heavy water which is the source of it.

Lastly, the conclusion that ordinary matter is not really space-filling has been illustrated by the discovery that certain stars have a density which is a fabulous multiple of the density of terrestrial matter. Although this is in some sense a deduction as distinguished from an observation, yet the steps required in the deduction are elementary ones entirely within the domain of the older physics.

This and many other points of view have seemed at first sight to contradict the direct indication of our senses. But it was not really so. They were obtained and could only be obtained by sense indications rightly interpreted. As in the passage from Lord Balfour already quoted the senses were not primarily developed for purposes of research, and we have in large measure to adapt them to that purpose by the use of artificial auxiliaries. The result of doing so is often to reveal a world which to the unaided senses seems paradoxical.

I have chosen for the main subject of this address a survey of some of the ways in which such adaptations have been made. I shall naturally try to interest you by dwelling most on aspects of the subject that have some novelty ; but apart from these there is much to be gleaned of historical interest, and when tempted I shall not hesitate to digress a little from methods and say something about results.

I shall begin with a glance at the mechanism of the human eye, so far as it is understood. I shall show how the compromise and balance between different competing considerations which is seen in its design can be artificially modified for special purposes. All engineering designs are a matter of compromise. You cannot have everything. The unassisted eye has a field of view extending nearly over a hemisphere. It gives an indication very quickly and allows comparatively rapid changes to be followed. It responds best to the wave-lengths actually most abundant in daylight or moonlight. This combination of qualities is ideal for what we believe to be nature's primary purpose, that is for finding subsistence under primitive conditions and for fighting the battle of life against natural enemies. But by sacrificing some of these qualities, and in particular the large field of view, we can enhance others for purposes of research. We may modify the lens system by artificial additions over a wide range for examining the very distant or the very small. We can supplement and enormously enhance the power of colour discrimination which nature has given us. By abandoning the use of the retina and substituting the photographic plate as an artificial retina, we can increase very largely the range of spectrum which can be utilised. This last extension has its special possibilities, particularly in the direction of using waves smaller than ordinary, even down to those which are associated with a moving electron. By using the photoelectric cell as another substitute for the retina with electric wire instead of optic nerve and a recording galvanometer instead of the brain we can make the impressions metrical and can record them on paper. We can count photons and other particulate forms of energy as well. We can explore the structure of atoms, examine the disintegration of radioactive bodies, and trace out the mutual relation of the elements. Indeed, by elaborating this train of thought a little further almost the whole range of observational science could be covered. But within the compass of an hour or so one must not be too ambitious. It is not my purpose to stray very far from what might, by a slight stretch of language, fall under the heading of extending the powers of the eye.

Most people who have a smattering of science now know the comparison of the eye with the camera obscura, or better, with the modern photographic camera—with its lens, iris, diaphragm, focussing adjustment and ground glass screen, the latter corresponding to the retina. The comparison does not go very far, for it does not enter upon how the message is conveyed to the brain and apprehended by the mind; or even upon the minor mystery of how colours are discriminated. Nevertheless, it would be a great mistake to suppose that the knowledge which is embodied in this comparison was easily arrived at. For example, many acute minds in antiquity thought that light originated in the eye rather than in the object viewed. Euclid in his optics perhaps used this as a mathematical fiction practically equivalent to the modern one of reversing the course of a ray, but other authors appealed to the apparent glow of animal eyes by lamplight, which shows that they took the theory quite literally. The Arabian author Alhazen had more correct ideas and he gave an anatomical description of the eye, but apparently regarded what we call the crystalline lens as the light-sensitive organ. Kepler was the first to take the modern view of the eye.

The detailed structure of the retina, and its connection with the optic nerve, has required the highest skill of histologists in interpreting difficult and uncertain indications. The light-sensitive elements are of two kinds, the rods and cones. The rods seem to be the only ones used in night vision, and do not distinguish colours. The cones are most important in the centre of the field of view, where vision is most acute, and it seems to be fairly certain that in the foveal region each cone has its own individual nervous communication with the brain. On the other hand, there is not anything like room in the cross-section of the optic nerve to allow us to assign a different nerve fibre to each of the millions of rods. A single fibre probably has to serve 200 of them.

The nervous impulse is believed to travel in the optic nerve as in any other nerve, but what happens to it when it arrives at the brain is a question for the investigators of a future generation.

The use of lenses is one of the greatest scientific discoveries: we do not know who made it. Indeed, the more closely we inquire into this question the vaguer it becomes. Spectacle lenses as we know them are a mediæval invention, dating from about A.D. 1280. Whether they originated from some isolated thinker and experimentalist of the type of Roger Bacon, or whether they were developed by the ingenuity of urban craftsmen, can hardly be considered certain. There are several ways in which the suggestion might have arisen, but a glass bulb filled with water is the most likely. Indeed, considering that such bulbs were undoubtedly used as burning glasses in the ancient world, and that the use of them for reading small and difficult lettering is explicitly mentioned by Seneca, it seems rather strange that the next step was not taken in antiquity. Apparently the explanation is that the magnification was attributed to the nature of the water rather than to its shape. At all events, it may readily be verified that a 4- or 5-inch glass flask full of water, though not very convenient to handle, will give a long-sighted newspaper reader the same help that he could get from a monocle.

The invention of lenses was a necessary preliminary to the invention of the telescope, for, as Huygens remarked, it would require a superhuman genius to make the invention theoretically.

The retina of the eye on which the image is to be received has structure. We may compare the picture on the retina to a design embroidered in woolwork, which also has a structure. Clearly such a design cannot embody details which are smaller than the mesh of the canvas which is to carry the coloured stitches. The only way to get in more detail is to make the design, or rather such diminished part of it as the canvas can accommodate, on a larger scale. Similarly with the picture on the retina. The individual rods and cones correspond with the individual meshes of the canvas. If we want more detail of an object we must make the picture on the retina larger, with the necessary sacrifice of the field of view. If the object is distant we want for this a lens of longer focus instead of the eye lens. We cannot take the eye lens away, but, what amounts to nearly the same thing, we can neutralise it by a concave lens of equal power put right up to it, called the eyepiece. Then we are free to use a long focus lens called the telescopic objective to make a larger picture on the retina. It must of course be put at the proper distance out to make a distinct picture. This is a special case of the Galilean telescope which lends itself to simple description. It is of no use to make the picture larger if we lose definition in the process. The enlarged image must remain sharp enough to take advantage of the fine structure of the retinal screen that is to receive it. It will not be sharp enough unless we make the lens of greater diameter than the eye. Another reason for using a large lens is to avoid a loss of brightness.

It seems paradoxical that the image of a star should be smaller the larger the telescope. Nevertheless it is a necessary result of the wave character of light. We cannot see the true nature of, for example, a double star unless the two images are small enough not to overlap and far enough apart to fall on separated elements of the observer's retina.

When the problem is to examine small objects we look at them as close as we can: here the short-sighted observer has an advantage. By adding a lens in front of the eye lens to increase its power we can produce a kind of artificial short sight and get closer than we could otherwise, so that the picture on the retina is bigger. This is a simple microscope and we can use it to examine the image produced by an objective lens; if this image is larger than the object under examination we call the whole arrangement a compound microscope.

Given perfect construction there is no limit in theory to what a telescope can do in revealing distant worlds. It is only a question of making it large enough. On the other hand, there is a very definite limit to what the microscope used with, say, ordinary daylight can do. It is not that there is any difficulty in making it magnify as much as we like. This can be done, e.g., by making the tube of the microscope longer. The trouble is that beyond a certain point magnification does no good. Many people find this a hard saying, but it must be remembered that a large image is not necessarily a good image. We are up against the same difficulty as before. A point on the object is necessarily spread out into a disc in the image, due to the coarseness of structure of light itself as indicated by its wave-length. I cannot go into the details, but many of you will know that points on the object which are something less than half a wave-length, or say a one-hundred-thousandth of an inch apart, cannot be distinctly separated. This is the theoretical limit for a microscope using ordinary light, and it has been practically reached. The early microscopists would have thought this more than satisfactory ; but the limit puts a serious obstacle in the way of biological and medical progress to-day. For example, the pathogenic bacteria in many cases are about this size or less ; and there is special interest in considering in what directions we may hope to go further.

Since microscopic resolution depends on having a fine structure in the light itself, something, though not perhaps very much, may be gained by the use of ultra-violet light instead of visible light. It then becomes necessary to work by photography. We are nearing the region of the spectrum where almost everything is opaque. In the visual region nearly every organic structure is transparent and to get contrast stains have to be used which colour one part more deeply than the other. In the ultra-violet, on the other hand, we get contrast without staining and, as Mr. J. W. Barnard has shown, the advantage lies as much in this as in the increased resolving power. For example, using the strong ultra-violet line of the mercury vapour lamp, which has about half the wave-length of green light, he finds that a virus contained within a cell shows up as a highly absorptive body in contrast with the less absorptive elements of the cell. So that ultra-violet microscopy offers some hope of progress in connection with this fundamental problem of the nature of viruses.

With ultra-violet microscopy we have gone as far as we can in using short waves with ordinary lenses made of matter, for the available kinds of matter are useless for shorter waves than these, and it might well seem that we have here come to a definite and final end. Yet it is not so. There are two alternatives, which we must consider separately. Paradoxical as it may seem, for certain radiations we can make converging lenses out of empty space ; or alternatively we can make optical observations without any lenses at all.

The long-standing controversy which raged in the nineties of the last century as to whether cathode rays consisted of waves or of electrified particles was thought to have been settled in favour of the latter alternative. But scientific controversies, however acutely they may rage for a time, are apt, like industrial disputes, to end in compromise ; and it has been so in this instance. According to our present views the cathode rays in one aspect consist of a stream of electrified particles ; in another, they consist of wave trains, the length being variable in inverse relation to the momentum of the particles.

Now cathode rays have the property of being bent by electric or magnetic forces, and far-reaching analogies have been traced between this bending and the refraction of light by solids ; indeed, a system of ' electron optics ' has been elaborated which shows how a beam of cathode rays issuing from a point can be reassembled into an image by passing through a localised electrostatic or magnetic field having axial symmetry. This constitutes what has been called an electrostatic or magnetic lens. It is then possible to form a magnified image of the source of electrons on a fluorescent screen, and that is the simplest application. But we can go further and form an image of an obstructing object such as a fine wire by means of one magnetic lens, acting as objective, and amplify it by means of a second magnetic lens, which is spoken of as the eyepiece, though of course it is only such by analogy, for the eye cannot deal directly with cathode rays. The eyepiece projects the image on to a fluorescent screen, or photographic plate. So far we have been thinking of the electron stream in its corpuscular aspect. But we must turn to the wave aspect when it comes to consideration of theoretical resolving power. The wave-length associated with an electron stream of moderate velocity is so small that if the electron microscope could be brought to the perfection of the optical microscope, it should be able to resolve the actual atomic structure of crystals. This is very far indeed from being attained, the present electron microscope being much further from its own ideal than were the earliest optical microscopes. Nevertheless experimental instruments have been constructed which have a resolving power several times better than the modern optical microscope. The difficulty is to apply them to practical biological problems.

It is not to be supposed that the histological technique so skilfully elaborated for ordinary microscopy can at once be transferred to the electron microscope. For example, the relatively thick glass supports and covers ordinarily used are out of the question. Staining with aniline dyes is probably of little use, and the fierce bombardment to which the delicate specimen is necessarily exposed will be no small obstacle. Certain standard methods, however, such as impregnation with osmium, seem to be applicable : and there is some possibility that eventually the obscure region between the smallest organisms and the largest crystalline structure may be explored by electron microscopy.

In referring to the limitations on the use of lenses I mentioned the other alternative that we might, in order to work with the shortest waves, dispense with lenses altogether : and in fact in using X-rays this is done. We are then limited to controlling the course of the rays by means of tubes or pinholes. This restriction is so serious that it altogether defeats the possibility of constructing a useful X-ray microscope analogous to the optical or the electron microscope. In spite of this the use of X-rays is of fundamental value for dealing with a particular class of objects, namely, crystals, which themselves have a regular spacing, comparable in size with the length of the waves. Just as the spacing of a ruled grating (say one 1/20,000th of an inch) can be compared with the wave-length of light by measuring the angle of diffraction, so the spacing of atoms in a crystal can be compared with the wave-length of X-rays. But here the indications are less direct than with the microscope, and depend on the object having a periodic structure. So that the method hardly falls within the scope of this address. How essential the difference is will appear if we consider that the angle to be observed becomes greater and not less the closer the spacing of the object under test.

Colour vision is one of nature's most wonderful achievements, though custom often prevents our perceiving the wonder of it. We take it for granted that anyone should readily distinguish the berries on a holly bush, and we are inclined to be derisive of a colour-blind person who cannot do so. But so far anatomy has told us little or nothing of how the marvel is achieved. Experiments on colour vision show that three separate and fundamental colour sensations exist. It is probable that the cones of the retina are responsible for colour vision and the rods for dark adapted vision which does not discriminate colour. But no division of the cones into three separate kinds corresponding to the three colour sensations has ever been observed. Nor is any anatomical peculiarity known which allows a colour-blind eye to be distinguished from a normal one.

Can artificial resources help to improve colour discrimination ? In some interesting cases they can. Indeed, the whole subject of spectroscopy may be thought of as coming under this head. We can recognise the colour imparted by sodium to a flame without artificial help. When potassium is present as well, the red colour due to it can only be seen when we use a prism to separate the red image of the flame from the yellow one. Such a method has its limitations, because if the coloured images are more numerous they overlap, and the desired separation is lost. To avoid this it is necessary to make a sacrifice, and to limit the effective breadth of the flame by a more or less narrow slit. And if the images are very numerous the slit has to be so narrow that all indication of the breadth of the source is lost. This, of course, is substantially the method of spectroscopy, into which I do not enter further. But there is an interesting class of cases where we cannot afford to sacrifice the form of the object entirely to colour discrimination. Consider, for example, the prominences of the sun's limb, which are so well seen against the darkened sky of an eclipse, but are altogether lost in the glare of the sky at other times. In order to see them prismatic dispersion is made use of, and separates the monochromatic red light of hydrogen from the sky background. A slit must be used to cut off the latter : but if it is too narrow the outlines of the prominence cannot be seen. By using a compromise width it is possible to reconcile the competing requirements in this comparatively easy case. Indeed, M. B. Lyot, working in the clear air of the observatory of the Pic du Midi, where there is less false light to deal with, has even been able to observe the prominences through a suitable red filter, which enables the whole circumference of the sun to be examined at once, without the limitations introduced by a slit. A much more difficult problem is to look for bright hydrogen eruptions projected on the sun's disc, and at first sight this might well seem hopeless. A complete view of them was first obtained by photography, but I shall limit myself to some notice of the visual instrument perfected by Hale and called by him the spectrohelioscope. A very narrow slit has to be used, and hence only a very small breadth of the sun's surface can be seen at any one instant. But the difficulty is turned by very rapidly exposing to view successive strips of the sun's surface side by side. The images then blend, owing to persistence of vision, and a reasonably broad region is included in what is practically a single view. I must pass over the details of mechanism by which this is carried out.

There are now a number of spectrohelioscopes over different parts of the world, and a continuous watch is kept for bright eruptions of the red hydrogen lines. Already these are found to be simultaneous with the 'fading' of short radio waves over the illuminated hemisphere of the earth, and the brightest eruptions are simultaneous with disturbances of terrestrial magnetism. At the Mount Wilson Observatory such eruptions have been seen at the same time at widely separated points on the sun, indicating a deep-seated cause. There are therefore very interesting and fundamental questions within the realm of this method of investigation.

We have so far been mainly considering how we may adapt our vision for objects too small or too far off for unassisted sight, and for colour differences not ordinarily perceptible. This is chiefly done by supplementing the lens system of the eye by additional lenses or by prisms. We cannot supplement the retina, but in certain cases we can do better. We can substitute an artificial sensitive surface which may be either photographic or photoelectric.

That certain pigments are bleached by light is an observation that must have obtruded itself from very early times—indeed, it is one of the chief practical problems of dyeing to select pigments which do not fade rapidly. If a part of the coloured surface is protected by an opaque object—say a picture or a mirror hanging over a coloured wallpaper—we get a silhouette of the protecting object, which is in essence a photograph.

Again, it is a matter of common observation that the human skin is darkened by the prolonged action of the sun's light, and here similarly we may get what is really a silhouette photograph of a locket, or the like, which protects the skin locally. In this case we are perhaps retracing the paths which Nature herself has taken : for the evolution of the eye is regarded as having begun with the general sensitiveness to light of the whole surface of the organism.

The sensitivity of at all events the dark adapted eye depends on the accumulation on the retinal rods of the pigment called the visual purple, of which the most striking characteristic is its ready bleaching by light. We can even partially ' fix ' the picture produced in this way on the retina of, for example, a frog by means of alum solution. This brings home to us how clearly akin are the processes in the retina to those in the photographic plate, even though the complexity of the former has hitherto largely baffled investigation.

There are then many indications in nature of substances sensitive to light, and quite a considerable variety of them have from time to time been used in practical photographic processes. But compounds of silver, which formed the basis of the earliest processes, have maintained the lead over all others. The history of photography by means of silver salts cannot be considered a good example of the triumph of the rational over the empirical. For instance, the discovery of developers came about thus. The first workers, Wedgewood and Davy (1802), had found that they got greater sensitivity by spreading the silver salt on white leather instead of paper. An early experimenter, the Rev. J. B. Reade (1837), was anxious to repeat this experiment, and sacrificed a pair of white kid gloves belonging to his wife for the purpose. When he wished to sacrifice a second pair, the lady raised a not unnatural objection, and he said, ' Then I will tan paper.' He treated paper with an infusion of oak galls and found that this increased the sensitivity greatly. It amounted to what we should call exposing and developing simultaneously. But, in using the method, it is easily observed that darkening continues after exposure is over, and this leads to beginning development after the exposure. This step was taken by Fox Talbot a year or two afterwards. Instead of crude infusion of galls he used gallic acid. Later pyrogallic acid was used instead of gallic acid, and still survives.

The use of gelatine as a medium to contain the silver halide was a more obvious idea. But it was not so easy to foresee that the sensitivity of silver salts would be much further increased when they were held in this medium. For long this remained unexplained, until it was noticed that some specimens of gelatine were much more active than others. This was ultimately traced by S. E. Sheppard to the presence of traces of mustard oil, a sulphur compound, in the more active specimens. This, in turn, depends in all probability on the pasturage on which the animals that afford the gelatine have been fed. The quantity present is incredibly small, comparable in quantity with the radium in pitchblende.

The value to science as well as to daily life of the gelatine dry plate or film can hardly be overestimated. Take, for instance, the generalised principle of relativity, which attempts with considerable success to reduce the main feature of the cosmical process to a geometrical theory. The crucial test requires us to investigate the gravitational bending of light, by photographing the field of stars near the eclipsed sun. For this purpose the gelatine dry plate has been essential: and here, as we have seen, we get into complicated questions of bio-chemistry. This is to my mind a beautiful example of the interdependence of different branches of science and of the disadvantages of undue specialisation (or should I say generalisation?). We may attempt to reduce the cosmos to the dry bones of a geometrical theory, but in testing the theory we are compelled to have recourse again to the gelatine which we have discarded from the dry bones!

To come back, however, to the development of the photographic retina, as I may call it. As is well known, the eye has maximum sensitivity to the yellow-green of the spectrum, but ordinary silver salts are not sensitive in this region. Their maximum is in the blue or violet, and ranges on through ultra-violet to the X-ray region. It was not at all easy to extend it on the other side through green, yellow and red to infra-red. The story of how this was ultimately attained is one more example in the chapter of accidental clues skilfully followed up which forms the history of this subject.

In 1873, Dr. Hermann Vogel, of Berlin, noticed that certain collodion plates of English manufacture, which he was using for spectrum photography, recorded the green of the spectrum to which the simple silver salts are practically insensitive. The plates had been coated with a mixture which contained nitrate of uranium, gum, gallic acid and a yellow colouring matter. What the purpose of this coating was is not very obvious. It rather reminds one of mediæval medical prescriptions which made up in complexity what they lacked in clear thinking. But Vogel concluded with true scientific insight that it must owe the special property he had discovered to some constituent which absorbs the green of the spectrum more than the blue : for conservation of energy requires that the green should be absorbed if it is to act on the plate. He then tried staining the plate with coralline red, which has an absorption band in the green, with the expected result. With much prescience he says : ‘ I think I am pretty well justified in inferring that we are in a position *to render bromide of silver sensitive for any colour we choose.* Perhaps we may even arrive at this, namely photographing the ultra-red as we have already photographed the ultra-violet.’ It was, however, half a century before this far-seeing prophecy was fully realised. The development of the aniline colour industry gave full scope for experiment, but it has been found by bitter experience that dyes which can produce the colour sensitiveness are often fatal to the clean working and keeping qualities of the plate. However, success has been attained, largely by the efforts of Dr. W. H. Mills, of the chemical department of this University, and of Dr. Mees, of the Kodak Company ; and we all see the fruits of it in the photographs by lamplight which are often reproduced in the newspapers.

It is now known in what direction the molecular structure of the sensitising dye must be elaborated in order to push the action further and further into the infra-red, and the point when water becomes opaque has nearly been reached, with great extension of our knowledge of the solar spectrum. The spectra of the major planets have also been extended into the infra-red, and this has given the clue as to the true origin of the mysterious absorption bands due to their atmospheres, which had baffled spectroscopists for more than a generation. These bands have been shown by Wildt to be due to methane or marsh gas. Neptune, for example, has an atmosphere of methane equivalent to 25 miles thickness of the gas under standard conditions. In this Neptunian methane we have a paraffin certainly not of animal or vegetable origin ; and I venture in passing to make the suggestion that geologists might usefully take it into consideration in discussing the origin of terrestrial petroleum.

The photographic plate is not the only useful substitute for the human retina. We have another in the photoelectric surface. The history of this discovery is of considerable interest. Heinrich Hertz, in his pioneering investigation of electric waves (1887), made use of the tiny spark which he obtained from his receiving circuit as an indicator. The younger part of my audience must remember that this was before the days of valves and loud speakers. His experiments were done within the walls of one room. When he boxed in the indicating spark so as to shield it from daylight and make it easier to see, he found that this precaution had exactly the opposite effect—the spark became less instead of more conspicuous. To express it shortly and colloquially, this action was found to depend on whether or not the spark of the receiver could see the spark of the oscillator. Moreover, seeing through a glass window would not do. It was ultra-violet light from the active spark that influenced the passive spark. Further, Hertz was able to determine that the action occurred mainly, if not entirely, at the cathode of the passive spark.

The next step was taken by Hallwachs, who showed that it was not necessary to work with the complicated conditions of the spark. He found that a clean zinc plate negatively charged rapidly lost its charge when illuminated by ultra-violet light.

The final important step was in the use of a clean surface of alkali metal *in vacuo* which responds to visible light and passes comparatively large currents. This constitutes the photoelectric cell very much as we now have it, and was due to two German schoolmasters, J. Elster and H. Geitel. English physicists who met them during their visit to Cambridge a generation ago will not fail to have agreeable memories of their single-minded enthusiasm and devoted mutual regard. Sir J. J. Thomson has recalled them to our recollection in his recent book. They could scarcely have foreseen that their work, carried out in a purely academic spirit, would make possible the talking films which give pleasure to untold millions.

The sensitiveness of the dark-adapted eye has often been referred to as one of its most wonderful features; but, under favourable conditions, the sensitivity of a photoelectric surface may even be superior. According to our present ideas, no device conceivable could do more than detect every quantum which fell upon it. Neither the eye nor the photoelectric surface comes very near to this standard, but it would seem that the falling short is rather in detail than in principle. The action of the photoelectric cell depends on the liberation of an electron by one quantum of incident energy, and under favourable conditions the liberation of one electron can be detected, by an application of the principle of Geiger's counter. The action of the dark-adapted eye depends on the bleaching of the visual purple. According to the results of Dartnell, Goodeve and Lythgoe it appears likely that one quantum can bleach a molecule of this substance, and in all probability this results in the excitation of a nerve fibre, which carries its message to the brain.

The photoelectric cell can be used like the photographic plate at the focus of an astronomical telescope. It might seem from the standpoint of evolution a retrograde step to substitute a single sensitive element for the 137 million such elements in the human eye. In this connection it is interesting to note that in certain invertebrate animals eyes are known which have the character of a single sensitive element, with a lens to concentrate the light upon it. Such an eye can do little more than distinguish light from darkness. But its artificial counterpart using the photoelectric surface has the valuable property that the electric current which indicates that light is falling upon it can be precisely measured, so as to determine the intensity of the light. In contrast with photographic action, the energy available to produce the record comes not from the original source of light, which only, as it were, pulls the trigger, but from the battery in the local circuit, and it may be amplified so as to actuate robust mechanisms. It has been applied with success to guiding a large telescope or, in a humbler sphere, to open doors, or even to catch thieves.

However, the scientific interest lies more in the possibility of accurate measurement. As an interesting example we might take the problem of measuring the apparent diameter of the great nebula in Andromeda. As is known, modern research tends to indicate that the Andromeda nebula and other like systems are the counterparts of the galaxy, being in fact island universes. But until lately there was such a serious difficulty in that all such systems appeared to be considerably smaller than the galaxy. Stebbins and Whitford, by traversing a telescope armed with a photoelectric cell across the nebula, have found that its linear dimensions were twice as great as had been supposed, reducing the discrepancy of size to comparatively little.

But, it may be suggested, could we not go further and make a photoelectric equivalent, not only for the rudimentary kind of eye which has only a single sensitive element, but for the developed mammalian eye which has an enormous number? Could we not build up on separated photoelectric elements a complete and detailed picture? In point of fact this has been done in the development of television; and since this new art which interests us all can properly be considered as an extension of the powers of normal vision, no excuse is needed for devoting some consideration to it. We must divide the photoelectric surface into minute patches which are electrically insulated from one another. This is not too difficult; but if it were proposed directly to imitate nature, and attach a wire, representing a nerve fibre, to each of these patches, so as to connect it to the auxiliary apparatus, we might well despair of the task; for there are probably half a million such connections between the human retina and the brain. In the artificial apparatus for television, one single connection is made to serve, but it is in effect attached to each of the patches in rapid succession by the process of 'scanning' the image. The photoelectric mosaic is on one side of a thin mica sheet, and a continuous metal coating on the other side gives the connection, which is by electrostatic induction. Each element of the surface forms a separate tiny condenser with the opposing part of the back plate. Scanning is achieved by rapidly traversing a beam of electrons over the mosaic line by line. The whole surface, and therefore each element, must be scanned at least twenty times a second. In the intervals an element is losing electrons more or less rapidly. The scanning beam comes along, and restores the lost electrons, discharges the little condenser formed by the element and the back plate and sends an electric signal into the wire attached to this plate. The strength of this signal will depend on how many electrons the element had lost since the previous scanning, and thus on the luminous intensity of that part of the image. An important point is that the element is in action all the time, and not only while it is individually being scanned. We have thus transmuted the momentary picture into a series of electric pulses occupying in all a time of one-twentieth of a second, and these can be amplified and sent out as wireless signals. How are they to be turned back again into a visible picture at the other end ? Well, that is not perhaps so difficult as the first conversion of the picture into signals. We must make a beam of electrons follow and imitate the periodic movements of the scanning beam at the other end. The beam of electrons falls on a luminescent screen, and makes it light up, more or less brightly according to the intensity of the electron beam. If we use the incoming signals to modulate the electron beam, we can make them correspond with the intensities at the sending end, and the original picture is reconstructed piece by piece. The reconstruction is completed in one-twentieth of a second or less, and the process begins again. The successive pictures blend into one another as in the cinema, and movement is shown with apparent continuity.

It seems not unlikely that the electric eye or iconoscope, as it has been called, may have applications apart from television. Dr. V. K. Zworykin, who took an important part in its development, suggested that it might be used to make visible the image in the ultra-violet microscope, which would be much too faint for direct projection on a fluorescent screen. For that purpose the sending and receiving apparatus would, of course, be connected directly, without radio transmission. It might also be used for rapid photography, if the photographic plate replaced the viewing screen. The beauty of the device is that the energy is supplied locally, the distant light source merely releasing it. The principle of amplification may thus perhaps be applied to the photographing of faint objects.

I come to the close of this part of my subject.

Much of modern scientific doctrine appears at first sight to have an elusive and even metaphysical character, and this aspect of it seems to make the strongest appeal to many cultivated minds. Yet upon the whole, the main triumphs of science lie in the tangible facts which it has revealed ; and it is these which will without doubt endure as a permanent memorial to our epoch. My main thesis has been that these are discovered by methods not essentially different from direct scrutiny. It is hoped that the present survey may remind you that if we allow for a reasonable broadening of the original meaning of the words, it remains true after all that 'seeing is believing.'

II.

Science and Warfare.

During the Great War itself, few scientific men in any country doubted that it was their duty to do what they could to apply their specialised knowledge to the purposes of war; nor was it often suggested by publicists that there was any countervailing consideration: on the contrary they urged strongly that our resources in this direction should be efficiently mobilised. It is chiefly in vague general discussions that the opposite view becomes vocal.

Science, it is urged, is the source of all the trouble: and we may look to scientific men for some constructive contribution to finding a remedy. It is worth while to inquire what basis there is for this indictment, and whether, in fact, it is feasible for men of science to desist from labours which may have a disastrous outcome, or at any rate to help in guiding other men to use and not to abuse the fruits of those labours. I may say at the outset that I have no sanguine contribution to make. I believe that the whole idea that scientific men are specially responsible is a delusion born of imperfect knowledge of the real course of the process of discovery. Indeed, very much the same complaint was made before the scientific era. Let me refer you to Shakespeare's play of *Henry IV*:—

> 'Great pity, so it was
> This villainous saltpetre should be digged
> Out of the bowels of the harmless earth
> Which many a good tall fellow had destroyed
> So cowardly.'

The quotation leads us to inquire how far the further development of this particular kind of frightfulness into modern high explosives was deliberate or not.

In the course of systematic study of the chemistry of carbon compounds it was inevitable that the action of nitric acid on substances like benzene, toluene, glycerine, cellulose and the like should be tried. No one could foresee the result. In the case of benzene, we have nitrobenzene, the key to the aniline dye industry. In the case of glycerine, Sobrero obtained in 1846 the highly explosive liquid called nitro-glycerine. He meant no harm, and in fact his discovery lay dormant for many years, until Nobel turned his attention to the matter in 1863, and showed how by mixing nitro-glycerine with other substances, solid explosives could be made which admitted of safe handling. Dynamite was one of them. They proved invaluable in the arts of peace, e.g. in mining and in making railway tunnels, such as those through the Alps. They were used by the Irish Fenians in the dynamite outrages of the eighties. These attempted outrages were not very successful, and so far as I know no one was inclined to blame science for them, any more than for the Gunpowder Plot. Like the latter, they came to be considered slightly comic. If anyone doubts this, he may agreeably resolve his doubts by reading R. L. Stevenson's story *The Dynamiter*. At all events, high explosives had been too long in use in peaceful industry for their misuse to be laid directly to the account of science.

Coming next to poison gas. We read that Pliny was overwhelmed and killed by sulphur dioxide in the eruption of Vesuvius in A.D. 79. During the Crimean War, the veteran admiral Lord Dundonald urged that the fumes of burning sulphur should be deliberately used in this way, but the suggestion was not adopted. Even if it had been, scientific research *ad hoc* would obviously have had little to do with the matter. During the Great War, chlorine was used on a large scale. I need hardly insist that chlorine was not isolated by chemists for this purpose. It was discovered 140 years before, as a step in the inquiry into the nature of common salt.

Coming to the more recondite substances, we may take mustard gas—really a liquid—as typical. It is much more plausible to suggest that here was a scientific devilment, deliberately contrived to cripple and destroy. But what are the real facts ?

Referring to Watts's *Dictionary of Chemistry* (edition of 1894), there is an article of less than forty words about mustard gas (under the heading of dichlordiethyl sulphide). After the method of preparation used by Victor Meyer has been mentioned, the substance is dismissed with the words ' oil, very poisonous and violently inflames the skin. Difference from diethyl sulphide.'

There are sixteen other compounds described at comparable length on the same page. So far as I know, none of them is of any importance. A not uncommon type of critic would probably say that the investigation of them had been useless, the work of unpractical dreamers, who might have been better employed. One of these substances, namely mustard gas, is quite unexpectedly applied to war, and the production of it is held by the critics to be the work not of dreamers, but of fiends whose activities ought to be suppressed ! Finally, at the bottom of the page begins a long article on chloroform. This substance, as you know, has relieved a great deal of pain, and on the same principle the investigator who produced it was no doubt an angel of mercy. The trouble is that all the investigators proceeded in exactly the same spirit, the spirit that is of scientific curiosity, and with no possibility of telling whether the issue of their work would prove them to be fiends, or dreamers, or angels.

Again, there is the terror of thermite incendiary bombs, spreading fire broadcast through our great cities. The notion is sometimes encountered that thermite was invented for this purpose. Nothing could be further from the truth. I first made acquaintance with it myself in 1901 by hearing a lecture at the Royal Institution by the late Sir William Roberts Austen on 'Metals as Fuel.'[1] He drew attention to the great amount of energy which was liberated when aluminium combined with oxygen, and showed how aluminium powder mixed with red oxide of iron would react violently with it, withdrawing the oxygen from the iron, and becoming brilliantly incandescent in the process. He showed further how this mixture, called thermite, could be used for heating metal work locally, so as to make welds, e.g. in joining two iron pipes end to end. I venture to say that it never occurred to him or to any of his hearers that thermite had any application in war.

In discussions of this kind a distinction is often implied between what I may call old-fashioned knowledge and modern scientific knowledge. The latter is considered to be the special handmaid of 'frightfulness.' The futility of this distinction is easily seen by considering a special case. Iron is thought of as belonging to the pre-scientific era, while aluminium is thought to belong to the scientific era. From the standpoint of chemistry both are metals, and the problem of producing them in either case is a chemical one. When produced they both have their function in 'frightfulness': iron to cut and stab; aluminium to make thermite bombs to burn and destroy. If modern science makes its contribution to 'frightfulness' in giving us aluminium, ancient craft did so in giving us iron. It is obviously absurd to make any distinction in principle between the two cases. Science properly understood includes all real knowledge about material things, whether that knowledge is old or new.

All these terrors have only become applicable against a civilian population by the development of aircraft. Military objects were certainly not the incentive of the successful pioneers of artificial flight. They were fascinated at first by the sport of gliding, and afterwards by a mechanical transport problem.

It is true that brilliant writers of imaginative fiction, such as Jules Verne and H. G. Wells, had foretold all, and more than all, the horrors that have since come to pass. But it is perhaps more to the point to inquire what were the contemporary views of practical men. The

[1] *Proc. R.I.*, Feb. 23, 1901, vol. xvi, p. 496.

Wrights made their first successful flight in 1903. In 1904 I myself heard the then First Sea Lord of the Admiralty repudiate with scorn the suggestion that the Government were interesting themselves in the matter ; and I know with equal definiteness that even as late as 1908 the Chief of the Imperial General Staff did not believe in the military importance of flight. Would it be fair then to blame the inventors for not having realised it, and for not having stayed their hands ?

Summing up what may be learnt from the experience of the past, I think we may say that the application of fundamental discoveries in science to purposes of war is altogether too remote for it to be possible to control such discoveries at the source.

For good or ill, the urge to explore the unknown is deep in the nature of some of us, and it will not be deterred by possible contingent results, which may not be, and generally are not, fully apparent till long after the death of the explorer. The world is ready to accept the gifts of science, and to use them for its own purposes. It is difficult to see any sign that it is ready to accept the advice of scientific men as to what those uses should be.

Can we then do nothing ? Frankly, I doubt whether we can do much, but there is one thing that may be attempted. The Association has under consideration a division for study of the social relations of science which will attempt to bring the steady light of scientific truth to bear on vexed questions. We rejoice to know that our distinguished American visitors are in sympathy with this aim, and we hope that our discussions with them will bear useful if modest fruit in promoting international amity.

SECTION A.—MATHEMATICAL AND PHYSICAL SCIENCES.

LOGIC AND PROBABILITY IN PHYSICS

ADDRESS BY

Dr. C. G. DARWIN, F.R.S.,

PRESIDENT OF THE SECTION.

Before coming to my main subject it is appropriate that I should begin by referring to the quite exceptional loss that physical science has incurred during the past year in the death of Lord Rutherford. It is not easy for contemporaries to judge what the future estimates of history will be, but in this case I think we may expect the verdict that he was the greatest of all experimental physicists—perhaps with the exception of Faraday. It was my good fortune to serve in his laboratory in Manchester during those days when he had got the subject of radioactivity into fair order, and was profiting by it to explore the structure of matter, the days when the α-particle was giving its best as a probe into the nature of atoms. I suppose that the most important thing that came out was the discovery of the nucleus. This arose out of investigations into the scattering of α-particles in a sheet of gold foil. A few of them were scattered through very much larger angles than they had any right to be, and from this hint Rutherford guessed that the atom must contain a powerful centre of force. I remember the occasion of a Sunday evening supper when he told us about it, in fact only a few minutes after he had worked out the consequences, and I remember being astonished at the use he could make of the vague recollections of what he had learnt at school about the hyperbola. It is easy for us now to say how reasonable it all was, because we have got used to atoms like that, but we have to remember that at the time it certainly caused a great deal of trouble. One single very rare phenomenon was explained, but every critical mind could point out endless objections. For example, there was the prime difficulty that until the advent of Bohr's theory there was nothing to hold the nuclear atom to a fixed size, a difficulty which was explained at any rate roughly by the older idea of a sphere of positive electricity; and then there was the trouble, which in fact worried Rutherford a good deal, as to how a β-particle could ever escape from such a strong attractive force. But he got it right; it was a process I have heard described by saying that if Rutherford went into a chemical laboratory for a reagent he would somehow always go to the right bottle even if there were no labels. Of his later work, disintegration and so on, I will not speak, but only refer to one characteristic he showed in it. This was his capacity for changing his methods. In the Manchester days the work was all done with astonishingly simple means—old tobacco tins for home-made electroscopes and so on—and it would have been easy for anyone to say, 'We have got many first-rate results out of these tobacco tins, and there are plenty more to get, so why change ?' There would have been plenty more to get, and he could have occupied a whole laboratory getting them, but all the time the world was going beyond them to costly apparatus on an engineering scale, which were beginning to yield results beyond the capacity of tobacco tins. Rutherford perceived this as early as anyone and was ready to undertake these engineering feats—and to collect the money to pay for them too. But I will say no more of this work, since it is only necessary to apply to him in this place and during our present week of meeting the words written on the tomb of another great man—*Si monumentum requiris, circumspice.*

When we try to assess the qualities, as opposed to the performance, of great men of science, we can make a dichotomy of them into two classes ; the dichotomy is of course not exact, but it divides them as well as such things do. There is the type of genius who seems to have been born with a knowledge of some branch of nature, so that he has only to grow up, learn to read and write, and then be told what the difficulties are in order to understand and explain them. Perhaps Lord Kelvin was the typical instance, for he seemed to know all about thermodynamics spontaneously at the age of 22. Then there is the opposite type of which the late Lord Rayleigh was an example, who seem not to have a natural understanding of things, but to know more precisely than common men what they do *not* understand and by mastering it to gain a very deep insight into nature. It is a question of whether it is easier to conquer the world by understanding it or by not understanding it. It is a matter of taste which type each of us prefers, whether the inspiration which seems to know what the world is like directly, or the careful induction, picking a cautious way between this difficulty and that objection. Each type may show the demerits of its quality, in the one case a certain inelasticity that follows the inspiration, in the other a sometimes too pedestrian rate of advance. On the principle that I like to see the machinery as well as its products, I confess that it is the second type that attracts me more. A rigid dichotomy of this kind is a misleading over-simplification, and most scientific men have shared the two characters in various proportions, but it is not hard to classify Rutherford. He had that quality of being prepared for anything, of making each discovery fit on to the last and suggest the next, of taking the world as he found it, which is typical of my second class, those whom I have called great by not understanding. But I must turn to my main subject, and will only add that the life and work of Rutherford is the best possible text I could choose for the kind of view which I want to put before you.

In choosing a theme for my address I was in some difficulty. The main subjects of present interest in physics, the nucleus of the atom, cosmic rays, and the phenomena at deep temperatures, are being dealt with in the discussions of our Section, so that they would be excluded even apart from the fact that I cannot speak on them with authority. It would have been possible for me to choose a narrower subject, but I could not feel that this would have possessed the general interest that such an occasion demands, and so with some trepidation I am venturing on an even wider theme and am going to touch on the philosophy of our subject. This is a dangerous thing to do for one who has never made more than the most superficial study of pure philosophy, but still I do not apologise for it, because it appears to me that recent scientific history has revealed a deep schism between the professional philosophers and the scientists, and this schism is worthy of examination.

General philosophy claims to be the critical subject which lays down for all of us what we may be allowed to think, and yet it has played no part whatever in the great revolutions of human thought of the present century—those connected with relativity and the quantum theory. It might have been expected that the scientists would have been constantly consulting the philosophers as to the legitimacy of their various speculations, but nothing of the kind has happened. Since no one can dispense with some sort of metaphysic, each scientist has made one for himself, and no doubt they contain many crudities, but it would seem that a deep interest in metaphysic is a disadvantage rather than an advantage to the physicist—at least I have the impression that those of my friends who are most inclined to speculate on the ultimate things appear to be the ones whose scientific work is most hampered by doing so. Now I propose to risk a similar indiscretion. I want to embody in it the practical philosophy of a physicist, and I do not mean it as an attack on the pure philosophers, who are very reasonable people, only chargeable with the minor offence of not having made me want to read their books!

I had better begin by stating shortly the ideas I intend to discuss. There is a notable contrast between the way we think about things and the way we think we ought to think about them. We have set up as an ideal form of reasoning the formal logic which has held the field since the days of Aristotle. We rarely conform to this ideal, but instead we usually make use of arguments having no accurate axiomatic basis, which compel belief because of some large accumulation of favourable evidence. I intend to develop the idea that the old logic was devised for a world that was thought to have hard outlines, and that, now that the new mechanics has shown that the outlines are not hard, the method of reasoning must be changed. The key to the modification has already long been in our hands in the principle of probability, but whereas in the past constant attempts were made to fit this into the old system, the new mechanics suggests the possibility of a different synthesis. Though I hope this subject will be found interesting in itself, I would not have ventured to bring it forward if I had not also a very practical purpose in doing so, and that is to urge that our mathematical education both at school and university has been gravely deficient in that it has put all the emphasis on matters susceptible of rigorous proof, while it has very completely neglected the equally important subjects of statistics and probability. I shall enter into these matters at the end of my address.

I have said that there is a contrast between the way we all think about things and the way we think we ought to think about them. This is true not merely of the scientist; the layman holds the same belief. I may exemplify this by a quotation from that epitome of the reasoned thought of the ordinary man, the detective story. After Watson has expressed admiration at one of the most brilliant guesses of Sherlock Holmes he is met with the reply: 'No, no; I never guess. It is a shocking habit, destructive of the logical faculty.' The reader is encouraged to revere the great detective by being told that all his arguments are Aristotelian syllogisms. The scientist forms his opinions in much the same way as Holmes really did, but he is apt to feel that this is a fault in himself and that he ought to be forming them by the severe principles of formal logic in the manner that appealed to Watson. Of course there are branches of knowledge for which this can be done, but somehow they are not the interesting ones; indeed, outside pure mathematics any subject is apt to become dead and uninteresting as soon as it is brought down to this form. The really live branches of physics call for a very different kind of thought, for a review of a system of interconnected facts and for a perception of conjectured analogies, and so on. This is vaguer, but it is more important, and our system ought to give importance to the important things, so that the actual habit of thought which the intelligent man finds the most useful is acknowledged as the right one.

In general literature there is a particular kind of writing which we all admire on account of its direct simplicity; it is to be found in much English of the seventeenth century, but specially in the work of many French authors, both early and late. It is a delight to read and often admirably achieves its aim of clarifying the subject-matter—but not always. There are two ways of writing simply about a subject. One is to understand and make clear the simplicity of it; the other is to leave out all the difficult parts. When we entertain the idea that everything can be brought down to the Aristotelian syllogism, are we not doing this last? Is it not possible that when a subject is brought down to these terms, it is merely that we have picked out from it the easy parts and concealed all the rest? If we turn our attention to the question of why we believe in our various theories, we can see that there is often a quite illusory simplicity in their presentation.

Why do we believe in the various theories that we are all agreed to accept? Once a theory has become well established someone usually gets to work to find a system of axioms, postulates, indefinables and so on from which it may be derived. For example, classical mechanics is based on Newton's Laws, or whatever system has been substituted for them by later criticism. The direct derivation of everything from an axiomatic basis has an attractive simplicity, but it tends to make us think we believe the theory because of the axioms, whereas the axioms are only a convenient shorthand summarising a wide field of information, and they are believed in merely because we believe in the theory. This may be seen by an occurrence of a few years ago. There was a letter to *Nature* pointing out a rather fundamental contradiction in the quantum theory—I do not think the author meant it as strongly as the accident of his wording implied. One's immediate feeling was that the idea must be wrong (as indeed it proved to be), but the point of my mentioning it is that I found I did not care much whether it was right or wrong, because the quantum theory must be right anyhow. The cumulative weight is so overwhelming that it is not conceivable that anyone could upset it in a single column of *Nature*. A little doctoring of the axioms would certainly put the matter right again, and hardly anyone would be any the better. We have then to believe that axioms are not important things, but that it is the whole body of accumulated doctrine that matters.

Take it from a different angle. The ' logic ' school of thought has in its repertory the idea of a ' crucial experiment,' that is the single experiment which gives the answer yes or no to a whole theory. I suppose the most striking crucial experiment ever done was the Michelson-Morley experiment on æther drift, which was made the basis of the whole gigantic theory of relativity. Michelson and Morley showed that to the order of the square of the earth's velocity there was no æther drift, and they showed it to the limits of the precision of their apparatus. For some twenty years the theory of relativity grew enormously, based on this one experiment, and then it was felt that it would be proper for some one else to repeat the work, and Dr. Dayton Miller undertook the task. We cannot see any reason to think that his work should be inferior to Michelson's, as he had at his disposal not only all the experience of Michelson's work, but also the very great technical improvements of the intervening period, but in fact he failed to verify the exact vanishing of the æther drift. What happened ? Nobody doubted relativity. There must therefore be some unknown source of error which had upset Miller's work. But as Miller was improving on Michelson, this contains the implication that Michelson's work must have had two unknown sources of error which happened to cancel one another. What has become of the crucial experiment ? We do not believe in relativity because of the Michelson-Morley experiment ; it is one, and an important one, of a number of cumulative pieces of evidence which all fit together, and it is this cumulation and not any one of its pieces that makes us believe in relativity.

From examples like these we conclude that an axiomatic basis, of the kind demanded for the operations of formal logic, is too narrow for the understanding of the physical world. Something wider is needed. Now for more than a century there has been growing up the recognition that probability plays a part in much reasoning, and that there must exist a wider system of logic which has probability as one of its features. Attempts have been made, and are still being made, to bring probability back into the narrow fold of the old logic. It appears to me that these attempts are hopeless, but before approaching the question directly I want to develop an analogy which seems to me important. Like everyone else I *feel* the compelling power of the old logic, and I cannot *feel* how when we try to go beyond it we can get the same compulsive force. But on the other hand I *know* of a case where our thoughts are driven in one direction by a force which seems to have the same psychological compulsion as does formal logic, and yet where the result is undoubtedly wrong.

To anyone who has thought at all seriously about the world, or at any rate to anyone who has made an elementary study of mechanics, I suppose there is nothing more absolute than the law of causality. By this I mean that the future is completely contained in the present. Passing over obvious examples where this is true, like the path of a projectile or the orbit of a planet, we may take an extreme case where we might expect our faith in the principle would be most severely tried. Take the typical case of chanciness, the tossing of a coin. We know that in a general way there is an even chance of heads or tails, even though we sometimes hear of gifted individuals with muscles so delicately adjusted that they can control the event. But in the ordinary way the tossing of a coin is complicated by being produced by a living organism, so let us simplify the problem by designing a catapult of some kind to project it. Which of us does not believe the coin would fall the same way every time if such a mechanism could be made with really complete precision? When the machine fails to make it do so, we say it is because there may have been a speck of dirt in the lubricant or something like that. In other words, we do not feel that the fall of the coin is determined by chance, but we regard the uncertainty we observe as due to our ignorance of all the detailed causes. Ignorance is a confession of incompetence, and so we regard the existence of chance as a blemish in our otherwise admirable characters. This feeling goes very deep, since we are prevented by it from having the complete control of our surroundings that we somehow think should be our due. We start prejudiced against probability and in favour of causality. So much for what we *feel* about causality; and about thirty years ago this feeling would have been regarded as a piece of inescapable reasoning, with the same kind of compelling power as a logical syllogism. We still have the feeling, but now we know it is wrong, and what is more, we know that it is wrong for a reason we never thought of. To understand this oversight we must review the recent history of atomic theory.

The history of the development of physics in the first quarter of the twentieth century will rank as one of the greatest in the advancement of knowledge, but it will also rank as one of the most curious in the history of human thought. In 1901 Planck started the quantum theory. Even this was curious. He was trying to find out the law of complete radiation by the use of ordinary statistical methods, and observed that he got his answer at what should have been the last stage but one of his work. The last stage would have involved proceeding to a limit, and he found that he got the experimental answer without doing so, and an absurd answer if he did. The work went rather deep into statistical theory and there were many for long afterwards who were not convinced of its compelling force, but it was the great merit of Planck that he knew that he had got something involving a quite revolutionary idea—the quantum. In succeeding years other phenomena were seen to involve the same revolutionary idea: Einstein's theory of the photoelectric effect, and of the ionisation produced by X-rays, his theory of specific heats, later improved by Debye, and Bohr's theory of spectra. All these things fitted in quite obviously with the quantum, but quite as obviously they violently contradicted the physics of the nineteenth century. What should a man think about a beam of light which according to Einstein had to be composed of arrows, whereas a hundred years earlier Fresnel had *proved* that it was a system of waves? What does a rational being do when faced with two mutually contradictory but both indubitable pieces of evidence? It was a nice test for the critical spirit, and it revealed a wide divergence of choices. In making a historical judgment long after the event, one of the hardest things to do is to recall the relative scale of importance which contemporaries were inclined to attach to the different branches of their subject.

The statistical theory of matter had already been well established by the work of Maxwell, Boltzmann and Gibbs, but it was not regarded as an essential part of a general mathematical-physical education. For example, in the various courses I was advised to undertake during my undergraduate career, no one at any stage ever suggested to me that I should learn anything about the kinetic theory of gases. I think that that period was one when the Cambridge mathematical school was not at its best, and very probably a little more was done at other places, but, to judge by the available text-books in any language, statistical theory was not regarded as one of the prime subjects of study, as it would be now. The period was essentially dynamic, and as such it was moderately easy for it to take in the new ideas of relativity, to which indeed the experimental work of the last century had been leading. But there was no common habit of thought on statistical lines, and so there was a sharp separation of opinion. The seniors, impressed with the vast mass of successful physics of the nineteenth century, with only a rather general knowledge of statistical theory but no facility of thought in it, found the new ideas completely contrary to their convictions. Such men would think that these ideas depended on the difficult and unfamiliar conceptions of statistics and would be inclined to judge that there must be a fallacy in the statistics which would be cleared up later. On the other hand the laboratory workers, dealing with atoms and electrons from day to day, could not fail to be more impressed with the discontinuous phenomena and the beautiful way these could be explained by the quantum. Such men would cheerfully accept the Bohr orbits as a complete explanation of the hydrogen spectrum, and certainly in many cases would be actually ignorant of the difficulty, the monstrous absurdity, of supposing that a sharp jump from one orbit to another could be responsible for a train of waves shown by the spectroscope to be lasting for quite a long time. So the majority of rational beings behaved in the natural human way of managing to forget all the disagreeable facts. But not every one, for there were Bohr and the other leaders who recognised the difficulties on both sides but could still maintain an attitude of balance and could believe that from somewhere there would come a higher synthesis by which everything would be fitted together.

As time went on the quantum got obviously stronger and stronger, and began to invade more fields. The nuclear atom in the hands of Bohr showed itself capable of giving all the broad details of the periodic table of chemistry, still with nothing done to meet the awful difficulties of optical theory. But about 1925, guided by the Correspondence Principle, things were moving towards a tentative theory of the refractive index, and it was this that finally suggested the break in the contradictions.

Acting on a hint given by the theory of refraction, Heisenberg was led to the suggestion that the contradictions of atomic theory would disappear if one adopted the idea of non-commutative algebra in dealing with the motions of electrons in an atom. Then the floodgates broke and the whole New Quantum Theory burst forth. It would of course be an incomplete account of it not to mention the quite different approach made independently by de Broglie and Schrödinger. If we are to trace this to its origin we must go back a century to Hamilton, for it was his work in geometrical optics which showed how a wave of short wavelength could be treated as a ray. It was de Broglie who worked out the modern analogies, but it was Schrödinger who succeeded in giving its full form, and by the invention of the *wave-function* placed in the hands of the mathematicians the most powerful of weapons for the technical discussion of atomic problems.

At first the work was of a formal kind, obviously right, and a complete synthesis of the rival doctrines of particle and wave mechanics, but there is a very interesting point that has gradually emerged in connection with the discovery. In his first paper Heisenberg laid great stress on the idea of building theory only on directly observable quantities. It is not very clear how the distinction was drawn. The electron's orbit is certainly not observable, but is it less so than the electric force which is the amplitude in the light-wave emitted by the atom ? It has seemed to me that it was not this idea of using the observable that was the merit of his work, but rather the contrary—the capacity for carrying through a formal mathematical analogy without ever asking what it all meant in terms of observable things. However that may be, it was only a year later that he remedied the defect by making a picture of his process by means of the Uncertainty Principle. I may remind you that the Uncertainty Principle asserts that it is impossible simultaneously to measure the position and velocity of any body, because the measurement of either inevitably produces a change of indeterminate amount in the other. The subject has been so often discussed that I am not going into it now, but as it concerns the centre of my argument, I want to emphasise its negative side, which as I think is much the most important. In this rôle the Uncertainty Principle is to be regarded as the argument used to defeat the old-fashioned physicist who claims that there is at any rate ideally no limit to the accuracy with which both position and velocity can be simultaneously measured. He has to admit the correctness of experiments such as the Compton effect, and we show him that by his own admission he will be defeated. On the positive side the principle is not so useful, because once we have seen the reason for the failure of classical ideas, we had better take advantage of the full technique of the quantum mechanics. Here my point is that the Uncertainty Principle showed up a fallacy in the old arguments about causality, and it was a fallacy about which we were so unconscious that we did not even know we were making it. It is now easy to see that there was nothing wrong with the old inference that if I know all about the present I can forecast the future exactly ; the trouble was the impossibility of knowing the present. Once this is seen the whole argument becomes obvious, but nobody saw it until Heisenberg. We had somehow to avoid the compulsory causality of the old mechanics, and there seemed no loophole allowing us to do so until the Uncertainty Principle. Knowing what we now know we may ask why no one discovered the loophole by applying a strict analysis, for example by the use of symbolic logic. Such an analysis would presumably have revealed the fault, but the trouble is that it would also have revealed other unwarranted assumptions which we have made but which we do not in the least want to doubt, so that it would not really have helped in pinning down the exact point of error. It is invention, not criticism, that leads to the advance of knowledge.

Following up the later history of the subject, the success of Heisenberg in exploiting the idea of observables for atoms seemed to repeat the brilliant success of Einstein twenty years earlier in using the same idea over relativity. It seemed to imply that what was wanted in physics was to free ourselves of all abstractions and only make theories about real things. There grew up a great cult of doubting the reality of unobserved things, and then a curious thing was found; the charm did not work again, and only a few minor things have come out of it. The work of the New Quantum Theory has in fact run most surprisingly in the opposite direction. The technique is largely concerned with wave-functions, which are quantities much more abstract than anything in classical mechanics. There is certainly nothing observable, or even picturable, about waves propagating themselves in many-dimensional space with absolutely unknowable phase, and with intensity controlled by the curious extraneous rule of normalisation. Largely by the use of these wave-functions the whole of atomic physics has been reduced to order, and so has molecular physics, except that it yields problems in which so many electrons are interacting that a full discussion is not feasible. So the doctrine of theorising only about observables was not really a useful doctrine; it merely provided a germinating idea. In fact we may well ask what an observable is, and if we go at all beyond direct sensations, which as physicists we certainly intend to do, the answer becomes perfectly indefinite. This opinion I heard admirably expressed a few years ago by the late Prof. Ehrenfest. It was in a physics meeting in Copenhagen and someone was proposing a way out of certain difficulties which involved, as he maintained, a reversion to the cult of the observable. Prof. Ehrenfest said: 'To believe that one can make physical theories without metaphysics and without unobservable quantities, that is one of the diseases of childhood—*das ist eine Kinderkrankheit.*'

I have dwelt at some length on the history of the quantum theory because I think it serves as an analogy to the deeper question of what is wrong with the old logical processes. Just as we used to feel the all-pervading compulsive force of causality, so we feel the all-pervading force of pure logic. Just as we felt that classical mechanics provided no room for anything beyond itself, so we feel that the old logic is the only admissible kind of reasoning. We knew that certain things led to the Old Quantum Theory and obstinately refused to fit into mechanics, and we know that the principle of probability can cover many things outside the old logic. Many men tried to force the quantum into the classical system, and many are still trying to bring probability within the fold of the old logic. I do not believe it can be done. This is not the occasion, nor have I the capacity, for a deep argument on the place of probability in logic, but one of the most convincing ways of seeing it may be found in the consideration of another branch of physical theory, the kinetic theory of gases.

In the early days of kinetic theory the central problem was the law of distribution of velocities of the molecules, and attempts were made to prove the law absolutely from dynamics, but the process always failed. Maxwell made the assumption that with the lapse of time a system of molecules would pass through all possible phases. There are technical difficulties in the discussion of this assumption which have never been overcome, and it is quite uncertain if it is even true. Indeed Kelvin, who disliked the whole kinetic theory, argued with some force that the only examples anyone could give contradicted the principle—for example, the motion of the planets. The greatest contribution to the subject was that of Gibbs, who recognised that there had to be a big assumption somewhere and made it quite frankly and without attempt at justification. The works of Gibbs are not easy reading; in both his great works he attends to every detail with a particularity that is really rather tedious, whereas his basic ideas are thrown at the reader almost without explanation. The idea of a canonical ensemble is a really beautiful idea once you understand it, but where does it come from? An ensemble is an idea which will be unfamiliar to many, so I had better explain it. We want to know something about the behaviour of a complicated system composed of a great many parts; say we want to know the pressure of the gas in some vessel. If we tried to attack the question by pure mechanics, we should be faced with an enormous number of mechanical equations for the motions of the molecules, and even if these could be solved the solution would be of no use, because it would depend on the initial positions and velocities of the molecules, and these we should not know. Instead of trying this impossible and useless task, Gibbs considers a very large number of possible states of motion of the set of molecules, which have some character in common such as their total energy, but which are otherwise unrelated. Though each specimen of the motions is quite independent of all the others, he looks at them all together; this explains the word *ensemble*—I do not know why he had to take a French word—and makes the assumption that the pressure of the gas is correctly given by the average of all the specimens. The actual gas in the vessel at any instant is one of the specimens; in its motion it passes into configurations corresponding to others, but only after a fantastically long time would it go through even a perceptible fraction of the whole ensemble. Gibbs is assuming that the behaviour of the actual gas will be determined by the average of the uncountable millions of specimens in the ensemble. Almost at the start one finds oneself presented with the ensemble with hardly an attempt to explain where it comes from or why it is right, and the beginner is usually troubled by the fact that, though the subject is obviously mechanical, all the mechanics he laboriously learnt in his youth seem to have faded into comparative unimportance. There are various kinds of ensemble, the chief of which is the canonical, corresponding to all the possible motions of the gas which would have the same temperature. Later, almost as a concession to human frailty, Gibbs introduces the micro-canonical ensemble, composed of much fewer specimens because they all have exactly the same energy. This is usually welcomed by the beginner because it seems closer to his familiar mechanics, but with more experience he will realise that the gap is still so great that he is really no better off, and he may as well accept the more general idea at once.

With the old mechanics all this involved ideas which for many readers were distinctly hard to accept. The principle of probability, embodied in the averaging over the ensemble, was frankly laid on top of the logical principles of Newtonian mechanics, and to anyone believing that probability would ultimately be brought down to the old logic the association was most repellent. But we can now see that Gibbs was a prophet far ahead of his time—and indeed, to be frank, far ahead of his own knowledge—for the new mechanics accommodates the ensemble very much more easily than did the old. The new mechanics has shown us that it is impossible to know how the individual molecules are moving, because when one undertakes an experiment to see, that experiment automatically alters the condition of the gas and so fails to tell what was wanted, the state of the molecules without the experiment. In the old days one used to feel that the validity of Gibbs's idea would be spoilt by some skilful experimenter who would really observe the motions of the individual molecules and would therefore rule out the legitimacy of averaging over the *whole* ensemble, but we now know that there is no danger of this. The real gas in the vessel is not merely one specimen of the ensemble, unrecognisable only because of our clumsiness; it is itself the whole of the ensemble. We used to think of the gas as *either* in the state A, *or* in the state B, *or* in C, but according to the new physics we have to think of it as in *all* the states A *and* B *and* C. The distinction is typical of the change we must make in our habits of thought, and most of us resist this change strongly, for we find we can hardly help asking: 'But which state was it really in?' As I have said, we used to be ashamed of ignorance, but we must now realise that *this* ignorance is one of the things that makes the world possible. The principle of probability, which used to be loosely superposed on the old logical principle, is now with the new mechanics fully united with it in a higher synthesis.

Before leaving Gibbs I would like to refer to one thing in his book, where I think he has not even yet come into his own. He considers various types of ensemble of increasing generality. In the micro-canonical the members all have the same energy. Now we never know the exact energy of the gas in a vessel, so that a better idea is the wider one of a gas at a given temperature which therefore has a certain range of admissible energies. This is represented by Gibbs's canonical ensemble, and it is the main one that he uses. In both these the number of atoms in the ensemble is constant. But in the last chapter of his book Gibbs introduces a still wider ensemble. He calls the ones with a constant number of atoms *petits ensembles*, which I shall translate as petty ensembles, and regards them as parts of a grand ensemble in which the total number of atoms is not fixed. He uses the idea to some extent in connection with semi-permeable membranes, but on the whole does not get far with it. As in much of Gibbs's work, it is the idea itself, rather than what he does with it, that is important. This idea of the grand ensemble is not yet incorporated in the new physics. In the quantum theory we take a number of electrons and nuclei and, allowing for their interactions, we construct something that is practically the canonical ensemble. But we take fixed numbers of them—this is partly reflected in the technical process of using normalised wave-functions. Now in an experiment dealing with a large number of particles we are never really sure exactly how many there are, and to assume this number is much like assuming a constant energy for them. If the canonical ensemble is a better idea than the micro-canonical, then the grand ensemble is superior to the petty ensemble. In the new mechanics nobody has yet succeeded in making anything of it, or has made any proposal how to do so, but I will venture the forecast that when some of our present difficulties in the quantum theory are cleared up, it will be found that we shall be using the grand ensemble with its indefinite number of atoms.

Reverting to my main theme, what is the moral of all this ? It is that the new physics has definitely shown that nature has no sharp edges, and if there is a slight fuzziness inherent in absolutely all the facts of the world, then we must be wrong if we attempt to draw a picture in hard outline. In the old days it looked as if the world had hard outlines, and the old logic was the appropriate machinery for its discussion. Things went wrong when it was found necessary to call in the help of the principle of probability ; this appeared first as an alien, but there was hope in the old days that the alien might be naturalised. It has resisted the process and we now recognise that it cannot be assimilated, because it provides the necessary step to a wider reason, that of the new fuzzy world of the quantum theory, a world which is not contained in the old. How far it will be possible to make a full synthesis of the new and the old I do not know, but I like to think there is something in my analogy from the history of the quantum theory, and to suppose that we are still in the condition corresponding to the Old Quantum Theory, and that some day a real synthesis will be made like that of the New Quantum Theory, so that there will be only one thing in the world that has not indefinite outlines, and that will be a new reformed principle of reasoning.

I may fitly conclude this part of my subject by returning to the point from which I started. As an example of what the ordinary man regards as correct reasoning I quoted some words of Sherlock Holmes. I must now confess that I was not quite sincere in my quotation ; the impression I gave was the impression the reader carries away, but on examining the text I was interested to find that the great detective had himself arrived at the ideas I have been putting forward. In the sentence before he said ' No, no ; I never guess. It is a shocking habit, destructive of the logical faculty,' he had said : ' I could only say what was the balance of probability—I did not expect to be at all accurate.' The master-mind uses the word logic in its modern sense.

There may be a feeling among some that the very general suggestions

I have been making are open to every sort of criticism. Perhaps they are right; as I have said, it is part of my doctrine that the details of a physicist's philosophy do not matter much. But whether it is wrong or right, my next point is one on which I do very much hope that there may be a consensus of agreement. This is that the subject of probability ought to play an enormously greater part in our mathematical-physical education. I do not merely mean that everyone should attend a course on the subject at the university, but that it should be made to permeate the whole of the mathematical and scientific teaching not only at the university but also at school. To the best of my recollection in my own education I first met the subject of probability at about the age of 13 in connection with problems of drawing black and white balls out of bags, and my next encounter was not till the age of 23, when I read a book —I think it was on the advice of Rutherford—on the kinetic theory of gases. Things are better now, but mathematicians are still so interested in the study of rigorous proof, that all the emphasis goes against the study of probability.

Its elements should be part of a general education also, as may be illustrated by an example. Every month the Ministry of Transport publishes a report giving the number of fatal road accidents. Whenever the number goes up there is an outcry against the motorists, and whenever down, of congratulation for the increased efficiency of the police. No journalist ever seems to consider what should be the natural fluctuations of this number. A statistician answers at once that the natural fluctuation will be the square root of the total number, and apart from obvious seasonal effects that is in fact about what the accidents show; the number is roughly 500 ± 25. The proof of this does not call for any difficult mathematics, neither the error function nor even Stirling's formula, but can be done completely by the simple use of the binomial theorem. There is no mathematical difficulty that should trouble a clever boy of 15; it is only the train of thought that is unfamiliar, and it is just this unfamiliarity that is the fault of our education. The ideas and processes connected with the inaccuracy of all physical quantities are much easier to understand than many ideas that a boy has to acquire in the course of his studies; it is only that at present they are not taught, and so when met they are found difficult.

This is not the place to describe a revised scheme of education. I would only say that it is not special new courses that are needed, but rather a change in the spirit of our old courses. When a boy learns about the weighing machine, emphasise its sensitivity, and consider the length of time that must be taken for the weighing. When he has a problem on projectiles, make him consider the zone of danger and not merely the point of fall. At a rather higher level, but still I should hope at school, introduce the idea of a distribution law; for example, in doing central orbits work out Rutherford's law of scattering. Calculate the fluctuations of density of a gas, or the groupings in time of the scintillations of α-particles. All these things ought to be examples of a familiar train of thought, and not merely a highly specialised side branch of mathematics first met at the university. It is the incorporation of probability in the other subjects on which I want to insist, but there will of course remain some higher aspects—things like least squares or significance tests—which are still to be treated in separate university courses. Even these I should hope would come to be recognised as subjects of central interest and not, as they are at present, relegated to a remote corner of specialised study.

If these reforms are carried out I shall hope that generations will grow up which have a facility that few of us at present possess in thinking about the world in the way which the quantum theory has shown to be the true one. The inaccuracies and uncertainties of the world will be recognised as one of its essential features. Inaccuracy in the world will not be associated with inaccuracy of thought, and the result will be not only a more sensible view about the things of ordinary life, but ultimately, as I hope, a fuller and better understanding of the basis of natural philosophy.

SECTION B.—CHEMISTRY.

RECENT INVESTIGATIONS IN THE CHEMISTRY OF GOLD

ADDRESS BY

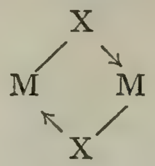
PROF. CHARLES S. GIBSON, O.B.E., M.A., Sc.D., F.R.S.,

PRESIDENT OF THE SECTION.

'How is the gold become dim! how is the most fine gold changed!'
(Lam. iv. i)

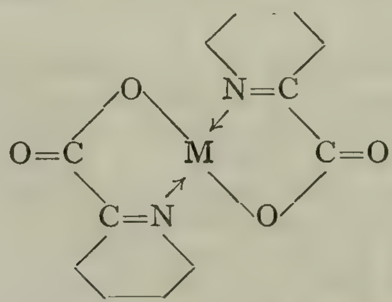
By the results of investigations in which workers in this country have played an important part during the last ten years it is now realised that fewer anomalies exist among the metals of sub-group 1B, copper, silver and gold, than was formerly believed to be the case. The only fundamental property which these metals have in common with the alkali metals is that they are all capable of being univalent. The metals of the sub-group differ from the alkali metals in their atomic structure; the former have eighteen electrons in their penultimate electronic group whereas the latter have eight electrons in that group. While there are differences in their multivalency, the multivalency of copper, silver and gold must be correlated with the eighteen electronic group of these metals. The univalency and bivalency of copper and silver are well established; the tervalency of silver must still be regarded as doubtful. On the other hand, while the bivalency of silver has only been established comparatively recently, modern investigations have shown that it is extremely unlikely that gold can exist in the bivalent condition and this metal continues to exhibit the anomaly, distinguishing it from copper and silver, of existing only in the univalent and tervalent conditions.

As far as the existence of normal salts is concerned, argentous silver differs greatly from cuprous copper and aurous gold. There is no evidence for the existence of any normal aurous salt and, for example, cuprous sulphate is at once decomposed by water with separation of metallic copper, and cuprous nitrate does not exist. On the other hand, in the solid state cuprous and silver halides have non-ionic lattices in their crystals which are isomorphous. Since cuprous chloride is bimolecular, it is reasonable to assume that in its halides the cuprous atom is 2-covalent. Recently, chemical evidence has indicated that this is also true of aurous gold in the analogous compounds and therefore under ordinary conditions the cuprous halides, the silver halides and the aurous halides would appear to have the general formula :



where X = halogen, indicating the 2-covalency of cuprous copper, argentous silver and aurous gold in these compounds.

Unlike bivalent copper, bivalent silver in its salts has been shown by Morgan and Burstall (1928) to exist only as a complex ion, e.g. *bis*-αα′-dipyridylargentic persulphate, [Ag 2dipy]S_2O_8, *tris*-αα′-dipyridylargentic nitrate, chlorate and perchlorate, [Ag 3dipy]X_2, from which it is obvious that bivalent silver may have co-ordination numbers of 4 and 6. On the other hand, Cox, Wardlaw and Webster (1936) have shown that bivalent silver is completely analogous to bivalent copper in giving like the latter metal a derivative with picolinic acid and which like the cupric compound has a planar structure. The constitution of these compounds is conveniently represented thus :

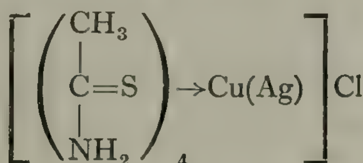


indicating the 4-covalency of bivalent copper and bivalent silver in this type of compound.

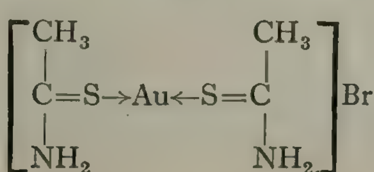
That cuprous copper and argentous silver can exhibit 2- and 4-covalency is well established. It was proved by Bassett and Corbet (1924) in the course of their phase-rule study of complex cyanides. They isolated the salts, $K[Cu(CN)_2]$, $K[Cu_2(CN)_3].H_2O$, $K_3[Cu(CN)_4]$, $K_3[Cu(CN)_4].H_2O$, $K[Ag(CN)_2]$, $K[Ag_2(CN)_3].H_2O$ and $K_3[Ag(CN)_4].H_2O$; but the only complex cyanide of aurous gold of which they were able to prove the existence and to isolate was the well-known potassium aurocyanide, $K[Au(CN)_2]$. The inability of aurous gold to exhibit a higher co-ordination number than 2 has also been recently emphasised by Mann, Wells and Purdie (1936 and 1937) in their studies of the trialkylphosphine and trialkylarsine derivatives of cuprous, silver and aurous halides. The cuprous and silver compounds derived from the iodides have the general formula $[R_3P(As)\rightarrow Cu(Ag)I]_4$ as shown by their molecular weights and are systematically named by the authors as *tetrakis*[iodotrialkylphosphine (or arsine) copper (or silver)]. Crystallographic investigations strikingly revealed the existence of these four-fold macro molecules in the solid state and the tetrahedral configurations of the 4-covalent cuprous and argentous silver complexes and, in addition, and for the first time, the tetrahedral configuration of 3-covalent iodine. The trialkylphosphine and trialkylarsine derivatives of aurous chloride and aurous iodide have, however, the general molecular formula $R_3P(As) \rightarrow AuCl(I)$ and the molecule has probably a linear configuration. The trialkylphosphine-gold compounds are remarkably stable and can be distilled at low pressures without decomposition. On the other hand, Mann and his co-workers have suggested that in the non-electrolytes, $Et_3P(NH_3)_2AuCl$ and $(EtO)_3P(NH_3)_2AuCl$ prepared by Levi-Malvano (1908), the aurous gold atom is 4-covalent, acquiring seven electrons and having an Effective Atomic Number of 86, the atomic weight of radon, the next inert gas. If this is the case these compounds are unique in the chemistry of gold; but it would appear that the determination of co-ordination numbers from ammonia derivatives is not always satisfactory. Aurous compounds having the compositions NH_3AuCl, $(NH_3)_2AuCl$, $(NH_3)_3AuCl$ and even $(NH_3)_{12}AuCl$ have been described. Of these monoamminochlorogold and diamminoaurous chloride having the respective constitutions:

$$H_3N \rightarrow Au - Cl \quad \text{and} \quad [H_3N \rightarrow Au \leftarrow NH_3]Cl$$

are by far the most stable and in these compounds the aurous gold atom is 2-covalent. The well-authenticated salts $(NH_3)_4HCl$ (Joannis 1902), $(NH_3)_4HBr$ (Bakhuis-Roozeboom 1885) and $(NH_3)_4HNO_3$ (Kuriloff 1898) may be compared with Levi-Malvano's compounds and with triamminochlorogold. It would appear more doubtful that such compounds afford evidence of the 4-covalency of hydrogen or aurous gold rather than that they indicate the existence of chain formation of ammonia molecules with links of co-ordinated hydrogen. If, however, by using a more suitable co-ordinating compound than ammonia, it could be established definitely that aurous gold may be 4-covalent as well as 2-covalent, it would be interesting to determine whether such quadricovalent aurous compounds like the quadricovalent cuprous and argentous compounds have a tetrahedral distribution of valencies. In this connexion, the use of thioacetamide by Cox, Wardlaw and Webster (1936) for the successful preparation of *tetrakis*thioacetamidocuprous and *tetrakis*thioacetamidoargentous chlorides:



does not appear to give an analogous aurous gold compound. The only aurous derivative of thioacetamide which Dr. F. H. Brain and I have been able to isolate is the somewhat unstable *bis*thioacetamidoaurous bromide (1937):



in which, as usual, the aurous gold atom is 2-covalent and has an Effective Atomic Number of 82. In the light of available information we must conclude that in all its compounds the aurous gold atom is co-ordinated and, with the possible exception of the two compounds mentioned above, it is always 2-covalent. At the present time there is no example of an aurous compound in which the gold atom is known to be 4-covalent, and attempts to produce such a compound have failed. Since the tervalent gold atom is also always co-ordinated and in its stable compounds always 4-covalent—there may be a slight tendency for it to become 5-covalent—its chemistry can have little in common with that of other tervalent metals which form normal salts.

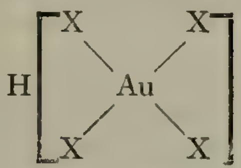
It will be shown later that the four valencies of the tervalent gold atom have a planar configuration and, since 5-covalent and tervalent gold compounds may always be too unstable, it would appear that the only type of gold compound capable of exhibiting optical activity must be a suitable 4-covalent aurous compound, if that can be prepared. These four valencies would be expected, according to Pauling's theory, to have a tetrahedral configuration. It has already been shown that various 4-covalent cuprous and argentous compounds having a tetrahedral configuration have been prepared and a 4-covalent argentous compound, the silver derivative of 8-hydroxyquinoline, appears to have been obtained by Hein and Regler (1936) in optically active forms.

Much of the confusion of knowledge regarding the chemistry of gold as described in almost all text-books and more comprehensive works arises from the fact that the simple halide and cyanogen derivatives are regarded as normal metallic salts and given the formulæ AuCl, AuBr, AuI, AuCN, $AuCl_3$, $AuBr_3$ according to the fundamental uni- or tervalency of the metal. This is all the more surprising in view of the long-established and well-known fact that whenever gold is in solution or in the form of a soluble salt it is always present as a complex. There is only need to mention as examples potassium aurocyanide, probably —on account of its application in the metallurgy of gold—the most completely investigated derivative of the metal and the very interesting sodium aurothiosulphate prepared as long ago as 1845 by Fordos and Gélis. Even at the time of its discovery, this latter compound was known to give neither the usual reactions for gold nor the usual reactions of a thiosulphate. It has long been used for fixing and toning silver photographic prints. Since its introduction in 1924 by the Danish physician, Mollgaard, for the treatment of tuberculosis and, later, by others for the treatment of rheumatoid arthritis it has been considerably investigated and has formed the basis of the modern 'gold therapy.' Curiously enough, in a standard text-book published as recently as 1937, the formula, $Au_2S_2O_3.3Na_2S_2O_3.xH_2O$ seems to be preferred to the correct $Na_3[Au(S_2O_3)_2].2H_2O$ which may be fully written

$$Na_3\left[\begin{array}{ccccccccccccc} & & O & & & & & & & & O & & \\ & & \uparrow & & & & & & & & \uparrow & & \\ \bar{O} & — & S & — & S & — & Au & — & S & — & S & — & \bar{O} \\ & & \downarrow & & & & & & & & \downarrow & & \\ & & O & & & & & & & & O & & \end{array}\right].2H_2O$$

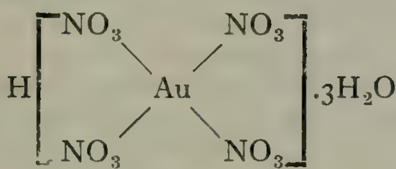
in which, of course, the aurous gold atom is 2-covalent, the compound being of the same type as the well-known potassium aurocyanide, K[N≡C—Au—C≡N], already mentioned.

The halogenoaurates, probably the best known auric compounds, have been long known as salts of acids which have been fully investigated and which have the constitution:

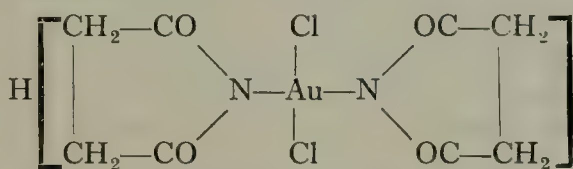

(X = halogen, Cl or Br).

Some more recently investigated compounds belonging to this type are:

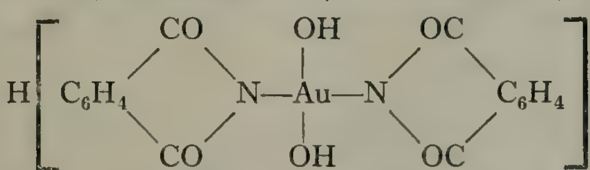
hydronitratoauric acid (Schottländer 1884, Jeffrey 1916)



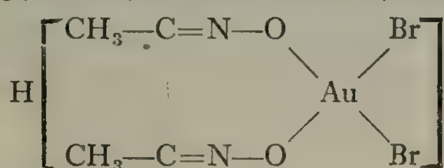
having the same number of molecules of water of crystallisation as hydrochloro- and hydrobromoauric acids as usually prepared, hydrodisuccinimidochloroauric acid (Pope 1931)



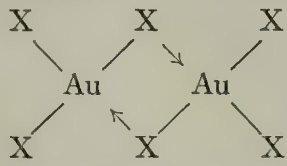
hydrodiphthalimidohydroxyauric acid (Gibson and Tyabji 1937)



and hydrodimethylglyoximinylbromoauric acid (Brain and Gibson 1937)



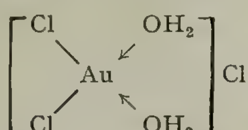
In all these compounds, the 4-covalency of the auric gold atom is obvious, but it is only recently that the persistency of the 4-covalency of auric gold in all its compounds has been recognised. Before the beginning of the present series of investigations in my laboratory, it was shown by W. Fischer (1929) that the molecular formula of auric chloride (trichlorogold) as determined by Horstmann's vapour pressure and transport method between 150° and 260° is Au_2Cl_6 and although it was not suggested at the time, and, indeed, not until 1931, the constitution of this and the analogous bromo compound is only adequately represented by the general formula:



(X=Cl, Br)

in keeping with the already recognised 4-covalency of auric gold. This constitution followed from the constitution of the dialkylmonohalogeno derivatives and from the direct determination of the molecular weight of auric bromide (tribromogold)[1] in boiling bromine (Burawoy and Gibson 1935). In a systematic discussion of the chemistry of gold, and allowing for the differences in fundamental valencies and covalencies of the metals, it would appear that gold is much more allied to palladium and to platinum—the pair of metals having the lowest melting points and the lowest densities of the six ‘ precious ’ transitional metals—than it is to any of the other metals. The comparison of gold with platinum is also of historical interest. Mendeléeff (1871) placed gold and platinum in the same horizontal series of his classification but for reasons which do not concern the present discussion.

[1] My suggestion for a modified nomenclature of certain gold compounds may be criticised as being, if not pedantic, unnecessary. It arises from obvious analogies of the organic compounds of gold with similarly constituted inorganic compounds of the metal ; its only object is to avoid further confusion in the chemistry of gold. Such confusion is constantly occurring. At the present time, in books of reference and even in original literature ‘ auric chloride ’ may imply hydrochloroauric acid in the presence or absence of hydrochloric acid, or it may imply a neutral salt—generally the sodium salt—of hydrochloroauric acid and, much less frequently gold trichloride or—to alter its name more profoundly in order to indicate that the compound is not a salt—trichlorogold. As a result of this confusion the statement is repeatedly found in the literature that ‘ auric chloride is soluble in ether.’ If this statement refers to the pure compound having the molecular formula $(AuCl_3)_2$, it is not true. Hydrochloroauric acid and hydrobromoauric acid containing water of crystallisation, the compounds $HAuX_4.3H_2O$, are soluble in ether but they are insoluble when anhydrous. Although the fact was known long before, the definite statement that gold chloride is soluble in ether appears to be due to Willstätter (1905) ; but it is clear that the material he was investigating was not $(AuCl_3)_2$, but an aqueous solution of hydrochloroauric acid which he termed gold chloride ; and, as a result, the above erroneous statement is still in text-books published as recently as 1937. The hygroscopic nature and solubility of ‘ auric chloride,’ i.e. gold trichloride, in water is not due to the solubility of the compound *per se*, but to the formation in the first place of a compound diaquodichloroauric chloride,



a type of co-ordinated auric gold salt, frequently met with in the present series of investigations, which is soluble in water and undergoes further changes in that medium resulting in the formation of hydrochloroauric acid and aurous choride (monochlorogold).

As far as the work on the chemistry of gold in which my colleagues and I have been concerned the chief advances have been achieved by studying in the first place the organic derivatives of the metal and, more recently, the gold derivatives of certain types of organic sulphur compounds.

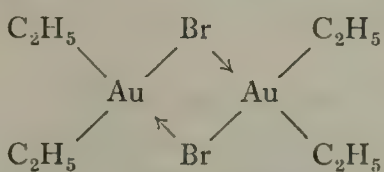
(*a*) *Dialkyl halogeno compounds.*

It is almost exactly thirty-one years ago since Sir William Pope and I (1907) prepared the first organic gold compound, then styled diethylauric bromide, just after he and Peachey (1909) had prepared the first organic platinum compound, trimethylplatinic iodide. But it was not until 1930 that the work could be resumed (Gibson and Simonsen 1930, Gibson and Colles 1931). In the earlier investigations the poor yield of the product of the interaction of the Grignard reagents and the ether soluble hydrochloro- or hydrobromoauric acids, $HAuX_4.3H_2O$, rendered the detailed study of these new organic gold compounds somewhat difficult. As starting material, the easily prepared pyridinotrichlorogold —less frequently, the corresponding bromine derivative—is now used and the following reaction is carried out in an ether-pyridine mixture using the relative quantities indicated :

$$2C_5H_5N.AuCl_3 + 4MgRBr \rightarrow 2C_5H_5N.AuR_2Br + 3MgCl_2 + MgBr_2$$

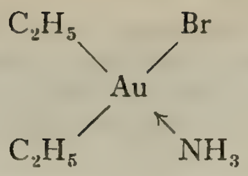
The pyridine co-ordination compound is decomposed later by the action of hydrobromic acid and the compound isolated through its water soluble co-ordination compound with ethylenediamine which, again, may be decomposed by a suitable acid. It takes some three to four hours to obtain the pure material and the average yield is rather more than 25% of the theoretical quantity and yields as high as 38% have been obtained.

The isolation of homologues of the first prepared compound was easily accomplished. The experimental proof that these are non-electrolytes, that they have molecular weights corresponding to those of twice their empirical formulæ and that they and their co-ordination compounds all contain 4-covalent auric gold led to the realisation that the 4-covalency is an essential feature of all auric compounds. The existence of gold compounds having the general empirical formula, AuR_3 (R = univalent hydrocarbon radical), is therefore impossible since such compounds could not contain 4-covalent gold. Taking the ethyl compounds as typical, the following are examples of some of the non-electrolytes which have been prepared:

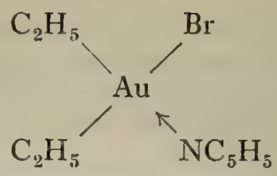


Diethylmonobromogold.

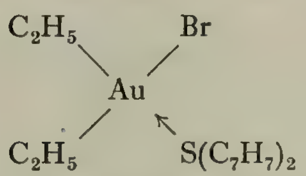
Col. anorthic needles, m.p. 58° (decomp.).
mol. wt. = 670. μ = 1.41 D in CCl_4 or C_6H_6.



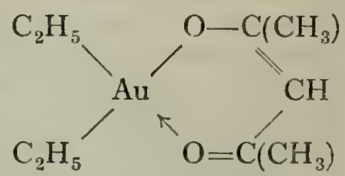
Amminodiethylbromogold.
Col. doubly refracting needles.



Pyridinodiethylbromogold
Col. needles.



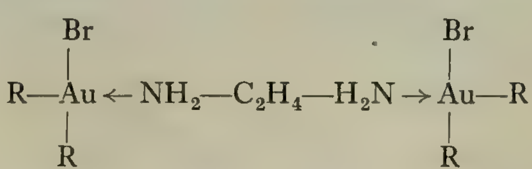
Dibenzylsulphidodiethylbromogold.
m.p. 91°. Col. needles.



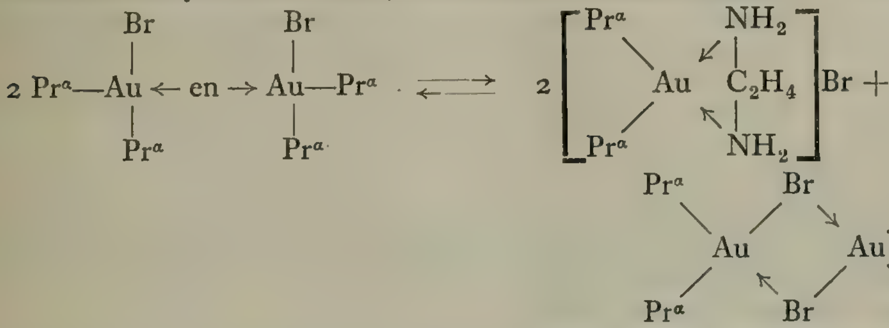
Acetylacetonediethylgold.
Col. plates, m.p. 10°.

All these and analogous compounds are soluble in hydrocarbon and other organic solvents and some interest attaches to the fourth and fifth compounds. Dibenzylsulphidodiethylbromogold (Brain and Gibson 1938) is the first auric compound of this type to be prepared by the direct addition of an organic sulphide to an auric compound. The compound separates rapidly and no reduction of the auric compound or oxidation of the sulphide takes place. Acetylacetonediethylgold is the first of these organic gold compounds containing no halogen to be prepared. It was the first organic gold compound from which brilliant gold films were obtained. These films are obtained when the compound in solution in a solvent such as ethanol is exposed to light at ordinary temperatures and also on gentle warming. Obviously, the compound prepared easily by the action of thallousacetylacetone on diethylmonobromogold is but one of a number of similarly constituted gold compounds which can be obtained from metallic derivatives of β-diketones. The decomposition of such compounds by suitable alkali salts is a convenient method of preparing chlorine and iodine derivatives of the parent dialkyl gold compounds.

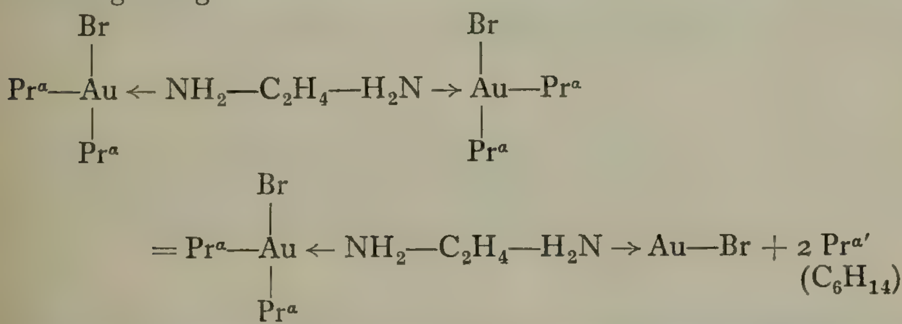
One would expect that non-electrolytes analogous to the above ammino and pyridino derivatives containing ethylenediamine should exist. The first compound of the type:



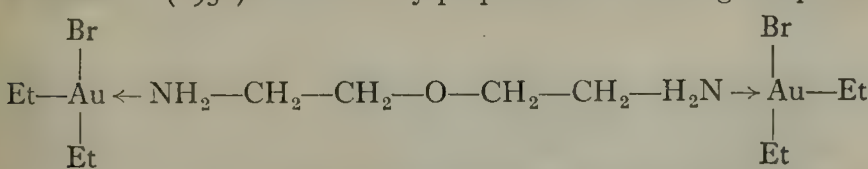
was prepared from di-*n*-propylmonobromogold ($R = Pr^{a}$) and no analogous compound was obtained from diethylmonobromogold; but analogous cyanogen compounds are easily obtained both in the ethyl and propyl series. Monoethylenediaminotetra-*n*-propyldibromodigold (Burawoy and Gibson 1934) is a fairly stable colourless crystalline, 4-covalent auric compound soluble in certain organic solvents. On standing at the ordinary temperature, its solutions in chloroform or benzene slowly become cloudy due to the following change:



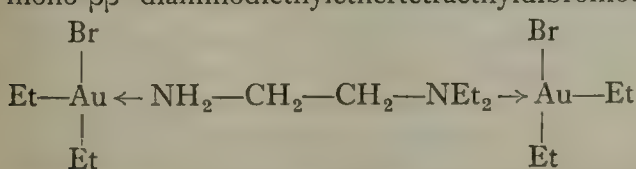
The salt, ethylenediaminodi-*n*-propylgold bromide, is insoluble in such solvents as chloroform and benzene. By repeatedly washing the compound with water the above change goes to completion from left to right, the di-*n*-propylmonobromogold being insoluble in water. The decomposition of the monoethylenetetraalkyldicyanodigold compounds follows a different course (see below). On being heated to its melting point, monoethylenediaminotetra-*n*-propyldibromodigold undergoes the following change:



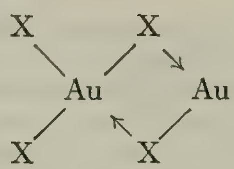
The solid product of the reaction is monoethylenediaminodi-*n*-propyldibromodigold and this reaction indicated that suitable organic gold compounds may be the potential source of free radicals (see below) and that it is possible to prepare mixed auric-aurous compounds containing 4-covalent auric gold and 2-covalent aurous gold in the same molecule. While the analogous ethyl compound has not been prepared, Dr. F. H. Brain and I (1938) have recently prepared the following compounds:



mono-ββ'-diaminodiethylethertetraethyldibromodigold, m.p. 87°,

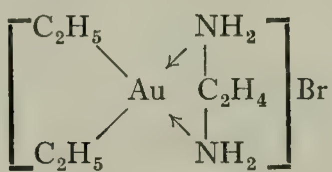


mono-asym.-N-diethylethylenediaminotetraethyldibromodigold, m.p. 84°, and both these compounds exhibit the same evolution of gas as does monoethylenediaminotetra-*n*-propyldibromodigold on being heated to their melting points. Other cases of the initial production of free radicals will be mentioned and their production from compounds containing two 4-covalent auric gold atoms in the molecule always results in the simultaneous production of a mixed auric-aurous compound in which the gold atoms are 4-covalent and 2-covalent respectively. Such decompositions as those just described raises the question of the possibility of establishing the existence of chlorine and bromine derivatives of gold having the formula Au_2X_4 (formerly written as AuX_2 and given as examples of bivalent gold). Such halides would be mixed auric and aurous compounds having the constitution :

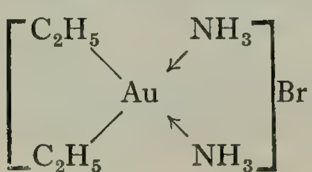


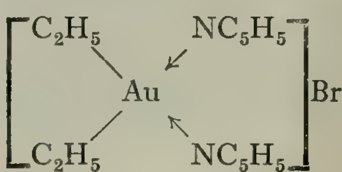
and they may be produced as intermediate products in the decomposition—not completely reversible—of the trihalides to the monohalides.

The first stable salt to be isolated in this series of organic gold compounds was the colourless highly crystalline auric compound ethylenediaminodiethylgold bromide,

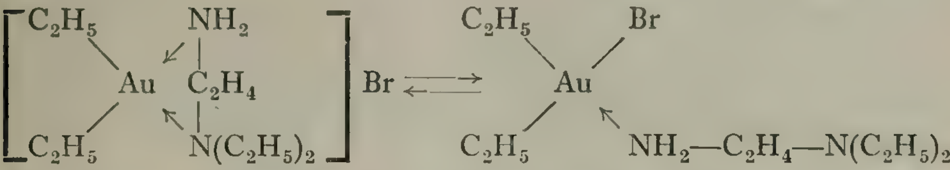


which with its homologues has proved of utility in the preparation of the dialkyl and diaryl compounds. Although evidence of the formation of the analogous ammonia and pyridine compounds

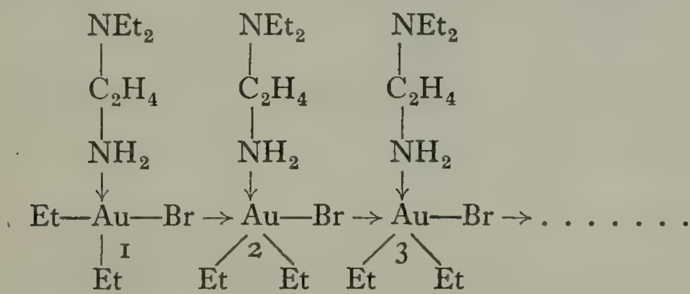




has been obtained they are too unstable (owing to loss of one molecule of the volatile base resulting in the formation of the non-electrolytes already mentioned) to be isolated. The corresponding co-ordination compound of diethylmonobromogold with the asymmetrical N-diethylethylenediamine is of special interest (Brain and Gibson 1938). The interaction of these two substances in molecular proportions results in the production of a colourless crystalline compound soluble in water and also soluble in benzene. The compound therefore appears to be both a salt and a non-electrolyte. Bearing in mind that the co-ordinating power of tertiary amines is less than that of primary amines it might be suggested that its constitution should be represented in some such way as :



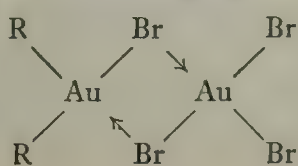
This appears to be an unique case of tautomerism, of course, not resonance. The compound, with others, is still under investigation and another anomaly remains to be explained. The compound is dissociated in aqueous solution, but shows considerable association in organic solvents (approximately bimolecular in bromoform and quadrimolecular in benzene), and it is suggested that the association may be explained thus :



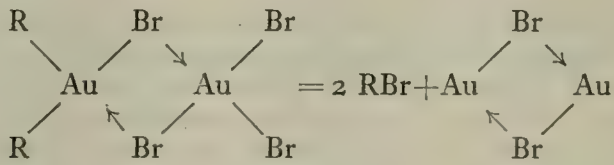
the auric gold atoms, (2), (3), etc., being 5-covalent—probably not a stable covalency—their Effective Atomic Numbers becoming 86 (the atomic number of radon) whereas the auric gold atom (1), as in a normal auric compound, is 4-covalent and has an Effective Atomic Number of 84.

(*b*) *Monoalkyldibromo compounds* (Pope and Gibson 1907, Burawoy and Gibson 1934 and 1935).

The monoethyl and mono-*n*-propyldibromo compounds have been studied in some detail. They are easily prepared by the action of the calculated quantity of bromine on the dialkylmonobromogold compounds in chloroform or carbon tetrachloride solution. They are highly crystalline and deep red in colour ; they are soluble in solvents which are not readily brominated or oxidised and therefore unstable in such solvents as ether, alcohol, acetone, benzene, ligroin, etc. Their molecular weights (determined in freezing bromoform) show that their general formula is $(RAuBr_2)_2$ and the high dipole moment in carbon tetrachloride solution of the *n*-propyl compound ($\mu = 6$ D) affords proof that the constitution of these auric compounds is correctly represented thus :

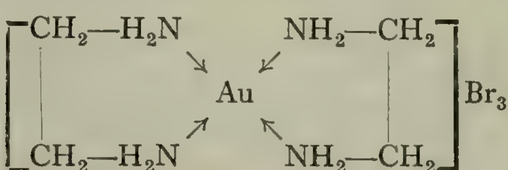


This is in keeping with their formation from equimolecular quantities of dialkylmonobromogold compounds and tribromogold and with their physical and chemical properties. Slowly, on standing, more rapidly, in a current of an inert gas at the ordinary temperature, and still more rapidly, when heated at a temperature just above the boiling point of the alkyl bromide, they decompose quantitatively into alkyl bromide and gold monobromide, thus :



This decomposition also seems to afford chemical evidence concerning the constitution of the aurous halides to which reference has already been made. The aurous bromide (monobromogold) is left in a state of purity as highly crystalline apparently pseudomorphs of the monoalkyldibromo compound, and, prepared in this way, it is suitable for X-ray investigation to which such an aurous compound does not yet appear to have been submitted.

Chemically, the monoalkyldibromogold compounds behave as equimolecular mixtures of gold tribromide and the dialkylmonobromogold. For example, in contact with the many solvents which decompose them they yield dialkylmonobromogold compounds and the solvents are either brominated or oxidised and aurous bromide is left as a yellow precipitate. With hydrobromic acid, they yield hydrobromoauric acid and the dialkyl monobromogold compound and with aqueous solutions of alkali halides the reaction is similar. With ethylenediamine, they yield equimolecular quantities of the ethylenediaminodialkylgold bromide, $[R_2Au\ en]Br$, and diethylenediaminogold tribromide (Gibson and Simonsen 1930, Gibson and Colles 1931), having the constitution :



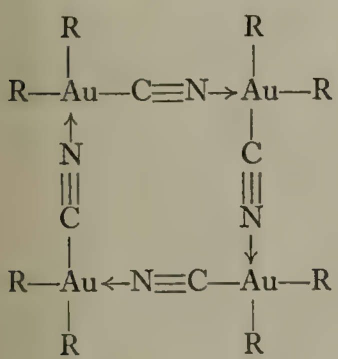
This is a crystalline yellow salt which is highly soluble in water and insoluble in ethanol. It is readily prepared by the action of ethylenediamine on gold tribromide or on a suitable salt of hydrobromoauric acid. It constitutes one of the few examples known in which the 4-covalent auric gold complex is a tervalent cation in halide salts. It is analogous to tetraamminoauric nitrate, $[Au(NH_3)_4](NO_3)_3$, the corresponding phosphate, $R PO_4.H_2O$, the oxalonitrate, $R(NO_3)(C_2O_4)$, the chlorate, $R(ClO_3)_3$, the perchlorate, $R(ClO_4)_3$, the oxaloperchlorate, $R(ClO_4)(C_2O_4)$, the sulphonitrate, $R(NO_3)(SO_4)$, and the chromate $R_2(CrO_4)_3$ where $R = [Au(NH_3)_4]^{+++}$ (Weitz 1915). Like the above diethylenediamino compound, these are very stable salts ; they retain their ammonia even in the presence of concentrated acids. The corresponding salts with the halogen acids, hydrocyanic acid and thiocyanic acid have, however, not been obtained.

(c) *Cyano derivatives of organic gold compounds* (Gibson, Burawoy and Holt 1935, Burawoy, Gibson, Hampson and Powell 1937).

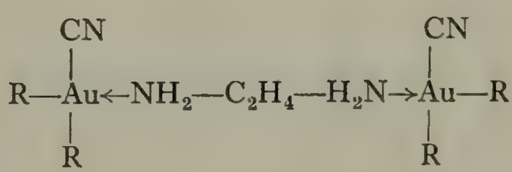
By the direct action of silver cyanide on the dialkylmonobromogold compounds, the corresponding cyano derivatives are easily prepared. These compounds have unique properties and the detailed investigation of the ethyl and *n*-propyl compounds have revealed a number of interesting features in connexion with the general chemistry of gold.

The dialkylmonocyanogold compounds are colourless highly crystalline non-electrolytes, soluble in hydrocarbon solvents, and their molecular weights in freezing bromoform are four times those required by their empirical formulæ. These compounds, therefore, unlike any other gold compounds so far described contain four atoms of tervalent gold in the molecule. In the molecule of such compounds, the gold atoms must be attached to the carbon atoms of the cyanogen groups and the nitrogen atoms must be co-ordinated to neighbouring gold atoms.

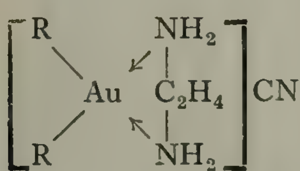
Constitution (I) ($R = Et, Pr^a$) indicating a symmetrical twelve atom planar ring structure is the only possible one in keeping with the stereochemical configuration of the cyanogen group, with the small dipole moment—$\mu = 1{\cdot}47$ D in carbon-tetrachloride at 25° for the *n*-propyl compound—with the 4-covalency of auric gold atoms and, as will be pointed out later, with the results of X-ray crystallographic investigation.



I

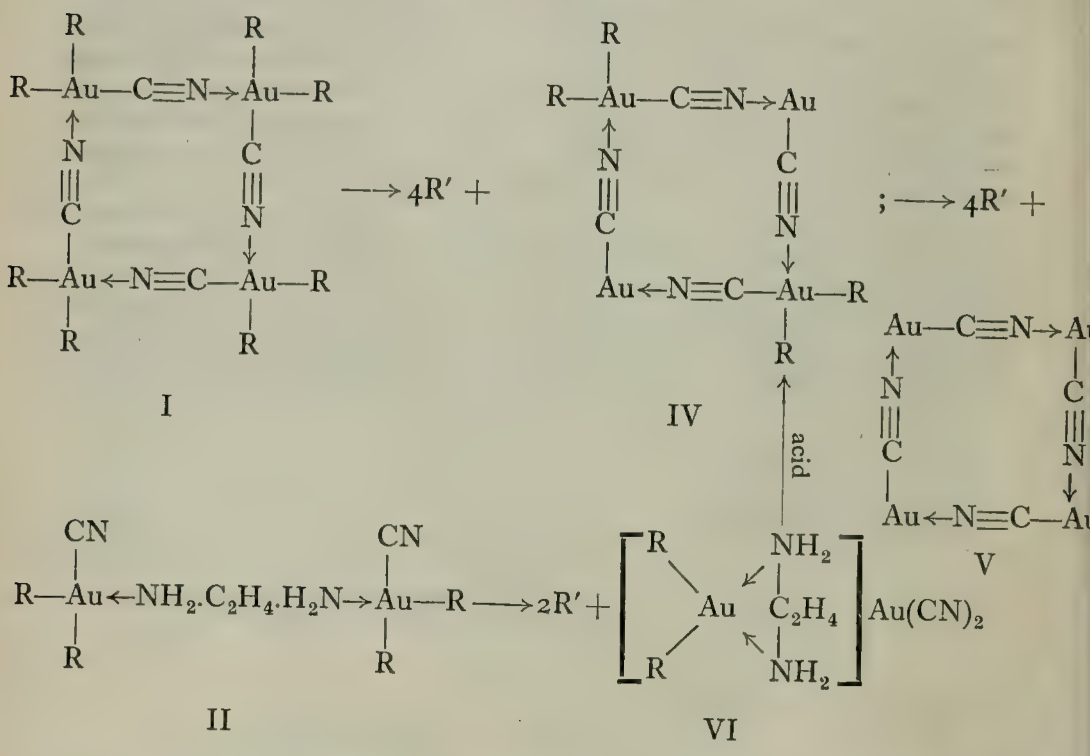


II



III

These compounds yield with ethylenediamine colourless crystalline non-electrolytes of type II — monoethylenediaminotetraalkyldicyanodigold—analogous to the above described monoethylenediaminotetra-*n*-propyldibromodigold, the molecules of which unlike those of the parent substances contain only two atoms of 4-covalent auric gold. In this series, it has so far been found impossible to isolate compounds of type III which would be analogous to the corresponding bromides. One particularly interesting feature of compounds of types I and II is the mode of their decomposition to give finally monocyanogold (aurous cyanide). Compounds of type I are converted into compounds of type IV on standing at the ordinary temperature and the ethyl compound undergoes this change explosively on mere rubbing ; at their melting points the change takes place very rapidly in all cases. These decompositions which have been quantitatively investigated are illustrated :



The dialkyldicyanodigold compounds (IV) are non-electrolytes like the dialkylmonocyano-gold compounds (I), but unlike the latter they are very sparingly soluble in organic solvents and they decompose without melting. They constitute further examples of mixed 4-covalent auric and 2-covalent aurous compounds. The decompositions I → IV → V afford chemical evidence regarding the constitution of monocyanogold (gold monocyanide, aurous cyanide), indicating that it is a non-electrolyte, that it is a 2-covalent aurous compound and that there are four such gold atoms in the molecule which like that of a compound of type (I) is planar. Pure gold monocyanide prepared in such a way should be suitable for X-ray crystallographic investigation. The ethylenediaminodialkylgold aurocyanides (VI) are electrolytes and mixed 4-covalent auric and 2-covalent aurous compounds, a type of compound of which the first example appears to have been described by Sir William Pope in 1929. It will be appreciated that the conversion of compounds of type VI into those of type IV by means of acid, which goes quite smoothly, is somewhat complicated involving changes of electrovalencies into covalent and coordinate linkages.

The free radicals were not identified as such although there is evidence for believing that they are initially evolved. The decompositions are of either solid or liquid compounds and so far free radicals have only been identified as such when they have produced by decompositions in the vapour phase. Actually the free radicals were identified as the paraffin hydrocarbons to which they should give rise, *n*-butane from the ethyl derivatives and *n*-hexane from the *n*-propyl derivatives. This was the first time that *n*-hexane had been obtained from the decomposition of a *n*-propyl compound. Previously, when *n*-propyl radicals had been anticipated as likely to be produced a mixture of ethane, butane and ethylene (Frankland 1877) or a mixture of only ethane and ethylene (Paneth and Lautsch 1931) had been obtained.

(*d*) *The structure of gold compounds.*

During the past two years, our knowledge of the structure of various types of gold compounds has developed considerably as a result of X-ray crystallographic investigations. Those on the organic compounds are being carried out at Oxford by Powell (1937) and his collaborators, those on Mann's co-ordination compounds of aurous gold at Cambridge by Wells (1936) while Cox and Webster (1936) have carried out their investigations on potassium bromoaurate at Birmingham. These investigations have established the planar configuration of the four valencies of tervalent gold and the linear configuration of the two valencies of aurous gold.

The X-ray investigation of the simplest organic gold compound, diethylmonobromogold, is attended with difficulties arising from the instability of the crystals to X-rays and even to light. In spite of these, Powell has been able to carry out his analysis satisfactorily. The results, are summarised in figures (1) and (2), reproduced by permission of the Chemical Society. The orientation of the molecule in the unit cell is indicated in the perspective diagram (Fig. 1) where, for convenience, the origin has been moved to $\frac{1}{2}\ \frac{1}{2}\ \frac{1}{2}$. The molecule, projected on the plane of the gold and bromine atoms, is shown in Fig. 2. The carbon atoms marked $\oplus$ and $\ominus$ are, respectively, above and below the plane of the other atoms. The distances marked on Fig. 2 are subject to a probable error of $\pm 0{\cdot}1$ A. The results show that two gold atoms and two bromine atoms lie close together near the origin and that the molecule is $Au_2(C_2H_5)_4Br_2$. These four atoms form a rough square in a plane somewhat inclined to (001). In order that the molecule may fit into the unit cell, all the atoms must lie approximately in one plane as is shown by the very small *c* dimension, and the four gold valencies must accordingly lie in one plane and will be approximately at right angles to one another. The suggested structure is in agreement with the needle habit of the crystals and the very high negative double refraction with the smallest refractive index roughly along the needle direction. The molecule thus has a centre of symmetry and the structure deduced from molecular weight determination and electronic structure is fully confirmed, although the substance has a small but definite dipole moment. The planar and symmetrical distribution of the four valencies of tervalent gold in a non-electrolyte thus confirmed the same results obtained by Cox and Webster in the case of the salt, potassium bromoaurate, $KAuBr_4.2H_2O$.

The crystallographic investigation of the much more complicated compound, di-*n*-propylmonocyanogold, $(Pr^{a}{}_2AuCN)_4$, has only recently been completed by H. M. Powell and R. F. Phillips and the results, which will be published in detail later, strikingly confirm the constitution deduced from the chemical and especially the physical properties of the substance.

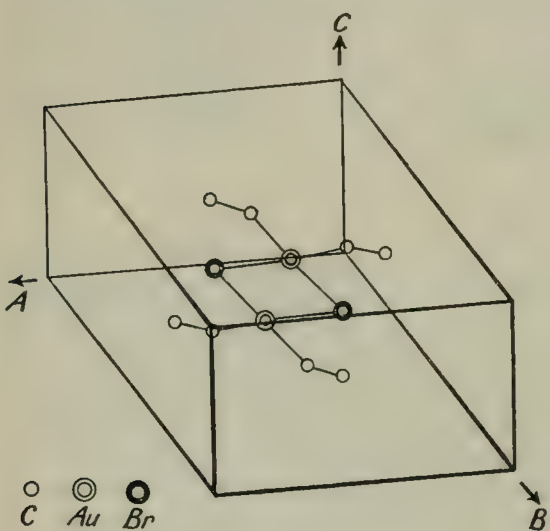


FIG. 1.

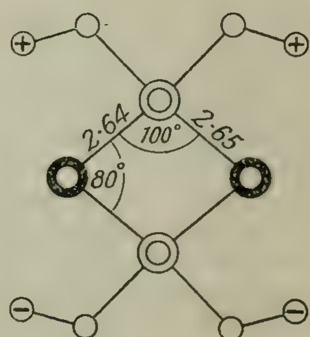


FIG. 2.

Di-*n*-propylmonocyanogold crystallises in the polar class of the orthorhombic system. The unit cell contains sixteen Pr_2AuCN units. Oscillation photographs and Weissenberg photographs about the three principal axes show the absences characteristic of the space group, Pca. The photographs were obtained with copper radiation and intensities estimated visually with the aid of a photographic intensity scale. A two dimensional Patterson analysis on the (hkO) spectra gave approximate *a* and *b* axis coordinates for the gold atoms and signs of the $F_{(hk0)}$'s could therefore be determined and the corresponding two dimensional Fourier synthesis carried out. Two successive approximations led to the final Fourier projection, Fig. 3. This shows the association of four gold atoms in one molecule, the peaks corresponding to the gold atoms lying at the corners of parallelograms in the projection suggesting that the plane of the molecule is considerably inclined to the plane of projection (001). The lighter carbon and nitrogen atoms are not resolved being, in any case, largely overlapping. Ridges of electron density indicate the

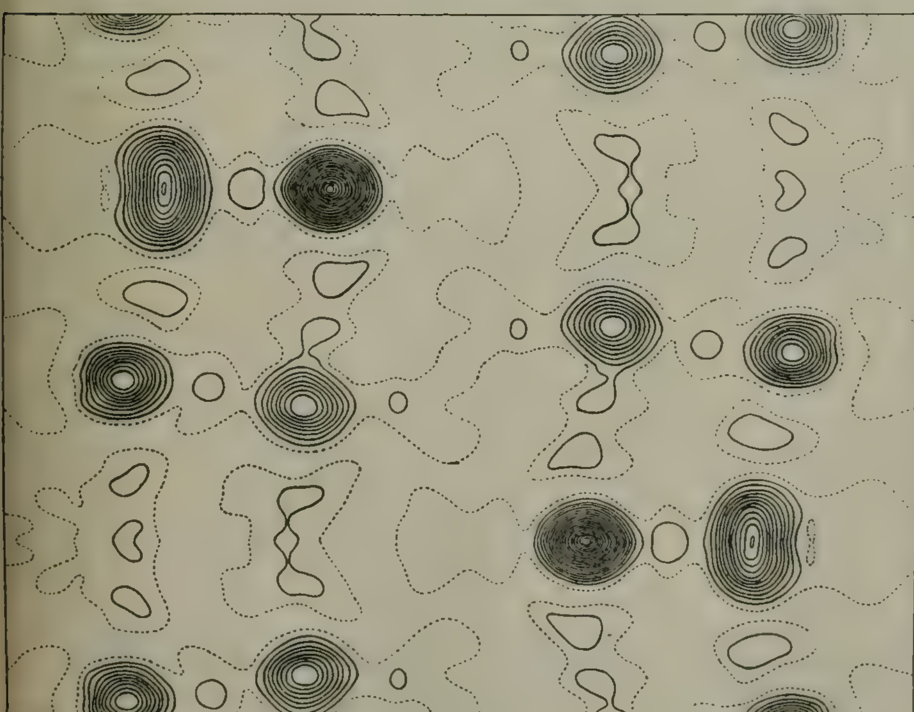


Fig. 3.—Relative electron density projected on (001), the lowest contour shown by broken lines.

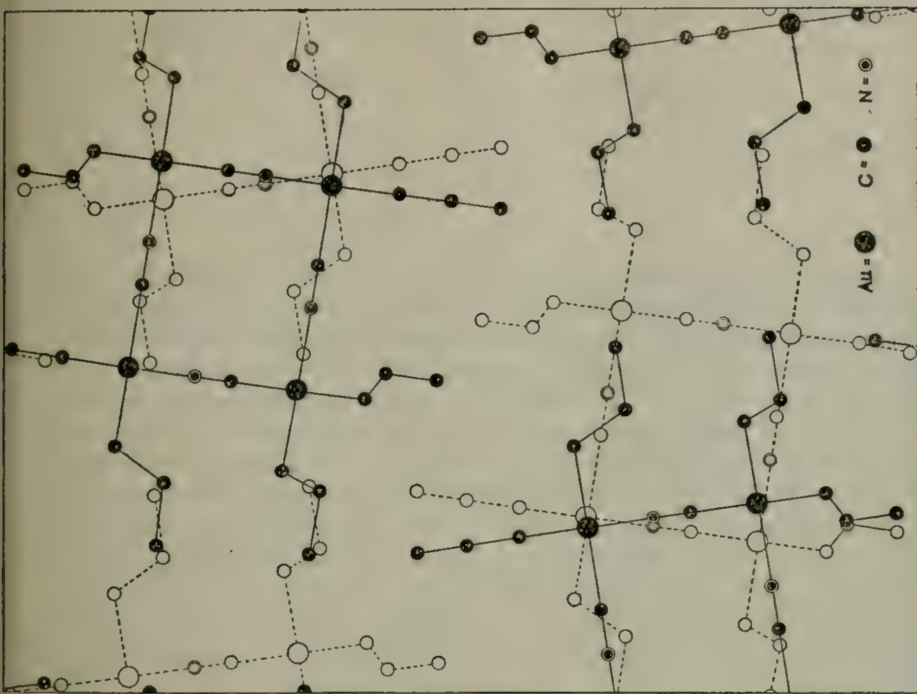
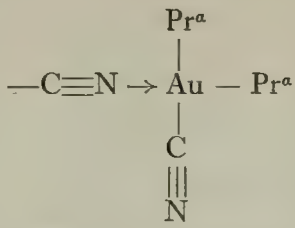


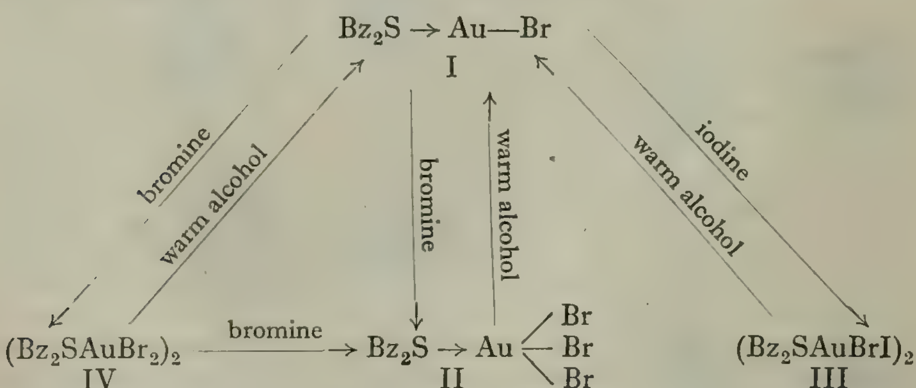
Fig. 4.—Plan of structure on (001). Heavy shading represents molecules lying above others not heavily shaded. The square appears foreshortened owing to the tilt of the molecule relative to the plane of projection.

Au—C≡N → Au group of atoms and the positions of the propyl groups are shown to be :



The projections on (100) and (010) are non-centrosymmetric, but owing to the overwhelming influence of the gold atoms, whose *c* coordinates can be estimated, the arbitrary phase constants corresponding to the $F_{(0kl)}$'s and $F_{(h0l)}$'s can be calculated for the completion of the projection on (010) and (100). The three projections so obtained give the coordinates of all the gold atoms and indicate approximately the positions of the carbon and nitrogen atoms. Consideration of these projections and of the space available permit the assignment of coordinates to all atoms. Between atoms in different molecules there is no approach less than the usual 3·6 A. The Fourier analysis shows clearly the general positions of the *n*-propyl groups and the ' square ' character of the gold valencies, but it must be understood that the details concerning the terminal parts of the *n*-propyl groups is only suggested to be as indicated, alternatively tilted above and below the plane of the square, in order to leave sufficient space between molecules (Fig. 4). The shape of the molecule approximates to a real square and the distance, Au—C≡N→Au, is the same for each side and equal to 5·18 A, thus confirming the suggested formulation, Au—C≡N → Au, which, from available data on bond lengths, should require 5·28 A.

(*e*) Among the studies of gold derivatives of organic sulphur compounds being carried out in my laboratory, I will only refer to one which presents certain unusual features. My collaborators and I have studied the reactions which may be briefly outlined, thus :



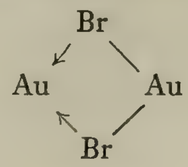
Analogous substances have been previously prepared by different authors. Substance (IV), for example, was described by McPhail

Smith (1922) as 'dibromogolddibenzylsulphide,' an addition compound of 'gold bromide' to which was assigned the formula, $Br_2Au.SBz_2$. On the other hand, Ray and Sen (1930) described the chlorine analogue of the same substance as auroaurichloride dibenzyl sulphide having the constitution, $AuCl_3Bz_2S.AuClBz_2S$. Ray and Sen, however, stated that the molecular weight of their substance agreed with the molecular formula they assigned to it. This does not agree with the results obtained by previous workers nor by Dr. Tyabji and myself for the bromine analogue (1937).

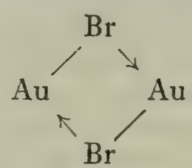
There is nothing unusual concerning the structure and properties of the colourless 2-covalent aurous compound (I), (dibenzylsulphidomonobromogold), or of the structure and properties of the deep red 4-covalent auric compound (II), (dibenzylsulphidotribromogold) ; but the structure and properties of the substances (III) and (IV), which, as compounds, would be termed *bis*(dibenzylsulphidobromoiodogold) and *bis*(dibenzylsulphidodibromogold) respectively, present interesting features. Of these, only (IV) in which the halogen atoms are the same needs to be considered in detail. All these substances are non-electrolytes and (IV), in addition to the method already indicated, can be prepared by mixing equimolecular quantities of compounds (I) and (II) in a suitable solvent, for example chloroform. The molecular formula of (IV) cannot be the same as its empirical formula otherwise it would be a 3-covalent derivative of bivalent gold. The apparent molecular weight of each of the substances (III) and (IV) in freezing bromoform is a little less than that indicated by the empirical formula affording no information as to their constitutions.

In this particular case, our knowledge of the chemical and physical properties is inadequate for determining the constitution of the substance in the solid state. All that is possible is for the chemist to suggest reasonable alternatives based on recognised principles of the constitution of co-ordination compounds. None of these alternatives is capable of being confirmed by chemical or physico-chemical methods and the only method of determining the constitution is by careful crystallographic and X-ray analysis. In the solid state the substance is obviously a mixed aurous-auric complex of a new type, since the mixed aurous-auric compounds already encountered are stable both in solution and in the solid condition. For the constitution of the substance in the solid state, two not unreasonable possibilities immediately suggest themselves. In the linking up of the aurous and auric compounds, (*a*) the aurous gold atom may become 4-covalent, its four valencies probably assuming a tetrahedral configuration, having now eight electrons in its outer shell and assuming an Effective Atomic Number of 86, or (*b*) the auric gold atom may become 5-covalent, its five valencies probably assuming a pyramidal configuration, having now ten electrons in its outer shell and assuming an Effective Atomic Number of 86.

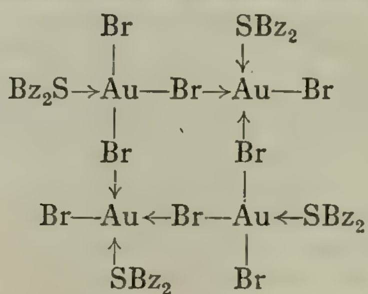
I have put the two alternatives in this order, because there seemed to be a possibility, if crystallographic and X-ray analysis proved it, of obtaining the first incontrovertible evidence of a 4-covalent aurous compound which, up to the present, remains a theoretical conception unsupported by experimental evidence to explain the constitution of certain complex compounds (compare p. 37). Arising from suggestion (*a*) there are two ways in which the two compounds may be linked together in the complex. In the one case, the gold atoms may be connected thus:



the aurous gold atom having four valencies—two co-ordinately attached bromine atoms (shown), a co-ordinately attached sulphur (of dibenzylsulphide) atom and a covalently attached bromine atom—tetrahedrally disposed, while the auric gold atom has its four valencies in a plane, three being covalent links attached to bromine atoms and a co-ordinate link from the sulphur atom of dibenzylsulphide. The dissociation of the complex in non-aqueous solvents might then be explained by the particular disposition of the co-ordinate Br → Au links since, in the stable mixed aurous-auric compounds already known and some of which have been referred to above, such linkages are alternately disposed thus:



In the other case arising from suggestion (*a*) the solid complex may consist of alternate auric and aurous units linked together by co-ordination of a bromine atom from the former to the latter, each aurous unit being linked to two auric units. The simplest possible molecule would thus be a ring containing two auric and two aurous units thus:



If, in this case, the four valencies (three co-ordinate and one covalent) of the aurous gold atoms are as we would expect tetrahedrally disposed, the structure becomes much more complicated than is represented by the above plane diagram. Arising from suggestion (*b*), there is the possibility that in the solid state an auric unit becomes linked to an aurous unit by the bromine atom of the latter becoming co-ordinately linked to the gold atom of the former. The auric atom now becomes 5-covalent and its five valencies may assume a pyramidal configuration whereas the four valencies of the auric atom had a planar configuration. This would appear to cause the minimum displacement of atoms in the two units concerned. Actually, it would only mean the altering of the position of the auric gold atom from the centre of a square to a position within the pyramid of which the base is the original square; this may be illustrated by Fig. 5:

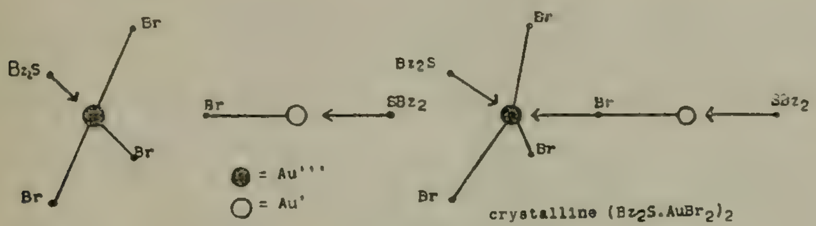


FIG. 5.

Such a constitution would not be out of keeping with the separation of the two parts of the complex compound in non-aqueous solvents since 5-covalent auric compounds may be expected to be unstable (compare p. 45).

I have elaborated this example to draw attention to the fact that not a few present-day investigations of which this is one are accumulating in which chemists will have to rely almost completely on crystallographic technique for the determination of the constitution of substances whose structures have so far not proved amenable to elucidation by treatment by the older methods. In the present instance, the above suggestions have been excluded at once by my collaborators, H. M. Powell and R. F. Phillips at Oxford. There are no indications of the 'bridge' and 'ring' structures now well known from previous investigations, and there are no indications of such a disposition of gold, bromine and sulphur atoms as required by the structure indicated in Fig. 5. In other words, this work again reveals the small tendency—if any—of aurous gold to become 4-covalent and of auric gold to become 5-covalent. Finally, having excluded the direct linking of the auric and aurous parts of the complex, there remains apparently only the possibility of the fitting together in the solid state of the aurous and auric compounds by a close packing arrangement which, again, only the crystallographer is capable of resolving. The result of this crystallographic investigation will be awaited by me with some interest, and it may be that investigations such as this will give some useful information about 'complex molecules' generally.

There are still many problems, some of them fundamental, in the chemistry of gold waiting to be solved. Even as it is, I have only mentioned a few of those which my co-workers and I have tried and are trying to investigate; but there is a limit to the topics which can be discussed at any one time and I venture to conclude with the apology of that interesting man of the world, chemist and theologian, Richard Watson, D.D., F.R.S., sometime Professor of Chemistry in the University of Cambridge, later Regius Professor of Divinity and Bishop of Llandaff: 'Chemists must excuse me, as well for having explained common matters, with what will appear to them a disgusting minuteness, as for having passed over in silence some of the most interesting questions.'

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SECTION C.—GEOLOGY.

DEVELOPMENT AND EVOLUTION

ADDRESS BY

PROF. H. H. SWINNERTON, D.Sc.,

PRESIDENT OF THE SECTION.

Those whose memories carry them back to student days at the end of the nineteenth century will remember how simple and straightforward the relationship between Development and Evolution seemed to be. 'The development of the individual,' we were taught, 'repeated the history of the race,' or more technically and concisely, 'Ontogeny repeats phylogeny.' To us, mere students, the names of Von Baer and Haeckel were in some way mixed up with all this, but we were not very clear what their respective contributions were, except that Von Baer lived long before Haeckel and therefore, we thought, his views must of necessity be a little out of date. But even then there were voices, like that of Hurst (1893), that spoke of a fundamental difference between the views of these two great workers, and maintained that Von Baer was nearer the truth than Haeckel. That difference is now much more clearly appreciated and finds expression in a tendency towards the division of thinkers into separate camps. On the one hand there are those who may be described as the lineal descendants of Von Baer, who propounded the view that 'the young stages in the development of an animal are not like the adult stages of other animals lower down the scale but are like the young stages of those animals.' On the other hand there are the corresponding descendants of Haeckel who maintained that 'the adult stages of the ancestors are repeated during the development of the descendants, but are crowded back into the earlier stages of ontogeny, therefore making the latter an abbreviated repetition of Phylogeny' (v. de Beer). This is variously referred to as the Theory of Recapitulation, the Principle of Palingenesis and the Biogenetic Law.

Year by year students of fossils, more especially those concerned with the invertebrates, have discovered an increasing body of facts which seem to them to fit in with and give support to Haeckel's theory of recapitulation. Meanwhile students of living forms have, as the result of new as well as old methods of inquiry, accumulated much additional evidence which seems to give the lie to this principle. Thus Garstang, whose survey of this field from the biological point of view has proved most helpful, wrote in 1921, 'The idea that form changes in ontogeny were preceded by similar changes in adult ancestry is an illusion.' A few years later (1929) he reiterated the same opinion in a yet more forceful way, saying, 'the theory of adult recapitulation is dead and need no longer limit and warp us in the study of Phylogeny.'

Though palæontology has long been the stronghold of the theory of recapitulation, there have not been wanting among its devotees those whose faith in the theory has waned and perished. Thus Spath (1924) with a touch of bitterness against the view he has forsaken writes, 'Of course it may be necessary to assume an inverted geological order if our views of the biological order of ammonites are to continue to be governed by discredited "laws" of recapitulation and omission of hypothetical stages.' Some years later (1933) his attitude seems to have become slightly modified, for he then described the law as merely 'inadequate.' In the same work he gives a useful summary of the views of a long array of previous writers who had expressed doubts concerning the 'law of recapitulation.'

It should be observed here that the bone of contention is not represented by the word 'recapitulation' but by the word 'adult.' Thus, for example, Garstang (1921), in spite of the apparently uncompromising statement quoted above, writes, 'as differentiation increases combinations of layers, tissues, organs, etc. at successive stages resemble more or less distinctly combinations characteristic of successive grades of evolution represented in phyletic classification. To that limited extent ontogeny epitomises phylogeny, in the true sense of the word recapitulation, i.e. sums up.' Other passages in his writings refer to a like parallelism. Again Spath (1933), speaking out of his wide experience in the handling of cephalopods, says of Perrin Smith that 'he constantly overlooked the fact that by heredity an ammonite was an ammonite, and that like other organisms it had to grow and therefore necessarily had to pass through more primitive stages.' It seems to me that this is no more and no less than a useful but incomplete paraphrase of the term recapitulation.

On the other hand, Raw, discussing the ontogenies of trilobites, after allowing for the presence of embryonic and larval characters in the Protaspis stage, recognises 'phylogenetic characters of ordinal and family value,' whilst 'in the next or Meraspis stage, as the embryonic and larval characters diminish in strength generic and specific characters appear.' In talking of higher divisions than species, Raw is obviously not thinking of a specific resemblance to any definite adult ancestor, but of a general resemblance such as exists between all the species of a genus, all the genera within an order, and so forth. Evidently he also has in mind grades of evolution, and in that respect his position is similar to that of Garstang and presumably also Spath. Garstang, however, visualises the structure not of primeval adults but of primeval young, whilst Raw quite definitely has in mind the general condition in the adults of the primeval stock.

The idea of recapitulation in the sense of summing up seems to me, as also to Crow (1926) and Lillie (1930), to be inherent in Von Baer's as well as in Haeckel's positions. The fundamental difference between them and their philosophical descendants is that for the former it is a recapitulation of *juvenile* conditions, for the latter it is a recapitulation of *adult* conditions. It will be helpful in further discussion if the two positions are referred to as juvenile and adult recapitulation respectively. In both cases the recapitulation may be either specific or general.

The main point at issue, therefore, is whether or not *adult* recapitulation, either specific or general, does occur. Some thinkers, especially upon the biological side, say emphatically ' No.' Others, especially invertebrate palæontologists, say ' Yes.'

Morgan (1925), whilst apparently subscribing to what is here spoken of as juvenile recapitulation, holds that adult recapitulation is quite ruled out of court by the fact that variations are germinal in origin and discontinuous in mode of appearance. There must, however, be some defect in his interpretation of the phenomena of variation, for, as will be seen presently, many well-authenticated cases of adult recapitulation are known.

At this stage it is well to remind ourselves that even the most ardent adult recapitulationist realises that the record is usually more or less vitiated and incomplete. Haeckel himself, by the phrase ' abbreviated repetition of phylogeny,' acknowledges that the record is curtailed. He also recognised that it was subject to falsification by the appearance during early life of features specially adapted to the conditions of life of the embryo or larva. For this phenomenon he introduced the term ' cœnogenesis.' As long as these features appeared to be limited in their influence to the early developmental stages very little importance was attached to them. In recent years, however, Garstang on the biological side and Spath (1932) and Schindewolf (1925) on the palæontological side have shown that their influence may extend in a marked degree even into adult life, a process for which the last-named writer proposes the term ' proterogenesis.' This discovery I fancy has played no little part in the recent intensification of activity in undermining the pre-eminent position held by the principle of recapitulation.

In matters of this kind there is a danger lest we should slip into the assumption that only one method has been pursued by Nature, but it is surely a grave mistake to assume that she is so bankrupt in originality. The fact that serious workers can hold such diverse views indicates the possibility that Nature's methods are equally diverse. It seems appropriate therefore that an attempt should be made to re-examine the evidence in the hope of gaining a clearer understanding of the relationships of the various view-points to one another. From such a survey, geology as well as biology has much to gain ; for as long as systematists drift along with only a confused appreciation of the laws of development and evolution they will be without fundamental principles to guide them in dealing with the multitude of specimens which are coming, in ever-increasing volume, from geologists in the field.

Naturally my approach to this survey will be from the palæontological side, but I hope there is still enough of the zoologist left in me to enable me to appreciate fully the more purely biological points of view. My illustrative material will be confined mainly to well-known examples, descriptions of which are easily accessible.

Any consideration of the relationship of development to evolution must deal with the subject from two aspects, viz. *retrospective* and *prospective*. On the one hand it must inquire whether the evolutionary changes of the past are reflected in development, and if so to what extent. On the other hand it must also inquire whether future evolutionary changes of sudden or of sequential character are foreshadowed in development. These two aspects are, of course, very closely interwoven with one another in the developmental record, and much confusion, which has crept into discussion in recent years, is due to a want of appreciation of their fundamental distinctness.

Retrospective Aspect.

In one form or another the retrospective aspect of the problem of the relationship of development to evolution has attracted the attention of embryologists even in the earliest stages in the growth of their science. This is exemplified by the principles enunciated by Von Baer and Haeckel, even though the former dates back to the pre-evolution age of biology. These two great workers, had they lived to-day, would have been the first to condemn any tendency to make a creed of the form of words in which they expressed the principles they detected ; they would have been the first to welcome any modification or amplification made necessary by the advance of knowledge. No attempt will be made here to trace the history of discussion on these problems, for it has been frequently summarised by various writers referred to in this address.

Concerning the Use of Terms.

Some of the disagreement that exists over the questions under discussion is due to diversity in the shades of meaning attached to terms in common use. It will therefore be helpful if some of these are briefly indicated here.

The term ontogeny is generally taken to mean the development of the individual, but it is not always clear how much of that development the writer has in mind. As long ago as 1909 Cumings pointed out that many biologists used the term ontogeny when they were really visualising only embryogeny. Even Haeckel himself did this. Strictly speaking, it includes all stages of growth from the embryonic through the epembryonic or neanic to the adult.

With regard to phylogeny, De Beer indicates a common though often quite unconscious restriction in the use of the term when he tells us that ‘ the distinction between adult and young, i.e. between structures which appear late or early, is drawn principally because it is only the structures of the adult which are concerned in phylogeny.’ Garstang (1921), it seems to me, gives it the correct significance when he defines phylogeny as ‘ the procession of ontogenies along a given phyletic line of modification,’ though he goes on to point out that ‘ it is expressed in terms of adult structure.' This is not, however, always the case. It is true for such organisms as belemnites and trilobites in which only adult features are visible in the fully grown specimen. But it is not quite true for organisms, like ammonites and gastropods, for which in any given specimen large portions of the ontogenetic record are exposed to view, and provide juvenile features which become automatically incorporated into the diagnosis of the species and consequently into the construction of the corresponding phyletic series.

Some of the shades of meaning that attach to the term 'recapitulation' have already been discussed. Another must now be mentioned. For Haeckel himself this term implied the idea of causation as well as of repetition. I doubt whether this causal relation of ontogeny to phylogeny has any place in the thinking of the average palæontologist. For him recapitulation is merely a descriptive term for the observed fact that there exists a striking resemblance between some stages in individual development and the ancestral types. But even as a descriptive term it requires the addition of certain qualifying terms such as 'general' and 'specific,' 'adult' and 'juvenile,' which have been already suggested.

A marked divergence occurs also between various writers in the use of the terms 'ancestor' and 'ancestral.' Thus Garstang, referring to recapitulation as enunciated by Haeckel, says (1921), 'The only way I can see of establishing this theory by purely embryological methods, is to show that the penultimate stage of the ontogeny of a given type of adult resembles the final (adult) stages of the ontogeny of some theoretically ancestral type more closely than it resembles the corresponding penultimate stage of the same.' He then repeats the same kind of requirement for the antepenultimate stages also.

Discussing various examples brought forward by other workers, he declares that they all fail to survive the imposition of this test. In coming to this conclusion he apparently does not realise that they fail for the simple reason that the material used in the examples quoted did not conform to that stipulated in his test. His test requires that the comparison should be between the ontogeny of a given type and that of an *ancestral* type. The examples he quotes deal almost exclusively with the ontogeny of forms co-existing at the present day, and therefore cannot possibly have the relationship to one another of ancestor and descendant. They are in fact merely collateral descendants. Had he selected his material from the works of his palæontological colleagues he would have found much which closely fulfilled the conditions required by his test. Some of these will be considered presently.

Another term that calls for consideration is 'race,' for by using it in quite different senses writers have fallen into much unnecessary contradiction. In seeking to discover the relationship between development and evolution it has always been the custom to think of development in terms of a specific individual, and of evolution in terms of the 'race.' Now about the development of the individual there is no ambiguity, for the individual is a single being having a range in time from the cradle to the grave. On the other hand the phrase 'evolution of the race' is used in almost as many different senses as there are examples quoted.

Thus the slits in the neck of the human embryo are compared with the gill clefts of a present-day dogfish, and the similarity between them has been referred to as evidence that the development of the individual, at least in this respect, tends to recapitulate the history of the race. On the other hand the much more striking dissimilarity between the human embryo as a whole and the adult dogfish is given as evidence against the 'law of recapitulation.' What is the sense in which the word 'race' is being used in these or similar cases?

Apart from the point already dealt with, that only collateral ancestors are being compared, the disputants on both sides have in mind the whole sub-kingdom of the vertebrates with a range extending across nearly the whole of geological time, and represented by masses of rocks twenty or more miles in thickness. But when a palæontologist compares the young stages of growth of *Gryphæa incurva* with the fully grown *Ostrea irregulare*, and maintains that the close similarity between them is evidence in favour of the 'principle of recapitulation,' the conception he has of the word 'race' extends only slightly beyond the bounds of a gens or sequence of closely related species, and corresponds to a range of time represented stratigraphically by only about 30 feet of rocks near the base of the Lias. The *Gryphæa* material forms practically an unbroken series, almost as continuous as the Great North Road, but the dogfish and human materials are relatively more remote from one another than are London and Edinburgh.

In both cases the evidence is valid only as far as it goes and no further. The evidence of the human embryo, relating as it does to the extremities of a sub-kingdom separated by several hundred millions of years, cannot in any way be quoted as invalidating the evidence of *Gryphæa* concerning the relationship of development to evolution within the limits of a couple of genera, ranging with almost complete continuity across possibly less than a quarter of a million years. In recent years the discussion of these problems has been marked by a strange lack of a sense of proportion, a sense which must be maintained if any progress of thought is to be made. To deny that there is any truth in the principle of recapitulation or, on the other hand, to talk as though it were universally applicable, does not conduce to clear thinking.

The Evidence of Zaphrentis delanouei.

Now that some of the confusion in the use of various terms has been cleared up we may proceed to lay a stable foundation for our subsequent thinking by making a detailed analysis of a well-established evolutionary series. For this purpose no better example can be taken than that provided by the work of R. G. Carruthers (1910) upon *Zaphrentis delanouei*. At this point I must express my indebtedness to Mr. Carruthers and to the Director of the Geological Survey for giving me every facility for making a careful re-examination of the salient material upon which this work was based.

This example has the great initial advantage that it nearly fulfils all the requirements of first-class evidence. In the first place it is based upon a large number of specimens which, though they exhibit a wide range of forms, make up a continuous series. From these Carruthers selected samples typical of various phases in the sequence and called them *Z. delanouei* (*s. str.*), *Z. parallela*, *Z. constricta*, *Z. disjuncta* (early, typical and advanced) respectively (Fig. 1). Between these types there occurred every gradation of form. In the next place these specimens were collected from a succession of known horizons in the Lower Carboniferous rocks of Scotland. Though some of these horizons were separated by relatively wide intervals the range of variation exhibited by the specimens collected at different levels overlapped to such an extent

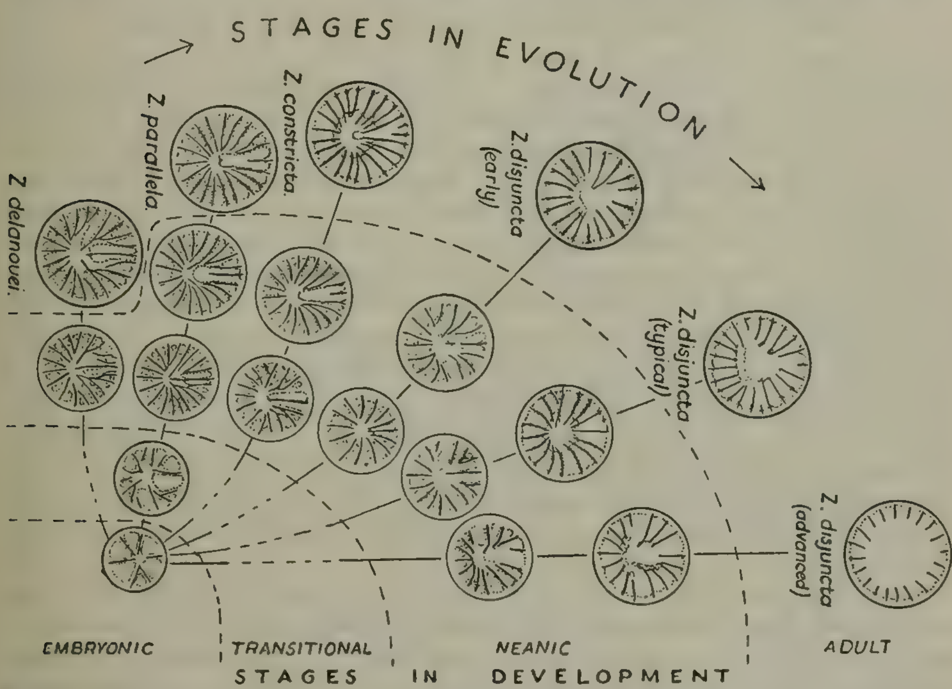

FIG. 1.—Diagram showing typical representatives of the gens *Zaphrentis delanouei*, the order of their appearance in time and the main stages in their development. (Modified from Carruthers.)

that the continuity in the sequence of forms, from the bottom to the top, was not broken. Further, the frequency of occurrence of each of the types was recorded, and when plotted produced a curve which conformed to the normal unimodal frequency distribution curve. When the curves for successive levels were compared it was found that the mode shifted with the passage of time from *Z. delanouei* (*s. str.*) at the bottom to advanced forms of *Z. disjuncta* at the top, thus showing that the stock was undergoing a corresponding evolutionary change during the period of its existence. The evolutionary character of this sequence was further supported by the very close similarity of the developmental stages of the later to those of the earlier types.

For the purposes of making a comparison between the development of the individual and the evolution of the stock, the specimens selected for developmental studies should be taken from among those which lie upon the mode at each horizon. It will be shown later that if this be not done a curious inversion of the truth may arise. In this connection it is interesting to note that, though Carruthers had not in mind the specific problems we are now discussing, four out of the six specimens whose development he describes fulfilled this condition exactly, and the other two lay only a little to one side of the mode. As to the sequence in time a little doubt attaches only to his middle pair, for from his records it is not quite certain which of the two specimens belongs to the lower and which to the higher horizon. Nevertheless the intermediate position in time of this pair between the other two pairs is beyond dispute.

It has been necessary to enter into all this detail because, as will be seen later, very great theoretical importance attaches to this material and it is well to know at the outset its precise standard of reliability. It will, I think, be agreed that the standard is a high one.

On examining the development of the individuals representative of the stages in the phylogeny of the *Z. delanouei-Z. disjuncta* gens it at once becomes obvious that the penultimate stage in the growth of *Z. parallela* bears a much closer resemblance to the adult of the ancestral species *Z. delanouei* (*s. str.*) than it does to the adult of *Z. parallela*. In like manner the penultimate stage in the development of *Z. constricta* repeats the sum-total of the characteristics which distinguish the adult ancestor *Z. parallela*, whilst the antepenultimate stage exhibits a similarly close resemblance to the ancestral adult *Z. delanouei* (*s. str.*). Here then is an example which fulfils almost, if not quite, perfectly the requirements of the test imposed by Garstang, and proved beyond dispute that specific recapitulation of adult characters does under some circumstances actually take place.

Turning now to the later stages in the evolution of this gens it may be observed that two tendencies, only faintly indicated in the earlier stages, now become more openly manifested. One is the tendency towards the establishment of radial symmetry. This is expressed, feebly in *Z. parallela* and more clearly in *Z. constricta*, by the central narrowing and peripheral widening of the fossula. In those later stages which are referred to as *Z. disjuncta* a second tendency is rapidly expressed in the shortening of the septa, and their withdrawal from the centre, a tendency which in the earlier members of the gens had affected only the cardinal septum. These tendencies are exhibited in progressive degrees of advancement not only in the late life of successive adult stages, but they also pass back into the penultimate and eventually into the antepenultimate developmental stages of the typical and later forms of *Z. disjuncta*. Thus the principle of specific recapitulation of adult characters holds good also for these two new tendencies.

In addition to being new, these two tendencies are also out of accord with and may involve a complete reversal and suppression of earlier tendencies. Thus the assumption of radial symmetry implies the disappearance of the tetrameral symmetry, so characteristic of the typical *Zaphrentis*; whilst the shortening of the septa is the reverse of the process of elongation by which each septum in early phases both of development and evolution attained the centre of the coral. Thus it comes about that in the later members of the gens there is as it were a conflict between these earlier and later discordant tendencies, with the result that the antepenultimate stages exhibit a mixed combination of features made up of the long cardinal of *Z. delanouei* (*s. str.*), the elongated septa of *Z. constricta* and the radial arrangement of *Z. disjuncta*. In these stages, therefore, there is merely a recapitulation of some of the individual features, but not a recapitulation of the combination of features of the adult of any preceding generation. It becomes advisable, therefore, to distinguish between *complete recapitulation* of the whole or part of the adult combination and the *limited recapitulation* of only isolated adult features.

Up to this point the earliest developmental stage which has been considered has always been one in which the individual was already sufficiently advanced to have attained a diameter of about half that of the adult. Obviously, therefore, important earlier stages still remain to be considered. Unfortunately very little information for these is forthcoming, for the pointed end in the specimens of this coral is rarely preserved. My re-examination of Mr. Carruthers' material, however, has enabled me to see several earlier stages than those which were figured by him. Of these there were three which only slightly preceded the earliest figured by him for *Z. delanouei* (*s. str.*) and for typical and advanced forms of *Z. disjuncta*. The two latter exhibit the same absence of resemblance, except in isolated features, to the adult of any previous species. On the other hand they did closely resemble the correspondingly young stage of *Z. delanouei* (*s. str.*). They provided, therefore, as excellent an example of the recapitulation of juvenile conditions described by Von Baer, and emphasised by modern biologists, as the later growth stages provided for the recapitulation of adult conditions reiterated above.

In the development of the typical *Z. disjuncta* a much earlier stage was fortunately preserved. In this there were only six septa, but these were arranged in an almost perfectly radial manner. Whether they were equal to one another in length was uncertain, for the section may have been slightly oblique to the organic axis of the coral at this level. Though the corresponding stage in the other members of this gens was not forthcoming in the material discussed above, it may be noted that it had been recognised by Duerden (1906), Carruthers (1906), and Butler (1935) in the earliest stages of development not only of other species of *Zaphrentis* but also in other palæozoic genera, viz. *Lophophyllum*, *Cyathaxonia*, *Dibunophyllum*, *Cyclophyllum*, *Streptelasma*, *Syringaxon*. Duerden sums up his investigations by saying, 'The rugose corals and the zoanthid actinians have both a primary hexamerism.'

The embryo in this case appears therefore to retain features characteristic only of the embryonic stages in the development of other members of the phylum, for as yet no adult coral of earlier date is known to possess them. The examination of this very young stage in the development of *Z. disjuncta* therefore furnishes further confirmation of Von Baer's principle. The careful consideration of the example before us, however, seems to necessitate a modification in the statement of that principle, for in this case the resemblance is not limited to organisms lower down in the scale, but extends to embryos of corals of later date and more advanced structure. Indeed, so far as it goes, the evidence indicates that this embryonic combination of features is one which may be characteristic of the whole class Anthozoa. Though Von Baer, like modern biologists, had in mind only contemporary animals, the facts show that the principle is applicable to forms belonging to different periods of time.

No doubt in the development of *Zaphrentis* there were, as in other Cœlentera, yet earlier stages, starting with the fertilised egg and passing on to a free-swimming larva, which of necessity are beyond the ken of the palæontologist. Keeping these in mind, as well as those discussed above, we may distinguish in the life-history of this, as indeed of other organisms, two main phases in development : the embryonic and the neanic respectively. The former covers a series of changes leading up from a single cell to a condition which, notwithstanding its relative complexity, has little or no resemblance to the adult but which, nevertheless, provides the basis out of which the adult may be produced. The latter covers that series of changes in the course of which the features which characterise the adult gradually emerge and ultimately attain full expression. These phases overlap one another and in so doing exhibit stages which are transitional in character between the two. In *Zaphrentis* the stages leading up to and including the corallum with six septa almost radially arranged belong to the embryonic phase. The immediately succeeding stages, during which the tetrameral symmetry is being established, are transitional. Those leading from this point onwards to the adult belong to the neanic phase.

In the embryonic phase the combination of characters seems to have attained a state of stability that furnishes a plan of structure which is common to widely separated members of the class. It must be regarded as the culmination of a long process of evolution of embryos in which many factors which concerned adult life have played no part, but in which factors foundational to adult development have been preserved. So far as known this embryonic condition in *Zaphrentis* bears no resemblance to the adult condition of any coral stock that could be regarded as ancestral to this genus. The features concerned in this ancient and stable combination appear to me to conform to those ' primitive types of structure ' for which Garstang (1921) proposed the term ' palæomorphic.' Should advancing knowledge bring to light adult fossils of an earlier ancestral stock and possessing these same features, then this term would have to give place to Haeckel's term ' palingenetic,' which Garstang rejects. This problem will demand very careful investigation. Thus, for example, the extraordinary resemblance between the attached dipleurula of echinoderms and the fossil *Aristocystis* among the blastoids may, apart from the presence of plates in the latter, be taken as recapitulation of adult characters. On the other hand the conditions of the resemblance may have arisen, not in the adult but in the larval blastoid.

In the neanic phase the organism exhibits a combination of less stable characters, superposed upon the stable embryonic foundation. These undergo, with comparative rapidity, a course of evolution the stages of which are very completely recapitulated during development. The fact must be emphasised that in so far as specific and complete adult recapitulation takes place it seems, in the example before us, to be limited to the neanic phase.

In the controversy briefly referred to at the outset biologists, in discussing the problems before us, have based their arguments almost entirely upon embryonic, larval or fœtal material. Palæontologists, on the other hand, have rarely had such material at their disposal, for such early developmental stages are either not capable of preservation in the fossil state, or they are such minute and delicate objects as the prodissoconch of lamellibranchs, the protoconchs of gastropods and cephalopods, the protaspids of trilobites, which are easily destroyed. The palæontologist's evidence therefore is usually drawn from neanic stages which, it may be noted, make up the major portion of the individual life-history and are more abundantly preserved in the fossil state. Inasmuch, therefore, as these two classes of workers are on the whole dealing with different portions of that life-history, their observations and the conclusions they draw are not contradictory but supplementary. As far as our study of *Zaphrentis* takes us we may say that the embryonic stages of development recapitulate the changes exhibited by corresponding stages of other forms belonging to the same general stock, and that the neanic stages recapitulate the adult condition exhibited by the preceding members of the gens to which the species belongs. Further, within the neanic stages the principle of acceleration or tachygenesis is perfectly exemplified, but its action, so far as the adult combination of features is concerned, does not penetrate back into the transitional and embryonic stages. In these latter the rate of acceleration does not remain the same for all features and consequently the adult combination undergoes disruption.

In the series *Z. delanouei* (*s. str.*)-*Z. constricta* the development of the later members runs parallel to but overlaps beyond that of the earlier. But with *Z. disjuncta* new tendencies enter, and though the earlier, typical and later members of this species exhibit in their development a like parallelism and overlapping, the direction they follow diverges from that of the former members of the gens. By acceleration these new tendencies ultimately cut out the older combination almost completely from the developmental record of the advanced members of *Z. disjuncta*. Here then is a very clear case of ' skipping of stages ' or lipopalingenesis of the kind referred to by Trueman as a ' straightening of ontogeny ' as opposed to ' mere shortening of ontogeny ' which results from tachygenesis. It should be noted that in this case the straightening is rendered necessary by the fact that divergent changes had previously set in.

Other Examples of Recapitulation.

With this general discussion, suggested by the study of *Z. delanouei* (*s. lat.*), as a basis to work upon we must now inquire whether the same general phenomena are recognisable in the development of other types of animal organisms. Unfortunately the number of cases in which the evidence is as satisfactorily established from the standpoints of quantity, development, variation and stratigraphical precision as for the example we have studied in such detail above is relatively small.

There is no group of organisms for which systematists have made such a full use of the principle of adult recapitulation as in the lowliest forms of life preserved as fossils, viz. the foraminifera. One illustrative example may be taken from the Orbitoides group. It includes the genera *Operculina*, which first appeared in the Cretaceous ; *Heterostegina*, in the Eocene ; and *Cycloclypeus*, in the Oligocene. In *Operculina* the shell is coiled spirally in a single plane and is divided by septa into chambers.

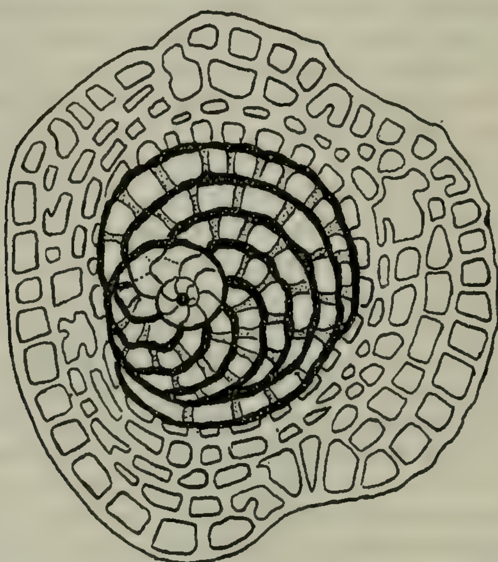

FIG. 2.—Diagram showing the structure and development of *Cycloclypeus post-indopacificus*. Protoconch (black spot). Operculina and Heterostegina stages (thick outline). Cycloclypeus stage (thin outline). (Modified from Tan Sin Hok.)

As the height of the coils increases with growth the chambers become tall and narrow. In *Heterostegina* the early development repeats in a typical manner the condition exhibited by *Operculina*, but in the outer or later coils the height increases greatly and the chambers become correspondingly much taller. Owing to an increasing forward bend in the middle the latter become practically semicircular, and at the same time each is divided into a series of chamberlets by the formation of walls across the chamber. In lowly species of *Cycloclypeus* (Fig. 2) the innermost coils are again typically operculine and are followed by others which are similarly heterostegine. In later development the height of the coils continues to increase until the chambers are quite circular. Henceforth the shell grows in size by the formation of successive chambers added to the outer margin of the shell. Tan Sin Hok, working upon material collected carefully from a series of strata in a continuous exposure, showed that with the passage of time there was a progressive reduction in the number of heterosteginal chambers. This was established by counting the number of septa between the chambers and treating the figures for a large number of specimens statistically. In the lowest part of the section the range in number was 30–18, with a major peak at 24. At the top of the section the range was 30–16, with the major peak at 21. The amount of reduction is not great, but that it took place was indisputable. When, however, material that has been collected from a series of isolated exposures representing a much longer range of time is examined the evidence for progressive reduction is much more striking. Workers on Tertiary deposits, when correlating one exposure with another, find that the evidence afforded by counting the chambers in these foraminifera is in complete accord with that derived from stratigraphical and faunal sources. The reduction indicated above is found to progress steadily from the base of the Oligocene, where the range is 38–21 with a maximum number of specimens having from 32–27, to the opening of the Quaternary, where the range is only 4–2 with a maximum at 3.

In this thoroughly well established evolutionary series we find that the relationship between Development and Evolution closely accords with that already seen in *Zaphrentis*. In the development of the earlier species of *Cycloclypeus* both operculine and heterostegine stages are well represented. In later species the operculine stage disappears and the heterostegine undergoes the progressive reduction described above. Here also the characteristic cycloidal chamber of *Cycloclypeus* appears first in late life and, with the passage of the generations, shifts back to earlier and yet earlier stages of growth until the heterostegine stage has almost completely disappeared also. In this example the principle of adult recapitulation with its accompanying phenomena of tachygenesis and lipopalingenesis dominates an even larger proportion of the life-history than it did in *Zaphrentis*, for apparently only the proloculum is unaffected by it.

Lack of time and space forbids a detailed consideration of other examples (cp. Cumings, and George), but lest it should be thought that the above are exceptional a number of others must be briefly mentioned. Among the lamellibranchs is the case of the gens *Gryphæa incurva* so well established by Trueman (1915). Here the earliest known developmental stage, the prodissoconch, is a minute embryonic bivalve shell identical with that of the oyster and of other lamellibranchs. This furnishes a clear case of juvenile recapitulation. The neanic phases of development are well known for all the members of the gens from *Ostrea irregulare* to *Gryphæa incurva*. In each case the resemblance of the young of later forms to the adults of the earlier forms is most striking. Here, however, lipopalingenesis plays no actual part. The same is true also for the gens *Inoceramus concentricus-sulcatus* established by Woods.

For the gastropods, Smith (1906) has worked out the evolution of *Volutilithes sayana* through *V. petrosus* from *V. limnopsis*. His material was abundant and its time succession based upon a series of good geological exposures. He shows that in the development of *V. limnopsis* the surface of the shell is at first quite smooth. It then becomes decorated for a short distance with curved transverse ribs upon which longitudinal ribs are next superposed, thus giving to the ornamentation of the shell a cancellated appearance. This condition persists to the end of life. In *V. petrosus* the curved rib and cancellated stages appear earlier in development and are succeeded in late life by a spiny stage. This latter, in turn, experiences an acceleration in the time of its appearance in later members of the species and in *V. sayana*. Senile characters also exhibit a like behaviour.

At one time the ammonites were the citadel of recapitulationists. Unfortunately so much scepticism is expressed by present-day specialists on this group concerning the work of their predecessors that it is difficult to find examples that are above suspicion. This is not, however, the case with the detailed work done by Bisat (1924) on the goniatites. Though he deliberately refrains from analysing his data from the standpoint of the principles of development, in dealing with *Reticuloceras reticulatum* he does allow himself to give, in tabular form, a valuable summary of his observations upon the changes undergone by the ornamentation of the shell in the course of the evolution of this species, and upon the times of appearance of these changes during the development of successive mutations. He thus provides us with yet another well-established case of adult recapitulation.

Among brachiopods many examples are forthcoming, but reference must here be limited to Fenton's detailed work (1931) on evolution in the genus *Spirifer*, a work that will repay careful study by all students of palæontology. The requirements of ample material in all stages of development collected with meticulous care from minutely zoned strata are sufficiently fulfilled to satisfy the most exacting critics. Space does not permit of the description of specific examples here. We must therefore be content to quote Fenton's own words. Speaking of the two gentes *S. varians* and *S. obliquistriatus* he says, 'there is also a close correlation between ontogeny and phylogeny in each pair of trends. Recapitulation is detailed, uniform, and generally involves the repetition of adult characters. . . .'

In the examples considered hitherto the study of the development has been easy because, as growth proceeded, the early stages were not destroyed but were retained, and the later stages were added to them. With many other organisms this is not the case. Thus the trilobite as it grew shed its skeleton, and with it all record of its juvenile features, at more or less regular intervals, so that specimens of adult trilobites show only adult features. Careful collecting from very fossiliferous beds may result in the discovery of series in various stages of growth, and workers who have collected such series claim, with good justification, to have discovered in them evidence of the working of the principle of recapitulation. But for our purpose, just at this moment such cases are not sufficiently well authenticated. The same is true also for the echinoids which reabsorb the earlier formed skeletal deposits as growth proceeds.

On glancing back over the general survey of facts made above, one overruling condition seems to emerge, viz. that the more lowly and simple the organism the more complete is the recapitulation. In more complex organisms the chances against a perfect repetition of the whole combination of adult features are greater, and consequently recapitulation is more likely to be less complete. Nevertheless, even in highly organised animals limited recapitulation is very common. Odd examples of this have always attracted attention, and indeed have been the basis upon which earlier workers, not excluding Haeckel himself, founded their belief in the general principle. But what was evidence for them can no longer satisfy us.

Localised Recapitulation.

Another set of facts which bears upon the problem may now be briefly discussed. They were first noticed by Jackson (1892), who grouped them under the heading 'Localised Stages in Development,' and are commonly shown in those parts of the body which are metamerically repeated or are reproduced by budding. Since it is in such parts that evidence is forthcoming only limited recapitulation may be expected.

Good illustrative examples of localised recapitulation are found among the echinoids, but unfortunately detailed stratigraphical evidence is usually lacking. In the palæozoic Lepidocentridæ, which appear to be the forerunners of the mesozoic echinoids other than the cidarids, the ambulacrum is narrow and made up of low laterally elongated plates pierced by a pair of pores. In the jurassic genus *Hemicidaris*, as in all the echinoids, new plates are added to the ambulacrum at its upper end. These move downwards towards the equatorial belt of the test and gradually assume the fully grown condition. The newly formed plates in the upper part are like those fully grown plates of the palæozoic genera in form and number of pores. As they pass downwards the plates become associated with one another first in pairs and then in threes having a common outline and decorated by a large tubercle. In its early stages of growth, therefore, each plate recapitulates the condition shown by the same structure in the ancestral palæozoic stock. Again, MacBride and Spencer (1938) have recently drawn attention to the interesting fact that, in the development of the ambulacral plates in certain modern forms, the podial pores first appear as notches in the lower border of the plate, a condition which characterises the adult condition in some ordovician echinoids.

The case of *Micraster*, though not so striking, is perhaps more valuable because the work of Dr. Rowe has produced a well-authenticated evolutional series, based upon very careful and detailed stratigraphical work. Unfortunately, owing to the mode of growth of the echinoids, the developmental evidence for the test as a whole is not forthcoming. So far as I am aware, attention has not hitherto been given to the existence of localised stages in the development of the ambulacra in these micrasters, but an examination of the uppermost portion of any well-preserved specimen of a high zonal variety of *M. præcursor* will show that it reproduces very closely the condition shown by the fully developed portion of the ambulacrum seen in low zonal varieties.

Recapitulation in Colonial Organisms.

A third body of facts which testify to the reality of the principle of recapitulation is yielded by colonial forms. This was concisely expressed by Lang in 1921 when, referring to a preceding discussion of criteria of relationship, he wrote, ‘ It was noticed that the colony, like the individual, exhibits growth stages of its own which recapitulate ancestral characters.’ In illustration of this reference may be made to the polyzoon genus *Stomatopora* in which he (1907) traces the evolution of the series of forms from *S. antiqua* and *S. gregoryi* of the Lower Lias (Sinemurian and Charmouthian respectively) to *S. smithi*, of the Cornbrash. In this series the angle between the branches at each dichotomy exhibits a progressive diminution from 180° in the earlier to 60° in the latest forms. In the development of successive types acceleration leads to the gradual elimination of the larger angles from earlier growth stages. The individual chambers or zoœcia exhibit a like phenomenon. In these the evolutionary change of form is from cylindrical or very slightly pyriform to markedly pyriform. Here again in the development of the later species acceleration of the quite pyriform stage leads ultimately to the elimination of the cylindrical zoœcium from the early stages of growth.

Summary.

Since the question of recapitulation, more especially of adult characters, has been the focus of controversy, it has been necessary, in order to secure its reinstatement in its proper place as an established principle of evolutionary thought, to deal with it at considerable length. It may be useful, therefore, before passing on to the next part of our subject, to summarise briefly the results of our discussion.

The divers shades of meaning which have been attached to the term ‘ recapitulation ’ by various authors in recent years reflect the phases which the phenomenon exhibits during the development of different organisms. In ontogeny two main classes of features have been recognised, viz. adult and juvenile, both of which undergo evolution in phylogeny.

Recapitulation of adult features occurs more especially during the neanic phases of development. It may be specific and complete, that is to say it repeats the combination of features exhibited by geologically recent adult ancestors. Cases in which this is manifested, even in some detail, appear to be limited to the lower and simpler grades of animal life. Adult recapitulation may also be specific and limited. That is to say it may repeat only a few of the features of the adult ancestor. In this case the features are not necessarily in correct combination shown by the adult ancestral species, they may indeed be drawn from several such types. Cases of this kind occur throughout the animal kingdom and include many well-known examples, such as the teeth in the young Ornithorhynchus, the three centres of ossification in the avian metatarsal, the horny claws on the wing digits of the unhatched duck.

Adult recapitulation is exhibited also during the growth of metamerically repeated parts, of colonial forms and rejuvenated individuals.

However complete and specific the recapitulatory record may be at the outset, in subsequent generations it becomes curtailed as the result of increasing acceleration in individual development. This leads to the 'skipping of stages' either by a 'mere shortening of ontogeny,' in cases where evolutionary trends remain constant, or by a 'straightening of ontogeny' where new trends out of accord with the foregoing set in. The record also becomes vitiated as the result of the fact that acceleration is not constant for all features, and consequently the combination of characters exhibited by the adult ancestor becomes broken up, or even eliminated from the development of the descendants.

Turning now to the juvenile features which characterise the embryonic stages of development, we find the problem is more difficult of elucidation. That these features, exhibited during the early development of the ancestor, are repeated during the corresponding or slightly earlier stages of the descendants may be regarded as established. The unsettled problem is the extent to which they reflect characters which in primeval times were in the first place peculiar to the adult, or had their first onset only in the embryo.

Prospective Aspect.

The task that lies before us now is to inquire the ways in which evolutionary changes may be foreshadowed during development. For this purpose our attention must be concentrated upon the new features which mark those changes, upon the mode and time of their appearance, and upon the way in which they fit into that framework of anciently derived characters discussed above.

In dealing with this aspect there is no need to stress once more the importance of basing conclusions upon ample evidence made up of numerous specimens precisely dated. It should, however, be urged that specimens used for developmental studies must be selected from among those which lie upon or close to the mode of the frequency distribution curve for varieties occurring at each stratigraphical horizon. If this be not done, conclusions of an extraordinarily contradictory character may be drawn. This may be illustrated by reference once more to the gens *Zaphrentis delanouei*. As already seen, specimens selected from the mode for varieties collected from the Cementstone and Lower Limestone horizons belong to *Z. delanouei* and *Z. constricta* respectively, and the development of the latter faithfully recapitulates the adult condition of the former. On the other hand, if by any chance the specimens selected happened to be varieties at opposite extremities of the curve, viz. *Z. constricta* from the lower horizon and *Z. delanouei* from the upper horizon, then the young stages of *Z. constricta* would appear to anticipate the adult condition of *Z. delanouei* and therefore to support the principle of proterogenesis, which will be discussed below.

In reading Garstang's discussions of kindred problems from the biological side, I find myself very largely in agreement with him as far as the evidence at his disposal takes him. Any difference that exists between us seems to me to be due to the fact that he had not before him any satisfactory evidence for the recapitulation of adult stages. He suggests

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therefore the dropping of Haeckel's terms 'palingenesis' and 'cœnogenesis,' apparently because the former implies that characters which appear in the adult stage are heritable, whilst the cœnogenetic characters are limited in heritability to the larval stages. Since the evidence at his disposal led him to believe that new characters enter the phyletic history only during early ontogeny he proposed the terms 'palæogenetic' for those ontogenetic processes which functioned early in phyletic history and 'neogenetic' for those which came into action later. He differentiates structures in a corresponding manner by using the terms 'palæomorphic' for primitive types of structures and 'neomorphic' for modified types of structures.

Cœnogenesis (the appearance of new characters at an early stage of development).

Though Haeckel's main emphasis was upon recapitulation he realised that certain factors were at work which tended to vitiate the developmental record. Among these was the appearance, in larvæ and embryos, of features which were adaptations to the conditions under which these immature organisms lived. He crystallised his observations by introducing the term 'cœnogenesis' for this phenomenon and by distinguishing a cœnogenetic stage in development, which he regarded as having no recapitulatory and therefore no phylogenetic significance. Just as with his principle of recapitulation the advance in knowledge has entailed modification, so is it also with the principle of cœnogenesis, but to a much greater degree. It may be noted in passing that in the cœnogenetic stage the resemblance of the young to those of preceding generations is not wiped out, but the new characters are superposed upon a combination of ancient or primitive (palæomorphic) characters, built up during the evolution of the young themselves.

Garstang seems to have objected to the term 'cœnogenesis' because it implied that these larval characters exerted no influence upon the subsequent growth stages in either development or evolution. Nevertheless some cœnogenetic characters and the evolutionary changes they undergo are confined wholly to early development, and apparently exert no appreciable direct influence upon the later stages. This point was indeed stressed by Garstang himself for certain adaptations to motile life exhibited by larvæ. Of these he says, 'the modification of the larva in this way need not affect the organisation of the adult.'

Fossil examples are perhaps less easily demonstrated. One clear case, however, may be quoted from among the ammonoids in which the protoconch undergoes evolutionary change. Those changes which find systematic expression in the terms 'assellate,' 'latisellate' and 'angustisellate' do not appear to have influenced the later developmental and evolutionary changes in any way. Considerable differences are recognisable between the protaspids of various trilobites and the protoconchs of gastropods. They also appear to have no effect upon the subsequent development of these organisms.

Some cœnogenetic characters may possibly have exerted a radical influence upon subsequent growth and evolution, though they themselves have undergone no change since their first appearance. An outstanding example of this has been claimed by Garstang (1928) from among the gastropods. In some of these, whilst the organism is still embryonic, the visceral hump with its shell rotates rapidly in relation to the rest of the body through nearly 180° in only a few hours. Though, for the sake of discussion, I shall use Garstang's example I am not convinced that he is right in concluding that this phylogenetic twist arose primevally in the larval stage. The twist exhibited by the embryonic gastropod is not more striking than is the contortion experienced by the equally young starfish. Starting from a condition of attachment by the preoral lobe and an outward attitude of the mouth, this passed rapidly to one of free movement with the mouth facing downwards. Bather (1915) has shown that the fully grown edrioasteroid of early palæozoic times must have been equally at home in either position. The Edrioasteroidea therefore provided a stock through which such freely moving forms as starfish and sea urchins could have been derived from the more primitive attached ancestors. When one watches a gastropod, as it crawls along, rotating its visceral hump and shell from this position to that, he cannot deny to its primeval ancestors an equal flexibility, and to those that most frequently rotated the mouth of the mantle cavity into a forward position possibly a greater selective value. Thus with gastropods, as with edrioasteroids and their descendants, a bodily position which at first was assumed by adults as a temporary convenience may have become stabilised into a permanency.

Consideration of some well-known facts among fossils brings to light other possible examples of the cœnogenetic origin of new characters which have influenced subsequent history. Thus in the oysters and in forms derived from them the process of cementation of the shell to other objects is confined to early life. It must, in all probability, have originated at about the close of the embryonic phase and remained with varying degrees of persistence into early stages of the neanic phase, but rarely if ever into later life. Here as with the torsion of the visceral hump of the gastropod the change was cœnogenetic, but it has brought in its train, or opened the way for, series of other changes such as the marked variability of form in the oysters and various degrees of coiling in *Gryphæa* and *Exogyra*. Nevertheless, even in such an advanced form as *Gryphæa incurva* this attached stage is preceded in development by the prodissoconch which exhibits primitive or palæomorphic characters. These observations appear to be equally applicable to other organisms which have become attached by other means such as a byssus or by spines. In each case the character that has been introduced cœnogenetically does not displace but plays its part along with those which belong to the ancient category.

Proterogenesis (the extension of new characters from early to late stages of development):

The cœnogenetically introduced characters discussed above are either confined to the early stages of development or, if they last into adult life, do not undergo further expression or change. In the case now to be considered they extend gradually into later stages. Recently Schindewolf in Germany and Spath in England have done good service by emphasising the existence of palæontological evidence for characters appearing cœnogenetically and extending, in subsequent generations, through later stages into the adult. In 1925 Schindewolf proposed the term ' proterogenesis ' for this principle of ontogenetic anticipation. In 1933 he wrote a fuller account of the principle and furnished a number of examples of his own as well as from other writers.

The conception that the larval characters may exert an important influence upon adult organisation is not a new one. It is indeed familiar to biologists under the heading of pædomorphism. It was also dimly foreshadowed in the writings of much earlier workers. Thus for example Haeusler (1887), dealing with certain foraminifera of the family *Miliolidæ* from the Lias of Banbury, showed that forms, now referred to the genera *Nodobacularia*, *Ophthalmidium*, *Spirophthalmidium*, form a series ranging from the condition in which the shell is straight, except for the beginnings of a coil at the embryonic end, to one in which the whole shell is coiled. He ventures to suggest that the first is the more primitive condition and that, during evolution, the process of coiling extended into later and yet later stages until it eventually dominated the whole shell. It should, however, be noted that he produced no evidence for the actual sequence in time of these members of this series, and it will be shown later that a quite different explanation is feasible.

Schindewolf, in a treatise which deals with this subject at some length, assembles a variety of illustrative examples. They are a mixed lot and include some which do not really exemplify the principle he is discussing, but, as will be shown later, belong to a quite different category. For the present our attention must be limited to genuine cases of proterogenesis (pædomorphism), that is, to cases in which new characters appear early in development and extend during evolution into later life.

The simplest, clearest, and at the same time the most fully authenticated example which Schindewolf describes is yielded by fossils from the Ordovician rocks of the Scandinavian Baltic belonging to the nautiloid family of the Lituitidæ. The central genus *Lituites* is characterised by the fact that while the major portion of the shell is straight, the early formed portion is coiled. On the basis of the principle of recapitulation it has usually been assumed that *Lituites* was the retrogressive descendant of a completely coiled ancestor. Schindewolf, however, describes a series of forms (Fig. 3) which commences in the Vaginaten Kalk with the genus *Rhynchorthoceras* in which the shell is wholly straight or only slightly curved. This is followed in the Platyurus Kalk by a variety of forms, including *Lituites* itself, which exhibit various degrees of coiling. The series ends in the Chiron Kalk in *Cyclolituites* in which the shell is almost completely coiled.

Other examples quoted by Schindewolf are far from being convincing. Reference to a paper on certain foraminifera by Rhumbler (1897) from which he culls several cases reveals a flimsiness of stratigraphical evidence which rules them out of court for any serious discussion of the problem.

Indeed, Rhumbler in summing up his own conclusions says, 'We see therefore that the adult stage is just as good for generating new structures as is the embryonal end of the shell.' This can hardly be regarded as giving strong support to the principle of proterogenesis.

I do not propose to discuss his references to evidence drawn from human development, for we cannot hope to understand the principles which govern the development of such highly complex organisms as man until we have straightened out some of the tangle in which our ideas of the development and evolution of simpler forms have become involved.

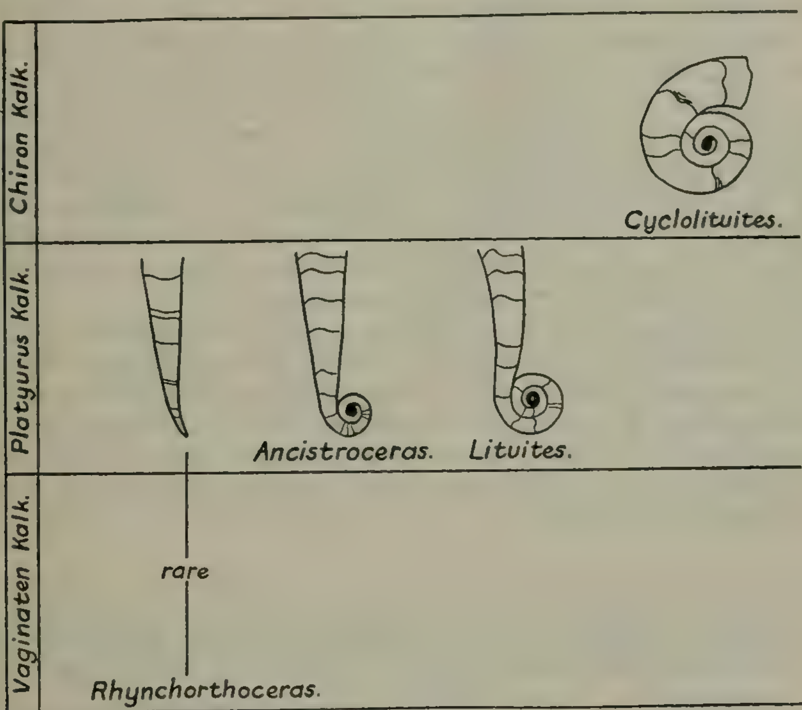

FIG. 3.—Diagram showing the distribution in time and the development of some representatives of the nautiloid family *Lituitdæ*. (Modified from Schindewolf.)

From time to time over a period of years Spath has made references to examples illustrative of the principle under discussion. They have, however, usually been buried in a mass of systematic detail that has made it difficult for others to extract who have not his unrivalled knowledge of a great multitude of ammonites. For this reason we look forward to the publication of his *Catalogue of the Liparoceratidæ* in which he has incorporated his evidence and views in a more accessible form. Meanwhile he has indicated the lines of his evidence in a recent paper (1936) and has done me the great kindness of giving me a personal demonstration with the help of a typical suite of specimens, and thus enabled me to give the following brief summary.

Dr. Spath claims that the various members of this family arose out of a stable stock of forms referable to the genus *Liparoceras*, which ranges from the upper part of the Jamesoni zone of the Lower Lias up into the Margaritatus zone of the Middle Lias. These are characterised by shells that are closely coiled and are very inflated. Associated with these are a number of variations which are less closely coiled and not quite so inflated. In the Ibex zone and the lower part of the Davœi zone occur forms, of the genus *Androgynoceras*, in which the inner whorls are not inflated, and are ornamented with stout ribs, and possess that cluster of features which characterises the capricorn ammonite. The outer whorls of these, however, revert to the condition seen in *Liparoceras*. In the upper part of the Davœi zone forms occur in which the capricorn condition extends into the adult stage to the complete exclusion of the *Liparoceras* combination of characters from the life-history. It seems, therefore, that the capricorn condition enters the stock as a set of new characters in the early neanic stage of development of the early generations and proceeds during evolution through late neanic stages to the adult in subsequent generations. Other features, such as the forward bend of the ribs on the venter, so characteristic of *Oistoceras*, and the deep lateral lobe in the suture line of *Becheiceras*, he assures me likewise put in their first appearance in early development and subsequently invade the later growth stages of succeding generations.

It may be noticed that the extension of the new features into later developmental stages is accompanied by a corresponding delay in the time of appearance of the older features. This latter phenomenon was also observed by Buckman, who proposed for it the descriptive term 'Bradypalingenesis' (1920). So near was he, and yet so far away from realising that some principle other than recapitulation was at work in development and evolution.

Deuterogenesis and Tachygenesis (the appearance of new characters at the latest stage in development and their extension to earlier stages).

Looking at the facts of recapitulation from the prospective point of view it will be realised that new trends of change, exhibited by the various gentes discussed in the former part of this address, show themselves for the first time in an incipient form in the adults of earlier species, and become increasingly emphasised in the adults of subsequent generations. Meanwhile the incipient phase passes by an acceleration of developmental processes (tachygenesis) into the early life of these generations. In other words, new characters, or rather trends of change, may enter the stock in the later stages of development, and the changes passed through in development in later generations go beyond and overlap those seen in the earlier. This mode of entry of new characters may be described as DEUTEROGENESIS in contradistinction to cœnogenesis.

There is no need to describe specific examples of deuterogenesis in detail, since every case of recapitulation looked at prospectively provides all that is required. Thus in the gens *Z. delanouei*, etc., the shortening of the cardinal septum starts in the adult of *Z. parallela*, and the shortening of all the septa starts in the adult of early forms of *Z. disjuncta*. Similarly the coiling manifested in late forms of the gens *Gryphæa incurva* were anticipated in late stages of growth in the earlier forms.

‘ *Mutation.*’

Fenton in the work already mentioned describes a very large number of new species, sub-species and ‘ forms,’ and provides numerous diagrams and tables which show their morphological relationships, their times of appearance and their ranges in time. An inspection of these reveals the fact that, whilst in some groups the new types appear in sequence at relatively wide intervals of time, in others they come on rapidly, and in yet others they appear almost if not quite simultaneously. In this last case he suggests that they may have arisen by ‘ mutation ’ in the De Vriesian sense. He finds difficulty, however, in definitely asserting this to be the case because the group of new types may be arranged in a continuous evolutionary series. Personally I feel no difficulty in believing in the simultaneous appearance, in a large population, of types which fit into a series, for such could be regarded as one more phase or degree

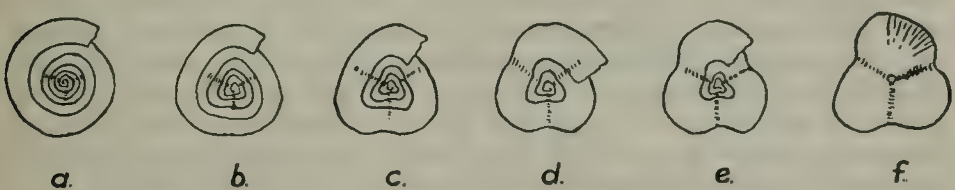

FIG. 4.—Diagram showing a series of clymenid ammonoids. *a–c*, Kamptoclymenia. *d*, Triaclymenia. *e*, *f*, Parawocklumeria. (From Schindewolf.)

of rapidity in production. There are not wanting facts which indicate that this simultaneous or sub-simultaneous appearance may be more common than is generally realised. Several examples may now be mentioned.

Among the examples quoted by Schindewolf in support of the principle of proterogenesis is one drawn from the cephalopod family the *Clymenidæ*, which lived during the Devonian period. It consists of a number of genera and species in which, at one end of the series, the shell has the normal type of spiral coil (Fig. 4) with an almost circular outline throughout development. In the next member of the series the innermost portion of the spiral has a triangular outline. In other members of the series the latter form of outline finds every degree of expression up to one in which it prevails at all stages of growth, including the adult. The series as it stands may be quoted in support of either the proterogenetic or the tachygenetic view, according to which end of the series is taken as the starting point. Schindewolf adopts the former. It is natural to look to stratigraphical evidence as the adjudicator between the two, but according to Schindewolf's account, all these grades appear simultaneously in the lowest stratum ; and the simpler forms, that is to say those in which the triangular outline is exhibited only in the innermost whorls, are progressively eliminated until in the uppermost strata only those remain in which all coils are affected. It may, of course, be said that more careful collecting from the lowest strata may show that these grades do actually follow one another in time; but taking the evidence at its present face value it points to the simultaneous appearance of all grades of the series, and suggests that the process at work is neither tachygenesis nor proterogenesis but simultaneous mutation in the biological sense of the term, and that the close resemblance to progressive evolution with the passage of time is not due to evolution but to elimination.

The conclusion thus suggested is so startling that the question naturally arises as to whether any parallel to it is furnished by other groups of fossils. For an answer it is unnecessary to seek further than Schindewolf's own examples. Among these is one, which has already been mentioned, from among the foraminifera in the family *Miliolidæ*, which bear a striking resemblance in form to the cephalopod family *Lituitidæ*. Haeusler, who is Schindewolf's authority for this example, tells us that the material he describes came from the Lias of Banbury and was supplied to him by Mr. Walford. The writings of the latter (1879) show that he collected the material from a layer of blue clay only three feet thick at the base of the Upper Lias.

In this connection it is interesting to note that Rhumbler in dealing with *Bigenerina* and *Textularia*, which are also quoted by Schindewolf in support of the principle of proterogenesis, says that the change from biserial to uniserial is so quick that the two forms occur side by side in the same geological layer. In view of this it is just as reasonable to regard this as a case of simultaneous mutation as of proterogenesis.

In dealing with the *Ostrea irregulare-Gryphæa incurva* series attention has already been drawn to the cœnogenetic mode of appearance of the habit of attachment. It may be noted here that the area of attachment already exhibits a very wide range of extent in this very early liassic species. Within the genus *Ostrea* this wide range remains quite constant even to-day, but in *Gryphæa* this is not the case. Hitherto it has usually been thought that in the evolution of the more fully coiled species the area undergoes a progressive reduction in extent. This is not, however, quite the correct way of stating the facts, for every grade of size already existed in the *O. irregulare* stock. What really takes place is not a progressive reduction of area but a progressive elimination of larger areas, leading to an increasing preponderance of small areas. This process of elimination was certainly a vastly slower process than the original production of the wide range of variation in size. So far as our knowledge goes at present it seems as though this wide range was the result of something akin to an explosion of mutations.

Among ammonites reference may be made to the subcraspedites fauna of the basement beds of the Spilsby Sandstone. Here side by side in the same layer, which is only several inches thick, occur a series of forms ranging from *S. primitivus*, in which the whole shell possesses a fine ornamentation, to *S. cristatus*, in which very coarse ribbing is dominant.

It will no doubt be said that the deposits containing these very varied forms are highly condensed, and that future work will prove that the several varieties follow one another in a definite order of time. Meanwhile the condition of preservation of the specimens in the actual deposits proves that the individuals whose remains have been found were practically contemporaneous with one another.

In conclusion it may be said that, though the case for such an explosion of serial mutants cannot be regarded as established, there is sufficient evidence to warrant us in taking the suggestion seriously. Should its occurrence be established it would provide a marked contrast to the type of mutation made familiar by experimental work. The contrast should probably be regarded as due to differences in method of study and of material. The experimenter breeds with isolated and controlled pairs, whilst nature breeds in a large freely mixing population with pairs drawn together by instincts which for the time being are beyond the experimenter's ken and across which his methods may be cutting. Made matches do not necessarily yield the same results as love matches.

The Inter-relationship of Processes.

While, for the sake of clearness, the several processes concerned in the survival during development of old characters and in the arrival of new ones have been considered separately, this is not the mode of their occurrence in Nature. Here the processes may manifest themselves side by side or in sequence in the same series of organisms or different processes may be dominant in closely allied forms.

Inasmuch as the particular individual that is being studied is the last of an almost infinitely long series of individuals each of which started life as a single cell, it seems inevitable that there should be some similarity between them in the succession of stages passed through in development and attained in evolution. This similarity is proportional to the proximity of the ancestor to the individual that is being studied. The facts put forward in the earlier pages lend strong support to this point of view and emphasise the importance of recognising this similarity, which is indeed the basis of all theories of recapitulation, as the background of all the other processes we have been considering. These processes do but render some portion more hazy and others they hide from view.

On to this background of survivals from the past are superposed all new characters. These, generally speaking, belong to one or other of two categories, viz. :

(*a*) *Unit characters* or *biocharacters*—features which appear fully expressed from the outset and undergo no subsequent change, e.g. torsion in gastropods, areas of attachment in lamellibranchs.

(*b*) *Trend characters* or *bioseries*—features which at the time of appearance are almost imperceptible but which in subsequent development and evolution become progressively more fully expressed, e.g. length of septa in corals, coiling in *Gryphæa*.

Unit characters may appear cœnogenetically, that is to say, at some early stage in development. Their appearance may open the way to other changes of a serial quality. Thus, for example, the twisting of the visceral hump in gastropods was followed by the progressive reduction.

of the gill and other structures on the morphological left side of the mantle cavity, and by a tightening up of the twist in some parts of the nervous system which is the despair of the student who is dissecting his first gastropod types. Though these changes cannot be followed in fossils, they are indirectly reflected by certain features in the shell such as the slits and siphons of the mouth margin, or they are associated with types of shell such as those of *Pleurotomaria*, *Littorina*, *Cerithium*, and thus throw some sidelights upon the geological history of these anatomical changes. Hitherto no cases of unit characters appearing deuterogenetically have been detected.

Trend characters, on the other hand, arise either cœnogenetically or deuterogenetically and proceed proterogenetically or tachygenetically towards later or earlier stages in life-history respectively in successive generations. In both cases the advancement of the trend is accompanied by a displacement of homologous characters—that is to say, characters situated in or on homologous parts. In the former case displacement is towards late life and culminates in the disappearance of the older characters at the end of life. In the latter displacement is towards early life and ends, usually at the junction of the embryonic and neanic phases, in the elimination of these characters. This phenomenon has long been known as lipopalingenesis.

A priori there seems to be no reason why both types of development should not proceed simultaneously in a series of solitary organisms for different sets of characters, but hitherto I have failed to detect any examples of this. That they may occur in sequence or simultaneously in closely allied organisms is well illustrated by the history of certain colonial forms, more especially the graptolites. As long ago as 1923 Miss Elles in her illuminating presidential address to this Section drew attention to a manifestation of the phenomenon now referred to under the heading proterogenesis. Speaking more specifically of thecæ she says, ' it may be noted at this point that all progressive development (anagenesis) occurs at the proximal and, therefore, youthful region of the rhabdosoma.' But, she goes on to observe, ' when retrogression (catagenesis) occurs it is in this same proximal region that the signs of former elaboration are retained.' In other words, the retrogressive changes proceed according to recapitulatory principles.

Miss Elles's statements have been recently amplified and fully illustrated in a paper by Bulman in which he gives a useful and suggestive summary of the present state of knowledge of the evolution of graptolites. Only two examples, selected from his account, may be mentioned here. The first is a progressive series, viz. *Monograptus raitzhainensis-Rastrites maximus*. Here the thecæ, that are tubular and distinctly separated from one another, appear for the first time at the proximal end of the stipe of the early member of the series. In later representatives the thecæ of this type extend progressively along the whole length of the stipe and thus clearly exemplify the principle of proterogenesis. Nevertheless it should be noted that the degree of elongation of the thecæ increases towards the distal end and that the degree thus attained in early types passes backwards towards the proximal end in later types. In this stock then proterogenesis of one feature and tachygenesis of another are proceeding simultaneously in one and the same series of organisms.

A second series, viz. *Rastrites peregrinus-Monograptus urceolus*, is retrogressive and illustrates the second half of Miss Elles's statement. In it the primitive closely approximated triangular type of thecæ reappear deuterogenetically at the distal end of the stipe, and in later members of the series they extend tachygenetically to early and yet earlier stages of the development of these. In association with this the less primitive tubular and isolated thecæ are gradually eliminated until only four or five are to be found at the proximal end.

Though in these two examples proterogenesis and tachygenesis happen to coincide with changes hitherto described as anagenetic and catagenetic it must not be supposed that these are synonymous terms, for in other cases the coincidence is reversed. Thus, for example, in the *Liparoceratidæ* the passage from the inflated whorls of *Liparoceras* to the much thinner whorls of the ' capricorn ' would usually be regarded as an example of catagenesis, nevertheless the origin and extension of the capricorn condition provides, as we have already seen, a typical example of cœnogenesis and proterogenesis. It is difficult to refrain from expressing the hope that Spath will now work out for us the other half of his story, to wit, the origin of *Liparoceras* itself. I am inclined to suspect that will it provide us with a good example of deuterogenesis and tachygenesis. But when we begin to hope, to suspect, and to prophesy, it is a sign that the springs of knowledge are drying up and that it is time to cease talking.

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SECTION D.—ZOOLOGY.

OCEANOGRAPHY AND THE FLUCTUATIONS IN THE ABUNDANCE OF MARINE ANIMALS

ADDRESS BY

STANLEY KEMP, Sc.D., F.R.S.

PRESIDENT OF THE SECTION.

In my title to this address I have used the term Oceanography, and I should like at the outset to enter a protest against the use of this word in a narrow and restricted sense, as a synonym of hydrography or the physics and chemistry of sea water. I must maintain that Oceanography is a comprehensive term, equivalent to the science of the sea. It includes within its scope not only physico-chemical work, coastal surveys, soundings and studies of tides and currents, which may collectively be referred to as hydrography, but marine zoology and botany as well, together with some parts of geology and even of meteorology. It is in this broad sense that the word is understood on the Continent.

The great advances which have been made in the study of oceanography may perhaps be said to have begun about seventy years ago, when the first marine biological station was established at Naples, when Maury was studying winds and oceanic currents, and when zoologists had just become aware of the new and unexplored realm of nature which exists in the depths of the sea. Before many years had passed H.M.S. *Challenger* made her celebrated voyage, and since then numerous expeditions have added to the wealth of our knowledge. Some of them, following the example of the *Challenger*, had zoological research as their main objective, but almost all of them made valuable contributions to our knowledge of the hydrography of the areas they explored, while, in recent times in particular, many research vessels have concerned themselves exclusively with this branch of oceanography. Notable results have also been obtained by ships with other primary objects : we owe, for instance, the greater part of our comprehensive knowledge of the Antarctic fauna to expeditions whose principal aim was geographical exploration.

The great marine expeditions have given us much knowledge that could not have been obtained in any other way ; it is to them that we owe our acquaintance with the oceanic and abyssal faunas and a very great deal of information on the currents and other hydrographical features of the ocean basins. For such work there is still a vast scope, many areas which would richly repay investigation by modern methods and many which are still unexplored. But oceanographic expeditions have their limitations, for as a rule they are only able to remain for brief periods in any one locality; in consequence they have seldom been able to obtain data on seasonal changes or fluctuations in hydrography, they cannot make any but the briefest observations on living animals and they cannot follow their life-histories. At marine stations, though only limited areas can be dealt with, these and many other studies can be carried out, and such work thus forms the counterpart to that of the expeditions. Once the success of Dohrn's station at Naples was perceived other similar marine biological laboratories were founded and in more recent times several institutions have been established which confine themselves to the study of hydrography. In Europe and in North America, though there is still ample room for expansion, we may consider ourselves well supplied, at least so far as biological stations are concerned, but in other parts of the world the facilities are for the most part quite inadequate. In many biological studies we are now reaching a point where observations on other faunas are essential to further progress, and a well-equipped tropical station in one of the richest areas of the Indo-Pacific region is rapidly becoming an urgent necessity.

There is, in this very brief outline, another and more recent development to be recorded. In 1895, when Sir William Herdman presided over this section at Cardiff, he spoke of the results of the *Challenger* expedition and urged that in the interests of the national fisheries an expedition should be fitted out, to last two years, to make a systematic exploration of the waters surrounding the British Isles. He evidently realised what is sufficiently obvious to us to-day, that expeditions of brief duration cannot supply all that we require and that for the study of the life history of almost any marine animal at least a whole year is needed. Matters did not take exactly the turn that Sir William Herdman advocated, the two-year programme was never undertaken; but a better course was adopted in the creation of state fishery laboratories, most of them with their own research ships. Though the activities of these departments are naturally restricted to economic problems they have contributed most handsomely to the study of oceanography. To marine zoology in particular they have brought great benefit, for by their intensive studies they have given us complete, or almost complete, accounts of the natural history of a number of fish, with detailed information far beyond what we possess for any other marine organism. To acquire this knowledge, which is clearly necessary for the scientific study of fishery problems, is a long and arduous task and as yet it is by no means finished. We are not yet able, and may never be able to make two fishes grow where one grew before; but the application of scientific methods is showing the way in which stocks of fish can be utilised to best advantage, and the success of fishery prediction must have struck even the most casual observer. It is not too much to say that the fundamental knowledge by which the major problem of the conservation of the stock can be solved has already been obtained, and this is a matter of vital importance to the fishing industry.

The rapid progress which has been made in oceanography is thus, in my estimation, due to these three agencies: to the expeditions, the marine stations and the fishery departments. The expeditions can reach areas which cannot be touched in any other way, while the marine stations and fishery departments have the great advantage of continuous observation. The only oceanic region in which continuous observation has been attempted is the Antarctic where the ships of the Discovery Committee have been working for the past thirteen years. Such work has proved to be highly remunerative in results and similar regular long-term investigations, designed to elucidate problems connected with the Gulf Stream, are now beginning in the western North Atlantic.

If I were asked to specify those branches of marine biology in which we have recently made the greatest progress I should say physiology and natural history. Very wonderful advances are being made in our understanding of function in marine animals, and this is due to the great volume of important work achieved by those whom I may call the zoological physiologists. Their researches are throwing a flood of light on many difficult problems, and if I do not discuss their work in detail to-day it is not that I do not recognise its significance and value, but rather that I feel incompetent to do it adequate justice.

In natural history we have made great strides. Work in this branch of biology has, I believe, been stimulated by the fishery departments, for when the importance and interest of the intensive study of individual species of fish was recognised, zoologists became anxious to apply the same methods to other marine animals. Though the need for correct identification will always remain fundamental, the days when the zoologist felt that his work was ended with a systematic diagnosis and the writing of a label have long since passed, and in recent times most excellent work has been done on the life-histories of marine animals and on their relations to their organic and physical environment. In all groups of organisms from diatoms to whales progress has been made, and for a goodly number of species we can now answer the simple questions that spring to the lips of every visitor to an aquarium : 'What does it eat ?' 'How does it breed ?' 'How long does it live ?'

A most important feature of animal life in the sea is the constant occurrence of large variations in abundance, and these, though they may not be greater, appear to be more general in their incidence than in land animals. We owe this knowledge mainly to the exact work carried out by the fishery departments, but though it is of fish that we have the best data there is no reasonable doubt that marine invertebrates are affected in the same way.

Annual fluctuations in the abundance of a fish may be very great. One year may be exceptionally favourable, with production far above normal, to be followed perhaps by several years of scarcity ; and it is not uncommon to find that fish belonging to one year class are fifty times as numerous as those of another. These great fluctuations, which are the foundation on which fishery prediction is based, are for the most part to be attributed to events which happened in the early months of the fish's life ; and when we consider the manifold perils, meteorological, physico-chemical and biological, to which the eggs and larvae of a marine animal are subject, it is little wonder that there may be such great differences from one year to another, nor is it a matter for surprise that the precise reasons for good and bad spawning seasons are as yet unknown.

Some very valuable information on fluctuations in year classes of fish has recently been collected by the International Council for the Exploration of the Sea.[1] The object of the Council was to summarise data on good and bad survival years in some of the principal food fishes, and the reports from the specialists who were appointed to undertake the work are of particular interest. For some fish the available information was found insufficient; but for cod, haddock, herring and plaice the data are adequate, at least for some areas. The results show that in different parts of the north-east Atlantic there are with rare exceptions no coincidences in good or bad spawning seasons, even if one species only is considered, and the evidence thus is that the fluctuations which are observed are regional in their incidence.

There is, I believe, good reason to hope that with improved knowledge of the spawning areas and more exact information on the environmental factors during the critical period the causes of these annual fluctuations will in due course be discovered.

But, of recent years, it has become apparent that in addition to the annual fluctuations there are other over-riding influences at work, which not only affect the abundance of marine animals, but may bring about great changes in their distribution. Since I have been at Plymouth I have been impressed with the very marked changes that have taken place in the western half of the Channel during the past seven or eight years, and the evidence points to the existence of long-period fluctuations which are superposed upon the normal annual fluctuations.

For the past thirteen years Mr. F. S. Russell[2] has been studying the young fish taken in the plankton at Plymouth and has made regular collections by standard methods in the neighbourhood of the Eddystone. His observations thus give a picture of what is happening on the offshore grounds in this area. He finds that from 1931 onwards there has been an alarming decrease in the abundance of larval fish. At first this decrease occurred in the comparatively small number of summer spawning fish; but it has now extended to the spring spawning fish also (see Table II, p. 91). If we compare the average numbers for the four-year period 1934–37, with those for the same period ten years ago, 1924–27, we find that the larvae of summer spawning fish have now been reduced to little more than one-fifth of their former abundance, while the numbers of the young of spring spawning fish have dropped to one-third. It is particularly to be noted that all species of fish are similarly affected, and bearing in mind the evidence I have already mentioned on good and bad survival years, this fact alone is sufficient to show that the decrease is not due to a chance coincidence in annual fluctuations.

This change which has come about in recent years is not shown only in larval fish; it is unfortunately apparent also in the Plymouth herring fishery, which has declined to such an extent that it is now virtually non-

[1] 'Comparative Studies of the Fluctuations in the Stocks of Fish in the seas of North and West Europe.' *Conseil Internat. pour l'Explor. de la Mer. Rapp. et Proc.-Verb. des Réunions*, CI, part 3, 1936.

[2] F. S. Russell, 'The Seasonal Abundance of the Pelagic Young of Teleostean Fishes in the Plymouth Area,' Parts I–V, *Journ. Marine Biol. Assoc.* XVI, p. 707; XX, p. 147; XX, p. 595; XXI, p. 679; XXII, p. 493 (1930, '35, '36, '37, '38).

existent. Mr. Ford, who has made a close study of this fishery since 1924, has kindly supplied me with the figures shown in Table I. This Table gives the returns of the fleet of steam drifters from Lowestoft which annually visit Plymouth in the winter, together with Mr. Ford's observations on the composition of the catch.

As with herring fisheries elsewhere, it will be seen that the catch, which is best indicated by the average weight per steamer landing, has shown marked fluctuations—the seasons 1924–25, 1927–28, and 1929–30 were much above the average. These, however, are normal annual fluctuations and they are due, as Mr. Ford has shown,[3] to the great abundance of five-year old fish : there were specially successful spawning seasons in 1920, 1923 and 1925.

TABLE I.—*The Plymouth Herring Fishery*, 1924/5—1937/8

Season (Dec.–Jan.)	Weight landed cwt.	Number of steamers	Average weight per landing cwt.	Percentage composition of catch by age[4]	
				6 years and under	Over 6 years
1924–5	83,600	86	40	91	9
1925–6	82,800	153	23	82	18
1926–7	45,900	129	17	66	34
1927–8	82,800	77	46	83	17
1928–9	42,200	81	27·5	81	19
1929–30	34,300	54	39	71	29
1930–1	44,100	75	33	72	28
1931–2	21,000	52	18	52	48
1932–3	47,800	85	34	35	65
1933–4	29,780	85	30	35	65
1934–5	46,600	91	28	25	75
1935–6	33,800	105	16	<20	>80
1936–7	1,700	56	6	<20	>80
1937–8	$28\frac{1}{2}$	1	9·5		

The significant point in this Table is, however, the marked change in the composition of the catch which began in 1931–32—that is to say in the winter of the year in which the summer spawning fish larvae showed their first signs of decline. Prior to 1931–32 the younger herring, not more than six years old, always formed at least two-thirds of the catch. In that season the younger fish were only 52 per cent. of the total and from then on there has been a rapid deterioration, until to-day there are less than 20 per cent. of the younger and more than 80 per cent. of the older.

[3] E. Ford, 'An Account of the Herring Investigations conducted at Plymouth during the years from 1924 to 1933,' *Journ. Marine Biol. Assoc.*, XIX, p. 373 (1933).

[4] Data obtained by E. Ford.

This change in the constitution of the herring shoals was not immediately reflected in the size of the catches, which for some years were maintained at a good level by the considerable stocks of older fish. But as these passed out they were not replaced by any adequate numbers of the younger year classes and in recent years the fishery has been profoundly affected. Formerly the number of Lowestoft drifters which visited Plymouth for the herring season rarely fell below 75 and was sometimes well over 100; during the past season only one came. And in similar fashion the weight of fish landed has fallen from a figure which sometimes reached 80,000 cwt. to one of under 30 cwt.

It is interesting and perhaps significant to note that as Mr. G. P. Farran has shown [5] the stock of herring on the north coast of Donegal has shown a pronounced decline in recent years. The decline began in 1930, some eighteen months before the change in the constitution of the Plymouth shoals was first seen, and the industry based on this fishery has suffered greatly. Conditions at Plymouth and on the Donegal coast are not identical, for the successful spawning seasons in the latter area were 1920, 1924 and 1925, whereas at Plymouth they were in 1920, 1923 and 1925. The annual fluctuations have thus not operated in exactly the same way. Mr. Farran tells me, however, that the shortage of herring in recent years has been accompanied, just as at Plymouth, by a great reduction in the numbers of the earlier year classes, and it is thus possible that the same long period fluctuation is affecting both areas.

Since 1931, when the depression in the Plymouth area began, there has been a marked change in the amount of phosphate in the offshore waters. Records made by Dr. W. R. G. Atkins and Dr. L. H. N. Cooper show that the phosphate is at its maximum in the winter, in December and January, and since the phytoplankton crop is limited by the amount of phosphate in the water, the winter records give a good indication of the quantity of food which will be available for fish larvae. The records show a heavy decrease in phosphate beginning in 1931, and, as seen in Table II, there is an evident relation between the amount of phosphate and the abundance six months later of the larvae of summer spawning fish. If the average phosphate values for the two four-year periods 1924–27 and 1934–37 are compared we find that the decrease has been about 35 per cent. The fact that the larvae of summer spawning fish were the first to feel the adverse conditions, and that those of the spring spawning fish were not seriously affected until 1935, can in theory at least be explained in terms of nutrient salts; a reduced crop of phytoplankton will mean a smaller supply of zooplankton, and this will mostly be consumed by the spring larvae, leaving little or none for those that come later in the year.

The herring on which the Plymouth fishery depends are mature fish running up Channel to their breeding places on the Cornwall and Devon coasts. On this migration they are not feeding and, presumably, they are unaffected by plankton conditions. It is possible that the disastrous change which has occurred is due to a long series of unproductive spawning seasons caused by the abnormal conditions of the Channel water and lack

[5] G. P. Farran, 'The Herring Fisheries off the north coast of Donegal,' *Journn. Dept. Agriculture for Ireland,* XXXIV, no. 2 (1937).

of food for the larvae ; if that is so the herring has failed in exactly the same way as the other fish whose larvae Mr. Russell has studied. There are, however, reasons for believing that this may not be the correct explanation, for the herring spawn in winter and thus differ strikingly from the majority of fish we have been considering. They are evidently able to find sufficient food at a time when the plankton is at a minimum and they are not dependent on the rich zooplankton which follows the spring outburst of phytoplankton. It is perhaps more probable that the earlier year classes of herring have responded to the abnormal conditions in the Channel by forsaking their usual line of migration, and that they now go to other spawning grounds.

TABLE II.

Year	Phosphate in preceding winter [6] % deviation from mean	Young Fish (less Clupeoids) [7]		*Sagitta*		
		Summer Spawners	Spring Spawners	Total no.[8] ÷ 1000	*S. elegans* %	*S. setosa* %
1922	> + 16					
1923	> + 27					
1924	+ 27	696	2,133			
1925	+ 9	140	1,510			
1926	+ 36	909	2,051			
1927	− 2	170	1,014			
1928	+ 23	..	..			
1929	+ 23	321	502			
1930	..	403	1,114	91·5	94·1	5·9
1931	− 7	230	1,395	117·3	16·7	83·3
1932	− 16	197	1,359	118·3	6·2	93·8
1933	− 5	117	1,220	117·4	4·7	95·3
1934	− 14	79	1,065	94·5	3·5	96·5
1935	− 25	37	393	48·2	3·6	96·4
1936	− 16	115	372	24·0	39·7	60·3
1937	− 14	174	382	26·1	3·8	96·2
1938	− 16					

Renewal of the phosphate in the Channel appears to be largely dependent on an inflow of mixed Atlantic water, which is rich in phosphate because it contains water that has upwelled at the edge of the continental

[6] For further particulars see L. H. N. Cooper : 'Phosphate in the English Channel, 1933–38, with a comparison with earlier years,' *Journ. Marine Biol. Assoc.*, XXIII, 1938 (in press).

[7] The numbers of young fish caught in half-hour oblique hauls of the standard 2-metre net, expressed as the sums of the monthly average catches. Hauls were made weekly, so far as possible, the number varying from 42 to 52 per annum. [Data by F. S. Russell, published in part in *Conseil Internat. Mer, Rapp. et Proc.-Verb. des Réunions*, C, part 3, p. 9 (1936)].

[8] The total number in the standard hauls referred to above. Data by F. S. Russell.

shelf; and from the evidence I have laid before you it seems probable that the normal water movements off the mouth of the Channel have undergone marked alteration in recent years. Direct proof of this is lacking, for we have no observations in the waters to the west of the Channel, but evidence of it is afforded by the very interesting discovery which Mr. Russell has made that certain planktonic species may be used as indicators of water-masses.[9] A relation of this kind has been found in a number of plankton species, but it is here only necessary to refer to those belonging to the genus *Sagitta*, and these owing to their abundance are the most useful.

Of the species of *Sagitta*, *S. serratodentata* is typical of the open Atlantic, *S. elegans* of the mixed Atlantic water and *S. setosa* of the Channel water. The first of these is only to be found on rare occasions off Plymouth when the inflow of Atlantic water is exceptional.

The importance of the species of *Sagitta* as indicators of water movement was first recognised by Prof. Meek, but Mr. Russell's data from Plymouth only began in 1930, and the records are therefore not as complete as could be desired. It is, however, known that for some years prior to this date the offshore plankton in the neighbourhood of Plymouth was of the kind characteristic of the mixed Atlantic water: it was a very rich plankton with such forms as *Meganyctiphanes* and *Aglantha*. It was this type of plankton which was found in 1930, and in the regular series of tow-net hauls made in that year Mr. Russell found that there was 94 % of *S. elegans* and only 6 % of *S. setosa*. In the following year, when the deficiency of phosphate and of summer spawning fish larvae first became manifest, there was, as will be seen from Table II, a conspicuous change in the *Sagitta* population: of *S. elegans* there was only 17 % while there was 83 % of *S. setosa*. Since then *S. setosa* has always greatly preponderated in the catches, with a percentage of 93 or over, with the single exception of 1936, when there was 60 % of *S. setosa* and 40 % of *S. elegans*. There is no doubt there was a small incursion of mixed Atlantic water in the Channel in this year, but it was apparently insufficient to alter the trend of events.

Attention may be drawn to the high sensitivity of this new method of distinguishing water-masses. Once the distinctions between the species of *Sagitta* have been mastered it is an easy method to handle, and it will no doubt be widely employed in the future.

We thus have evidence from four separate sources of the changed conditions which have prevailed in the Channel since 1930–31. These sources are (i) the winter phosphate maximum; (ii) the numbers of fish larvae; (iii) the constitution of the spawning herring shoals; and (iv) the predominance of one or other species of *Sagitta*.

The picture, to my mind at least, is convincing: one gains the impression that if only we had fuller knowledge corroborative data from many biological sources would be forthcoming.

The view that the large alteration which has occurred is linked with

[9] F. S. Russell, 'On the Value of certain Plankton Animals as Indicators of Water Movements in the English Channel and North Sea,' *Journ. Marine Biol. Assoc.*, XX, p. 309 (1935); 'Observations on the Distribution of Plankton Animal Indicators . . . in the Mouth of the English Channel, July, 1935, *ibid.*, XX, p. 507 (1936).

hydrographical changes is corroborated from farther afield. Since 1926 continuous records of the currents in the Straits of Dover have been made from the Varne Lightship with the Carruthers drift indicator. Water can enter the North Sea both from the English Channel and round the north of Scotland and Dr. Carruthers infers that these water-masses are opposed to one another and act in a sort of ' buffer relationship.' At the Varne Lightship the relative strengths of these two forces are indicated by a change in the direction of the current. Dr. Carruthers [10] has calculated the direction of the residual current for each year since 1926 and the figures which he has given show that from 1931 onwards this residual current has swung towards the north and has considerably less of the easterly component which it possessed in the earlier years when high winter values for phosphate were observed at Plymouth.

A point worthy of consideration is whether a similar series of adverse years has occurred in the past, but on this unfortunately we have no reliable data. The statistics of the herring industry are almost the only source open to us, for we have no regular observations on fish larvae prior to 1924, and it was not until five years later that the importance of *Sagitta* was recognised. But before the War, the herring industry was conducted on different lines, from sailing vessels, and we have figures only for the aggregate catch from which it is not possible to draw any conclusions.

In 1915 and 1916 Mr. D. J. Matthews first began the determination of phosphate in Channel water. His results, though not obtained by the methods now in use, have a high degree of accuracy, and they suggest that in those years there was a deficiency of phosphate comparable with that in recent times. Unfortunately the Plymouth herring fishery was greatly reduced during the period of the war and we have no reliable statistics for comparison.

We may suppose that this long-period fluctuation at the mouth of the Channel will end in due course, but we have no means of knowing when this will happen. When the change comes it will be heralded, we believe, by the return of *Sagitta elegans* in large numbers, and by a marked increase in the winter phosphate maximum. The fisherman will presumably not find any immediate improvement in the bottom fish. As yet he has perhaps scarcely realised the full extent of the depression which started some years ago, and when there is a return to better conditions he must wait until the increased numbers of larvae grow to fish of marketable size. It is possible, however, that bottom-living fish have been migrating into the area and that he may thus in some measure escape the worst effects of the depression. If the younger herring have forsaken their spawning grounds and gone elsewhere, we may hope that they will at once return in force when conditions improve, and that the Plymouth fishery will rapidly be re-established. If, however, they have throughout held to their former migration routes, and the present dearth is due to lack of suitable conditions for the larvae, they are in the same position as the bottom fish and a number of years must elapse before the fishery can be resumed.

[10] J. N. Carruthers, ' The Flow of Water through the Straits of Dover," Part II. *Min. Agric. Fisheries, Fishery Invest.*, ser. ii, XIV, pp. 15, 64, Table VI (1935).

You will, I think, have noticed that in this outline of recent events I have made no reference to other hydrographical data, such as salinity and temperature, and I must needs do so now lest you suspect me of suppressing evidence which is not in accord with the story I have told you. For the plain fact is that the observations we have of salinity and temperature do not fit into the picture.

For many years past Dr. H. W. Harvey has followed the temperature and salinity changes at the western end of the Channel,[11] and during the period since 1924 he has found that the most conspicuous movements were large incursions of low salinity water in May 1928 and in March and April 1936, while in 1932, 1933 and 1934 (especially in 1933) patches of water with unusually high salinity moved eastwards up the Channel. So far as can be seen these movements show no correspondence with the marked biological changes which have occurred : it is in the phosphate data only that a correlation can be found.

In the year 1921 there was an exceptional influx of Atlantic water, which filled the Channel[12] and flooded into the North Sea. Salinity and temperature were much above normal and numbers of unusual planktonic organisms of Atlantic origin were found in the North Sea.[13] Recent experience at Plymouth might lead one to think that such an influx as this would bring benefit to the herring fisheries, but actually it was just the reverse, for at Plymouth and in the North Sea, at Lowestoft, Yarmouth, Grimsby and North Shields, the herring fishery was much below normal.[14]

In recent years also a number of unusual planktonic forms have entered the Channel, brought apparently by incursions of low salinity water flowing round Ushant ; but these movements have had no effect on the depleted phosphate supply.

It thus appears that incursions of Atlantic water into the Channel may bring advantage to the biology of the area or may be detrimental, that no obvious connection between the biological data and temperature and salinity is noticeable, and that so far as we can at present see the only correlation that can be established is with phosphate. The explanation lies, I believe, in our very considerable ignorance of the constitution and origin of the water-masses which from time to time enter the Channel.

There is evidently more than one way in which an influx of Atlantic water may be advantageous. It may, in the first place, bring water with a high content of phosphate and other nutrient salts which will subse-

[11] H. W. Harvey, ' Hydrography of the Mouth of the English Channel, 1925–28 and 1929–32,' *Journn. Marine Biol. Assoc.*, XVI, p. 791 (1930) ; XIX, p. 737 (1934).

[12] J. R. Lumby, ' The Salinity and Temperature of the Southern North Sea and English Channel during the period 1920–21,' *Publications de Circonstance*, no. 80 (1923).

[13] A. C. Hardy, ' Notes on the Atlantic Plankton taken off the east coast of England in 1921 and 1922,' *ibid.*, no. 78 (1923).

[14] J. R. Lumby, ' Salinity and Water Movements in the English Channel during 1920–23,' *Min. Agric. Fisheries, Fish. Invest.*, ser. 2, VII, no. 7, p. 18, fig. ix (1925). H. W. Harvey, ' Hydrography of the English Channel,' *Conseil Internat. Rapp. et Proc.-Verb. des Réunions*, XXXVII, *Rapp. Atlantique*, 1924, pp. 82–84 (1925).

quently yield an abundant plankton. Or, secondly, though deficient in phosphate, it may bring in large quantities of phytoplankton or zooplankton, the product of a former richness in phosphate. This plankton will afford an immediate food-supply for larval fish and other animals, and when it dies down the phosphate will be regenerated and will serve for further plankton production in the future.

It is thus what we may call the biological condition of the water that is of importance, and this no doubt is to some extent determined by the season of the year. At times, in summer, the surface water may be largely devoid of both plankton and phosphate and an influx of such water, even though its high salinity may indicate an oceanic origin, will bring no improvement to biological conditions and may indeed be harmful. In winter, when the thermocline has broken down and surface phosphate has been renewed by convection and by stormy weather, an influx may prove of advantage. But it is perhaps more probable that upwelled water, rich in the nutrient salts which are always to be found in the lower layers of the ocean, is the potent source of surface enrichment, and of the conditions in which such upwelling occurs we are very largely ignorant. We lack the necessary data and can merely speculate on what may be happening from analogy with what is known in other areas.

Some twenty-five or thirty years ago Mr. D. J. Matthews [15] published a series of papers on the physical conditions in the English Channel and adjacent waters, and to this day his work remains one of our principal and most valuable sources of information. He showed that to the south of Ireland there is an extensive cyclonic or counter-clockwise circulation, which may at times reach as far south as $48\frac{1}{2}°$ N., and nearly a quarter of a century ago he suggested that this circulation might prove of considerable biological importance. ' If the strength of the cyclonic system varies from year to year, so will the character of the water at any place within its influence, such as the areas of the drift-net fishing off the mouth of the English Channel and off the south coasts of Ireland.' There can scarcely be a doubt that the vagaries of this circulation have a profound effect on conditions in the Channel. If we possessed, as unfortunately we do not, a continuous series of observations on the oceanographic conditions to the south of Ireland and on the edge of the continental slope, the variations in this cyclonic system could be traced, and even though it might then appear that the ultimate causes of the present depression are linked with changes in the Atlantic circulation, and thus still out of reach, the information which would be gained would undoubtedly throw new light on the problem.

I have dwelt at some length on these events in the Plymouth area because they afford a good example of a long-period fluctuation and illustrate the way in which observations drawn from widely different lines of inquiry are linked together. From other sources also there is

[15] D. J. Matthews, ' Report on the Physical Conditions in the English Channel and adjacent Waters in 1903 : in 1904 and 1905 : in 1906,' *Internat. Invest. Mar. Biol. Assoc.*, Rep. I, 1905 ; Rep. II, part ii, 1909 ; Rep. III, 1911. ' The Surface Waters of the North Atlantic Ocean, South of 60° N. Lat., Sept. 1904 to Dec. 1905,' *ibid.*, Rep. II, part i, 1907. ' The Salinity and Temperature of the Irish Channel and the waters South of Ireland,' *Fisheries, Ireland, Sci. Invest.*, 1913, iv, 1914.

good evidence of long-period fluctuations in fisheries, and though the hydrographical changes to which they may ultimately be traced are not, as it appears, the same as in the Channel, they show that major alterations extending over a long term of years are by no means unusual.

In 1925 the Norwegians discovered great numbers of cod on the banks surrounding Bear Island, and ever since that year, except in 1929 when ice interfered with the operations, the fishery has been maintained, many trawlers visiting the banks annually to take toll of their wealth. Iverson,[16] from whose paper my information on this fishery is derived, states that there was a former occasion when cod were plentiful in this area. That was from 1873 to 1882. Between 1883 and the time when the present fishery began the grounds were examined on a number of occasions, but very few cod were found and the results were unprofitable. It was so in 1924, the year which preceded the present period of abundance.

Another instance is afforded by the cod fishery in West Greenland. At certain times large concentrations of cod appear on this coast and spread as far north as Disko Bay, affording a profitable fishery; but after a term of years their numbers suddenly decline and a protracted period of scarcity follows. In 1917 cod were found in West Greenland in great abundance and the fishery on this coast has been maintained up to the present day. Prior to that, as Jensen and Hansen show in their interesting historical account,[17] the grounds were tested on a number of occasions without finding stocks of cod in marketable quantity; but early records indicate that there were at least two periods, in 1820 and in 1845–49, when cod were present in great numbers. In recent years it has been found that some of the cod spawn in Greenland waters, while others migrate for this purpose to Iceland. Marking experiments show that there is an interchange of cod across the Denmark Strait, and there is reason to believe that most of the fish found on the Greenland coast during periods of abundance have come from Iceland, either as fry carried in the west-going current or by migration of mature fish.[18]

To these two instances of large-scale changes in the fish population in northern waters many others could be added and all are apparently due to the same cause—to the fact that in recent years the entire area from Greenland to Bear Island has become appreciably warmer. Berg [19] has collected much information on the effects of this rise in temperature, Saemundsson [20] has given an interesting account of the alterations which have occurred in the fauna of Iceland, while Stephen [21] has shown that marked changes have also taken place in the British marine fauna. Berg, quoting from Schischow, gives figures of the very

[16] T. Iverson, *Rep. Norwegian Fishery and Marine Invest.*, IV, no. 8 (1934).

[17] S. Jensen and P. M. Hansen, 'Investigations on the Greenland Cod,' *Conseil Internat. Rapp. et Proc.-Verb. des Réunions*, LXXII, pp. 1–41 (1931).

[18] E. S. Russell, 'Fish Migrations,' *Biol. Reviews*, XII, pp. 324–5 (1937).

[19] L. S. Berg, 'Rezente Klimaschwankungen und ihr Einfluss auf die geographische Verbreitung der Seefische,' *Zoogeographica*, III, Heft 1, pp. 1–15 (1935).

[20] B. Saemundsson, 'Probable influence of change of temperature on the marine fauna of Iceland,' *Conseil Internat. Rapp. et Proc.-Verb. des Réunions*; LXXXVI, no. 1, pp. 1–6 (1934).

[21] A. C. Stephen, 'Temperature and the incidence of certain species in Western European Waters,' *Journ. Animal Ecology*, VII, p. 125 (1938).

remarkable increase in the herring taken on the Murman coast since 1931. The quantities taken in that year and in 1932 and 1933 were respectively 23 times, 29 times and 68 times as great as the largest catch in the ten preceding years.

It is clear that an increased sea-temperature, probably of the order of 1·0 to 2·0° C., has allowed various species of fish to extend beyond the normal limits of their distribution, with the result that it has been possible to establish productive fisheries in areas which formerly would not have yielded an adequate return. It is evident, I believe, that at some future date conditions will revert to normal and that a time will come when these lucrative fisheries will cease to exist.

In the present state of our knowledge we can do little more than guess at the reasons for the increased temperature in these areas; but the only source from which warm water can come is the Atlantic drift, and it therefore appears that in recent years this drift must either have increased in volume, or, if the volume remains constant, in the temperature of the water it carries.

As you will have seen, I have in this address tried to draw a distinction, which I believe to be a real one, between two kinds of fluctuations, both of which have a pronounced effect on the marine fauna. Normal annual fluctuations are a constant feature. They form the basis of fishery prediction and our information, such as it is, is that their incidence is restricted: a fishery for a certain species in a particular place will be affected, while other species in the same place, or the same species in another place will be unaffected. And it is to be assumed that the causes of such annual fluctuations, though of these we know but little, are also restricted both in space and in time.

In contrast are what I have called long-period fluctuations, which extend over a term of years and involve much larger areas. Such fluctuations as these are due to a widespread change in one or more of the hydrographic factors in the environment, and large numbers of species, if not all, are affected simultaneously or within a short period. Long-period fluctuations may mask the effects of the annual fluctuations and at times they will render fishery prediction unreliable.

In the illustrations I have given you I have spoken chiefly of fish, because it is of fish that we have best knowledge; but it will I think be evident that invertebrates are influenced in the same way and I believe it may truly be said that all marine animals show great variations in abundance. You will also not fail to note that though these fluctuations are of the greatest economic importance they are equally of very high scientific interest.

The evidence I have given you indicates that long-period fluctuations may be brought about in entirely different ways. In the Channel, as it appears, the change can be traced to a deficiency in phosphate, while in more northerly areas it is due to an increase in sea-temperature. But, though there is this wide difference, the two sets of circumstances have this in common, that they originate in the open Atlantic, at the edge of the continental slope or farther to the west. It is here, in oceanic waters, that the causes of these large alterations in European fisheries must be sought.

Vladivostok, though it is ice-bound each winter, lies in the same latitude as Marseilles. This is only one of many facts which impresses us with the climatic advantages that we derive from the warm water of the Atlantic drift, and it might be thought that an investigation of the causes which underlie this phenomenon would long since have been undertaken by those who reap such great benefit. Yet, to the present day, these problems remain unsolved and, as Dr. Iselin has recently shown,[22] three mutually conflicting theories are extant regarding the circulation of water in the North Atlantic.

Fortunately there are signs that a period will be set to our ignorance. On the American side of the Atlantic the Woods Hole Oceanographic Institution is making a study of the Gulf Stream and of the effect of wind velocity and direction on the strength of a current. There is to be British co-operation in this programme, based on the Bermuda Biological Station. The Royal Society is administering a Government grant which has been given for the purpose, and additional staff for the Bermuda station and a small research ship have been provided. Data recently obtained by the Woods Hole Institution show that the transport of water in the Gulf Stream has varied by as much as 20 per cent. in fourteen months, and it may well be that this figure is below the normal range of variation. When the observations over the five-year period which is contemplated have been carried out we may hope to know far more than we do at present of the Gulf Stream and its effects on circulation in the North Atlantic.

During the present year a German research ship is making a prolonged investigation of the hydrography of the North Atlantic, and only two months ago research ships from Denmark, Norway and Scotland were co-operating with her in studying extensive areas from the Azores to Iceland.

From such combined attack we shall learn much and there is every reason to believe that the main features of the circulation in the North Atlantic will shortly be understood. But though we may look for results of the highest importance from these investigations it is evident that they will not solve the biological problems with which we are faced; for the work in the eastern Atlantic is an isolated set of observations, most valuable as a contribution to our knowledge of the general conditions, but affording little help in solving the problem of long-period faunistic fluctuations of which I have spoken. It is the deviations from the normal which are of paramount importance to the biologist, and it is only by repeated observations made over a series of years that they can be detected.

To make such observations at sufficiently close intervals of time and space over the whole of the north-east Atlantic is clearly not within the bounds of present possibility; but when we have gained an adequate knowledge of the normal system of circulation it is to be expected that certain critical positions or regions will be discovered, and that regular data from those places will give information from which the variation in the whole system can be deduced. Even such a programme as this is far beyond the resources we now possess; but I believe that the need for

[22] C. O'D. Iselin, 'Problems in the Oceanography of the North Atlantic,' *Nature*, vol. 141, p. 772 (1938).

systematic oceanographic work in the eastern Atlantic will be more and more acutely felt as time goes on, and I feel convinced that it is the only way in which we can ever reach an understanding of the reasons for the large fluctuations in our fisheries.

There is much work to be done nearer at hand in improving and co-ordinating the collection and publication of data from our own coastal waters—a matter to which the International Council for the Exploration of the Sea is now giving careful attention. It appears, however, that the research ships employed by the maritime countries of Europe are for the most part fully occupied with their own domestic fishery problems and can only occasionally find opportunity for oceanographic survey. Thus, unfortunately, it is not a question of devising a programme which will give the regular data that are needed, but of attempting to obtain the necessary information with resources which will almost inevitably prove to be inadequate. Yet, with the knowledge we now possess and the new methods which have been evolved, it is certain that very valuable results could be achieved by a comprehensive study of the fluctuations in the hydrography and plankton, and the work that is now beginning in the western Atlantic will lose much of its value if we are unable to obtain comparable data in our own waters.

Before concluding this address I feel I should call attention to the urgent need throughout a very large part of the British Empire for greater activity in the scientific administration of the fisheries, for to me at least it is apparent that the lessons which long years of experience have taught us in this country are not generally understood elsewhere.

The plain fact is that in the Empire as a whole we are deplorably deficient in fisheries administration. To this broad statement there are of course some exceptions. By reason of its situation in Europe the Irish Free State is obviously one of them, and it has taken its full share in the progress that has been made during the present century. Another exception is Canada, where a vigorous fisheries service, with a competent scientific staff, has been at work for many years. Newfoundland, a country whose fisheries are of predominant importance, not long since suffered a shattering blow in the loss of the whole of its laboratory buildings by fire, but it will recover from this disaster and we may hope that the work which had such a brilliantly successful beginning will shortly be resumed. Australia has now made a fresh start after the tragic loss of the *Endeavour* and has at last taken the wise step of founding a Commonwealth fishery department. These are the high lights, and there are one or two colonies, such as the Straits Settlements and Ceylon, which give relief to what is otherwise a very sombre picture. In South Africa with its astonishingly rich fishing grounds and vast length of coast-line the fishery staff is utterly inadequate, and in India, where fisheries research has immense possibilities, there is apparently little hope that proper action will ever be taken. In India fisheries are what is known as a transferred subject : that is to say they have been handed over by the central Government to the provincial administrations. The result is that some provinces may have a scientific staff of one, others have none at all, while Madras, which is much the most enterprising and publishes a Fisheries Bulletin, has three. In such conditions fishery work on any adequate scale is clearly out of the question and it is not possible even to begin the acquisition of the fundamental knowledge that is essential to future progress. Japanese trawlers, taking advantage of the complete lack of development of the Indian off-shore fisheries, are now visiting the Bay of Bengal in increasing numbers, and there is perhaps a possibility that their activities will cause the Government of India to realise how backward they are in fishery administration. It is evident that little or nothing can be expected from one or two men working in isolation and that only an all-India service, with the esprit de corps that such a service would have, can be sufficient for India's growing needs.

It has taken more than a quarter of a century of intensive co-operative effort by most of the leading European nations to build up the information that we now possess of the fisheries round our coasts, and though with existing knowledge and the better methods that have been devised it might be possible to reach the same stage in a shorter time, the accumulation of the necessary facts must inevitably be a slow process. Administrators are still prone to expect a rapid solution to any question which they submit to scientific inquiry; but in almost every problem which touches marine biology it is essential to possess a background of fundamental knowledge which can only be acquired by long years of patient study. If there is one lesson to be learnt from the history of fisheries research—one that cannot be too heavily stressed—it is that the opportunity of dealing effectively with a fishery problem will generally be lost unless this basic knowledge has been obtained in advance and is ready for application.

Even in our home waters, which have been examined so long and so closely, our information is not within sight of being complete: in almost every branch of fisheries work there are new fields to be explored, new methods to be tried, and many large gaps in the knowledge we possess. But it may at least be said that we have made a beginning, that we are aware of the deficiencies and are trying with the facilities we possess to make improvements.

In many other parts of the world, however, not even a beginning has yet been made; ignorance is profound and there is no background of knowledge which can be utilised. It is no great exaggeration to say that in Africa and throughout almost the whole of the vast stretch of the Indo-Pacific region there is scarcely a fish whose life history is fully known and whose various stages from egg to adult can be recognised. Of such matters as age, rate of growth, spawning periods, food and migrations we are equally ignorant, nothing is known of the incidence of fluctuations and nothing of the seasonal or other changes in the environment. It is surely time that the importance of such knowledge was recognised and that early steps were taken to lay the foundations of fishery science throughout the Empire.

When speaking of long-range fluctuations I expressed the view that the facilities we at present possess in Europe are insufficient to give us all the data we need: regular observations over a much extended area are required if we are to reap the full advantages of the knowledge we have gained. In the present state of international politics we can expect little; but when, in God's good time, the nations begin to turn their armaments to better uses, and the mass production of ploughshares begins, let us hope it will not be forgotten that there is also a harvest of the sea.

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SECTION E.—GEOGRAPHY.

CORRELATIONS AND CULTURE

A STUDY IN TECHNIQUE

ADDRESS BY

PROF. GRIFFITH TAYLOR,

PRESIDENT OF THE SECTION.

CONTENTS

A. GEOGRAPHY AND THE SOCIAL SCIENCES.

I MUCH appreciate the honour of addressing the Geographical Section in my old Alma Mater, especially in this fine monument to the importance of Geography directed by my former sledge-mate, Prof. Debenham. Many presidential addresses have been devoted to a survey of the progress made in one or another branch of our very varied discipline during the *past* twenty years. This is a safe and sane programme—but is not, I think, so likely to stimulate research as the unsafe and, as some would say, insane attempt to forecast somewhat of the advancement of Science in our special field during the *next* twenty years. There seems indeed something incongruous in a scientist from the Antipodes trying to say something new on the subject of culture amid these colleges renowned for their study of arts and letters. How can the experience of a geographer, based on the study of contours, isobars, isotherms and all the other isopleths which adorn modern maps, help us to obtain a more valid interpretation of that elusive concept which we call culture? In my address I propose first of all to consider the field of cultural geography; then to discuss a technique which I have found invaluable in research in that subject; and finally to suggest that modern education would do well to reduce greatly the study of certain fields of culture which were too strongly emphasised even in the Middle Ages, but which still occupy our young students to the exclusion of other far more important aspects of culture.

Our field, fellow geographers, can, I believe, be made the most interesting in the realm of a general education. Partly because it deals with the vital facts of our environment; partly because it is so comprehensive; and partly because it is so objective. In these days of queer ideologies and freedom from canons, it should be all the more valuable that we, in *our* discipline, can chart our data and so make clear our problems, and in a sense prove our conclusions. At Chicago, one of the three leading universities in U.S.A., the whole of the various disciplines were grouped into the four divisions of Physical Sciences, Biological Sciences, Social Sciences, and Humanities. But there were a few *liaison* subjects which were too widespread in their interests to fit into any rigid division. Geography was one such subject, so that our large staff (and we had four full professors) was given a place on the Boards of both the Physical and Social Sciences. It is this feature of Geography which helps to give it a special place in a general education.

If we look back at the relation of education to these four divisions of knowledge, we see a most interesting evolution. First of all, in the fourteenth century, the protagonists of the new Humanism waged a bitter fight against the Church and the Schoolmen. In the end the modernistic views of the humanists won, and we call this epoch the Renaissance. Next around 1600, the physical sciences were damned by the leaders of reaction, only to emerge triumphant in their turn. Some eighty years ago the biological sciences, in the persons of Darwin and Huxley, advanced truths which were anathema to the orthodox. Few educated folk attempt to oppose these truths now. But to-day the social sciences are challenged by the forces of reaction. I will only instance the perverted use of anthropology and sociology to advance the views of some of the totalitarian nations. We geographers can do yeomen service, as I see it, to clarify some of these issues if we teach tolerantly and scientifically what is becoming known as Cultural Geography.

I could talk for half an hour on the question of the field of geography and yet not make my meaning so intelligible as the impression you will gain from the study of Fig. 1 for a few minutes. The diagram suggests that the field of geography (the large circle) contains eight subdivisions which in turn are linked with eight major disciplines (Griffith Taylor 1937). Thus geography links the four 'environmental sciences' of Geology, Physics, Astronomy, Botany, with the four 'human sciences' of History, Anthropology, Sociology and Economics. There are vast uncharted areas on the borders of regional geography—the core of our discipline—which merge into the eight subjects specified. Among professional geographers the great majority will always carry on the vital work in the central fields—but we may always hope for Raleighs, Drakes, Hawkins, and Dampiers, who will explore far afield and extend our realms. They will perchance trespass on other empires; and doubtless some conservative historians and anthropologists will call them buccaneers or pirates. Dropping metaphor, I firmly believe that by applying techniques learnt in the realms of geography, biology and geology—and carried across to anthropology, history and sociology—such pioneers will ultimately earn the respect of the leaders in the ' purer ' social sciences. But I must caution any piratical young geographer who cruises in strange waters that his reward, if any, will probably be a posthumous one.

It seems advisable to consider for a moment definitions of the fields of geography. Like many other geographers, I have put forth my own definition, and it runs somewhat as follows, ' Geography is concerned with description, localisation and explanation of the data which relate man to his material environment.' As I see it, the essential feature is the *localisation* (i.e. charting of the data in question) with a view to explaining their distribution. In a word we should make maps not solely as an

Fig. 1.—The Liaison Character of Geography, using ' Environmental ' Sciences to explain Social Sciences. The map of the continents suggests the ecological character of Geography.

end in themselves, but with a view to *explaining* the phenomena in question (Griffith Taylor 1935). Perhaps, before proceeding farther, some apology is necessary for the introduction of so many diagrams into a presidential address. My excuse is that my subject is Geography—and Geography without maps is, to my mind, as little worth while as *Hamlet* without the Prince.

B. Geography and History.

Let us now consider how the techniques of geography and allied sciences can be usefully employed in helping the social sciences. There are, of course, many ways in which charting data is helpful to the historian or anthropologist, but curiously enough many workers in the sister disciplines are extremely sceptical of the value of such a technique. I cannot do better than quote a paragraph from a recent paper by my good friend Ellsworth Huntington on this very topic (Huntington 1937).

> 'The majority of historians feel that they need a knowledge of geography. Therefore, those among them who belong to what we may call the standard group devote an early chapter to a somewhat elementary but accurate account of the geography of their selected region, and then forget about it. Many historians are conscious that the Alps are really a barrier, and that the climate of Russia as well as of India is different from that of Belgium. Nevertheless, taking their work as a whole, an astonishing number of historians seem to regard a court intrigue as more important than the influence of climate, relief, occupations, and so forth, upon national character, or upon specific historical situations. This is not the fault of the historians. The fault lies simply in the fact that both history and geography are still in a very crude state of development.'
>
> 'The route to a higher development has been explored a little by the economic historians. According to their view, man's need of food, clothing, shelter, and the other good things of life, has been the keynote of history. Like the standard historians they have done yeoman service, and no word here said should be interpreted as disparagement. Yet many of them seem to have little knowledge of the way in which geographic environment influences not only the available resources, but man's desires, and the degree of energy with which he works to satisfy them. . . . These differences arise in part from the geographical environment as well as from the historical development of a culture. Their effect on economic conditions and historical events is profound.'

I am reminded of a recent congress of historians in which I heard one of the chief speakers hold up to ridicule the idea that certain historical sequences in Scotland could be correlated with the Old Red Sandstone. From the applause, his fellow-historians agreed with him. To the present speaker nothing is more likely than that such a relation existed; and indeed I propose to show one or two examples of the same type.

The first example is taken from the finest collection of *liaison* studies in English with which I am acquainted. Here the various periods of English history are treated as separate stages of growth; in each of which the effect of the environment on man is shown to be as important as it is to-day. I refer to the *Historical Geography of Britain*, edited by H. C. Darby. Here is history of an unusually valuable type; and it is food for thought that the authors are, as far as I know, all geographers. Is it going too far to say that most historians have felt so little need to study physical correlations that such a work could not be presented by them?

No historian would deny the vast importance of the wool trade in the fourteenth century. We owe to Dr. Pelham a map of the Sussex Weald (Fig. 2) which shows clearly how closely this trade depended on a geological condition—the outcrop of the Cretaceous Chalk. I do not assert that this is the most vital feature of the wool trade in this period—but it did determine the site, which no historian can ignore.

Another example from America explains a peculiar and characteristic culture-complex in the State of Kentucky. Everyone has heard of the Blue-Grass Country around Lexington, and its association with horses and racing. It is rather sharply marked off from neighbouring areas, and its site is exactly determined by the geological structure (Fig. 3). The Blue Grass Region is an 'Eroded Dome' much like the Weald. Here the fertile Trenton formation (of upper Ordovician age) is surrounded by rather sterile Carboniferous rocks. A similar eroded dome surrounds

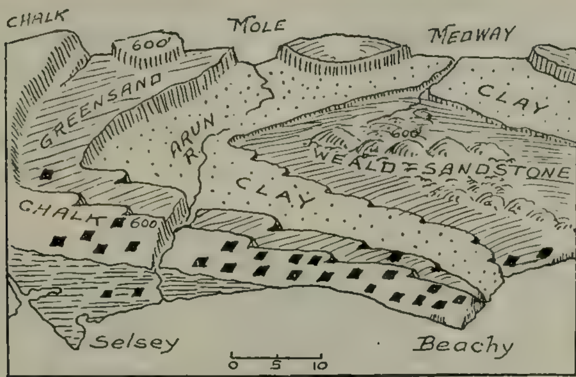


FIG. 2.—The Wool Industry in Sussex about 1350, determined by the Chalk Cuesta of the South Downs (based on R. A. Pelham). The black squares represent from 500 to 1,000 sheep in a parish.

FIG. 3.—Correlations between Geology (Eroded Domes) and Horse-breeding in U.S.A. Figures indicate approximately 'Horses per square mile.'

Nashville to the south. In Fig. 3, I show the close correlation between the Ordovician beds and the density of horses in these regions of Kentucky and Tennessee (Finch and Baker 1917). Such comparisons show how the geographer can help the historian to elucidate culture in almost any district in which he may be interested.

Few students seem to have made use of graphical methods in investigating their historical problems. These are, of course, the chief characteristic of geographical research. Especially is this true in regard to the use of *Isopleths* (lines of equal abundance), which can be applied to cultural facts, almost as readily as to such features as temperature or elevation. In Fig. 4, a number of isopleths illustrating the spread of the Renaissance are charted.

In diagram *I* some of the chief teachers of Renaissance ideas about 1350, such as Petrarch and Boccaccio, are localised. Later writers dealing with the ' life of the times in living languages ' (a phrase which in part describes the Renaissance) were Wyclif, Froissart and Chaucer. Hence toward the end of the fourteenth century we see the new ideas moving north up the ' Way of Light.' In diagram *II* (Fig. 4) I have stressed the *spread of printing* as perhaps the most characteristic feature of the second period of the Renaissance (1450 to 1550). Modern research (by J. H. Hessels and others) seems to refer the invention of movable type to Costar of Haarlem about 1446. It has spread to Mainz and the vicinity by 1460, moving along the ' Rhine-Way,' and reached Rome by 1465 and

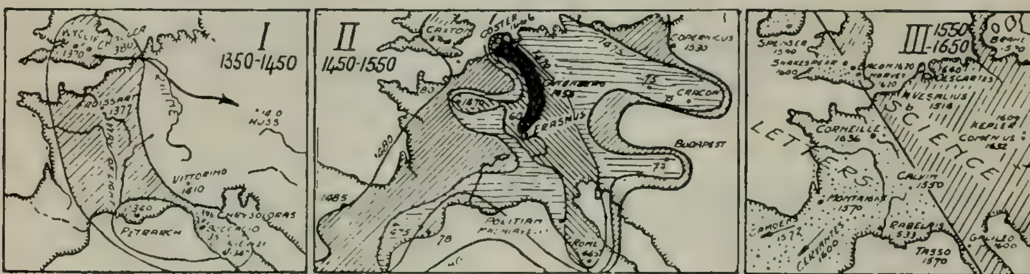


FIG. 4.—The spread of Renaissance ideas in three stages, showing the effect of the ' Way of Light ' and the ' Rhine Way.'

Paris by 1470. We have here an interesting example of a culture spreading along a new route, far removed from the familiar ' Way of Light.' Other isopleths showing the rapid spread of printing throughout western Europe by 1480 are also charted.

In the third diagram of Fig. 4, I have plotted the ' schools ' of the famous teachers in the third period of the Renaissance (1550 to 1650). Here I have not attempted to draw isopleths. But when I labelled each teacher as concerned either with science or letters, it was surprising to find that practically all the former were to be found in the eastern portion of the map, and all the latter in the western part. This is an interesting distribution which is in part no doubt associated with the leading religions of the two areas. The conservative west held by the old Catholic faith for the most part, while the eastern region was that where the reformed religion had the chief control. This distinction in turn is of course bound up with the deep-seated inheritance of Roman culture in France and Italy, which was wanting east of the Rhine. The votaries of mediaeval science were not encouraged by the orthodox Roman Catholic Church, so that naturally they were not numerous in the western part of diagram *III*.

In the social sciences, we are dealing with disciplines of an intermediate character. In much of their content, they are not so susceptible to rigorous proof as are many of the problems in the physical sciences, and in this they resemble the humanities. But like the latter they have the great educational advantage that they deal definitely with *man* rather than with lower forms of life or with physical phenomena. A disadvantage inherent in geography and allied subjects is the immense number of *facts* whose assimilation would seem to be necessary in the study. This is wearisome in a scheme making for an all-round education, and in my opinion *memorising facts* should never be the vital factor in geography.

You may have heard of the despondent negro preacher who complained that his flock was either so ignorant that they believed too much in '*de deuce*,' or so sophisticated that they *doubted* everything. Students of cultural geography should also learn to 'doubt and deduce,' rather than to memorise the innumerable facts often presented without coordination. It is this training in deduction, accompanied by a healthy scepticism of orthodox dogmas until they have been tested and confirmed, which should be our aim.

C 1. Evolution of Life and Culture.

To the geographer interested in culture-spreads, it seems likely that the one outstanding fact has often been neglected by sociologists. It should be clear that as long as man was controlled primarily by the same factors as the higher mammals his evolution is likely to proceed along somewhat similar lines. We shall find in many fields of research that we are dealing with the same phenomena, i.e. with progressive stages of evolution developing in the Old World 'cradle.' This concept can be illustrated in Mammals, Human Race and Human Culture alike.

Matthew has shown that the cradleland and stages of evolution for various related groups of the higher mammals *can be deduced wholly from their distribution* in time and space (Matthew 1915). In Fig. 5 at B, I have summarised his conclusions in a block diagram, which shows that we are dealing with a typical example of what I describe later as the 'Zones and Strata' phenomena. Here is illustrated the problem of the vast biological changes involved in changing something like an antelope into a sheep. Needless to say millions of years have elapsed while this occurred. But the salient control was the marked environmental stimulus *centred in south-central Asia*.

There is no reason to doubt that these special conditions continued to operate in this region from early Tertiary times up to the development of the first stable civilised communities of man—say, around 10,000 B.C. If we grant this postulate, then it would seem obvious that the variations in the *human* species (i.e. racial groups) would almost inevitably arise in the same region of great stimulus. These might be expected to develop in a much shorter period, say of the order of half a million years. The writer has demonstrated this thesis in many books and papers.

Finally, major *culture-changes* are also essentially responses to environment—though far more rapid than biological changes. There is, to the writer, no region more likely than south central Asia where the tremendous development from the nomadic hunter to settled village-dweller was so likely to occur. I pointed out this inherent geographical advantage nearly twenty years ago; and since that time I have watched the students of culture driven from Egypt to Mesopotamia, and finally to some, still indefinite, region to the north in their efforts to find the cradle of civilisation. (I shall return to this aspect of the subject later.)

Thus we arrive at the interesting result that major racial evolution and major cultural evolution occurred in much the same region; in spite of the fact—often pointed out—that there is no inherent connection between a given race and a culture associated with it. The time-factor is very different in the two phenomena. In the field of Race, during the short period of the recent centuries, we have only seen the origin of a few hybrid groups, all unimportant except perhaps for the Mestizos of Latin America. But we have observed new cultures travel all over the world; their speed of expansion increasing with every passing year. Thus tobacco spread far and wide within a century after Raleigh brought it to Europe. Nowadays the son of the head-hunting Papuan delights to drive a motor launch, and the second generation from the cannibal Fijian is filling the medical services in those tropic isles.

C 2. General Discussion of the Zones and Strata Theory.

It was his use (on world maps) of the isopleth method in charting the criteria of race, in conjunction with the findings in W. D. Matthew's memoir 'Climate and Evolution,' which led the writer to publish the 'Zones and Strata Classification of Races' in 1919. The general principles of this concept are illustrated in Fig. 5. Here three parallel cases of

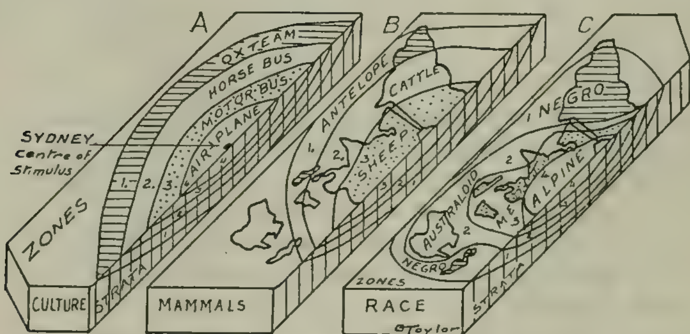


Fig. 5.—Block Diagrams illustrating the 'Zones and Strata' Concept applied to Culture (Evolution of Transport); Mammals (Artio-dactyls); and Major Races. In each case the centre of evolution is in the centre of the zones, and the most primitive types have been thrust to the margins. The strata appear on the vertical edges (at right). All much generalised.

evolution are considered. All anthropologists will agree as to the explanation of the block diagram on the left. Here we see zones of *Methods of Transport* (ox-team, horse-bus, motor-car and aeroplane) arranged round the city of Sydney—the only settlement of note for sixty years in Australia. The 'strata' resulting from this evolution in Sydney and gradual migration to margin, are indicated on the vertical edge of the block diagram. Clearly there is a common cradleland, where commercial activity is greatest in the centre of the zones—and the primitive types now occur precisely where they did *not* originate.

Turning to Fig. 5 B, we find the same process illustrated in the evolution of the Artio-dactyls (or even-toed mammals) based on data given by Matthew (1915). The antelopes are earliest and are displaced farthest from the centre. The sheep are latest and still characterise the common cradleland. The fossil strata are in accord, using the palæontologist's 'Law of Superposition.' No biologist doubts that the zones and strata in the case of these mammals indicate the order of migration and of *evolution* for the Artio-dactyls.

The writer believes that primitive man was differentiated into the five major races long before the later races reached Western Europe. This evolution almost certainly took place in Asia and occurred before the last Ice Age. It certainly far antedated early Neolithic times.[1] Hence early man of such a primitive type can surely be considered as obeying the same laws of migration as the higher mammals. If now the pre-Columbian distribution of the major races (Negro, Mediterranean and Alpine) be plotted in a block diagram (Fig. 5 C), we find a series of *zones and strata* closely resembling the two already charted. It is difficult to escape from the conclusion that the centre of Asia is the common cradleland where evolution progressed most actively in the case of primitive man—just as Matthew has shown it progressed most actively here to produce new types of the earlier mammals. Indeed, we can almost exactly parallel the spread of the rhinoceros from Asia with the spread of the negroes, while the spread of the Pleistocene *Equidae* is the same as that of Alpine man (Matthew, Figs. 20 and 17).

The centre of stimulus in Fig. 5 A was the commercial progress in the city. In the case of the mammals and man it was the stimulating climate of south central Asia. I have shown in a number of books and papers (see bibliography) that this region in the past has been characterised to a marked degree by such climatic features, but lack of space prevents my covering this ground again.

C 3. Corridors into the Continent.

It is of considerable interest to use our knowledge of the relative accessibility of the other continents from Central Asia, and to see how the consequent migrations agree with the 'Zones and Strata' hypothesis.

Most anthropologists accept Asia as the cradleland of the later, i.e. the Alpine, Mediterranean and Australoid, Races. If we are to assume that the earlier negroes or negritoes evolved in Africa, then we are faced with several cumbrous inconsistencies. Where did the negroes (and negritoes) of Melanesia and thereabouts come from? If Africa is suggested, the obvious reply is that it is far simpler to assume that both African and Melanesian negroes come from south Asia, i.e. the same centre of racial evolution as did the other races. Moreover, the 'Zones and Strata' hypothesis leads us to believe (even if this be not actually proven) that primitive races persist in the marginal lands, precisely where they did *not* evolve.

[1] It is probable that the first Alpine peoples reached France (Solutré, etc.) in Aurignacian times (*vide* A. Keith); and Koeppen dates this as far back as 74,000 years ago. Neolithic times in France were only 8,000 years ago (Keith 1931 and Koeppen 1932).

The same arguments apply to the negritoes, and lead us to accept an Asiatic cradleland.

What was the relation of Africa, Australasia, and America to the Eurasian land-mass during the later Ice Ages—when we may surely picture these earlier racial migrations as occurring ? Surely something like this. The easiest of access was Africa, for only the Red Sea—probably much less of a barrier then—separated that region of deserts and savannas from the South Asiatic cradleland.

Australasia was the next most accessible. During the Ice Ages no doubt the broad low area of Sunda Land with the almost dry Bali-Timor ridge led man to the large low ' Sahul Land ' and so to Australia (Griffith Taylor 1937). In the Interglacial period both Sunda Land and Sahul Land were drowned as the result of the filling of the oceans by the melting ice caps. Hence we may postulate that Australia and Melanesia were, on the whole, much harder to reach than was Africa in those early days.

As regards America, all migrations must pass *via* north-east Siberia. In the Ice Ages this was covered with an ice cap (Griffith Taylor 1930) which would definitely discourage migrations. During inter-glacials the Behring route might be quite feasible—and doubtless during such a period a few tribes of Australoids or kindred folks reached America. Possibly during the close of the Wurm Ice Age the Eskimo reached America while their congeners, late Palæolithic man, were reaching Western Europe. The main migrations into America seem to have occurred in the warmer periods (say of the Achen retreat of the ice, or between the Buhl and Gschnitz minor advances of the ice in Europe) some ten to twenty thousand years ago.

Now, assuming these geographical relations, what should we expect to find ? Primitive man was thrust out of south central Asia (primarily by climatic changes leading to greater cold or aridity) and would know nothing of the outlying areas. He would, no doubt, move off in several directions (to south, south-west or south-east) more or less equally. Thus the greater proportion of the earliest (Negro) migrations would inevitably reach Africa (the easiest outlet, on the whole), while a smaller number would reach Melanesia by circumventing the very difficult tangle of mountains in south-east Asia and crossing the ' stepping stones ' of the East Indies ; and, if fortunate enough, making use of the alternately open and drowned corridors of Sunda and Sahul Lands. This ' paired ' dispersion to west and east is illustrated in Fig. 7.

As millennia passed the more accessible lands of Africa would fill up, and Australia would receive a much larger proportion of later (Australoid) migrations. Finally as the latest migrants were thrust from Asia, the American corridor became available—and this is why we find so large a proportion of the last or Alpine-Mongolian Race in the New World. A glance at the arrangement of the zones (Fig. 7) will show that this series of migrations is fully corroborated.

C 4. Scandinavian Climate.

Let us now consider the environmental conditions somewhat more in detail. In the first place the migrations were probably extremely slow, and were made quite unwittingly by the primitive peoples concerned. They would all be hunters, preying on wild animals or upon wild fruits and grains. With the onset of any Ice Age, the forests, steppes, and tundras move slowly but *en masse* to the south. A fall of temperature of 12° F. is the maximum effect. This temperature range (by the ordinary ratio explained in any text-book of climatology) [2] is normally equivalent to a journey of some 800 miles toward the Pole. Such a migration of vegetation would perhaps change half the Siberian forest into tundra, and change the whole central Asiatic desert belt into steppe, while much of the southern forest belt would gradually turn into desert.

Research in Scandinavia has made it much easier for us to reconstruct

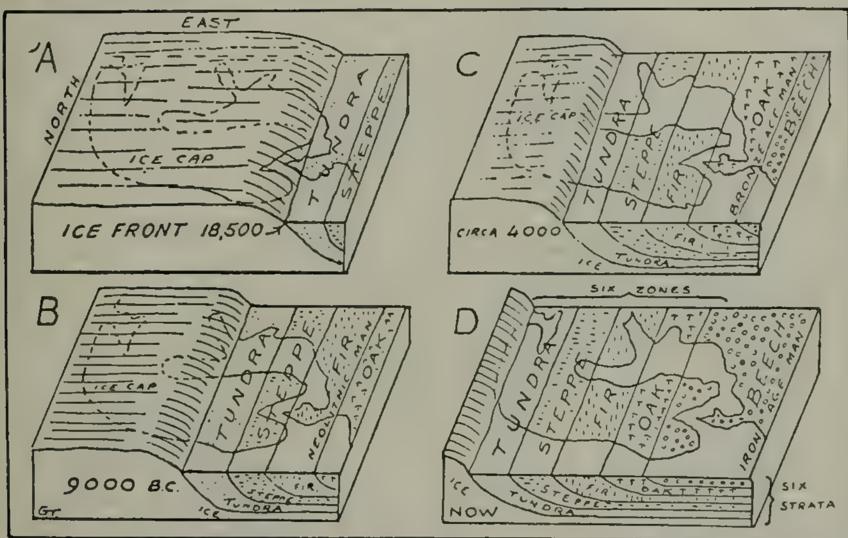


Fig. 6.—Correlations of Climate and Culture, showing the Northward March of the Ice-Cap, the Vegetation Zones and Primitive Man in Scandinavia since the close of the Wurm Ice Age. The front of each diagram shows the Strata in section.

the movement of ice-caps, vegetation-zones, and of man himself (Griffith Taylor 1934). De Geer and others working on the Varve-clays have dated the moraine of the waning Wurm Ice Age as it developed in South Sweden. They place it about 18,500 B.C. This is shown in Fig. 6, at A, where Sweden is shown buried under the great ice-cap. Peat bogs in North Germany and Denmark show that tundra plants were growing south of the ice-cap at this time. Man had apparently not yet appeared in Sweden.

In block-diagram B (Fig. 6) we see that the ice-front has retreated northward half-way along the Swedish Peninsula. This is dated about 9000 B.C. At that time the peat bogs in Germany show remains of fir-trees, and here also we find the artefacts of Neolithic man. Apparently Palaeolithic man found the tundra and steppe very unattractive and so never settled on the Baltic. The next diagram C shows us a further retreat during 5,000 years. The fir now covers Southern Scandinavia

[2] Off China the world isotherms change 1° F. for about 1 degree of latitude.

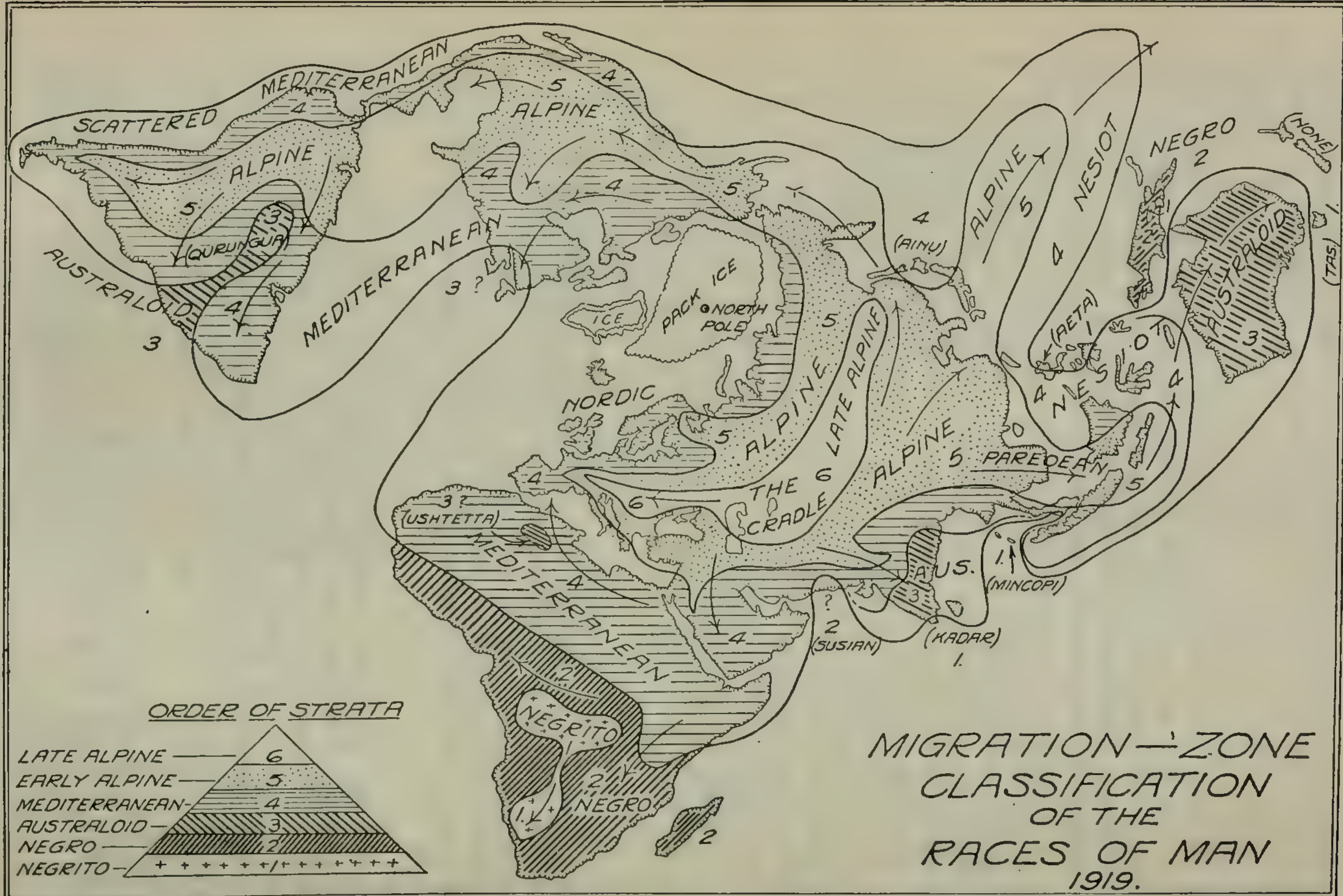


FIG. 7.—The logical classification of the Races of Man, based on racial history and ecological principles as well as on head index, hair, etc. The five major races about A.D. 0 had spread all over the world. Ecology gives the clue to their common cradleland and order of evolution. (Revised map from *Environment, Race and Migration*, Oxford, 1937.)

and oak-trees cover North Germany. Bronze tools are found in the bogs in the oak stratum—showing that a higher culture has moved north *with the ice-retreat*. Finally, at the dawn of history, conditions were like those to-day. The beech is now the dominant tree on the Baltic—and its advent was marked by the coming of Iron Age man. Here, then, we have a *dated* set of zones and strata, and we can be sure that similar movements of vegetation and man, northward and southward, accompanied every one of the Ice Ages throughout the Pleistocene.

The general distribution of Races over the World, before the period of modern marine migrations, is given in Fig. 7. I have devoted nearly twenty years to this 'Migration-Zone' theory of racial evolution and classification, the main features of human ecology. I mention it here primarily because it demonstrates that all the progressive nations of the world are built up of the same three stocks, 'Alpine, Nordic and Mediterranean.' When this thesis is accepted, then much of the evil structure based on 'race-prejudice' must fall to the ground. In my opinion, race prejudice is but another name for ethnological ignorance.

This is a very encouraging idea, for cultural differences of language, education and religion can be entirely changed in a generation, whereas a real racial barrier is much more difficult to overcome. Thus the world must wait a long while for the negro problem (based on a real *racial* difference) to be solved. But racial differences exactly like those separating Europeans, Japanese, Chinese, Indian, Polynesians and Amerinds have all been smoothed away in Europe itself; where (in my opinion) their component stocks came into contact with each other long ago in Neolithic times.

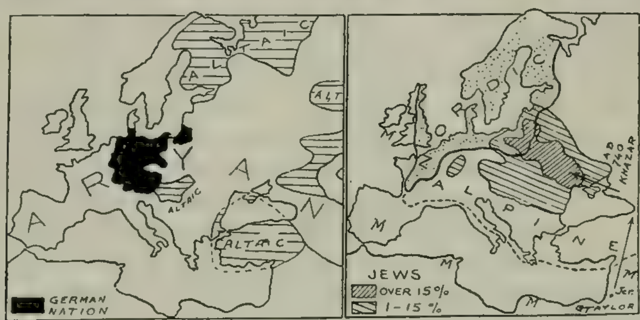
D. Relations of Culture and Race.

One of the main results of a knowledge of cultural geography is a much clearer conception of the distinctions between race, nation, language and religion than most educated people possess. It can be well illustrated, as we shall see, by maps in connection with the spread of the Jewish people. Moreover, this study clearly defines the danger resulting from powerful political groups dabbling in sciences of which they are ignorant.

We are surely all agreed that the term *Aryan* can only be applied to *speech*; and that *Nordic* indicates a 'breed' and can only be applied to *race*. But few folk realise that the term 'Jew' should only be used in connection with religion. It is much too common an experience to have to argue with folk, however influential, who insist on talking of a 'Jewish Race.' We need a new term to express a group linked by purely *cultural* characters such as language or religion. For such groups I have been extending the use of the word '*cult*.' For instance, in Canada, we have in reality no 'French Race' (since Frenchmen may belong to one of *three* distinct races), but only a 'French cult' linked by common language and religion. So also we should learn to speak of a 'Jewish cult,' since this large group is linked closely by religion and to a lesser degree by language. The Jews, like the Germans, are of two different races. If they come from Poland they belong to the Alpine race; if from Spain they are of Mediterranean race, like all the original Jews of Palestine.

Since this problem illustrates very concretely the way in which the social sciences are vitally concerned in world politics, I will dwell on it briefly. It is illustrated in the two maps in Fig. 8. On the left we see the logical *linguistic* divisions in Europe, which are undoubtedly Aryan and Altaic. Here also is shown in black the realm of the German *nation*. In the right-hand map (Fig. 8) are the *race* divisions in Europe, i.e. Nordic, Alpine and Mediterranean. The German nation is half Nordic and half Alpine. The Jew belongs to a 'cult,' but the dominant Jews in Europe, including about three-quarters of the whole body, are broad-headed Alpines like the rest of the mid-European peoples. The most logical explanation is that the Polish Jews are the result of the widespread conversions carried on by the Jews in eastern Europe. For instance, in A.D. 740 the Khan of the Khazars (who lived north of the Black Sea) was converted, and many of this large nation of medieval traders

(*a*)



(*b*)

FIG. 8.—(*a*) Map of Europe showing the distribution of Aryan and non-Aryan (i.e. Altaic) Languages. The area of the German Nation is also charted. (*b*) Map of Europe showing the distribution of the Nordic Race (dotted), and of the folk who profess the Jewish Religion. The Alpine and Mediterranean Races (M) are also indicated.

followed his lead (Griffith Taylor 1936b). Racially the Khazars were akin to the Turks, i.e. they were Alpines; and they are also known as the Royal Scythians. Indeed the Polish Jews still call themselves *Ashkenazim*, which is a Hebrew word meaning 'Scythian.'

A year or two ago the German authorities were specifically excluding the German Jews as of 'non-Aryan race.' Of course racially they are Alpines like the south Germans, and their language is best called Judeo-German. Anyone with a slight knowledge of German can understand the following sentence '*Es ist gar alles kein Neues nicht unter der Sunn.*' Yet this is Yiddish, which is mostly a medieval German dialect learnt in the Rhineland (Wiener 1899). It has a considerable addition of Hebrew words, in the same way that English includes much Latin, but English does not thereby become a Romance language. Nor does English change its Saxon character when it is written in Pitman's shorthand. The Semitic script used in Jewish newspapers disguises, but does not change, the essentially Aryan basis of Yiddish. Thus the accurate student of social science would describe the Jews as *a composite culture-group* ('*cult*') *with a specific religion, most of whom are Alpine in race, and speak an Aryan language* (*a dialect of German*) *which is written in a peculiar script.*

E. General Ecological Approach to Problems in Culture.

Let us see how ecology can help the study of the evolution of languages. The early settlers of New England came mainly from Suffolk and the adjacent south-east of England. They carried to America the pronunciations of early Stuart times, and some of these have changed considerably in England since their departure. In the fifteenth century words like *dark*, *far*, *farm*, *star*, etc. were spelled and sounded *derk*, *fer*, *ster*, etc. So also *clerk* and *new* were pronounced *clerk* and *noo* in this part of England. About the time of Elizabeth the pronunciations *clark* and *nioo* were growing in favour, and have since become universal in England.

The older forms were carried to America and survive in rustic New England. So also certain Elizabethan and Stuart ballads are perhaps better preserved in the isolated mountain hamlets of the Appalachians than in most of England. Many old words have become archaic in England which are still in common use in much of America (Wyld 1920).

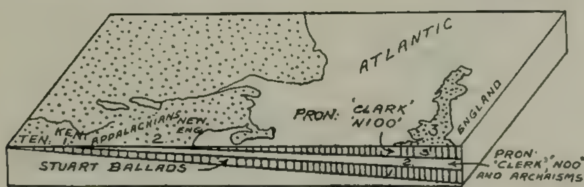


Fig. 9.—Linguistic Evolution indicated by the distribution of early pronunciations and folk-lore, which survive in marginal regions. An illustration of the 'Zones and Strata Concept.' Three 'strata' are suggested on the front edge.

Examples are *fall* (autumn), *guess* (think), *sidewalk*, *whittle*, *greenhorn*, *cordwood*, *gotten*, *cracker* (biscuit), *shoat*, etc. Here again, as we saw in race, the primitive is 'pushed to the wall' far from the cradleland. No one imagines that Shakespeare lived in the Appalachians because some of his language is now perhaps more common there than at Stratford! But many philologists have thought that Sanskrit *originated* in Lithuania; whereas Lithuanian is a *marginal* survival of an early Aryan akin to Sanskrit, thrust out from the common cradleland in south central Asia.

These stages in the evolution of the details of a language are charted in Fig. 9 which illustrates the principle of the 'Zones and Strata Concept' fairly clearly. As before, we see that the primitive type is pushed to the margin, while the later types appear first in the central cradleland. Of course conditions have changed so greatly in America in the last fifty years that it is now an independent centre of stimulus—possibly the greatest in the world in regard to modern culture—and Britain is borrowing new terms from the U.S.A. There is, however, not much difficulty in detecting such new centres of culture in dealing with problems of the evolution of early culture in the Eur-Asian world. For the most part they progressed fairly regularly from south-east to north-west. This is indicated in the following graph dealing in a general fashion with certain phases of progress in the last 6,000 years.

This diagram (Fig. 10) illustrates the necessity for defining the amount of correlation involved in a given comparison. It is, of course, obvious that the shift of power is not wholly determined by the lower temperature of high latitudes. There is, however, no doubt that physical vigour is somewhat higher at lower temperatures, though Huntington is convinced that the optimum occurs at 63° F.; while the best mental work is done in regions with an average temperature near 45° F. (Huntington 1938). These facts must have a bearing on the evolution of all forms of culture. Probably of equal importance in the shift of empire are other factors such as 'freedom from invasions.' Invaders attacked Europe from Asia and Africa at first; and later, Britain was saved by her insular position from many continental attacks. Command of the Atlantic seaboard, and, chief of all, readily accessible coal supplies also contributed to this shift to the north-west.

We may use as an illustration of the value of the 'Zones and Strata Concept' that complex of races and cultures which characterises the

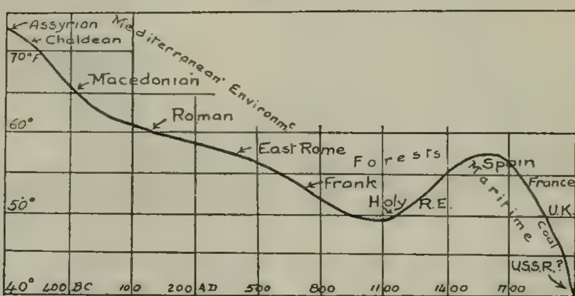


FIG. 10.—Correlations of Temperature and Empire. Other factors are discussed in the text.

Indian and Pacific areas. The writer has had the advantage of travelling widely in Eastern Asia and in the Pacific, and this has focused his attention on the general principles underlying dispersion in this area. It is quite obvious that every widespread characteristic in Polynesia has migrated from west to east—and that any cultural contacts with America can be completely ignored in a general study. Let us examine the data in Easter Island—the farthest of these isolated groups from Asia (Fig. 11). It is almost 14,000 miles from the Caspian area to Easter Island, yet I hope to demonstrate a culture sequence stretching across all this vast expanse.

Two remarkable features in Easter Island are the well-known stone statues and the undeciphered script incised on wooden tablets. There is no reasonable doubt (as the Routledges (1919) have shown) that the statues, with their bird-man decorations, are of the same culture-complex as is common in the Solomon Islands, some six thousand miles to the west (Fig. 11). Hevesy (1933) and Hunter (1934) are satisfied that the script, the only one used by Polynesians, is connected with the remarkable Mohenjo Culture which flourished in the Indus region about 3000 B.C. It is true that objections have been raised by Métraux (1938) that the script was not understood by any living Polynesian, and that the tablets of mimosa wood, etc., are not likely to be many centuries old, some indeed being modern in origin. The present writer thinks that these objections are not very relevant. Our own alphabet is said to originate from not very similar signs used by miners in Sinai, though all the links are not yet clear. The question surely is to determine the origins of the remarkable Easter script—and to my mind, the Mohenjo theory is plausible and indeed probable. Moreover it offers a good illustration of clues which may be furnished by an ecological approach.

Let us consider some of the major culture changes in the Indus region. Gordon Childe (1934) gives data as to the races which have been discovered at Mohenjo. Australoids, Mediterranean, Armenoids and Mongoloids were all present. There can be little doubt that the first settlers (before 3000 B.C.) were the aboriginal 'Australoids' who spoke a Munda language. Many members of this zone of peoples are now found 'pushed to the margin' in the East Indies and in Australia. It is represented by

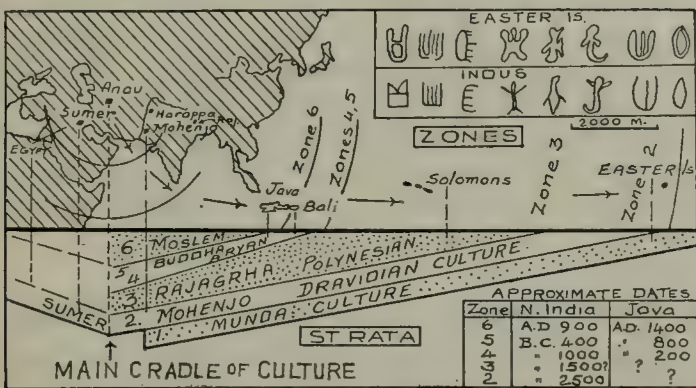


FIG. 11.—The spread of cultures from India eastward; showing the Munda, Australoid culture at the bottom, covered by Dravidian, Polynesian, Aryan-Buddhist and Moslem 'strata.' In the Inset are compared some signs from the Mohenjo and Easter Island scripts. All much generalised.

Stratum 1 in Fig. 11. The general belief is that the Mohenjo culture was due to the later 'Mediterranean' races who spoke Dravidian languages. This constitutes Stratum 2, and in the writer's opinion is to be linked with Dixon's 'Caspian Race' in the Polynesian area (Dixon 1923).

We have little knowledge of the period from 2500 B.C. to 1500 B.C. in India, when the great Aryan migrations overwhelmed North India. But it is significant that the earliest stone monuments in India, which are found at Rajagrha (Rajgir) near Patna,[3] are of a cyclopean character quite unlike the work of the later Aryan builders, and rather resemble the mysterious early stone monuments of the Pacific (Fergusson and Burgess 1880). I have suggested that this culture-complex spread out as Stratum 3. The Aryan-Sanskrit complex (Stratum 4) never reached Polynesia, but was carried to Java and dominated that region for several centuries after 200 B.C. In North India Buddhism (Stratum 5) flourished after 500 B.C.

[3] The Jarasandha monument (of unknown date) is a square truncated pyramid 85 ft. wide and 28 ft. high. It is built of large uncemented blocks of stone 5 or 6 ft. across. It resembles the truncated pyramids and Maræ of Polynesia.

and was carried to Java about the eighth or ninth century of our era. It did not displace the older Hindu pantheism—but flourished alongside. Around A.D. 1400 the Moslems (Stratum 6) conquered Java, and the Indian religions found a refuge in the island of Bali further east, where they still flourish. It is not, of course, suggested that the Polynesians migrated from India, for they probably lived originally in south-east Asia. But their culture probably followed the same route as that used by the Buddhist and Moslem teachers in historic times.

We may dwell for a few minutes on the recent discoveries in the vicinity of Persia. In Mesopotamia the earliest culture of Sumer is known as 'al Ubaid' (Childe 1934), and this contained copper tools and is younger than cultures from Susa and the adjacent Persian Plateau.

To the north near Nineveh is the 'Tell Halaff Culture' with wheeled vehicles, but with no metal. This is much older than anything discovered in Sumer near the Persian Gulf. Still older are the lowest cultures of *Samarra* in the same region, where they occur in debris seventy feet below a temple dated about 2450 B.C. Childe corroborates my statement (of 1919) as to the cradleland of man, with his comment that the early cultures of China resemble those of Anau in Turkestan (Fig. 11). It is significant that Zoroaster, the first great religious teacher, lived in this same vital region. Thus we see that the centre of the zones of the *races* of man in Turkestan (as charted in Fig. 7) is also likely to be near the cradle of civilisation.

F. Correlations in the Distributions of Languages.

The evolution of nations is one of the most interesting and important problems engaging the attention of the social scientist. A common language is often the chief 'cement' which links the various races and 'cults' to form a nation. Hence languages merit our careful study. Few problems in Science are so difficult as those concerning the interrelations of the main language-groups. Since here we have to do with an evolving complex arising in something like a cradleland and affected by wide migrations, it seems likely that some light on the subject may be obtained by charting the data in the form of the 'Zones and Strata' technique.

The distribution of the main groups of languages is given in a generalised fashion in Fig. 12. The ecology of language indicates that the order of evolution in the Occidental area is in the following sense, the marginal languages being the earliest:—Bantu, Hamitic, Semitic, Basque, Sumerian, '*K*' Aryan, '*P*' Aryan, and latest or 'Satem' Aryan. The problem is, of course, complicated (as in Biology) by the fact that independent evolution takes place after the branching of the parent languages. Thus it seems likely that Proto-Gothic branched off from the Aryan stock before Sanskrit. Yet English (a descendant of Proto-Gothic) is a more advanced language (i.e. more analytic, simpler and easier to learn) than is Sanskrit.

The following summary (Worrell 1927) gives simple definitions of the language classes. In primitive languages like *Bantu*, parts of speech were differentiated by the attachment of different relationship-words—which,

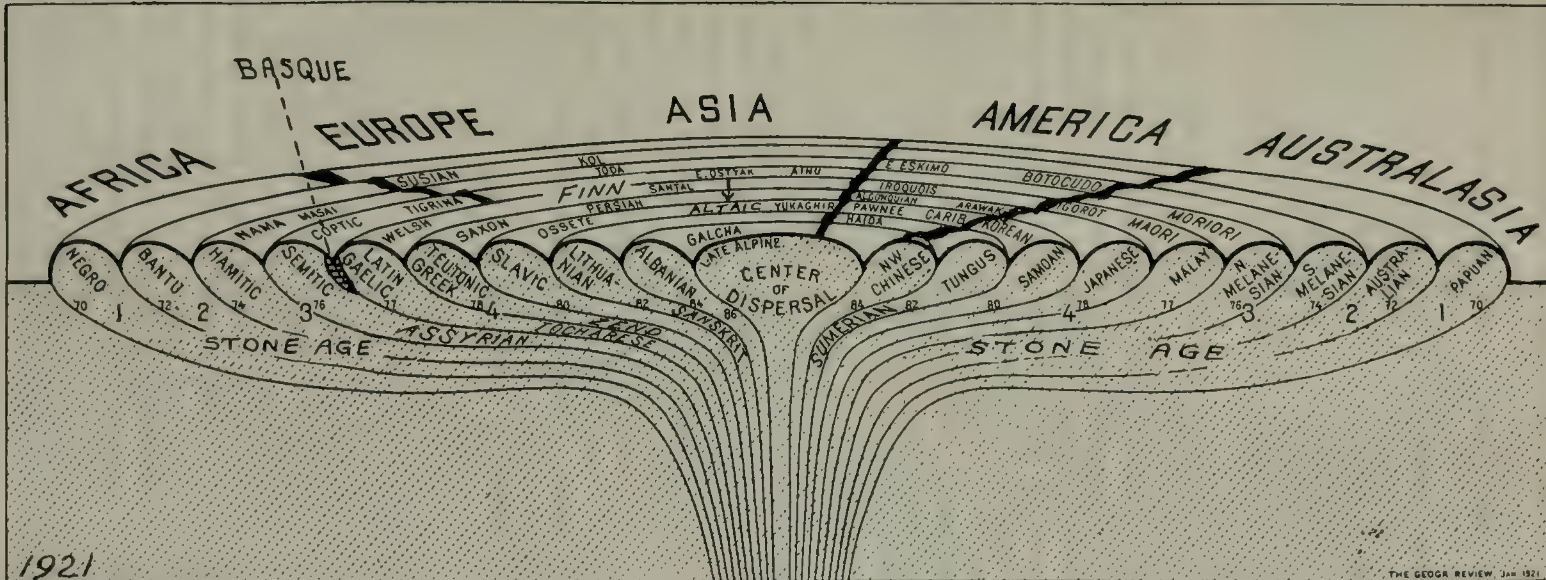


FIG. 12.—An illustration, published in 1921, of the 'Zones and Strata' technique. It is highly generalised. The lava-flow analogy applied to studies of evolution and linguistics. The centre of dispersal in Central Asia is pictured as a sort of 'fissure eruption' sending forth streams of lava in all directions. Each new eruption arises from the centre and, while covering some portion of the previous flow, pushes most of the previous lavas out to the periphery. Fossil languages, buried under existing tongues, are seen in section. On the surface are shown existing languages typical of the several migration zones. (From *The Geographic Review*, New York, 1921.)

however, did not fuse with them. Vowel harmony of all the syllables of such a compound often developed as a means of marking off the group. This is *Agglutinative* speech. It survives in the Bantu tongues (*prefixing* type) and in the *Altaic* (e.g. Turkish) as a *suffix* type.

The Hamitic-Semitic group carried agglutination so far that the relationship-words fused at last with the chief words to which they were affixed or prefixed, and speech became *amalgamating*. Words were also systematically modified by internal vowel-change to give regular alterations of meaning. Dravidian speech is agglutinative—affixing and infixing, so that it is rather generalised and may be the ancestor of several of the other main languages.

The Aryan group developed external suffixes to indicate variations, and so produced *inflectional* language. The three groups Hamitic, Semitic and Aryan also tend to rely on relationship-words and on word-order, and increasingly to neglect the word-forms. Thus they become *analytic*. There is little doubt in the writer's mind that this sequence (e.g. from Bantu to such an Aryan language as Persian) represents linguistic evolution, in much the same sense as the sequence ' amoeba to man ' represents zoological evolution. In both cases many groups branch off from the main stem producing minor independent evolutionary groups. In both cases some descendants stagnate, while others advance rapidly, as stated earlier.

Before Aryan scholars yield to despair because the foundations of Aryan are ' wrop in mystery,' a promising field would be to explore Dravidian or Altaic for the ancestors of the Aryan. For instance, there are three possible explanations for the accepted resemblances of Finn to Teutonic. The one usually accepted is that Finn has borrowed from Teutonic. But it is also possible that Teutonic has borrowed its ' peculiarities ' from Finn. A third view worth considering is that border (e.g. primitive) languages like Keltic or Teutonic, still retain speech characteristics which have been carried over from the more primitive speech (now preserved as a *marginal* language-zone) from which the Aryan group as a whole evolved. On this view the features common to German and Finn may be an inheritance from the common mother-tongue of Aryan and Altaic.

The lesson to be derived from the ' Zones and Strata ' technique is that marginal languages should be compared with each other. This means that *far-distant* speeches may be very well worth comparing. From this point of view, we should actually *expect* that Basque would resemble some Amerind language ; that Gaelic would resemble Pharaoh's tongue ; and that early Sinitic, early Altaic and early Dravidian (i.e. marginal languages) should be studied to learn something about Proto-Aryan. Thus the writer by no means despairs of the solution of the Aryan problem.

These ideas have long been engaging the writer's attention. In 1921 he published a generalised diagram, which was probably the first utilising the ' Zones and Strata Concept ' as applied to Linguistics. With a few minor alterations it is reproduced as Fig. 12. Here the concept of a *central* cradleland of culture is adopted. But we must ever bear in mind that we are primarily concerned with events which occurred before

5000 B.C., for all the major language families had differentiated before that period.

The cradleland is represented as sending forth successive flows of lava from a centre of eruption. These form concentric zones about south central Asia, and each flow pushes its predecessor to the margin. The effect of one flow on its neighbour—involving some contact and assimilation—is also rather usefully indicated. The flows reach the four 'peninsulas' of Asia (i.e. Europe, Africa, America and Australasia) according to the relative advantages of the connecting corridors. I have elaborated this concept in several essays already published (Griffith Taylor 1936c).

G. Ecological Notes on the Aryan Problem.

We may use the stage-diagram to correlate our scanty knowledge as to the early wave-fronts of the Aryan languages. There are three fairly definite subdivisions of Aryan: (1) the early *Kentum* or '*K*' speeches like Gaelic and Latin, (2) the Intermediate '*P*' languages like Welsh (with which we may associate Teutonic and Greek for convenience), (3) the later *Satem* languages like Slav and Indian.

Turning to Fig. 13, some idea of our knowledge of the language distribution in Sumerian times is given in the lowest map of the series. At this time Hamitic languages were used by the Pharaohs in Egypt, akin to those still spoken by the Berbers in the Atlas Mountains. Semitic languages characterised Arabia and Syria, as they still do. Sumerian itself has some resemblances to the Altaic, though its affinities are not yet clearly understood. In Europe at this early date there were racial allies of the present-day Hamitic-speakers—all of Mediterranean race—living in the western regions, who probably spoke Hamitic according to Rhys and Jones (Griffith Taylor 1936a). Central Europe was occupied by early migration of Alpine 'Brakephs' (broadheads), of whose language we know nothing. It was almost certainly not Aryan, and something akin to 'Basque' seems most likely. This problem is taken up later. In view of the important corridor linking Turkestan with China by way of the Tarim Basin, I have ventured to suggest that a linguistic kinship between early Chinese (Sinitic) and Sumerian or early Aryan is only to be expected.

In the second map (Fig. 13 at B), for the period around 1200 B.C., we are on surer ground. Vast migrations of 'Satem'-speakers had poured into India from Turkestan. The Hittites, who seem to have spoken an Aryan tongue somewhat akin to the Kentum Group, were in control of Anatolia. Semitic was now the chief language of Egypt and Mesopotamia.

In central Europe (if we adopt the suggestions of H. Peake) Kentum languages were spoken in the regions east of the Alps, while Brythonic (one of the Intermediate 'P' type) was that used by the Cimmerians of the Ukraine and Caucasus areas. It seems logical to assume that many Satem-speakers still remained in Turkestan, and were perhaps allied to the Sarmatian tribes.

In the next map (for 400 B.C.) we see the first great Aryan conquest in the Near East, that of the Persians. They spoke a Satem language, and it is probable that their Sarmatian kin were occupying the European

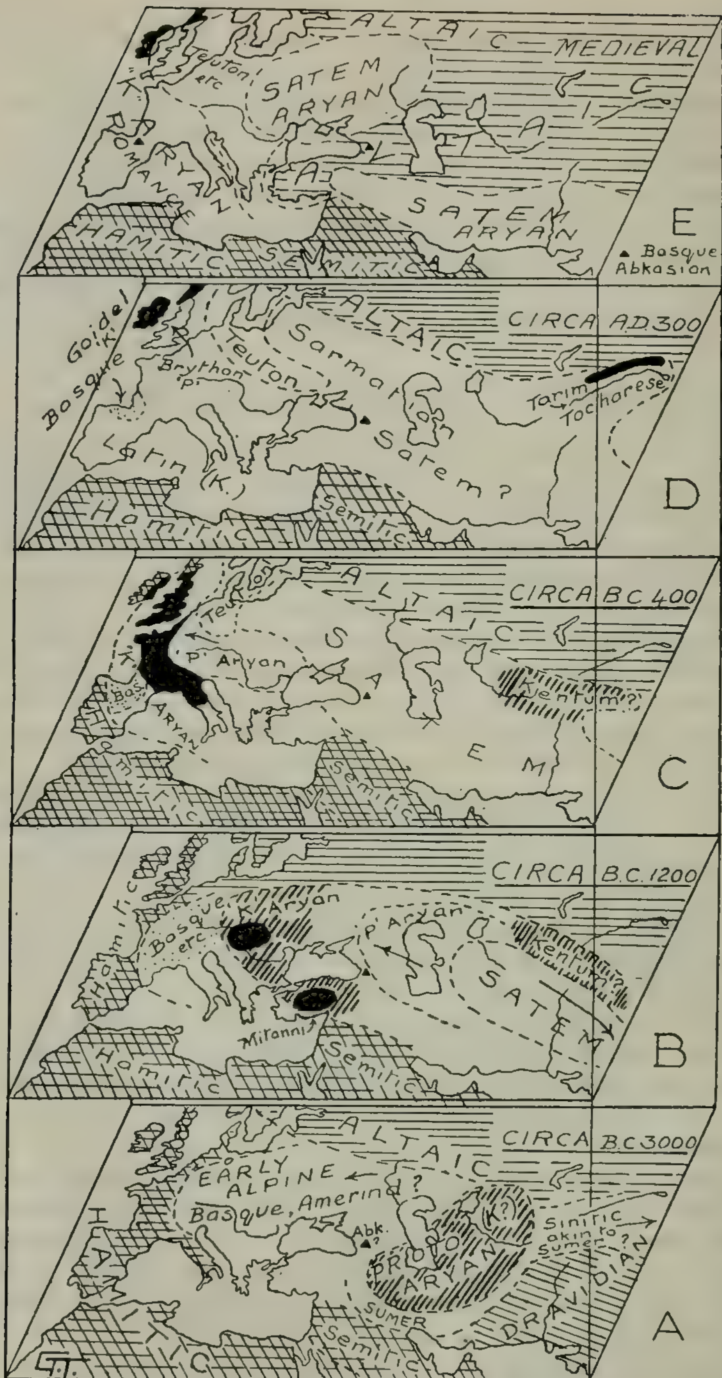


FIG. 13.—A Stage-Diagram giving a tentative reconstruction of the distribution of European languages at various epochs. Black areas are the Primitive (marginal) '*K*' *Aryan* languages. It is suggested that the early Mediterranean Race spoke *Hamitic*, and that the early Alpines spoke languages akin to *Basque, Abkasian* (and Amerind ?). Aryan developed near the Caspian Sea and spread out in waves. The outer 'ripple' (*K*) was akin to *Gaelic*; the second (*P*) akin to *Welsh*. The latest type was the *Satem* group.

steppes about this time. The latter may have been the ancestors of the *Slavs*, who already seem to have settled in the Vistula Basin. Meanwhile the marginal *K*-speakers (Gaelic, etc.) had reached Britain and Ireland, and still occupied parts of France. The distribution of place-names in Central and Western Europe clearly shows the migration of waves of Gaelic and Welsh speakers across much of these areas.

The conditions some seven centuries later (A.D. 300) are shown in the next map which deals with Europe during the zenith of the Roman Empire. The marginal primitive Aryan language Latin had been carried far and wide ; so that it later gave rise to Italian, French and the other Romance tongues—which are clearly offshoots of the ' *K* ' group of Aryan. Brythonic (Welsh) was spoken in England, South Scotland and Wales at this time, and probably in parts of the continent beside Brittany. Possibly some Hamitic dialects still persisted in the Scottish Highlands, as suggested by some of the Ogam inscriptions. Gaelic (a ' *K* ' language) was spoken in Ireland and in most of Northern Scotland.

Of great interest is the discovery that a Kentum language, called Tocharese, was still in use north of the Tarim Basin in Central Asia about this time (Fig. 13, at D). Tocharese seems, however, to have some affinities with the Intermediate and Satem groups also. Hence it may well be fairly close to the *generalised* Aryan ancestor from which all three groups of Aryan have descended (Childe 1936). It is suggested in the diagrams that this Kentum speech had been continuously used east of Turkestan since early Aryan times.

The medieval distribution of languages, and of the three subdivisions of Aryan, is shown in the top map. To-day Gaelic is almost the sole representative of a little-altered primitive Aryan speech—though the much-evolved derivatives of primitive Latin are still very important languages (Jespersen 1894). Hamitic has died out in Europe. Altaic has encroached in Hungary and Finland, and displaced Hittite and Greek in Anatolia. Semitic has driven out Hamitic in much of North Africa. ' Satem ' Aryan, in the form of Russian, is in turn displacing Altaic throughout much of U.S.S.R.

The conclusion to be drawn from this tentative geographical approach to the Aryan problem is that the waves of language have spread from Turkestan towards India, Persia and Europe. There seems to be no support for the origin of Aryan in the German or Lithuanian regions, a theory which has been strongly upheld by a number of notable continental philologists.

H. The Race of the Early Aryan-Speakers.

Let us turn to another aspect of the Aryan problem. What race first spoke the Aryan languages ? (The name *Wiro* has been given to this unknown ' race.') To-day Aryan is spoken by Alpines in central Europe, by Nordics in the north and Mediterraneans in the south. There can be little doubt that originally these dissimilar races ' despised ' each other as bitterly as any pair of ignorant and opposed ' cults ' do to-day. It seems logical to assume that each of the three original races at first had a somewhat distinct culture, including language. How can we advance our knowledge of their early linguistic history? There are several techniques in use. The oldest method is to study the syntax and etymology of related languages (i.e. Aryan), and learn, from their many variations, which was the original. Unfortunately, for many years this technique was fatally hampered by the idea that language 'decayed' by losing inflections—whereas this phenomenon is one of the clearest signs of evolution. However, there can be little doubt that the unwieldy Kentum languages of the western margin are closer to the original Aryan than the *Satem* languages of the centre and east of the Aryan realm.

Let us use an ecological approach. If we plot these languages on the map, it seems highly likely that the Mediterranean folk of south and west Europe spoke Hamitic (or its derivative Semitic) before they were conquered by Aryan speakers. The research of Rhys and Jones (*The Welsh People*, 1908) makes this entirely probable as regards Gaelic and Welsh. But our chief interest is concerning the original speech of the Nordics. It is usually taken for granted that they spoke Aryan, and of a type not far from Proto-German! I do not know any reason for this belief except the volubility of the high priests of the Nordic fetish. We have seen that it is possible that the marginal European of the south-west originally spoke Hamitic. Let us apply the same reasoning on the northern margin of the Aryan realm. Here dwell the Nordics—and it is very important to remember that many of the Finns are Nordic, as are some of the northern Asiatic peoples such as the Ostiaks of the Yenesei. These Finns and Ostiaks speak Altaic. Applying our general Ecological Law of Linguistics, we should expect that some marginal languages (i.e. Altaic) have been replaced by later languages (Aryan) migrating from the cultural cradle.

It is very significant that some of the characteristic features of the *Teutonic* group of Aryan remind one of similar features in the speech of the Finns and Ostiaks. For instance, 'strong' verbs like those of German (and English) are quite common in Ostiak (*Encyc. Brit.*, 1929). Moreover, Finn and Ostiak have more inflection and less of agglutination than most other Altaic tongues. There is, therefore, some ground for the suggestion that the Nordic folk of Germany and Scandinavia, originally spoke an Altaic language (like Finn or Ostiak): and only relatively lately in linguistic history learnt an Aryan tongue. Indeed, there seems little doubt that the earliest-known Nordics of central Sweden, like those of Finland, spoke Finn, not Teutonic. This speculation as to the ancestral tongue of the 'Blonde Overlords' is not likely to be acceptable in Teutonic circles.

The Basque problem has intrigued philologists for a century. The language is quite different from Aryan, but has some slight affinity with three distant groups, Abkasian in the Caucasus, Altaic, and certain Amerind (i.e. American Indian) tongues (Fig. 13). So far as I know no one but myself has suggested any satisfactory reason for the relation between Basque and Amerind. Yet if we study the zones and strata of Europe there is one curious feature. Peake (in *The Bronze Age*) has suggested that the earliest Aryan tongues (Gaelic, etc.) reached central Europe from Asia by way of tribes of *Alpine* race about 1500 B.C., or thereabouts. But there were folk of Alpine race in Europe for some four millennia before this period, for instance all the Danubian peoples who moved across Europe in the third millennium B.C., and many still earlier, such as the Men of Ofnet. What did they speak? Look further afield to the other side of the Old World (Fig. 7). At this date (say the sixth to the third millennia B.C.) it is generally believed that hordes of Amerinds were pouring into North America from central and east Asia. As I showed in 1919 they were of much the same race as the Alpines entering Europe. I suggest that the Pre-Gaelic Alpine invaders of Europe were members of the same linguistic zone and spoke Basque. Later, Europe was invaded by the last-evolved group in the cradleland who spoke Aryan. These 'Wiros' transferred their languages to almost all the other tribes in Europe excluding the Basques and Finns. In the rugged valleys of the Caucasus, relics of Pre-Aryan language, such as Abkasian, seem to have survived. In its syntax it resembles both Basque and many Amerind tongues.

Following the principle of 'doubt and deduce,' I venture to sow many seeds of linguistic heresy which I hope will prove fertile in the minds of some young researchers. Let us consider the typical marginal languages of Europe, i.e. Gaelic and Welsh. It seems to be rather generally believed that these have always been spoken by the dark highlanders and their allies of Mediterranean race in Wales and Ireland. A great deal of natural pride is based on this belief in this age-long association. But our 'Zones and Strata' theory suggests that these marginal peoples only recently learnt these languages from entirely different races. The writer believes that Hamitic speech (akin to the language of the Pharaohs) was spoken in most parts of Britain while the Greeks were learning Homer. The Berbers, Tuaregs and other still more marginal folk of North Africa are of the same race and still preserve their old speech without change (Fig. 13). It is to be hoped that Berber will not be made compulsory for the unfortunate youngsters in the Irish Free State as the result of this address!

As regards France, we have very little knowledge of the languages spoken as late as 200 B.C. which are called Gaulic. Study of the migrations of the Kentum Aryans shows that the western tribes probably spoke something close to Gaelic—but intermediate between this speech and Latin. The writer has never been satisfied with the general belief that French is *entirely* a derivative of Latin. If the western *K* Gauls used the same linguistic roots as did the Romans, why is not French largely *based on Gaulic roots*, with the presumably characteristic suffixes, etc. 'worn off' according to the usual development of a language? I may make my point clearer by an exaggerated analogy. Supposing we knew nothing of the English language before 1750, we should be far from correct if we assumed (because English resembled German) that it was largely due to the Hanoverian culture of that date.

Few developments in world history are more remarkable than the spread of the Romance languages which are of course *largely* based on Latin. It is well to realise that Latin is one of the most marginal and, therefore, one of the most primitive of Aryan tongues. Jespersen pointed this out many decades ago—but there is such a halo around Latin that this has not yet become generally known (Jespersen 1894). Many philologists still maintain that the striking change from Latin to French and from

Anglo-Saxon to English is one of 'decay.' They would seem to deplore the loss of the cumbrous and countless inflections. A study of language-distribution would show them that the languages still more marginal than Latin have even larger development of inflections. For instance, they are peculiarly abundant in Australian aboriginal speech and in West African languages! I venture to predict that philologists will soon accept the following general ecological and cultural 'law': Marginal languages are primitive, and characterised by cumbrous inflections; they evolve by loss of inflections and by the development of an analytical character; this gradual change is illustrated by the concentric zones of actual languages. This 'law' was pointed out by the writer in a tentative discussion of Language Evolution in 1921 (Griffith Taylor 1921), when the general 'key' to the whole process (suggested in Fig. 12) was first published.

I. Graphs of Culture Growth.

It is often of great help to the research student in cultural problems if he can make a mental picture of the processes involved in culture-spreads. In the following section I have endeavoured to realise such a picture.

City growth of a type which is familiar to every geographer is illustrated in the stage-diagram given in Fig. 14. Here four stages in the development of the City of Chicago are represented in a sort of isometric projection. The years chosen were 1846, 1873, 1899 and 1926. The diagram is a modification of the 'Zones and Strata Concept,' and emphasises the fact that 'the Zones of to-day are the Strata of to-morrow.'

The maps extend about 16 miles south and 10 miles north of the geographical centre of Chicago. In the lowest map is shown the site of the fort which was built in 1816; just where the *voyageurs* paddled up the South Chicago River to reach the boggy divide, which alone separated the Mississippi Basin from the Great Lakes Basin. In 1828 there were three inns and about a dozen huts near the Forks of the small Chicago river. Farms existed in Madison Street in 1833, though to-day the nearest farm is about 15 miles away to the south. So also private houses have been displaced about 8 miles to the south. Forest covered much of the area south of Madison and this has been displaced about 30 miles to the south (Hoyt 1933).

In 1873 factories developed along the small river, while the best business section was still near the Forks. The old negro quarter was 1 mile south of the river near State Street. It now occupies an elongated belt from 3 to 7 miles away from the old city centre. The better residences are also migrating to the periphery. In the last map (for 1926) skyscrapers have developed along the lake front for a dozen miles, as well as in the old centre of the city. Very large Polish, Czech and Italian communities build up most of the population to the west of the city centre.

Crop-farming has now been pushed right out of the city limits, though small truck-farms are still to be found on some vacant lots. Within this fringe comes a zone of small two-story houses which are built mostly of wood. Still nearer the centre is a zone of better residences both north and south of the city, wherever industrial pursuits are not too prevalent.

The factories alternate on the west with large areas of wooden or brick houses occupied by foreign labourers.

The heavy broken lines (linking the four stages in Fig. 14) indicate the gradual growth of the city. Obviously this growth (as shown in Fig. 14) resembles a *cone*, starting at the original humble houses of the first settlement and gradually expanding (as a ' small-house zone ') on all sides as time progresses. The later type of building can also be

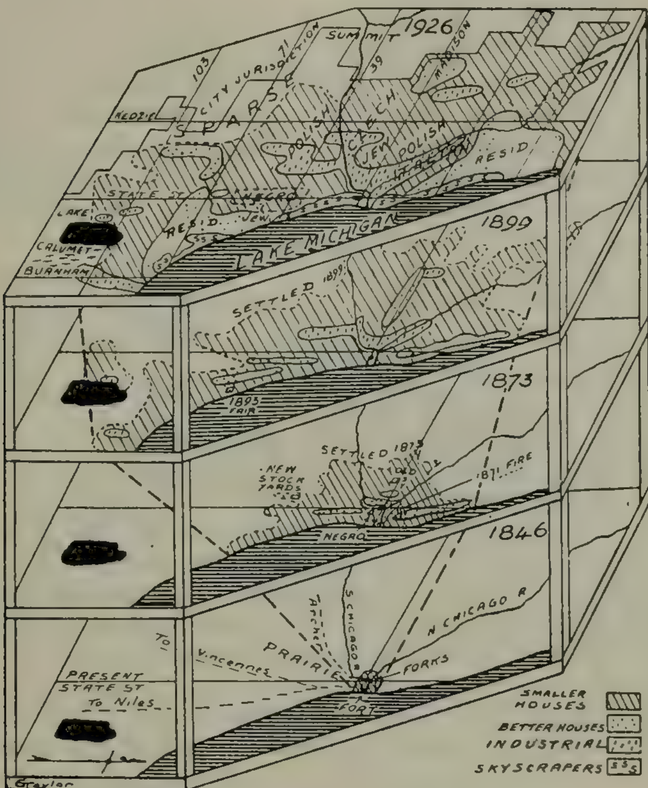


FIG. 14.—A ' Zones and Strata ' Diagram Showing the Evolution of Chicago for Eighty Years.

Notice that the older types of structure now occur on the periphery. The (skewed) squares are four miles across. The heavy broken line linking the four diagrams indicates the spread of the outer zone of simple wooden houses. (*Cf.* Fig. 15.)

represented by such conical growth-forms. The phenomenon is much like the series of concentric *craters* built up by the lavas in a gradually increasing volcano. Each lava-flow may be supposed to cover part of the preceding flow, but to push some of the mobile earlier lava still farther to the periphery. The whole concept as applied to culture-spreads has been illustrated in Fig. 12 earlier in the address.

Since these ' craters of growth,' as I have named them, help us considerably in our search for affinities of isolated tribes, speeches or cultures, I have developed the concept somewhat further in the next diagram (Fig. 15). Here the second ' crater ' illustrates how the Mediterranean

Race has developed in space and time. Starting from a small group of people, who originally lived somewhere near the Caspian Cradleland, this race has gradually spread out as the millennia passed until to-day it is found all round the margins of Eur-Asia, as well as in North Africa. On the west the Mediterranean zone is complete—as the rim of 'crater 2' suggests. On the east the later Alpines have burst through in their journeys to Polynesia and America. But isolated tribes of the Mediterranean Race, such as the Ainu and Lolo, still maintain the growth and spread of their forefathers. (This zone is shown, in plan, in Fig. 7.) Within the 'Mediterranean Crater' we may picture an 'Early Alpine Crater,' which however is not sketched. Below (and concentric with) Crater 2 we may imagine 'Crater 1,' which shows a much earlier racial distribution, that of the negritoes. Here the crater did not develop symmetrically—but extended primarily to the south-west (Congo, etc.) or to the south-east (New Guinea, etc.). However, the general conditions

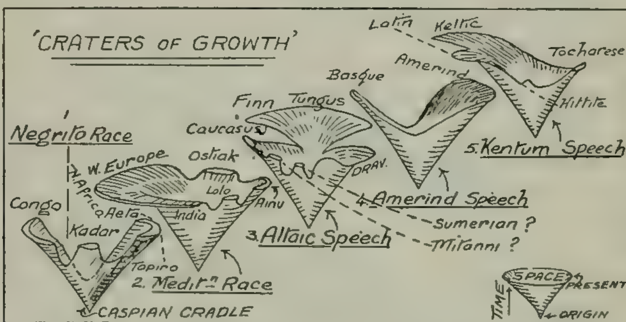


FIG. 15.—'Craters of Growth' are Space-Time diagrams illustrating the irregular spread of a culture-type from the point of origin (as suggested in the corner.) Compare the broken line in Fig. 14.

of growth are the same as in the other craters, and definitely explain the connection of the Congo negritoes with the Tapiro and Aeta off south-east Asia.

Turning now to the realm of language, we can illustrate their distributions by the same technique. As I have explained earlier, it seems highly probable that the main language-stems originated in south central Asia, just as did the races—but far later. It must again be emphasised that there is not likely to be any *permanent* connection between an expanding race and an expanding language. Both may originate in the same area—but language moves so rapidly and is so easily transferred that it is highly unlikely that its limits would long agree with those of race.

We may if we like picture the three 'craters' on the right of Fig. 15 arranged one above the other, Altaic below, Kentum on top. As regards No. 3, it is suggested that the resemblances of the Indian Dravidian languages to the Altaic (of Siberia) is due to their having a common origin somewhere in the region between. Two 'dead' languages may have a place in the south-west of this growth-pattern (Fig. 15 at 3). These are *Sumerian*, spoken near Bagdad in 3000 B.C., and *Mitanni*, spoken in north Syria between 1550 and 1350 B.C. Both of these, according to some authorities, have resemblances to Altaic and Dravidian. They may be near 'Proto-Aryan,' which almost certainly next developed in this same area of culture origins.

There is little doubt that the main Aryan-speakers of Europe entered the western continent (following many Alpine migrations who did not speak Aryan) long after the majority of Mediterranean and Alpine tribes (constituting the Amerinds) had migrated to the north-east and so reached America. I have elsewhere suggested that the Basque-speakers belong precisely to this 'horizon' (if I may use a parallel geological concept) between Hamitic and early Aryan. Hence on 'Crater 4' I have suggested that Basque is the sole relic in west Europe of a pre-Aryan tongue which may be equated with some forms of Amerind speech.

Still later in development, and occupying the upper position of our three concentric language craters, is the Kentum type of Aryan. It spread only a little to the north-east from our hypothetical cradle—where *Tocharese* was spoken in Chinese Turkestan (near Turfan) as late as A.D. 200. Some Hittites spoke something like a Kentum tongue (as their numerous scripts indicate) in Anatolia about 1300 B.C. Kimmerians from the Ukraine probably carried the Keltic languages westward across Europe, though perhaps their language was more like Welsh than Gaelic. Latin, the typical Kentum speech, seems to have reached Italy (via the Alps) about 1200 B.C. These data build up 'Crater 5' in Fig. 15.

To the cultural geographer the chief value of these representations of growth is that they emphasise the necessity for looking for missing kindred-groups *around the margins* of a given centre of culture. Secondly they suggest the way in which one group of languages may give rise to another. For instance, the Satem-Aryan group developed *near the cradle-land* from the Kentum-Aryan. The hypothesis also suggests that many Amerind languages will be found to have risen from a Proto-Altaic. As stated in an earlier paragraph the writer, on the whole, thinks that the key to early Aryan may be found in the Dravidian tongues of Southern India.

J. Determinism *versus* Possibilism.

During the twentieth century the trend of geography has been away from the belief of Ritter in *Providential* control, and from *Environmental* control as expounded by Ratzel towards the '*Possibilism*' concept of Vidal de la Blache and his school. The latter geographers picture any particular region as offering almost innumerable possibilities of exploitation to Man. Our material evolution, in their opinion, is essentially a matter of our own choice depending on which of the possibilities *we* choose. I have come to a different conclusion, no doubt primarily owing to my experience in pioneer countries like Australia and Canada, where the possibilities offered by Nature to Man are more meagre than in Britain or U.S.A. Indeed of these three schools, which we may label the Theocratic, the Geocratic and the 'We'-ocratic, I definitely belong to the second. However, I propose to illustrate by the correlative method first in a pioneer country like Canada, and secondly in the old-established culture-complex of Europe, that man is not really a free agent—but definitely a product of his environment.

The stage-diagram forming Fig. 16 may help to explain how this idea of ' choice of possibilities ' has arisen. It is true that in Southern Ontario we have seen man at first dependent chiefly on fur, then on lumber, then on farming, hydro-electric power, and mining. But all these in turn depend on Nature's bounty, and, given sufficient knowledge, could be predicted as the inevitable development of an expanding nation in the given environment.

In the lowest stage in Fig. 16, we see a generalised economic map about 1750, showing that fish, farms, and fur had expanded to the limits

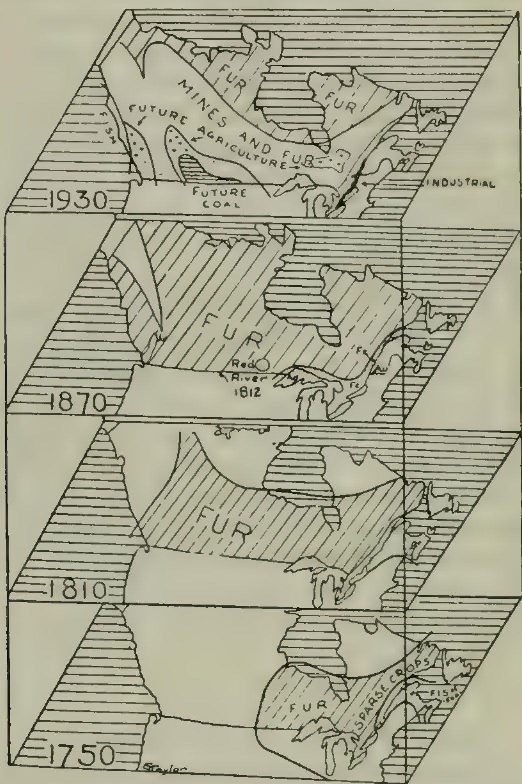


FIG. 16.—A Stage-Diagram of Industrial development in Canada from 1750 to 1930. It supports a deterministic attitude towards man's occupation of a country.

approximately there shown (Griffith Taylor 1936d). Some sixty years later, by 1810, farming had spread approximately to Detroit ; while Mackenzie was exploiting for furs the river-basin named after him. By 1870 mining was becoming of some importance, and gold (Au), silver (Ag), and iron (Fe) mines were being exploited both near the St. Lawrence and on the Fraser River. Still more important, Selkirk had, over fifty years earlier, settled his isolated band of farmers on the silts of Lake Agassiz in the heart of the continent. About 1880 the modern migration to the wheat-fields of the prairies began. In the last and uppermost stage we see in a generalised fashion the conditions to-day. The whole north of the Dominion is being exploited not only for furs but for metal mines; the latter in part by air-transport. Agriculture has covered most of the inland prairies, and will extend north (and into the Clay Belt) as indicated by the crosses. Manufactures have spread along the St. Lawrence from Montreal to Ottawa and Windsor, in large part owing to the bountiful water-power (Taylor 1936d).

But while there have been these striking advances and changes in the type of industry, man has not really been a free agent. His advance from fur-hunting to wheat-growing is only possible where rain and sun and soil are satisfactory. *All* the fur country cannot be utilised for wheat, even if man so wishes. Using a foreign example, we shall never see hydro-electric power or coalfields leading to the development of factories

FIG. 17.—The evolution of the social groups in Europe is almost wholly determined by the environmental controls of Climate, Topography and Coal; as shown in the Insets. Figures are density per sq. mile.

in that half of the southern continent known as ‘ Empty Australia,’ however much man may wish to replace the sparsest of pastoral occupation by better-paying industries. On the other hand, it seems clear to the writer that in the future the immense coal resources of Alberta must inevitably be utilised, as the more accessible coalfields are used up elsewhere. Man may very probably some day ‘ choose ’ (as the ‘ Possibilist School ’ would say) to give up ranching in the drier parts of Alberta, and turn to manufacturing based on the almost inexhaustible coal. But he is none the less controlled by his environment.

Exaggerating somewhat, I feel that Man’s part in the programme of a country’s evolution is not unlike that of a traffic policeman. He can accelerate, slow or halt the traffic, but he does not alter its *direction*. This ‘ Stop and Go Determinism ’ has no supporters among the historians, and not many even among geographers. But it expresses something of the conclusions that I have arrived at from my lengthy study of the difficult environments of Australia and Canada.

Let us turn now to Europe, where the population has approached closer to a state of equilibrium under modern conditions than anywhere else in the world. Do we find that the present groups depend on *racial* or on *national* or on *tribal* characteristics ? Only in a very minor degree. The ultimate pattern of the European population is ' Geocratic,' i.e. it is almost wholly determined by *Environmental Control*. In Fig. 17 is shown this population-pattern, and in the small insets A, B, and C, are shown the climatic and structural correlations. The sparsely settled areas in the north (A1, A2, A3) are in the realm of King Frost, who has resisted all invaders (Fig. 17, inset at A). In the south-east (B and E) are regions ruled by King Drought. Of the remaining sparse areas, F is also too dry for notable settlement, while G and G2 are Young Mountains (inset B). The remainder of Europe has a good climate and is accordingly somewhat densely populated. The densest areas of all (T, X, Y and Z) have their populations determined by King Coal, i.e. by the presence of the coal trough (Inset C), which in turn results from the geological environment of 200 million years ago.

To sum up, we can safely affirm that man's use of Nature's endowment in various countries must be based on a scientific understanding of their relative values. Systems of high protection and of autarchy run directly contrary to this ' natural ' law ; and, as usual, if Man tries to direct his industrial evolution in a way for which his environment is not suitable, he himself is the sufferer.

K. Culture in the Twentieth Century.

As in all preceding ages education is still a battleground between conservatives and liberals, or, in less polite terms, between reactionaries and iconoclasts. Too often the reverend elders in charge of a nation's education forget that their chief purpose should be to train the *young* idea rather than to protect the literature of a bygone age. It is a dangerous task to attack vested interests, and in the field of culture, classical interests are still powerful in school, college and university. Since most cultured folk receive the main part of their education in the years from fourteen to twenty-two it is vital to spend these precious eight years wisely. I do not believe that the curriculum in many of the schools in the British Empire—at any rate those with which I am familiar in Australia and Canada—is wisely chosen. It is because I feel that the social sciences, especially such topics as are discussed in this address, are far more important to the youth of this generation than almost any other branch of culture that I raise the somewhat trite issue of Classics *versus* Modernism. The youth of to-day has not time for both.

We have noted in our discussion that primitive ideas persist in marginal areas. Perhaps it should not surprise us, therefore, that in the two distant Dominions mentioned, classical teaching is given an undue place in schools and colleges ! When St. Augustine reached Britain about A.D. 600 with a mission to educate the barbarians, he first found it necessary to establish grammar schools to teach the classical grammar. Not until students knew Latin could they begin to study the mathematics, logic, music, theology and the rudimentary science of the day. Undoubtedly we are a conservative people ; so that many folk seem to think what was good enough for St. Augustine ought to be good enough for us !

This is perhaps the main reason why so many of our schools still give the lion's share of their time to the acquisition of an inadequate knowledge of the Latin language. Latin is, of course, of small importance in helping the average man to-day to pass on to other subjects which he really needs. Be it understood that the Latin and Greek languages should, in my opinion, most certainly be studied in the universities for the same reasons that we study Anglo-Saxon and other sources of our language, and to the same extent. There is no need to learn Greek to understand Greek philosophy.

Would that the classical protagonist realised the real value of the Greek education as taught by Aristotle, and would encourage its adaptation to modern times. Plato and Aristotle in 350 B.C. did not occupy the invaluable time of their students by wearisome repetition of the vocabularies

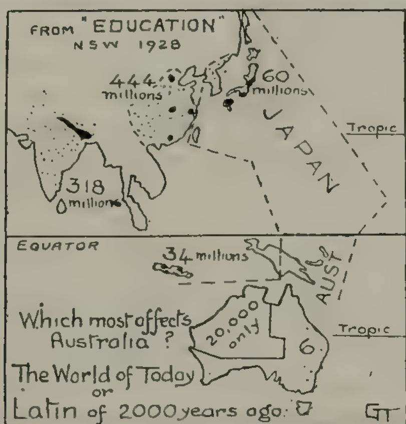


FIG. 18.—A knowledge of the fundamentals of *modern* culture is far more vital to the youth of to-day than studies which have survived since the days of St. Augustine.

of the Egyptians, from whom the Greeks derived much of their culture ; or of folk-tales written in some foreign language two thousand years before their date. *They trained youth to deal intelligently with existing conditions.* We may not all agree with the scathing words of a well-known writer (E. D. Martin), who states that culture did not mean for the Greeks 'the accumulation of dead and inconsequential knowledge, the only purpose of which was a pedantic display of erudition ' (Martin 1926) ; but we *must* suit our education to the twentieth century.

We must train our young folk to deal intelligently (i.e. scientifically) with existing conditions. It will be of interest to draw from my own teaching experience in this connection in Australia, Chicago and Canada. In the former continent we see six million Anglo-Saxons living in a hot arid continent with an environment resembling North Africa and quite unlike any portion of Europe, not to mention the British Isles (Fig. 18). Bordering the Commonwealth on the North are 300 million Indians, 400 million Chinese and 60 million Japanese. The empire of the last reaches to the borders of Australasia. Yet it is safe to say that the students in the better schools, as I knew them in Sydney, learned little or nothing of these environments and of their effect on the inhabitants ; or about the huge over-populated areas to the north menacing the Australian Commonwealth. Conditions of matriculation meant that they spent one-fifth of their time in schools studying classical languages—though only a few per cent. would ever use this subject at the university or in later life.

In Ontario I find that conditions are not much better. Here also we have a pioneer country, with the welfare of the citizens far more directly controlled by the natural environment than in older regions like Britain. Here also in my opinion we have too great a stress laid on the classics in the *general* education. The schools here as elsewhere are controlled by matriculation conditions. It is perhaps sufficient to compare some of the social sciences with classics at the University of Toronto.[4] There are twenty professors (i.e. assistant professors or higher) in classics, while the total for the five independent and newer departments of Sociology, Psychology, Anthropology, Archæology and Geography is only sixteen. The number of students in the second group is of course far greater. On the other hand at Chicago—one of the three leading universities in the States—Geography (as regards both students and staff) is on an equal footing with classics ; and throughout most of the U.S.A. the legend that culture is impossible without classics is nearly dead. I hope and believe that education in this respect is better planned in Britain than in the Dominions.

There is something very wrong with the world to-day. Our outlook on life is confused whether we are concerned with material, mental or spiritual values. As Lord Samuel has recently pointed out, *Science* in modern times can be trusted to look after material things, but philosophy and religion are still in the melting pot. In his opinion, that frontier *where science and philosophy meet*, and where the conclusions of one are handed across to be the premises of the other, should be taken as the vital centre in the wide realm of thought. To my way of thinking this explains the value of a study of cultural geography. It is a fair example of such a transfer of concepts from science to a somewhat philosophical field.

What should be the training of the educated man to-day ? If we omit the specialised knowledge he needs for his profession, then we might do worse than adopt Aristotle's idea, ' To deal in the best way possible (i.e. scientifically) with existing conditions.' Let us replace the bygone *Trivium* of grammar, rhetoric and dialectic by one in sympathy with modern aims. I like Wells' summary in this connection. ' The end and aim of all education is to teach . . . of the beginnings of life upon this lonely little planet, and how these beginnings have unfolded ; to show how man has arisen through the long ages from amidst the beasts, and the nature of the struggle God wages through him ' (Wells 1911). In effect, to make folks realise that evolution is still progressing, and that *they themselves are living factors in the process*.

Three subjects seem to be vital in the scheme of education outlined above. First *Biology*, which deals with the evolution of man as an animal ; secondly, *History* which deals largely with the growth of his ideals and institutions ; and thirdly *Geography* which deals with his present, often

[4] Professors in classics and philosophy are almost wholly appointed by the affiliated Colleges themselves.

varying, environment which is inevitably moulding himself. This to my mind is the ultimate reason why Geography can be claimed as one of the three fundamentals of modern education. I should like to see Wells' *Outline of History*, or some similar generalised study of cultural evolution (and I have tried to write two, myself!), made compulsory for all high schools. A knowledge of this aspect of culture will be far more helpful in the present world-crisis than much of the present curriculum.

The aim of civilisation, as I see it, is not to prepare for a better world *beyond* this earth, but to prepare a better world *on* this earth. Our immediate objective should be a world of peace. This can only be attained by studying world problems, especially those involving other nations and cultures. It would seem desirable to swing the attention of youth for a generation or two from the problems of physical science to the more difficult and dangerous problems of social science. There is no risk to-day, though there was in the past, in stating that the earth is a globe, revolves around the sun, and is of infinitely small importance in the Cosmos. But there is grave danger in many circles in stating the truth about Communism, Socialism, Judaism, Nordicism and many other -isms which conflict with established or dictatorial interests. These creeds are cultural facts, which can be most readily understood by a *graphical* presentation. It is no quibble to say that they are to-day more vital to the man of culture, i.e. with a well-rounded education, than is the well-recognised and valuable culture based on art, music, or classics. Thus the geographer whose interests lie not only in the economic but also in the *cultural* field can feel that he is working right on the battle-front in man's progress towards a higher type of civilisation.

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SECTION F.—ECONOMIC SCIENCE AND STATISTICS.

SCOPE AND METHOD OF ECONOMICS

ADDRESS BY

R. F. HARROD,

PRESIDENT OF THE SECTION.

In my choice of subject to-day, I fear that I have exposed myself to two serious charges: that of tedium and that of presumption. Speculations upon methodology are famous for platitude and prolixity. They offer the greatest opportunity for internecine strife; the claims of contending factions are subject to no agreed check, and a victory, even if it could be established, is thought to yield no manifest benefit to the science itself. The barrenness of methodological conclusions is often a fitting complement to the weariness entailed by the process of reaching them.

Exposed as a bore the methodologist cannot take refuge behind a cloak of modesty. On the contrary, he stands forward ready by his own claim to give advice to all and sundry, to criticise the work of others, which, whether valuable or not, at least attempts to be constructive; he sets himself up as the final interpreter of the past and dictator of future efforts.

My sense of immodesty is greatly enhanced by the occasion and place of this gathering. As economists we are singularly happy in having this meeting of the British Association in Cambridge. There is no need for me to emphasise the unique contribution which this University has made to economic studies in recent times; the great names of masters dead and living are in all our minds. And here I come, a tyro from a University, which, albeit the home of revered economists—may I be forgiven for mentioning Locke, Senior, W. F. Lloyd and Edgeworth—must in the modern period recognise its own juniority of status, and dare to lay down the law in this holy of holies. In the sphere of methodology the Cambridge economists have contributed much both by way of parenthesis in their major works and by occasional papers. I must refer also to the classic treatise on Scope and Method by Dr. John Nevile Keynes, who is still happily with us.

As a small extenuating circumstance I may mention that after taking my degree at Oxford I spent an all too brief but highly stimulating period here as the pupil of Mr. Maynard Keynes. And it is a source of particular pride and pleasure to me that on the first meeting of the Association in Cambridge thereafter I should re-visit it in this honourable capacity.

My substantial excuse for choosing methodology to-day is that I feel a strong inner urge to say something. Also the time appears to be fitting. English writers have been on the whole wisely chary of the subject; but recently there has been an outcrop of speculation upon it. There is Prof. Robbins' brilliant essay. My differences from him on certain matters of emphasis will become manifest; his effective and conclusive exposure of many popular fallacies regarding the nature and assumptions of pure theory considerably lightens my burden. Prof. Fraser has contributed some important articles, and his book on *Economic Thought and Language* lies on the borderland of methodology. Most recently we have Mrs. Wootton's jeremiad.[1] While her case against too grandiose claims for our subject is unassailable, I am confident that a circumspect statement of its achievement and utility would be proof against her shafts. Most melancholy of all I find her unappetising programme for the future development of economics.

A word of warning is in place at the outset. In view of the prospective intensification of economic studies in this country, it might be thought timely to lay down the lines or set up some finger-posts for the work which might most profitably be done. Such an attempt would indeed be presumptuous and would depart altogether from proper methodological procedure. The principles by which progress in a science proceeds can only be reached by observing that progress. They cannot be deduced *a priori* or prescribed in advance. There are no doubt certain general logical rules to which all genuine advance in knowledge is subject. The study of these constitutes logic itself. Each science or discipline has its own special limitations and conditions; its method of progress has its own special characteristics; within the wide field of logical possibilities some are selected as especially adapted to its problems; it is with this selection that methodology is concerned. And for this reason the methodologist is bound to occupy the rear and not the vanguard. He studies the specific nature of the selected principles after the selection has been made. Methods of course change from time to time; but the actual worker on special problems is more likely than the methodologist to be able to judge the best line of advance. The methodologist's contribution is more indirect.

It is when they endeavour to adopt a forward position that the methodologists are most apt to lapse into barren controversy. The historical school scolds the deductive school and the deductive school scolds back. Captions and battle-cries are devised. The 'institutionalists' appear on the scene. These rival schools endeavour to prescribe what economic method *ought* to be. The function of the methodologist is to say what it in fact is, or, more strictly, has so far been. The proper and final reply to the would-be reformer is, 'Stop talking and get on with the job; apply your method, and, if it is productive, you will be able to display your results.'

On first glance this relegation of the methodologist to the rear might seem to give public endorsement to what has all the time been the inward suspicion of the pioneer that he is an utterly useless being. But in fact by reducing his claims he at once becomes much more useful. The

[1] Cp. also Dr. Lancelot Hogben, *Political Arithmetic*, Introduction.

forward worker is inevitably influenced by methods used in the past; methods that have already achieved good results may be expected to achieve more; tools ready to hand are taken up. By going over the old ground and making a stricter survey, the methodologist may considerably modify this influence of the past upon the present. For instance, by a minute examination of assumptions he may show that there are certain limitations in principle to the productiveness of a given method and that it has in fact already yielded all the results that its assumptions allow. Or, he may show that propositions usually deemed to constitute constructive knowledge do not in fact do so, but consist essentially of definitions of the terms employed. Or, he may show that conclusions often presented as the fruits of deductive reasoning were suggested by observation of the facts and have no other support, the premises used in the pedagogic demonstration being hypotheses otherwise unsupported. These elucidations may alter the forward worker's sense of proportion and the reliance he implicitly places on certain tools. They may give him a greater understanding of the nature of past achievements and so insensibly influence him in his gropings towards fresh discovery. To do this is very different from trying to lay down the lines on which he *ought* to work.

This survey of economics is confined to what may be called its scientific aspect—namely, the formulation of general laws and maxims. Many economists are, naturally, concerned with much besides this. They are concerned with the bare description of institutions, with compiling statistics and presenting them in an informative way. Study of this sort may be regarded as contemporary economic history. It has serious methodological problems of its own, which are not considered here.

It must not be inferred that this paper is solely concerned with so-called deductive economics. Quite the contrary. Its purpose is to emphasise the limitations of deduction and the importance of observation of the facts. Facts may be observed for their own intrinsic interest, or as tending to establish or overthrow some generalisation. It is the latter type of observation that falls within this survey.

It may be of assistance at this point to sketch out certain broad conclusions which the following reasoning seeks to establish. An advance statement of this kind may make the course of the argument more easy to follow.

I propose to divide what is commonly regarded as the pure theory of traditional economics into two sharply distinguished sections. Confusion appears to me to have arisen from the failure to make this distinction. On the one hand there is the theory of value and distribution; on the other there is the maxim that productive resources should so be distributed among occupations as to yield an equi-marginal social net product.[2]

The theory of value and distribution seeks to show how a number of circumstances taken as given (the fundamental data)—namely, the preferences and capacities of individuals and the available resources—serve

[2] Cp. Prof. Pigou, *Economics of Welfare*, 1st ed., pt. ii, ch. 2, sec. 5.

to determine a structure of output and prices. If a change in these data occurs, the theory professes ability to predict the consequences within certain limits on the price-output structure. This professed ability to predict implies that we have available certain general laws concerning the succession of events, causal laws in fact. Rigid demonstrability and certainty, of an almost geometric kind, are claimed for them. Since the laws concern the succession of phenomena they must have an empirical basis; and since the phenomena of economics are notoriously highly complex and unamenable to scientific handling, it is a paradox that the laws derived from their study should have the high degree of certainty claimed for them.

The paradox is resolved when we consider that the laws in question are deducible from a single simple principle (Robbins), itself based on experience, but on an experience far wider than that vouchsafed by the study of markets and prices and extending back to the earliest phases of man's self-conscious existence—namely, the Law of Diminishing Utility or the Law of Demand, to be defined more precisely presently. The experience is so broad that the principle may be taken as an axiom of the highest possible degree of empirical probability.

But against this very high degree of probability of the principle and the laws deduced from it must be set their complementary degree of generality. The degree of generality is indeed so great that, I shall submit, the power of prediction vouchsafed by them is almost nugatory.

Next, economists, even the most theoretical, have been prone to give advice on the basis of theory. And I believe that economists would claim that much of the advice so given since Adam Smith has been valid. A type of the advice I have in mind, though this by no means covers the whole field, is the recommendation of free trade. Now it will at once occur to the critic to ask how, if it is true that the laws of value and distribution are so general that they yield but a nugatory power of prediction, can a quite copious array of advisory propositions, admittedly based on pure theory, be justified.

The reply is that these prescriptions are based on the other department of what is commonly regarded as pure theory. They are derived from the maxim that productive resources should be so distributed among occupations as to yield an equi-marginal social net product. The nature and justification of this maxim must be considered.

In order to derive from it precepts, which are applicable in the real world, certain knowledge about that world is necessary. This knowledge does not, however, relate primarily to causal sequences, nor does it consist of a bare enumeration of particular features and events. It arises rather from a simultaneous chart or survey of the economic field and the main work of the cartographer is analysis and classification. This analytical work is required *both* as a preliminary to the construction of the map and to the derivations of specific causal laws from the law of demand. I venture to submit that it is this identity of the preliminary groundwork which has tended to obscure the fundamental distinction between the set of conclusions which relate to causal sequences and involve predictory power on the one hand and the comprehensive but simultaneous conspectus of the field as a whole, on which the validity of the prescriptions depends, on the other.

I regard this division of analysis into two departments as of importance, (i) because it reconciles the fairly copious array of economic precept with the very limited power of prediction, and (ii) because only by it can the empirical grounds of our general propositions be properly sorted out. I should add that recent methodological speculation appears to attach too much importance to the part played by the general theory of value and too little to that of the equi-marginal maxim in the history of economic thought.

Recently economists have had the very proper ambition of obtaining greater knowledge of causal sequences than is vouchsafed by deductions from the Law of Demand. The phenomena of the Trade Cycle have been a special stimulus in this direction. But once they leave the plane of high generality which pertains to those deductions, their generalisations are likely to have a much lower degree of probability. All the difficulties associated with the complex and unamenable nature of the phenomena, which they have to study, come to the surface. They must say goodbye for ever to the claims to certainty which they could make, so long as they remained within the confines of their geometrical system. From being one of the most exact, albeit narrowly circumscribed, sciences, economics of necessity becomes one of the most conjectural.

Yet the conjecture of the trained observer may be of value. In the recent period economists have already offered advice on the basis of their conjectures in this dubious field. To this department belong many of the recommendations concerning control of the trade cycle; they are based on propositions concerning causal sequences not derived from the Law of Demand: on propositions, therefore, which are to some extent conjectural. Hence the recent conflict of prescriptions, of which we have heard so much. Thus we may account for the transition from the unanimity of advice, common in the last century, of which free trade is a good instance, to present-day disagreements. The former was based on the analytical map, making no claim to causal knowledge; the latter is based on the necessarily conjectural propositions of cycle theory, which must make such a claim and are conjectural precisely because they entail such a claim.

But the new realm of conjecture, though it may drive out the old knowledge from its position of central interest in the economist's mind, does not invalidate that knowledge. It will be a thousand pities if the conflicting nature of prescriptions of the new type, which economists are right to give, albeit without claim to certainty, since they must give of their best, undermines the authority of the advice given on the basis of the analytical map.

I now proceed to a more detailed examination. What remains is divided into four parts. The first I call the economic criterion, which deals with the nature and authority of the prescriptions given on the basis of the analytical map. The second is the theory of value and distribution, which considers the scope and validity of the causal knowledge derived from the Law of Demand. There remain the recent strivings after causal knowledge outside that ambit. Within this field I carve out a section named dynamic theory for reasons which will be explained. The residual section I call empirical study. This must not be taken to imply that the knowledge considered in the earlier sections is not based on experience. I expect the studies falling under this fourth head to be the most important in the future ; but owing to my rearguard position I shall not be able to say much about them. I hope that appreciation of the necessary limitations to the scope of the other types of knowledge may serve to stimulate the new empirical work.

I.

The Economic Criterion.

The train of thought here to be considered is derived from Adam Smith. His chief claim to fame consists in his origination of it, his work on this topic having far greater cogency and authority than his particular formulation of the labour theory of value or his speculations on the forces determining wages, profit and rent. Furthermore I conceive it to be the central core of classical economics, entitled to an easy priority over the theory of value and distribution to which more recent writers, by reason of the growing precision of its formulation, have tended to give pride of place.

The contribution of this department of theory must be considered under two heads : (1) the choice of the criterion itself ; (2) the mechanism for testing how far existing or proposed arrangements and practices fulfil its requirements.

The criterion may be defined dogmatically as follows : If an individual prefers a commodity or service X to Y, it is economically better that he should have it. Similarly, if the individual prefers work X to Y, or dislikes it less, it is economically better that he should do it. The economic good is thus the preferred. If we may adopt Prof. Robbins' method of regarding the inner structure of thought rather than the verbal formulation of it, this choice of a criterion may be attributed to Adam Smith.

The act of choice cannot be regarded either as a discovery or a hypothesis, though it partakes to some extent in the nature of each. He perceived that by means of it, it would be possible to make sense of the confused and conflicting arguments of economic doctors and reduce chaos to order. This choice involved scientific insight of a high order. Its merits may be judged by its fruits.

In appraising institutions and practices and making recommendations the economist has this criterion in mind ; it constitutes his standard of good and bad.

Zealous protagonists for the scientific character of economics have been disposed, especially recently, to define the advisory capacity of the economist somewhat differently. Realising that in fully developed sciences, laws of causation have primacy of position and practical maxims issue as corollaries from them, they have been unwisely eager to assimilate economics to this category. Consequently they have suggested that the economist in his advisory capacity should state that a given interference will lead to certain consequences X, Y, Z . . . and then remain silent, leaving his client to decide whether X, Y, Z . . . is a state of affairs which he wishes to bring about. This formulation is in manifest conflict with the actual practice of economists. If the methodologist urges that this *ought* to be their actual practice, he trespasses beyond his proper province, which has already been defined. Also this formulation claims both too much and too little.

It claims too much because it gives an exaggerated idea of the economist's power of prediction at the present juncture. It claims too little because it entails that his advisory power is confined within the narrow limits of his predictory power. Moreover it would make him present his information in a form in which it would be of no use to his client.

Suppose, for instance, an import duty on what is under consideration. He may feel confident that this will cause the price of wheat and wheaten bread within the country to be higher than it would otherwise be. He knows also that the duty will have effects on the prices of other commodities, on the incomes of various classes, on the foreign exchanges and the circulation of money. But he cannot put these effects into quantitative terms and in some cases he may not know the direction of the consequential movements. To do so he would have to have much more detailed causal laws at his disposal than there is any immediate prospect of his having.

But even if he could know all these things, his advice would still be in a form of little use to his client. Having heard all the prospective charges, the client will want to know whether the last state of affairs is in sum better or worse than the first and will be unable by his unaided intelligence to decide.

By resorting to his analytical map, presently to be described, the economist may be able to come by a short cut to the required answer. He may be able to say outright and with substantial authority that on the whole the individuals of the community will be in a worse position, even although his power of predicting the actual course of prices and incomes is negligible. Any definition of the economist's advisory scope, which does not recognise this, is unrealistic and fails to do justice to the usefulness of the economist even with his present limited powers.

Strictures upon the economist's proneness to give advice come also from another quarter—namely, politicians or moral philosophers. What right, they say, has the economist to lay down that such and such ought to be done, since this depends in part upon the ends sought ? Surel the economist must wait until the ends are furnished to him by the politician. This criticism is not valid.

The economist is entitled to his criterion of individual preference. The politician may then say to him, ' I am not so much interested in individuals getting what they prefer, as in the country being self-sufficient. What I want to know is how to achieve this.' But there are an infinite number of ways of achieving it. Which shall the economist prescribe ? The politician may add : ' Oh, well, I want to do it in the most economical way.' The economist then interprets this as meaning that subject to the overriding condition of self-sufficiency, individuals are to get what they prefer. Without his own criterion he cannot choose among the infinite variety of possibilities. Thus he has to employ it, even when a specific end is furnished to him.[3]

He uses his criterion both to give advice *simpliciter* and to give it subject to an overriding end furnished to him. If it were true that there is a latent ethical or political bias when he gives advice *simpliciter*, it would be equally true when he advises on the means to achieve an end laid down by moralists or politicians. Without his own criterion, he is entirely stultified. With it, he can give advice of precisely equal validity and freedom from ethical bias whether a specific end is furnished to him or not.

We proceed to our second head within this field of thought : the mechanism for testing whether the requirements of the criterion are fulfilled. Here again our main debt is to Adam Smith. He perceived that the complex phenomena of markets and prices might be regarded as the result of the efforts of individuals to inform each other of their preferences. This is the basis of the analytical map. He correctly maintained that economic study arises from the fact of division of labour. Robinson Crusoe directs his energies in relation to his own standard of preferences ; he needs no outside advice. He may indeed misdirect his efforts from ignorance of agriculture or engineering ; in this the technicians in these subjects can alone correct him ; the economist has no place. The need for the economist arises from the division in person between the producer and the consumer.

Economists have constructed a map or model in which individuals are seen informing each other of their preferences. (It may help the reader to regard this map as ' the theory of perfect competition,' provided that all reference to the sequence of events is excluded from that ' theory.') In order to construct the map in a way which corresponds with the observed phenomena of the real world, certain important analytical work was necessary. The relevant propositions may be stated in the form of truisms or tautologies, such as that the price of an article is equal to the sum of rewards to all persons contributing to its production, or again, if services of the same type get equal rewards in different occupations, the prices of commodities will be proportional to the quantity of services required for their production.[4] The intellectual intuition behind these formulations is primarily one of classification. Indeed, it may be said that the major part of traditional economic theory consists of classification. Classification is a highly respectable scientific activity of which economists have no need to be ashamed. By referring more to it and less to so-called ' laws ' their claim to scientific status, albeit more modest, would be less suspected.

[3] The position may be more complex. The economist may be asked to provide not for absolute self-sufficiency but for a higher degree of it than obtained before. He will then be able to lay down the conditions for the attainment of the greatest amount of economic advantage in connection with any given degree of self-sufficiency, and he may be able to give some idea of the successive rates of economic sacrifice involved in the attainment of successively higher degrees of self-sufficiency.

[4] More strictly, the prices of commodities will be the sums of parts *a*, *b*, *c* . . . charged in respect of services A, B, C . . ., the value of each of which parts will be proportional to the quantity of the corresponding service used.

The map is to some extent hypothetical. It supposes that various activities may be interpreted as notifications of preferences. On the other hand it is drawn with reference to the facts of the situation, assuming, if appropriate, such matters as private property, private ownership of land, unequal division of wealth, even special types of banking institution, company organisation, etc., and traces how the mutual notification, which it supposes to be intended, operates in these conditions.

Two points may be noted. (1) By means of the map we are enabled to get a view of the economic field as a whole. This is necessary for prescription. A particular piece of legislation may be well designed to secure its specific object. All reasonable men will wish to know, and it is the economist's task to say, how this fits in with the larger purpose, for which the whole economic mechanism is designed. To what extent does the specific objective militate against or further the more general purpose?[5] This can be studied by reference to the analytical conspectus. (2) Our right to interpret observed phenomena as constituting the mutual expression of preferences depends in the last analysis on introspection. An observant visitor from Mars who knew nothing of the nature of desire, purpose and will, might well be unable to make this necessary link; he could become expert in the knowledge of causal sequences, but for lack of the necessary interpretation would be unable to give advice on the basis of the conspectus.[6]

The map is related to the criterion of preference by this principle, that the more effective the system of mutual notification attained, the more fully are preferences likely to be realised. Reference may be made to the example of an import duty on wheat. We may know enough of the existing organisation of markets to be sure that this will impose an obstruction to effective mutual notification. We infer that in the presence of this obstruction preferences are less likely to be secured. The validity of this inference depends upon the correctitude of our interpretation of existing market processes. It is independent of knowledge how individuals will react to the obstruction,[7] namely, the consequent course of prices, wages, etc., which we should have to know if we were required to give a full statement of consequences before prescribing, but which we only could know if our causal knowledge were fuller than it is.

How far the facts of real life correspond to those envisaged in the map is a matter of observation and it should be subjected to continuous check. Economists of the past were perhaps too hasty in assuming exact correspondence. On the basis of the assumption and the criterion that the

[5] If I interpret him aright, this account is in accordance with the view expressed by Prof. Robbins in his section on 'rationality' in the concluding section of the *Nature and Significance of Economic Science*. Cp. also Prof. G. Cassel, *Fundamental Thoughts on Economics*, p. 14.

[6] This is in principle the position to which Prof. Cassel would reduce economists by extruding all reference to utility from economics. Cp. *Fundamental Thoughts on Economics*, pp. 66–70. In another place, however, he recognises the fundamental part played by the notion of need, which is only another word for utility, cp. *Theory of Social Economy*, vol. i, pp. 8–9 (tr. McCabe).

[7] In exceptional cases the precise nature of this reaction might be relevant. Our map read in conjunction with our interpretation of the market should warn us if there is any probability of this.

economic objective was to achieve the preferred position the maxim of *laissez-faire* was exalted and a wealth of recommendations vouchsafed.

These may be defended at least negatively. A given interference, unless specifically designed to shape the real world to a closer approximation to the map, is likely to distort it further from it. In this case reference to the criterion makes valid condemnation possible.

More recently there has been a proper tendency to go beyond this negative attitude and to consider what interferences might be introduced to make the real world more like the map. Recommendations of this sort must be based on a vigilant observation of the actual working of real institutions (but they do not rest on causal laws or predictory powers).

In this connection reference may be made to the formulation by Prof. Pigou, already referred to, that the marginal social net product of resources in different occupations should be equal. Time forbids me to consider the definitions and classifications required to support this. It is the necessary but not sufficient condition for the fulfilment of the criterion that individuals should get what they prefer and may be regarded as a (partial) re-statement of it.

The fact that a large part of Prof. Pigou's *Economics of Welfare* consists in the appraisal of institutions and proposals in the light of his criterion is evidence that this line of thought still has vitality.

Recent theorems relating to Imperfect Competition, which in my own mind at least have a direct intellectual connection with Prof. Pigou's consideration of Increasing Returns in the light of his criterion, appear to have their principal value, not in the realm of causal laws or prediction, but as an endeavour to show in an orderly and systematic way how real markets are distorted by comparison with those of the map.

In spite of these interesting developments I feel that there is a danger that this part of economic speculation, the field of its most signal triumphs in the past, may suffer an undeserved neglect, whether owing to the economist's absorption in rival interests or to his discouragement at the overthrow of free trade. A mistaken methodological ban on advice-giving might also contribute something.

The widespread growth of government interference makes this function more and not less important. Officially sponsored rationalisation schemes, arrangements for the semi-public operation of services, public policy with regard to road and rail transport, marketing board arrangements all require vigilant scrutiny in the light of the criterion, to say nothing of more full-blooded socialist programmes. Even if public policy appears to violate the advice which the economist would give *simpliciter*, this is no excuse for him not to take an interest in the fulfilment of his criterion subject to the overriding demands of policy. He may think that there is no case for giving agriculture special protection ; in the face of the opposite policy he has scope enough to criticise the arrangements introduced to give effect to it. If he loses interest in this field of thought, the country is only too likely to get tied up with red tape and be subject to vast avoidable wastage.

One further topic remains for consideration in this section.

The preference criterion which forms the basis of the kind of investigation here considered was stated in a form not involving the comparison of the claims of different individuals with one another. The preferences notified in the model market are of the form that a given individual prefers an *n*th unit of X to an *m*th of Y. The need of one individual is not compared with that of another.

Yet one is tempted to make such comparisons. For example, Marshall says in the *Principles* that the marginal utility of twopence is greater in the case of a poorer man than in that of a richer. If such comparisons are allowed, recommendations for a more even distribution of income seem to follow logically. They give scope for a wide range of recommendations not sponsored by our original criterion.

Objection to this enlargement of the field of prescription may be based on two grounds.

(i) It may be urged that the economist hereby goes outside his proper 'scientific' field. This point is strongly urged by Prof. Robbins. Whether the *n*th unit of X has greater or less utility than the *m*th of Y to a given individual may be made the subject of test. He can be given the choice. But there are no 'scientific' means of deciding whether the *n*th of X has greater or less utility to individual P than the *m*th of Y has to another individual Q. The choice can never be put. This implies that we cannot decide whether two pence have more utility to a millionaire or a beggar. Yet we may have a shrewd suspicion. But this, we are told, is 'unscientific,' for lack of a test. This objection would be very weighty if economics itself were a mature and exact science. Yet in fact its achievements outside a limited field are so beset on every side by matters which only admit of conjecture that it is possibly rather ridiculous for an economist to take such a high line. πεπαιδευμένου γὰρ ἐστιν ἐπὶ τοσοῦτον το ἀκριβὲς ἐπιζητεῖν καθ' ἕκαστον γένος, ἐφ' ὅσον ἡ τοῦ πράγματος φύσις ἐπιδέχεται.[8] Can we afford to reject this very clear finding of common sense ? Of course great caution must be exercised in not pushing the matter too far. Since the evidence is vague, we must not go further than a very clear mandate from common sense allows.

It is not altogether certain that the gulf between the prescriptions of the classical economists and those of, shall I call them, the welfare school is as great as Prof. Robbins implies. There is no doubt that the marginal utility of twopence to a given man at a given time and in given other circumstances is less if he has £1,000,000 a year than if he has £25 a year, since he will spend the £25 on things which he prefers per 1*d*. of cost to the things on which he would spend the remaining £999,975. The further postulate that the twopence has lower utility to a millionaire than to a £25 per annum man is based on some sort of assumption about the equality of men in regard to their needs, which must not be pressed too far. But so also do the prescriptions favourable to free markets. For the individuals who gain by the opening of a market are often different from those who suffer some loss. Consider the Repeal of the Corn Laws. This tended to reduce the value of a specific factor of production, land. It can no doubt be shown that the gain to the community as a whole exceeded

[8] Aristotle, *Ethica Nicomachea,* 1094b. 'For an educated person should expect to obtain precision in each branch of study to the extent which its nature permits.'

the loss to the landlords—but only if individuals are treated in some sense as equal. Otherwise how can the loss to some, and that there was a loss can hardly be denied, be compared with the general gain? If the incomparability of utility to different individuals is strictly pressed, not only are the prescriptions of the welfare school ruled out, but all prescriptions whatever. The economist as an adviser is completely stultified, and, unless his speculations be regarded as of paramount æsthetic value, he had better be suppressed completely. No; some sort of postulate of equality has to be assumed. But it should be carefully framed and used with great caution, always subject to the proviso 'unless the contrary can be shown.' In the case of the free market arguments there is usually no characteristic attaching peculiarly to the beneficiaries of restriction other than that they are beneficiaries. In the case of the uneven distribution of income, there are many special characteristics of the rich as a class to which due consideration must be given.

(ii) Objection may be raised on more general grounds which appear to me to have greater weight. The distribution of income is intimately connected with the balance of social and political forces, the study of which is outside the economist's province. In prescribing here he knows without being told that there are other considerations. This is not to say that he should avoid all questions with political entanglements, for then again he would be almost completely stultified. Most vested interests can whip up some political support. It is a matter of degree and sense of proportion.

It might further be urged that since redistribution is a straightforward matter widely understood, the economist might well leave it alone, since he can but reinforce in technical language an argument already before the public. Projects of redistribution, however, may have complicated ramifications which the economist is especially qualified by his other training to trace out. For instance, in his *Public Finance* Prof. Pigou has worked out with great elaboration the principles and consequences of a redistributive system of taxation. It may safely be said that this work would have been beyond the powers of any but a highly trained economist.

II.

General Theory of Value and Distribution (Static Theory).

We now enter the territory which has increasingly come to be regarded as the special domain of the economic theorist. It is here that we find the laws relating to the succession of phenomena, claiming a high degree of authority, on which prediction is based.

It is not altogether clear why this department of thought has been so greatly elevated. The trouble may have begun with Ricardo. He wrote: 'in different stages of society, the proportions of the whole produce of the earth which will be allotted to each of these classes, under the names of rent, profit, and wages will be essentially different . . . to determine the laws which regulate this distribution is the principal problem of Political Economy.' Why the principal problem? We are not told.

The method of procedure is to take certain elements in the situation as given—namely, the preference lists of individuals for goods and services, the terms on which they are willing to contribute their assistance in production and the current state of technology; and to take other elements as unknowns—namely, the prices of all commodities and of factors of production, the amounts of commodities which will be produced and of factors which will be employed, and the precise methods of production among the variety of these technically possible which will be used. If the elements taken as known were in fact known, it would be possible to write down a number of equations expressing some of the unknowns as functions of the others. The object of this procedure would be to provide means of showing how changes in the fundamental data, desires, etc., will govern the course of events.

I regard the most notable intellectual achievement in this department to be the classification of factors of production required as preliminary to the formulation of the equations. (This classification has also proved of great service in elaborating the analytical map already considered.) There is the analysis of the contribution of capital to production as consisting essentially of waiting. There is all the work concerning the relation between direct and overhead costs. The so-called law of rent has given rise to a number of dichotomies of great interest. The concept of profit as a reward for skill and judgment has been rendered fairly precise. Prof. Knight has shed a penetrating light upon the relation of profit to uncertainty-bearing, but some puzzles here remain. Meanwhile Mr. Keynes has produced another concept, liquidity-sacrifice, which bids fair to find a place as an independent factor; it needs further elaboration, and its relation to the general concept of uncertainty-bearing requires precise definition.

These concepts are then applied and their values are expressed as unknown quantities in a number of forms of functional equations. These relate to the demands for commodities considered as functions of the prices of commodities, the quantities of factors used to produce commodities considered as functions of the prices of factors, and the quantities of factors on offer considered as functions of their prices. Satisfaction is expressed if there are as many forms of equations as there are unknown quantities.

But we run at once into this difficulty that the matters taken as known for the sake of argument are in fact not known. We may write down that the quantity of a commodity demanded depends on its price and on the prices of other commodities. But this does not take us far unless we know the precise law of dependence. We can only say that there should be an equation here, and if it could be written out along with a number of other equations, we should be able to determine the value of the unknowns and the effect of any specified change upon them. But in fact we have not got these equations, but only a number of blank forms, which are nothing more than aspirations to have such equations!

If this were the end of the matter, this department of theory would yield no causal laws and no power to predict whatever. The situation is not quite so bad. It is at this point that the Law of Demand is brought into play. With its aid we are able to say something about the demand equations. We say that they will have this in common, that the quantity of a commodity demanded will be less the higher its price.[9] We are still unable to formulate the demand equations precisely, but we have this very general piece of knowledge about their structure. Having regard to it and also assuming that the other equations relating to supply and productive methods *are not of a very odd structure*,[10] limited powers of prediction with regard to the direction, though not the quantitative value of changes consequent upon a change in fundamental data, are rendered possible.

How do we come by this law of demand? Here we are certainly at the very centre of traditional economic theory. I do not believe this to be based on an observation of markets in the ordinary sense. There the confusing influence of many forces is operative, and though scatter diagrams may give a faint suggestion of the law, we hold it with much more feeling of assurance than they would vouchsafe.

Consider the Law of Diminishing Utility. Is this based on some psycho-physiological principle, the diminishing reaction to stimuli? Is the main constructive part of our theory based on a generalisation borrowed from elsewhere, the verification of which depends on the observations of others? I do not think so. I believe the matter to be simpler.

It appears to me that we have here an *a priori* axiom, albeit based in an indirect way on observation. In markets we are concerned with commodities divisible into parts. The parts are homogeneous in one respect, namely in all their sensible properties, so as to be perfectly substitutable one for another, but heterogeneous in another respect, namely the use to which they may be put. The parts may be used separately. Each occasion of their use has its own importance. Not each occasion is likely to have precisely the same importance, save in an exceptional case. This is all that is required for the law of diminishing utility. If supply is restricted, use will be confined to the most important occasions. This appears more general than, and independent of, the law of diminishing reaction to stimuli. The axiom arises directly out of homogeneity in one respect and heterogeneity in another. That homogeneity and heterogeneity thus reside together in exchangeable objects is of course known by observation, ultimately by introspection and the assumption that other selves exist and have similar states of consciousness to our own. The existence of the law explains how it is possible to make prediction on the basis of equations, which themselves seem and claim to be independent of detailed economic investigation.

With the aid of the general law of demand we are able to predict some immediate consequences of changes in fundamental data. But we cannot

[9] Even to this there may be exceptions. Cf. Marshall, *Principles of Economics* (8th ed.), p. 132.

[10] It is possible that the crucial point in the argument by which Mr. Keynes throws doubt on the consequences usually supposed to flow from certain changes, on the basis of the theory of value, is his demonstration that the real supply schedules of the prime factors are, owing to actual offer terms being expressed in money, precisely of the odd structure required to invalidate the reasoning.

go far. In the absence of more precise quantitative knowledge we soon run into alternative possibilities.

This being so, the next step would appear to be to obtain more precise knowledge. This must come from empirical investigation. But when we leave the sure ground of the law of demand in its general form, we are at once confronted with the appalling problems which the shift and change in the economic scene with its plurality of causes and unamenability to experiment present. Heroic attempts have been made by such workers as Dr. Schultz to obtain quantitative laws of demand, and Prof. Douglas has made assaults on other parts of the structure of equations. Interesting results have been obtained and more are to be expected.

If this is really the heart and centre of economic science, all our resources should be put at the disposal of such investigations. But is it ? We come back to the *obiter dictum* of Ricardo. Can it be justified ?

It may be hazarded that there has been some concentration on the development of this part of pure theory, precisely because to a certain point it was possible to proceed by way of deduction from our demand axiom. But when we proceed beyond this point it is necessary to make hypotheses about alternative possibilities, and, although with the aid of mathematical tools elaborate chains of deduction may be forged, the basis remains hypothetical. It does not seem probable that the predictory power in the theory of value can be enlarged, save by such empirical observations as make it possible to fill in the blank-forms of equations with quantitative data.

This may be done. It should be noted that the results obtained will at best not have a very high degree of probability. Yet it must be said that if real equations could be substituted for the present empty forms, even if the former were conjectural and hazardous in the extreme, economics would be on its way to looking much more like a mature science than it does at present. Only by abandoning the theological claim to certainty and explicitly allowing a wide margin of error can economics rebut the charge of scholasticism and claim scientific status.

To sum up. The adoption of individual preference as the criterion for testing arrangements has proved convenient for getting a systematic ordering of thought. Incompletely but validly formulated as the principle that the marginal social net product of productive resources should be equal, it may be used to test existing arrangements or proposals. A map may be constructed, resembling our economic system, in which individuals notify each other of their preferences. Interferences may be condemned for not taking account of this map. Alternatively interferences may be recommended designed to make our economic system resemble the map more closely. Both kinds of advice spring from and are dependent on a vigilant observation of the actual working of our system. It is highly important that this part of the economist's function should not fall into desuetude.

The causal laws of static theory are deducible from the law of demand. This is well based on a very wide experience ; it is in no need of verification ; further attempts to verify it could not add to the assurance with which we already hold it. But the laws are of a very general form and little prediction can be based upon them, nor are they the source of the recommendations of traditional economics. More specific laws would have to be based on detailed empirical research and would be highly conjectural. While great interest attaches to such empirical work, it is not clear that this should be the main avenue for future developments; but, if it is not to be, then the general theory of value must itself be displaced from its central position.

III.

Dynamic Economics.

There is no reason why the quest for causal laws should be limited to those propositions which may be derived from the law of demand. We may well expect future progress to lie outside that ambit.

Out of the wide field of possibilities I choose for first consideration one department, which I propose to call dynamic economics. In using this terminology I am aware that I am departing from recent usage. There has been a tendency to use the expression broadly for any set of generalisations lying outside static theory. More specifically it has been used for the study of the influence of expectations—but these may find full expression in a system of static equations—or, again, for the study of time-lags in a process of adjustment to a new static condition. These studies all have their own place.

I believe that there ought to be alongside of static theory a body of laws relating to the increase (or decline) of economic magnitudes, and that with the aid of a very few empirical generalisations, having high authority if somewhat less than the law of demand itself, it may be possible without more ado to construct such a body of laws. I conceive the analogy between the relation of dynamics to statics in mechanics and that of this branch of economics to the static theory to be much closer than that implied in recent uses of the word dynamics in economics. While the equilibrium price determined by the maintenance of a steady flow of demand and supply corresponds to a state of rest, new equations would be formulated to determine regular movements in the economic magnitudes under the influence of growth of population, savings, inventions, etc.

This line of thought is not, of course, new. The classical economists attached great importance to the alleged tendencies of rent to rise and profits to fall. Such considerations are not absent from Marshall. But generalisations of this kind have tended to recede from view owing both to their conjectural character and to the more precise formulation of static propositions in a mathematical garb. The existence of this formulation has in turn tended to lead monetary and trade cycle theorists, who are interested in change as such, to regard the phenomena of their study in terms of transitions from one static equilibrium to another. It may be that they would be greatly assisted if they could regard them as departures from or oscillations about a path of growth; but they can only do this effectively if the laws governing increase are as precisely formulated as the static laws. We need a system of fundamental equations using simplify-ing assumptions; *cf.* the frictionless surface, etc., in which rates of increase will themselves figure as unknown terms.

One reason for holding development along these lines to be needed is the unsatisfactory condition of the theory of interest in static economics. I refer now not to the results reached by Mr. Keynes in his important study of the dual nature of capital supply (waiting and liquidity-sacrifice), but to a still more fundamental difficulty.[11] Using the assumptions required for static price determination, namely persistence of tastes, technology and supply of factors unchanged, the demand for new saving at any given rate of interest is zero, since so long as the fundamental conditions and the equilibrium are maintained, the volume and method of production must be unchanged. To put the same thing in other words, the static equations determine the price of *capital* and the quantity of it which will be used. It is the quantity of capital in use which, along with the quantity of land and labour in use, remains unchanged throughout the maintenance of a given equilibrium. But if the quantity of capital in use is the same the rate of saving is zero. I have the impression that writers, other than the most careful, tend to get one dimension wrong at this point, and suppose that the 'laws of supply and demand' (static theory) may determine not the quantity of capital but the amount of saving, i.e. rate of increase in the quantity of capital at a given level.[12]

That it is possible to reach interesting conclusions on the basis of the static assumption of no saving may be seen from Mrs. Robinson's article on the 'Long Period Theory of Employment.' The paradoxical air of that essay may well be due precisely to her strict adherence to the static assumption. The fact that she quite properly compels us to consider the true effect of any change in the light of its consequences in the state of equilibrium only reached when all saving has fallen to zero, suggests that it would be expedient to tackle the problem more directly. In place of a succession of static equilibria we need the concept of motion under the influence of steadily operating forces.

[11] I regret that it is not possible within the scope of this paper to consider, from a methodological point of view, the great contributions to thought recently made by Mr. Keynes. My division into sections was necessarily guided by reference to economics as a whole, and his contribution, although internally highly coherent and constituting a unified structure, belongs in part to all my divisions, so that a full discussion would not be wholly relevant to and would unduly swell any one. See *Econometrica*, January 1937; R. F. Harrod, *Mr. Keynes and Traditional Theory*.

[12] We might imagine a static state as follows. People would save out of *earned* income in their early years and invest in life annuities such sums as would make their income rise at a rate which would make its marginal utility fall at a rate equal to the rate of interest. Meanwhile the rate of interest would be fixed at a critical level, sufficient to make them hand on their *inherited* capital intact, despite their inferior regard for their heirs. These conditions would, on the assumption of a stationary age distribution, make saving equal to zero. If their regard for their heirs happened to be as great as their regard for themselves, then, with a positive rate of interest and supposing the state of bliss described by Ramsey in his well-known article not to be reached, there would be some positive saving, and the assumptions of the static theory would be mutually inconsistent. Similarly a socialist state in conditions otherwise static should arrange for positive saving.

The laws will govern the relation between and determine the mutual consistency of the rates of increase of various magnitudes, e.g. working population, technical powers, quantity of capital, of circulating medium, etc. Some empirical foundation is necessary. Bare study of mutual implications will not yield much, since there is an infinite variety of possibilities. But I have the impression that a few basic empirical laws, of a generality not much inferior to that of the law of demand in statics, may yield, in connection with the study of mutual implications, an elaborate structure of deductive theory.

An example of a basic empirical generalisation may be found in the proposition put forward by Mr. Keynes in his recent work, that at a given rate of interest people will save a larger absolute amount from a larger income. We could get still further if we could establish, but this is perhaps too audacious for the early stages, that people save a larger proportion of a larger income. Both these propositions are clearly open to empirical verification. They will be subject to *ceteris paribus* clauses regarding the distribution of income and institutional arrangements, but these would probably not impair their high scientific utility. The statistical work of verification required is no doubt substantial, but light compared with that required to fill in the blank forms of the static theory equations. The phenomena are much more amenable to the attainment of reliable results in this field than in that of static supply and demand schedules. The *de facto* growth of society assists the former while it hinders the latter type of statistical enquiry.

May I be excused for touching on a theory in which I believe, subject of course to the eroding researches of historians of thought, that I have certain proprietary rights? If it is true that the most important factor governing the demand for new capital is the rate of growth of the system, and the most important factor governing its supply is the absolute size of the system, then, having regard to the truism that demand must be equal to the supply, a host of interesting conclusions should follow. Premises containing these peculiar mathematical relations should surely be a gift, precious beyond compare, to economists of mathematical bent seeking new conclusions. I risk saying that if, when trade cycle theory comes to be established on firm and agreed foundations, these relations are not judged to have central causal significance, I shall be dumbfounded.

IV.

Empirical Studies.

I now come to the most difficult, the most tentative, and withal the most important section, the search for causal laws outside the realm of deductions from the law of demand or the simple laws of growth.

Having previously tended to belittle the causal significance of the theory of value and distribution, I should like to pay tribute to the high importance of the work of classification, not achieved without much toil and the insight of genius, which is the groundwork of that theory as well as of the analytical map. This is likely to prove a valuable and indeed indispensable tool for further investigation, and the empiricist, however radical, is likely to flounder if he is unable to use it. In the classificatory work I include truisms like the quantity theory of money, and the wages fund theory, which serve to give precision to the concepts.

How shall I proceed into this unmapped territory? At this stage there should be no dispute on matters of principle. On the one hand, for every proposition purporting to relate to the succession of events it must be possible to point to the empirical evidence. Any attempt to assume superior airs may be met with the rejoinder that if empirical evidence is lacking, the proposition can be no more than a definition of the terms which it employs. On the other hand, attention must be paid to the mutual consistency of generalisations and each one must be valued according to the extent to which it contributes to making the whole system more coherent.

One might draw up a methodological classification by reference to how the investigator spends his day. There is armchair cogitation; there is the application of statistical technique to the great body of statistical raw material already available, which may well require an elaborate apparatus and assistant workers; there is the compilation of fresh statistical material by work in the field; there is also the field work directed to gaining a closer knowledge of how institutions actually work and the motives which govern behaviour. It may safely be said that all these kinds of activity have utility; they may be regarded as 'factors' in the production of economic truth to be mixed in due proportions in accordance with the general principles of production; what is a due proportion depends in part upon the abilities and temperaments of the workers available. I will only add that the institutional arrangement whereby most professional economists are heavily burdened with teaching and administrative duties may militate against a sufficient admixture of the more laborious forms of statistical and field work. The remedy for this, now already in process of application, is the endowment of full-time workers of the right temperament and the provision of adequate laboratory equipment and skilled assistants. It may be noticed with satisfaction also that statistical method, on which economic advance depends, has recently displayed a great vitality under the influence of such distinguished pioneers as Dr. Ragnar Frisch.

There is, however, a more fundamental difference between the outlook of the more and the less empirically minded. This consists of a difference of judgment as to the most hopeful source of clues for the future development of the subject. On the one hand there are those—I believe that it is fair so to represent the view of Prof. Wesley Mitchell—who believe that clues are most likely to be obtained by the diligent scrutiny, arrangement and rearrangement of the empirical data. The facts will one day speak for themselves. By patient and continuous observation the investigator will find the appropriate generalisation borne in upon him. On the other hand, some believe that clues are more likely to be found by an inspection of the existing body of theory. Close examination of it will reveal gaps, and in those very gaps may be found clues suggesting new generalisations which will render the theory more coherent, or even wider generalisations leading to a revolution of the kind which occurs from time to time in physics. Or, more moderately, they may lay some stress on observation, but urge that this should be done very much in the light of existing theory, to test hypotheses directly suggested by that theory.

Both schools must be given our cordial blessing. Past achievements are still too exiguous for us to be sure which is the method most naturally adapted to our study.

It is sometimes claimed that the major part of established generalisations have been reached in the less empirical way. But my feeling is that the great fruitfulness of the analytical map in yielding valid prescriptions has obscured the extreme paucity of our knowledge with regard to causal sequences. Two circumstances militate against the more deductive method. One is the impossibility of the crucial experiment. In the mature sciences which rely mainly on this method, such as physics, or, to name a more recent comer, genetics, the crucial experiment is of central importance. Secondly, it is extremely difficult to test hypotheses by the collected data of observation. The operation of the plurality of causes is too widely pervasive. Thus numerous hypotheses are framed and never submitted to decisive test, so that each man retains his own opinion still.

I do not wish to press these considerations hard, but only sufficiently to upset the complacency of dogmatic upholders of one exclusive method. To give a contrary example, I believe that in so far as the monetary explanation and the demand-for-capital-goods explanation of the trade cycle be regarded as *rival* hypotheses suggested by theoretical considerations, the course of events in this country and the United States in the last ten years enhances the probability of the latter. It should be possible to devise statistical methods to increase the cogency of this indication of experience. I assume that even the more deductive or hypothetical method of advance should be fortified by statistical verification.

It is a doubtful point whether the more radically empirical method has been as barren as is sometimes suggested. To give a rather trivial example, Gresham's Law is an instance of the facts speaking. However convincing the ex-post theoretical explanation of the phenomena, the process of discovery was by observation rather than hypothesis. A more striking example may be derived from trade cycle studies. It is an accepted generalisation, not indeed possessing the universal validity of the law of demand but none the less of substantial authority and interest, that in the upswing of production prices have a rising tendency and in the downswing a falling tendency. It may safely be said this could not be deduced from the propositions of static theory nor from that part of monetary theory which is deducible from them. Falling prices would be regarded as an equally (if not more !) likely accompaniment of rising output and *vice versa*. The generalisation is a direct result of observation, an excellent example of the facts speaking for themselves. And if theoretical explanations have subsequently been woven round it, this must not blind us to the true source of our knowledge. If rather crude observational data can yield appetising morsels of this sort, may we not legitimately hope that when subjected to refined statistical treatment they will yield more fruit in plenty? It will still be necessary to relate such generalisations to each other and to those of a more deductive origin in an orderly fashion.

Having made this plea for the more radical empiricist, I will conclude by mentioning one or two types of investigation suggested by the present condition of theory. If I make no mention of others now under way, I hope it will be understood that this is not because I regard them as unimportant, but for lack of space and because the former happen to have caught the speaker's eye first.

Emphasis has recently quite properly been placed upon the importance of expectations with regard to the future in determining the present actions of the individual, and upon the slender basis of knowledge on which he is obliged to form his expectations. Speculation upon the consequences of this may therefore be regarded as arising directly out of theoretical considerations.

Ignorance with regard to the future drives the agent back to an imperfectly rational dependence upon past experience, particularly his most recent experience. It is reasonable on this basis to make the hypothesis of a time-lag between certain adjustments. By introducing a systematic lag it is possible to give a mathematical demonstration that an oscillation of behaviour must result. The interesting survey by Dr. Tinbergen (1935) in *Econometrica* discusses a number of hypotheses of this nature.

Statistical verification may proceed from two ends. On the one hand it may be possible to verify the particular lag assumed by reference to two statistical series. On the other the cycle mathematically deducible from the assumption of such a lag may be compared as to its general features with the real cycle. One might hope that even with the data already available the determination of lags in this empirical manner might give us a theory of the trade cycle, which would be self-consistent and consistent with the broader generalisations of theory and also subject to fairly approximate empirical verification at both ends. Fortified by such tests, with what far higher degree of confidence might we call upon legislatures to take remedial measures! I may add that the framework of equations within which the lag hypothesis should be applied are those of dynamic economics. This gives another reason for wishing an early precise formulation of these.

I now pass to an entirely different type of empirical work. General considerations suggest that the entrepreneur acts under the influence of certain defined forces. When we come to examine these, it is surprising how largely the entrepreneur must be ignorant of their precise value. This is evident enough in the case of capital outlay, decisions regarding which must be based on prognostication. But even current output is properly determined by reference to the value of the loss or gain of customer goodwill and to that of 'user cost' (Keynes), both of which depend upon prognostication. And apart from the future, there are other matters of uncertainty. Correct behaviour in the field of imperfect competition, and this is the greater part of the whole field, presupposes knowledge of the value of marginal revenue, which in its turn requires knowledge of the current elasticity of demand. Yet even that magnitude of central importance, which theorists are apt so glibly to take as given, is one about which many entrepreneurs are quite in the dark.

Having regard to the fog of uncertainty, by which the entrepreneur is thus shrouded, it has seemed to some of us in Oxford that valuable information about how he does in fact steer his course might be gained by the method of direct question. It is desirable to obtain a wide sample and to conduct the questionnaire in such a way as to make it probable that the victim will speak his true mind. I select two lines of thought for mention.

1. Theory may assume that the change in a certain magnitude, e.g. the rate of interest, will cause a defined change in the entrepreneur's behaviour. But in fact if his margins of possible error owing to uncertainty about various factors are very wide, such a specific change, even although definitely known, may be treated by him as of too small account to affect his reckoning. The method of direct question does not seem an unreasonable one for obtaining reliable information about this.

2. The entrepreneur lives by action; even if ignorant of the relevant data, he must decide one way or another. Nor can each and every decision be reached by an independent act of judgment; some rules of thumb are necessary to the efficient conduct of a business. In the absence of data, the rules must be supplementary to those envisaged in static theory. What are they? Again this seems a suitable subject for direct question. Generalisations may be possible and valuable, even if confined to certain types of industry. For instance, an irrational but systematic and consistent treatment of overhead costs might give rise to a pattern of behaviour of significance in the trade cycle.

I believe that we may be on the eve of a great advance in economic theory, taking us right outside the ambit of the static system of equations. The wealth of statistical data, together with the indications resident in the trade cycle that the succession of events is governed by laws still undiscovered, should be a spur to the inventiveness and enthusiasm of every student to whom the ways of science make appeal. He may reasonably feel that any day he may light upon some general relation of wide validity, satisfying to the intellect and capable of yielding vast benefit to humanity. The prospect is an inspiring one.

Kindled by it, the worker who is an economist at heart will reject with contempt proposals for relegating him to the banausic work of the mere cataloguer. Nor will he be likely to wish to take up a position of polite subordination to the sociologist or anthropologist, as Mrs. Wootton has recently suggested. All honour be to those allied branches of investigation into human behaviour. I hope that I have indicated that the economist should take a broad view; he should be very much awake to the possibility of obtaining hints from and using the results of workers on the periphery of his subject. But if the status of a subject may be judged by the number and width of its general laws established on a firm foundation, then, even adopting my very modest assessment, the economist may still claim without insolence that his subject is more mature than other sociological studies. And it may be added that the wealth and precision of the data at his disposal suggest that a further advance on broad front is likely to occur in the near future. The notion that investigators in other branches of social study should be asked to help forward their lame brother economist and guide him on his proper path must, in the interest of intellectual honesty, be set down as fatuous and derisory.

To some minds it may seem that in the field of the social studies, workers who treat of human values in direct, simple and intelligible terms are the most useful members of the fraternity. But not to minds well informed of the progress of the sciences. To reach general laws it is usually necessary to abandon the straightforward terms of common sense, to become immersed for a time in mysterious symbols and computations, in technical and abstruse demonstrations, far removed from the common light of day, in order to emerge finally with a generalisation which may then be retranslated into the language of the workaday world.

Zealous humanitarians may be impatient for quick results. All men of goodwill may see without more ado that there is much amiss with the world. Should not social students postpone their abstruse intellectual problems, of fascination mainly to themselves, and get together in a sort of academic tea-party to list our known abuses and our known resources and arrive at a programme of reform on the basis of mutual goodwill? And do they not in fact, so the critic proceeds, bury themselves in unintelligible jargon, because they fear that, if they proceeded with their more immediate duties, they would disturb vested interests, incur social odium and signally fail to feather their own nests?

The criticism misconceives the duty of the student and the true source of his power for good. It may be the case that much could be put to rights without further scientific knowledge. But the sociologist will agree that if known abuses are not redressed it is not for lack of a catalogue of them or even for lack of men of goodwill. He may not be able to formulate the sociological or psychological laws by which society is held in a fatal equilibrium of internecine hostility. But his experience will lead him to suspect that the equilibrium is not likely to be shattered by the breath of an academic tea-party. Nor have academic students a monopoly of goodwill or the power to express it.

Only in one way can the academic man change the shape of things, and that is by projecting new knowledge into the arena. In goodwill he may partake in greater or less degree along with more practical persons, and he is at liberty to join with them in political parties or social welfare groups. His specific contribution is the enlargement of knowledge and particularly of the knowledge of general laws. The task of the economist is rendered arduous by the intractable nature of the phenomena which he has to study; but he is better placed than other social students, and, if he turn a deaf ear to cavillers, the past achievements of his subject and its present vitality may buoy him with a reasonable hope.

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SECTION G.—ENGINEERING.

THE CHANGING OUTLOOK OF ENGINEERING SCIENCE

ADDRESS BY

PROF. R. V. SOUTHWELL, F.R.S.

PRESIDENT OF THE SECTION.

1. By custom Section G, in conferring the high honour of its presidency, turns alternately to the practising and to the academic sides of the engineering profession. I suspect that the practical man has less trouble in preparing a presidential address, for his work has wide appeal. By contrast few are interested in teaching or in specialised research, and consulting reports of recent meetings I have not been surprised to find that academic presidents, for the most part, have either dealt with semi-political matters like the state of patent law, or given reviews of progress in particular fields of engineering.

Seeking a theme for my own address this morning, I resolved to find if possible some topic that concerns us all; and one topic I thought might usefully engage our attention, which past presidents seem to have left alone. We have had discussions of particular problems—the organisation of applied research, the training of recruits for industry; but in no year since the war have we attempted a general stock-taking—to view the trend of engineering science regarded both practically and academically, both as an art and as a field for study, teaching and research. And meanwhile all the circumstances which should influence our policy—the trend of modern physics, the attitude of industry towards the university graduate, the nation's organisation for applied research—have altered profoundly. Is not the time appropriate for an attempt to bring them under review, seeking now to foresee and plan for changes that are inevitable, rather than wait for action to be forced on us by pressure from without?

Here then is the reason for the title of my address. The outlook of engineering science is changing, as I believe, for reasons which for the most part are beyond our control; *and it is changing fast.* From day to day, absorbed in immediate duties, we may not be conscious of the change: it is not fast enough for that. But now and again (and a meeting such as this affords a convenient opportunity) we ought as I think to step back and take a wider view. The trend of engineering concerns us all, and the policy to be adopted in the face of changing circumstances. Neither can we, whose profession is teaching, afford to disregard changing conditions in industry, nor can you, because your work is practical, afford to be unconcerned in the state of engineering schools where now the men are being trained who within another quarter-century will be leaders in their profession.

I will not waste your time protesting my inadequacy for the task I set myself: it is patent, and I think it does not matter. Even if I could speak with authority, the ground is too wide to be covered in an hour's address; nor am I so much concerned now to win adherents for my views, as to provoke thought and discussion where I believe them overdue.

2. Such as they are, I shall try to present my views under three main headings: (1) our policy in regard to the teaching of engineering science; (2) our policy in regard to engineering research; and (3) 'foreign policy': our relations with the community. Throughout it will be the keynote of my argument that whatever may have been the circumstances of the past, those of to-day forbid a policy of isolation; so that whether academic or practical we must do our planning in collaboration, because ultimately our objectives are the same. It will make for brevity if I may use the words 'engineering' for the practical, 'engineering science' for the academic aspect of our profession, so making partial distinction between application (the art) and study (the principles). But the separation is artificial, and should be permitted only for temporary convenience. Our objectives are the same, and frontiers should be ignored in our discussion of common policy.

3. Engineering was defined by Thomas Tredgold as 'the art of directing the great Sources of Power in Nature for the use and convenience of man': engineering science I define, conformably, as 'science studied with a view to application'. It can trace its ancestry (I suppose) back to Archimedes or even further; for its name shows geometry to have originated in surveying—a branch of engineering science as defined just now. But notwithstanding this very respectable pedigree it was not, I think, until 1840 that our subject was admitted into the select circle of university studies, not until much later that its status was acknowledged by the award of an honours degree. Here as in other subjects wise conservatism will resist light-hearted innovation, but here a die-hard conservatism may not take shelter in long-established tradition. Our history is short, and it covers very eventful years: a policy that was right before the war may not be the best policy to-day.

Nor can we safely argue from experience gained in allied subjects of university teaching. For engineering science is not, like chemistry or physics, a separate branch of natural philosophy, but natural philosophy studied from a particular standpoint and with a special purpose. Thus the planning of instruction for our undergraduate students is a problem very different from the planning of an honours course in chemistry; because the chemist will use later, for his work in the world, the same technique that he has used in his university laboratory, whereas the engineer is being prepared for work quite different—his lectures and laboratory courses are not so much of practical value in themselves as a means of training him to think.

There is a further point of difference, in that the content of our subject is determined not only by the growth of knowledge but by the trend of practice; it includes all natural science that has been applied to the service of man. It is a commonplace that the boundaries of natural science have so extended that no man now can hope to comprehend the whole of physics, or chemistry, or any other field: specialisation has become imperative. But engineering science embraces all these fields; its boundaries extend not only continuously, as knowledge grows in tracts already surveyed, but at times by a sudden accretion of new territory—as when recently the new technology of plastics came to replace, for many purposes, older methods of fabrication in wood or metal. Thus a problem strictly speaking insoluble confronts, and will always confront, all schemes of training for industry: What should be the content of a university training? What is to be our policy in the face of this continuous accretion of knowledge, seeing that there is no corresponding increase in the capacity of undergraduates to absorb?

4. I mean to offer later some tentative answers to these questions, but now I am concerned with something more important. The time I say is past when they could be discussed as it were *in vacuo*, without regard to developments outside; and the same is true of the other main activity of engineering schools, which is original research. Policy must be dictated by circumstances, and in research our circumstances have changed most drastically since the war: first, by the trend of modern physics, which has profoundly altered the relations of pure and applied science; secondly, by a quite unprecedented growth of industrial and governmental institutions concerned with scientific experiment. We see this change of environment if we study the records in past reports of grants made by our Association to special committees charged with the study of particular problems. From 1832, when it called for a report on the state of knowledge in Hydraulics (a report which ended on the wistful note: 'It only remains for us to notice the scanty contributions of our own countrymen. While France and Germany were rapidly advancing upon the traces of Italy, England remained an inactive spectator of their progress'), through the 'sixties, when it made its invaluable contribution to electrical engineering by providing accurate standards, and up to quite recent times, the Association has done much through the agency of these special committees. But the fact that now its funds are less widely devoted to such aims need not, I think, be matter for regret. Provision exists elsewhere, and its contributions now are of different kind.

Supposing that like the fat boy I were minded to 'make your flesh creep', could I not find argument here for pessimism in regard to the future of engineering schools? As to research, I have always held that in universities it must find justification not in what is consequential—the utility of its results—but in what is intrinsic: the urge of the scientist to discover, like the urge of an artist to create, is something that will not be denied. But will the engineering laboratory continue to be essential, if more and more the trend of engineering practice is towards applications of fundamental chemistry and physics, and especially if provision for *ad hoc* experimentation continues to extend as it has in the past twenty-five years? Can we gainsay that our science is not, like chemistry and physics, a separate branch of natural philosophy, but natural philosophy studied from a particular standpoint and with a special purpose? Well then, does it not follow logically that we, as non-specialists, must look to be ousted ultimately as specialisation becomes more intense? Will there continue to be a demand for engineering graduates? Will not the demand of industry be more and more for specialists, trained in laboratories appropriate to the purer sciences?

5. I have my answers to these questions: I am not really pessimistic! But I have put the case for pessimism, being convinced that these arguments must be faced and countered now, by intelligent foresight, if they are not to accumulate uncomfortable force in years to come. They are, as I maintain, arguments that concern us all, though more immediately the concern of academic engineers like myself. That view may find acceptance or it may not, but in one field you will agree, I think, that engineers whether practising or academic must stand side by side: I mean in the field of 'public relations', of their concern with the bearing of their work on the life of the community. It is a matter which of late has greatly exercised the minds both of our Association and of thinkers and publicists in the world outside, and it too must receive attention in a conspectus of engineering such as I attempt to-day. Inevitably, as I believe, its consideration will lead us into wider and deeper issues, and for that reason I shall turn to it last in my address. Then more than ever I shall be conscious of my inadequacy to my theme; but the task must be attempted, and I take shelter behind my statement that the only purpose of this address is to provoke discussion by others better qualified.

And now, having outlined my headings, I confront the necessity of committing myself. It is no light ordeal to one who represents the smallest of our engineering schools, that he should incur the reproach of pretending to know how things should be done! I implore the indulgence of my listeners.

I.

6. No one I think will question that a dilemma confronts all teachers of engineering science: no two of us, I fancy, will agree in detail regarding the action by which it should be met. On the one hand more and more specialised knowledge finds application in engineering practice: on the other our industrialists—now, with rare exceptions, well disposed to the engineering graduate whom once their predecessors regarded with a blend of amusement and contempt—seem agreed in demanding that students shall come to them not as incipient specialists or as trained technicians, but as men who have been educated to take wide views, trained to think and qualified to negotiate and to control. Here are conflicting demands, to be reconciled as best we may in the construction of our time-tables. Inevitably they conflict, since days have not lengthened, nor is there any noticeable increase in the power of the average undergraduate to absorb.

Faced with this dilemma, different teachers propound different solutions—none claimed as wholly satisfactory or as disposing finally of a problem which inevitably will become more acute. That as I see the problem is its crux. It is not enough to be opportunist and to find a makeshift solution now, because of all sciences engineering is the least static. Unless we plan radically, though our trouble be allayed for the time it will inevitably return.

Since I claim no authority for my views, some vigour in presentation is perhaps allowable. I maintain that it is both an easy and an unsatisfactory solution that they propound, whose recipe in effect is either a lengthening or an intensification of our academic courses. The lengthening may be overt—the addition of another year to the honours course—or it may be concealed in a demand for a higher standard at entry, which would mean if adopted a more severe specialisation at school. The alternative, which I have termed intensification, is to load still further our already heavily loaded time-tables: whatever knowledge will or may be useful to the practising engineer, that knowledge must be acquired and *therefore* (for this is the essence of the argument) it must be represented by courses in our lecture lists.

It will be said that I am entitled to call this policy unsatisfactory, not to call it easy. But I am impenitent; for what I mean by easy is this facile assumption that a subject once on the lecture list will be taught and therefore learned. I am aware that at the age of twenty or thereabouts a student's power of memorisation can be almost uncanny; moreover it can be (and I am afraid often is) stimulated by intensive 'coaching'. But what concerns us now is his ability to absorb, and this I believe to be a quantity much more obstinately constant. Excepting the really first-class man (who is not the essence of our problem) I maintain that planning must be conditioned, first and foremost, by ineluctable limits to the instruction we can give *with confidence that it will really be assimilated.* It is easy, I repeat, to proceed on the assumption that lectures delivered are lectures absorbed; but the fallacy of that assumption will be shown by our third-class students in their examination scripts, by our better students when they come to attempt research.

As it seems to me, the real and difficult duty of a professor is to decide, not what subjects of instruction should be included because of value, but what can be omitted on the ground that, pushed into a mind already taxed, it will push out something still more valuable. Choice is hard, for there is so much that he would wish to include, so much that has undoubted value; yet the choice must be made. It will be made harder for him by his colleagues, though from motives of the highest. Abraham Lincoln used to tell of the farmer who said, as to wanting more land, 'I ain't greedy; I only wants what jines mine.' So every lecturer worth his salt will want, as he approaches the allotted boundary of his subject, to move that boundary just a little back, into fields he sees that are rich and fruitful. It is as though a raft were being equipped for passage on a course as yet unknown, and every lecturer were proffering stores of some different kind. All are of excellent quality, and every kind may be needed in some circumstance which can occur. Yet attempting to take all, the raft will surely founder: that is the dominating consideration, and we forget it at our peril.

Neither in a lengthening nor in an intensification of engineering courses, as I believe, shall we find more than a temporary and makeshift solution of our problem; and this for a reason that is fundamental. However long we make our terms, however full our time-tables, and however great be the capacity of our students to absorb, still we shall have failed to satisfy the demand of industrialists for men of *personality*, educated to take wide views. I hold it a profound mistake to believe (or to plan as though we believed) that all that universities can give to the young engineer is given in their engineering schools. Specialisation is easy, if the demand of industry were for specialists; but that, as I tried just now to show, would be a sorry outlook for ourselves, and we ought to be glad that in fact the demand is plainly different. Glad, but not complacent: for to meet that demand deliberately, instead of merely assuming that it will be met, we shall have to adopt a standpoint very different from what is customary in discussions of 'training for industry'. We must not lightly assume that 'first year work' can be done at school without detriment to the cultural education which industry has begun to value; or that we have done our duty by our students, when every hour of the working day is absorbed by some lecture or laboratory course, and no appreciable time is left for those divergent pursuits which we lump together under the heading of 'undergraduate activities'. In my experience it is these activities—too often forgotten in our planning—which do most to develop the qualities that are desired in our products. Their scientific and technical training must be our first concern; but seeking to fulfil this duty we must not plan as though automatically, in any odd hour that we leave vacant, our other aims will be realised.

7. I am not so foolhardy as to obtrude my personal views in detail, but in principle I will try to state them plainly, and I will outline now one possible scheme of action in this business of planning. First of all we must decide the purpose which our honours courses are meant to serve. Here my view is at least clear-cut: their purpose is to train recruits for industry, and the taking of honours in a final examination should indicate an assimilation of engineering principles adequate in a man who is starting a professional or industrial career—*but not more than this.* It may be objected that this view makes no provision for the really first-class man: I agree that it makes no special provision, but not that this is an objection; because to me, as I have said already, the first-class man does not seem the essence of the problem.

For what, after all, is this first-class ability, that it should demand an examination specially designed to detect it? Is it something that would escape detection otherwise? If you mean qualities of such value to industrialists that they should seek it even at the cost of higher salaries, then I suggest that we ought to inquire of industrialists, whether in their view these qualities can be expected to reveal themselves in a written examination. I suspect that the answer will be something of this kind: 'In examinations as they are to-day, it matters little to us whether a man has taken a first- or second-class, provided that his personality is suitable. He *must* have the requisite personality, and his knowledge of engineering principles must be real and ready—ready to be turned to the various problems that arise in our particular activities. But what we want we are as likely to find in your second class as in your first.'

If, on the other hand, when you talk of first-class ability you mean ability to do research, then I am prepared to hazard an answer of my own. Research ability reveals itself as ability to do research. Examinations are not its best detector: their proper function is to test that what has been taught has been absorbed, and research cannot be taught—or even its methods—except informally, in the course of some actual investigation.

If this is first-class ability, let it reveal itself in the only way that leaves no doubt, by research actually performed. Restricting our examinations in the way that I have suggested, we shall provide the requisite opportunity; for what the normal student can absorb only in three years the 'really first-class' student will be able to absorb in two. We shall have time to give him what he really needs, which is training specially suited to the individual.

Do not think I want my views (even assuming that they are sound) to have result in closer standardisation. As Sir Henry Tizard emphasised in his presidential address to Section L at Aberdeen, in education diversity is a sign of health. But speaking for myself alone I would say: On all counts let us shun 'harder papers' in our examinations! No one could claim that they are tests of personality, and very seldom, as I believe, are they concerned with new principles not to be covered in easier papers. (How should they be, seeing that ours is not a separate branch of science, but science studied with a special purpose?) At the worst they are founded on some special course of lectures, delivered with a view to some special paper: a vicious circle in truth! At best, too often they provide for the intending specialist a test of knowledge in mathematics, chemistry or physics which could be acquired better, and tested as well, if a more restricted examination in principles were followed by further study of those subjects in their special schools.

8. Secondly, in discussing this and consequential problems I would call industrialists into council. In the jargon of Section F, they and we are in the relation of consumers and producers; and though in the past it was our part to stimulate demand by producing something that they needed without realising the fact, we cannot now afford to disregard the consumer's point of view—as in some fields, it seems, British producers are prone to do. But I mean more than this: I mean that the time is past, or all but past, when his three years at a university and his two years of apprenticeship could be regarded as wholly distinct phases in the training of an engineer, to be planned both separately and independently. Most of us will remember, either from hearing or from reading it, the paper on 'Training for Industry' which last year, at Nottingham, Mr. Fleming and Dr. Willis Jackson (now Professor) presented to this Section. To me its most striking feature was its view of engineering training as an integrated whole, as five years devoted to a single objective. Whether or not we should agree regarding details in our planning for those five years, this I believe to be a most important principle. Much in the same way medicine (of all professions the nearest to ours, I think, in its nature and requirements) calls for university preparation followed by practical experience in the hospitals. Like medical schools we should plan, I think, with all five years in mind—not think of our responsibility as ending with the conferment of a degree.

And to industrialists, having called them into council, I would say: 'Let us seek to work out a plan whereby you may be provided with the recruits you say you want—men who with adequate knowledge of engineering principles combine some breadth of background, who by intercourse with men of other training have gained some maturity of bearing. To achieve this end it is essential, as I believe, that we forbear to regiment them too strictly in the five years we are apportioning; we must not forget the importance of leisure to the formation of personality. And here I fancy that you no less than ourselves will find your details apt to negative your principles: you too in my experience are inclined to fill the whole of every available day. But to show that we mean business we teachers now, as a first step, ask you to scrutinise our syllabuses and say from your experience whether items could be omitted either (1) as never likely to be applied in practice, or (2) as being easily and more appropriately learned in works. *We do not engage to drop a subject because you have not found it useful*: that may be an accident of your particular interests, and even though no industrialist finds it useful (speaking professionally) we must still reserve a right to teach what we believe to have educational value. But every item on your list we will undertake to scrutinise carefully—to put, so to speak, on trial; and I for my part do not doubt that thereby we shall find much that has crept into our courses more by accident than design.'

I leave the problem there, for in detail my views should be expressed in the council that I advocate, where they can be countered, rather than here as it were *ex cathedra*. Stated broadly, my thesis is our need of 'lightening ship', and here I would only emphasise that I am not advocating the exclusion from lectures of all matters excluded from a syllabus. As engineering advances, inevitably as it seems to me things that were essential tend to become rather of academic or historical interest. Concrete examples are dangerous; but I feel that forms of link motion, with which every engineer had to be familiar in days when the reciprocating steam engine had no serious rival, should be discussed now (in a non-specialised course) rather as examples in the theory of velocity and acceleration images, and ought no longer to have a place of their own in the syllabus.

II.

9. I turn to research. Other teachers will feel as I do that life would be a duller thing if teaching were all, if we ceased to have that zest for the unsolved problem, and the rarer thrill of a problem solved, that every researcher knows, though his problem be of interest to himself alone. What answer then can we make to the pessimistic forecast, that engineering research at universities is doomed to ultimate extinction, because as engineering comes to make ever fuller use of mathematics, physics and chemistry, more and more its problems will be such as only specialists in those subjects can investigate, while for *ad hoc* experimentation generous provision exists, and will increase, in government institutions and in the research departments of our larger works? Here too, as I see it, is a challenge we must face together, whether we be users or purveyors of research. Demand will react on supply, and supply on demand: unless in collaboration we shall not plan aright.

For my own part I am persuaded that here, where the case for pessimism seems at first most strong, it is most easily answered. I do not believe that departments of engineering will either cease from research activity or be merged in departments of physics or chemistry, for the reason that though engineering is not a separate branch of natural philosophy, but natural philosophy studied with a view to application, yet the attitude of the engineer to his problems is as I believe something both peculiar and worth preserving.

It will suffice to explain my meaning if I make comparisons with the mathematician and physicist, leaving others better qualified to deal in like manner with the chemists. Wherein, then, does the outlook of the engineer differ from that of the physicist? Mainly, I think, in that his problems are inexorable, and he recognises them as such. The physicist despairing of progress along a path attempted, is free to try some other: the engineer has to solve the problem as it is presented, and some solution he must have, even though it be only approximate. It has been the fashion of late to jeer at the engineer's 'factor of safety'—changing its name to 'factor of ignorance', and asserting that like charity it covers a multitude of sins. We must I think admit the criticism to be largely true as regards the past: too often factors of safety have been a refuge and an excuse rather than the extra assurance that they ought to be. But they have come down greatly of late, since aeronautics set an added value on weight-saving achieved without loss of efficiency; and the time I think is near when they will have values strictly dependent on the reliability of our materials. As 'factors of uncertainty' they will always have a *raison d'être.*

Now uncertainty of this kind does not, as I see the matter, enter into the physicist's scheme of things at all. (He has his own 'uncertainty principle'—so quaintly advanced in recent years as an argument for human free will; but I can conceive no argument for free will based upon the variability of constructional materials,—the Victorians missed no path to spiritual comfort there!) The physicist's problems are fundamental, and he is not the man to let them be complicated by additional difficulties. If corrosion is a potential source of trouble, then he will use gold if need be; if magnetic flux is calculable only for one or two particular shapes, then he will use those shapes. *Because throughout he is free to choose*; his shapes are not dictated by constructional or manufacturing requirements, nor his materials by considerations of strength or cost.

Simple illustrations are best: let us visualise the attitude to elasticity of a physicist who still retains some interest in nineteenth-century physics. He will be interested in Hooke's law, and in its interpretation as a statistical average of effects due to forces from very many atoms. He will recognise two distinct types of strain, the first involving change of dimensions without change of shape, the second change of shape without change of volume; and he will devise ingenious experiments for measuring the two relevant elastic moduli. In this connection he will study Saint-Venant's theories of torsion and of flexure, and he may even pursue the harder parts of elastic theory with the aim of eliminating errors in measurement that result from straining due to weight. But speak to him of the strength and distortion of an engine crankshaft—a matter of interest in practice, so long as engines tend to fail by torsional vibration; and if you find him interested then—well, he is an engineer in disguise! For speaking *qua* physicist he will say: 'I see that both torsion and flexure are involved—that is, both of the two fundamental types of strain; but why study these in a body of such appalling shape ?' And the engineer can only reply : 'Because I must. This shape was not evolved for its intrinsic interest, but its strained form is important none the less—and very difficult to calculate.' There you have the clash of interests : the physicist wants his problems unalloyed, the engineer is not free to choose. 'Go to the applied mathematician, thou sluggard !' is likely to be the final word.

Well, and suppose he does ? Will he find what he is seeking—a power of analysis that turned on his problem will lead to its solution ? No : he will find that mathematical analysis, developing in its own way, has come to include a very beautiful technique for solving the general equations of elasticity, but the body in question must have one of a number of shapes—among which his crankshaft is not included ! Again he is sent away empty-handed, but now for a different reason. The applied mathematician is not, as the physicist was, interested only in principles (usually—as was said by Sir Horace Lamb (1924) in writing of early elasticians—it is a relief to him when he finally arrives at his differential equations, and feels really at home) ; but he is interested in method, and *his* zest of discovery is experienced in applying new methods, let their limitations be what they must.

10. So, as I see the matter, in this and countless other problems of practical engineering—problems far too difficult for routine investigation—there will still be scope for academic engineers : they have a point of view, and it is needed. In particular they possess a sense which the modern 'high-brow' mathematical physicist at times seems almost to boast of having discarded : they can *visualise*—which is what is meant, really, by this talk of 'nineteenth-century model-making'. Hard things have been said in recent years about the Victorian physicist : one gathers that his love of 'models' was a vice which led him from the light, acquired by debasing association with engineers. '. . . When the physicist sought an explanation of phenomena his ear was straining to catch the hum of machinery'. Well, the work of nineteenth-century physicists is still, I fancy, a fairly potent argument in their defence ; and I hope that we engineers, working in fields that they explored, will avoid undue humility in our answer to these taunts. I for one am defiant—and therefore perhaps impertinent ; but as a gesture of defiance I will maintain that the tools of these mathematico-physical critics—theories of orbits, elastic solids, fluids compressible and incompressible, wave motions—were made for them by men who could visualise—'model-makers'—and are applied by them now to problems which often they do not understand or even seek to understand, relying instead on intermittent experimental verification to show that they haven't yet gone wrong ! I think it quite a sound line to follow in a fog, but I cannot see reason for so much self-congratulation.[1]

[1] So H. Jeffreys in *Nature*, April 23, 1938 (p. 718) : 'The modern quantum theories have begun by direct and successful attempts to co-ordinate what we know, without attending to the details of any deeper interpretation, and as a matter of method I think that their procedure is right. I should disagree, however, with the elevation of the rejection of unobservables into a magic philosophical principle.'

However that may be, engineering as I see it still calls for this nineteenth-century gift of visualisation, and if now mathematical analysts see fit to eschew visualisation, that is no concern of ours except as meaning that we must go our own way. I feel profoundly certain that in the engineering student who intends research a gift of visualisation must be fostered deliberately : he must develop intuitions not only in geometry plane and solid, but of membranes, gases, elastic solids, incompressible fluids. It is a gift very different from a gift for observation, because a solid may be visualised clearly which is unlike any solid that he has ever seen. So in hydrodynamics the fluid that he visualises has no colour, scent, taste, viscosity, compressibility, surface-tension : it is a fluid in his own brain, and it is unlike real fluids in this at least, that its presence there does no harm.

11. I ought not to spend more time on this heading of my thesis, yet one point I would try to make because it has been very much in my thoughts during the past three years. So far from our being always dependent on professional mathematicians, I suspect that the time is coming when we shall have methods of our own for doing most of what, hitherto, we have looked to them to do for us. Those methods will not be exact in the mathematical sense, but I think they will be none the worse for that, even philosophically speaking. For there is, as it seems to me, something wrong philosophically in an approach which envisages even the possibility of an exact solution to any actual problem. In practice data are subject to a margin of error, no less than the quantities required ; yet in theoretical work (perhaps as a bad result of the examination system) we almost invariably start as though the data had absolute certainty.

On two occasions in the past three years this Section has borne with patience my exposition of ' Relaxation Methods '—an attempt to construct a ' mathematics with a fringe '. Grateful for that indulgence, I will not weary you this morning with a recital of problems which have been attacked with success up to date. Some you will hear of later, when short papers are given by my research students in accordance with a scheme which Section G is trying as an experiment this year. I will only say that I have been astonished as well as gratified by the way in which problems regarded as difficult have yielded to the new attack.

12. You are thinking, perhaps, that I lay too much stress on theory and on calculation, that I have been talking only of the ' high-brow ' sort of study that would have been the preserve of physicists before the war. But that is how I visualise the trend of university research, considering how generous is the provision which now exists for more *ad hoc* and expensive studies. Inevitably, as I believe, there will be some shift of the focus of our interest,—schools of engineering will find problems different from those which engaged their energies a generation ago. It is not that those problems have lost importance or been solved, but that better facilities now exist elsewhere, and can be made available. When a problem can be turned over to trained men who will work on it full time, common sense suggests that it is uneconomic both of brains and money to pursue it at universities in hard-won spells of leisure from the duties of teaching and administration.

Moreover, though paradoxical is it not the fact that engineers, usually regarded as more practical than the pure physicist, are for that very reason more concerned to calculate correctly? The physicist at every stage can test his theory by experiment: in engineering, nine times out of ten, the only real check on calculation—a test to destruction—is too expensive and dangerous to contemplate. Here, I think, is the real explanation of what I have termed 'factors of uncertainty': they are needed because we can rely neither on our materials nor on our calculations, and only improved methods will enable us to reduce them.

Confessedly (for I do not claim to be propounding more than a personal point of view) I think of university research as approximating more and more closely, with the passage of time, to what in the last century was called pure physics. Avoiding mention of the living, I would say that it is in Osborne Reynolds and Ewing—yes, and Clerk Maxwell, Rayleigh, Kelvin, Heaviside in some of their manifold activities—that future professors of engineering will find the models which they should aspire to emulate. Their aim will be, not so much to make inventions in the manner of Bessemer, Parsons, Otto, Diesel, or to test the working of large prime movers (that will be done at works and in the research institutions), as to break new ground in the physics that has application to engineering—more especially near the 'border-lines' that tend always to be drawn too sharply when research is highly organised. Where controlled research has become too systematic, there they will try to be a disturbing factor; and having made their small disturbance, they will seek not to pursue the new problems themselves, but as soon as possible to turn them over to men who command greater facilities but have less freedom of choice. As I envisage the future, it is the universities who must maintain that irresponsible quality which otherwise research is in danger of losing, precisely because now it is taken so seriously, as a matter of national concern.

III.

13. So I come to my third heading—'public relations', or engineering as it concerns the community. Time is short, and here my remarks must be very brief. In any event I should not have wished to say much—conscious that I am trespassing on ground belonging to a specially appointed joint committee of the Engineering Institutions, and should be better occupied listening to its chairman Sir Clement Hindley.

Briefly, here too my thesis is that we should avoid undue humility! The times are out of joint, and having attained to command of Nature greater than the world has seen before, because man has not learned to use his mastery wisely, illogically now (as it seems to me) he inclines to question the value of that mastery, and the labours that have given it. In particular I want to record my protest against what seems to be an implication in much that is written nowadays, that because the range of engineering includes guns, battleships, aeroplanes, tanks, therefore engineers are to be regarded as a class more than others responsible for the horrors of modern war.

Here are words spoken by Sir Alfred Ewing, in a presidential address to the Association (1932) which I keep to read ever and again, for its showing of what at the best an engineer's outlook may be:

'An old exponent of applied mechanics may be forgiven if he expresses something of the disillusion with which, now standing aside, he watches the sweeping pageant of discovery and invention in which he used to take unbounded delight. It is impossible not to ask, Whither does this tremendous procession tend? What, after all, is its goal? . . .

'The cornucopia of the engineer has been shaken over all the earth, scattering everywhere an endowment of previously unpossessed and unimagined capacities and powers. Beyond question many of these gifts are benefits to man, making life fuller, wider, healthier, richer in comforts and interests and in such happiness as material things can promote. But we are acutely aware that the engineer's gifts have been and may be grievously abused. In some there is potential tragedy as well as present burden. Man was ethically unprepared for so great a bounty. . . . The command of Nature has been put into his hands before he knows how to command himself.'

Here too are words spoken somewhat earlier, in his wonderful James Forrest Lecture, 1928, on 'A Century of Inventions'. In them still more clearly, as I read them, he seems to feel as engineer a sense of special responsibility:

'I used, as a young teacher, to think that the splendid march of discovery and invention, with its penetration of the secrets of Nature, its consciousness of power, its absorbing mental interest, its unlimited possibilities of benefit, was in fact accomplishing some betterment of the character of man. . . . But the war came, and I realised the moral failure of applied mechanics. . . . We had put into the hand of civilisation a weapon far deadlier than the weapons of barbarism, and there was nothing to stay her hand. Civilisation, in fact, turned the weapon upon herself. The arts of the engineer had indeed been effectively learnt, but they had not changed man's soul. . . .

'Surely it is for the engineer as much as any man to pray for a spiritual awakening, to strive after such a growth of sanity as will prevent the gross misuse of his good gifts. For it is the engineer who, in the course of his labours to promote the comfort and convenience of man, has put into man's unchecked and careless hand a monstrous potentiality of ruin.'

To which I personally would answer: 'Yes, for the engineer as much as any man, *but no more*.' And when, in more recent pronouncements, I find the charge so glibly formulated—'It is engineers who have given men these potent weapons of destruction: on them *more than others*, then, rests the responsibility for their use'—then, admitting the premise, I protest against the deduction. I would say rather: 'On them as much as on others (but no more) rests the responsibility for their use.' Do not think that I imagine the load thus shared will be light for all. I have no illusion about the weight of responsibility—it is appalling; but I hold that we must share it equally, as citizens, not look for scapegoats when we have been free to choose either our path or leaders to direct us.

14. I can conceive no subject in which, more than this, clear thinking is wanted to-day: the desire to hand on responsibility is so deep-seated, and the will to believe that we could have had the benefits of science without its risks and its temptations. But knowledge is of good and evil: it is of its essence that we cannot know how to cure poison without knowing poison and its action, how to control and use explosives without acquiring power for harm as well as good. We may elect either to shun it or pursue, but we cannot have it both ways. Either we must choose, deliberately, impotence as preferable to the power of doing evil, or we must accept knowledge for the double-edged tool it is, vowing to use it

wisely. We may not say to the scientist, ' Keep searching, but let your discoveries be such as must benefit and cannot hurt us ' ; or pretend that the use we make of science is something outside our responsibilities as citizens, a thing imposed upon us by science itself. *Knowledge is not moral*: good and evil are its opposite sides, inseparable in its very nature. I have no quarrel (though no sympathy) with the plea we sometimes hear, for a cessation of scientific activity : it is arguable that on balance knowledge is undesirable. But when men talk of ' beneficent ' and ' destructive ' science as though we were free to pick and choose, then I say that they have not even begun to understand what science is.

Holding these views, I find it matter for regret that so often our concern with the impact of science on the life of the community, which is good and healthy, is expressed in a manner that is neither. Too often we seem to be weakly apologising for results that have followed our activities, as it were because we did not take sufficient care. Need the geneticist apologise for having increased the earth's fertility, because we have found no better use for plenty than to destroy food while thousands are in want ? Ought doctors to regret that by coming to a fuller understanding of disease they have lengthened the span of life in a world where birth rates are falling ? Here and in countless other instances, science impinging on the life of the community has set problems that will tax to the utmost its courage and intelligence ; the hardest and clearest thinking will be wanted, and it is right that engineers and scientists should seek to contribute their share. But I think that we only confuse the issue when we intervene *as specialists* in discussions which concern us really not as specialists but as members of a community.

15. Whether in these days, when all but a small minority seem convinced of the necessity of rearmament, the engineer is still regarded as scapegoat or has (for a time) been transferred to the rôle of saviour, I have no means of judging. But if any still reproach him for making what all men now seek to buy, I would answer that horror is not peculiar to modern war ; all war is horrible, both in nature and by purpose, and wars are made not by engineers but by communities. No war is righteous, though it seem so at the time ; or inevitable, except as a penalty of national sins : pride, greed and indolence ; and those more contemptible because weaker sins, vacillation of purpose, persistence in shams, clinging to safety even at the loss of honour.

More and more frequently, in lectures and in editorials, the decline of international standards is noticed with consternation and lament. Naturally, perhaps, in this country we are apt to see it mainly as an increasing tendency towards ' repudiation of law and order in favour of brute force ',[2] revealed most clearly in states that have abjured the democratic ideal. But I think that the malady is at once deeper and more general. Dare we claim that our own policy has shown no falling away from earlier belief in straight-dealing, generosity, and the sanctity of contracts ?

Increasingly, as it seems to me, nations incline to put trust in the adroitness rather than the sincerity of their statesmen. Ethics are out of fashion, and while as individuals we may still admit the moral imperative, the

[2] Vide *Nature*, May 28, 1938.

notion that motives recognisable as moral can have place in international affairs seems now to be rejected as impracticable idealism. Force and deceit, it appears, although unpleasant are held to have ' survival value ' : the gangster compels our unwilling admiration, at least in the field of world affairs. But what if there should be something in the notion, that because success in the life-struggle can come not only by individual strength but also by ability to associate and combine, morality has survival value as being (thus regarded) one of the factors which make association possible ? A bank may come to ruin not only through fraudulent or incompetent direction, but because its depositors, panic-stricken, seek each his own legitimate interests at the expense of the common weal : may not a less narrow concept of moral obligation be necessary to the continuance of our civilisation, even as wider than national horizons are necessary in the spheres of economics and finance ? Perhaps this ' idealism ' is not so impracticable after all ?

Collective security attained by higher standards of fair dealing—it is an epitome of man's progress from the cave to association in the village, in towns, and in nations, and I see no ground for believing that the notion can never transcend national barriers. Men write as though it were new—a product of post-armistice utopianism. That it is not new let these sentences, none written less than 100 years ago, bear witness (Guedalla 1931):

> ' Soyez sûr qu'en politique il n'y a rien de stable que ce qui convient aux intérêts de tout le monde ; et qu'il faut regarder un peu plus loin que soi-même.'

> ' . . . although the aggrandizement and security of the power of one's own country is the duty of every man, all nations may depend upon it that the best security for power, and for every advantage now possessed, or to be acquired, is to be found in the reduction of the power and influence of the grand disturber.'

> ' If we lose our character for truth and good faith, we shall have but little to stand upon in this country.'

> ' I would sacrifice Gwalior, or every frontier of India, ten times over, in order to preserve our credit for scrupulous good faith, and the advantages and honor we gained by the late war and the peace. . . . What brought me through many difficulties in the war, and the negociations for peace ? The British good faith, and nothing else.'

If this be utopianism, then some of our historical judgments will need revision ; for all were said or written by Arthur, Duke of Wellington—a man not lightly to be charged with saying what he did not mean.

16. You will say, now I am drifting perilously near to politics ! It is precisely the point I want to make : I say that inevitably, when instead of science we discuss its impact on the life of the community, we *must* verge on politics, because what concerns the community *is* politics, both etymologically and in fact. The old convention, that science should have no politics, seems to me sane and wise : how to preserve it if as scientists we are to concern ourselves with the life of the community, that is a question I must leave to others more subtly-minded. For myself I see no reason why as scientists we should meet to discuss anything but science. Contrary to common belief, it is not our habit to pursue science throughout the whole of every day ; and on all counts I hold it were better that we came to political discussions in hours of leisure, unlabelled, than give

support to a notion that political problems will yield to something known as 'the scientific attack'. Talk to me of the scientific approach in physics, and I shall have an idea of what you mean, though you will easily bewilder me with detail: talk to me of 'scientific approach' to problems of real life, I shall suspect you of indulgence in mere jargon.

This is not to assert that science unfits a man for political discussion: if only because by training men of science are prepared to believe that problems of urgency may yet be hard, I hold on the contrary that some scientific leaven is beneficial in almost any body of administrative humanists. It is a protest against our facile modern use of the word 'scientific' (which if it means anything connotes a special kind of approach to special problems) where 'trained common sense' is the faculty which is really needed. In science we seek to explain phenomena which we believe to be outside man's control: it is the faith in which our work is done—for if the facts were not inexorable, and could be altered at man's pleasure, how could we hope to find enduring 'laws'? But politics is concerned with action in fields where we believe that we can influence results: I see no reason to believe that the same technique will serve.

17. Rather than seek to defend our activities from the charge that evil can come of knowledge misapplied, might it not be better that we undertook a harder task, trying to instil into the mind of the public a clearer notion of the aims with which real scientific work is done? For what is that notion now, in these days of 'popular science'? At best, a picture of life lived monastically by men who care nothing for the world outside their laboratories, but spend their energies unceasingly in the quest for more and more knowledge of less and less. (Is it surprising if the public question the right of such men to leisure, seeing that by their carelessness, as it appears, forces are unleashed which may bring our civilisation to utter ruin?) At worst, an uncomprehended picture of modern 'wonders of science'—gifts which these same men have conferred upon their fellows, altruistically wresting from nature the secrets of spiritual and material benefit; so that somehow, while the astronomer fosters humility by telling the vastness of interstellar space, Heisenberg's principle of determinacy is thought to bring mystic comfort to men oppressed by the notion of all-pervading law. Equally unfounded, it is I believe the other side to that sense of responsibility for the consequences of science, about which I have spoken already; and on a more material plane it is the mainstay of the patent medicine business! For it has given us a public superficially acquainted with 'recent progress in science', yet in reality no less ignorant, and more gullible, than was the public of Victorian days.

Never have greater powers of exposition been devoted to the 'popularisation' of science: when, I wonder, shall we find like powers devoted to the harder task of a real *apologia*? To telling, not of the treasure found, but of the quest; to showing the true man of science (for it is the fact) neither as care-free *dilettante* nor as philanthropist, but seeking truth like the artist, because he must? That, I maintain, is the real spirit of science, be it pure or applied; a spirit that breathes in every book of science worth the name: to make of difficulty a stimulus, to be unwearied in determination to do good work. Is it not there, rather than on a favourable trade-balance of benefits conferred, that we who have chosen science should stand in our defence? Were it not better that the public be told plainly: This is our work, which we do because we must?

18. A lead has been given, and we may be proud that the giver was an engineer; for the gleam is seen in that noble presidential address by Sir Alfred Ewing from which I have quoted already:

> 'The quest of truth goes on endlessly, ardently, fruitfully. And yet with every grain of knowledge we realise more clearly that we can never really know. To understand, as Einstein lately said, is to draw one incomprehensible out of another incomprehensible. From time to time we discover a fresh relation between observed phenomena, but each of the things which are found to be related continues to evade our full comprehension; and that is apparently the only kind of discovery we can achieve. Our joy in the quest itself never fails; we are constantly learning that it is better to travel than to arrive.'

That I say is the spirit! Let us have the courage of the artist to exalt our calling, and while deploring the folly that has led us and other men to misuse them, let us not weakly question that the gifts of science hold potential good. Fairly regarded, the record of engineering is not such that we need feel ashamed of our calling. Again to quote Sir Henry Tizard (1938):

> 'There is nothing new in the fact that experiment and invention are transforming the habits of men and are adding to their problems. What is new is that we are all more aware of it, because the rate of change has been steadily increasing. . . . Bad news is, as a rule, better copy than good news. But can it seriously be argued that any section of society is worse off and living under worse conditions than a hundred years ago? Broadly speaking, the natural result of all scientific discovery has been greatly to improve the conditions of life and all our social relations, in spite of—or possibly even because of—the fact that scientific workers have been too busy doing their own jobs well to worry about other people's.'

So Dr. Johnson to Mr. Boswell: 'My dear friend, clear your *mind* of cant. . . . You may say, "These are bad times; it is a melancholy thing to be reserved to such times" . . . You may *talk* in this manner; it is a mode of talking in Society: but don't *think* foolishly.'

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SECTION H.—ANTHROPOLOGY.

THE ORIENT AND EUROPE

ADDRESS BY

PROF. V. G. CHILDE,

PRESIDENT OF THE SECTION.

I do not intend to inflict upon the Section an abstract discussion of archæological method. Ten years' of excavation throughout the Old World have yielded results startling enough to affect our concrete picture of human history. From this vast field I want to gather together some new facts that should mould our total synthesis. But my aim in so doing will be not to attempt in an hour an impossible reconstruction of human history. I shall rather focus attention on some new data which will permit a concrete answer to a rather abstract question. Why is a prehistorian asked to preside over a section in this Association from which historians, as such, would be *de facto* excluded ? And if a prehistorian have some title to occupy this eminence, denied to a historian, how far are British prehistorians, including my humble self, conforming to the obligations conferred by this privilege ? In a word, on what grounds can prehistory in general and British prehistory in particular claim to be a Science ?

Is prehistory experimental ? Yes, but only within very narrow limits, and in a restricted sense. I have recently described experiments to show how the puzzling phenomenon of vitrifaction may have been produced by our prehistoric forerunners. We knew the results of their activities ; we formulated hypotheses to explain how these results were attained ; we actually conducted under controlled conditions some of the suggested operations, and found that one would produce the desired result. In the same way Breuil and Coutier have demonstrated how an Acheulian hand-axe may have been made. But possibilities of this sort of experiment are very limited. Normally only one sort of experiment is open to the archæologist, an experiment that can never be repeated—I mean excavation.

Or does prehistory work ? Can it formulate general rules that serve as guides to successful action ? Yes, but again in a very limited sphere. No one who has been privileged to see Dr. Wheeler's excavations at Maiden Castle, can fail to recognise in his brilliant sections the effective application to the particular of general laws inferred from accumulated experience and from observation of the site. Prehistorians can indeed go further. In many regions the general aspects of prehistoric culture have been so far reduced to a system and pattern that we can say where a given phenomenon should be sought, and what should be found at a given site. Given the right maps, one can soon find out where in Great Britain to look for an unrecorded long barrow or hill-fort. When Anatolia was still a blank archæologically, Dr. Frankfort was able to predict in a general way what would turn up when excavation began. This sort of prediction, verifiable by experiment with the spade, clearly gives the same sort of guidance as hypotheses in natural science. But only, mark you, as to how to acquire fresh knowledge.

In the end our claim to be scientific is mainly this. We base our deductions upon solid facts—relics and monuments—which are available for all to examine and relations between them which, if no longer subsisting, have been objectively recorded with photographs and diagrams and verified by the greatest possible number of independent observers. We rely in our discussions on such substantial and public data, not upon any mystic revelations, vouchsafed once and irrevocably to a prophet or a Führer, nor upon the ambiguities of sentences spoken or written by individuals now dead. Prehistorians ' first enquire diligently into the nature of things and then proceed more slowly to hypotheses for the explanation of them.' Or to quote a leader in *Nature* last April, ' The study of man must be based on impartial and objective study of the facts, and not on forcing the facts to fit a biased and distorted dogma.'

How slender is this thread that links prehistory to natural science has been demonstrated all too glaringly by the distortion of the subject that we can recognise beyond the Channel. ' The forcing of facts to fit a biased and distorted political dogma ' has, as *Nature* showed, in certain quarters made anthropology ' a science travestied in masquerade.' The writer was referring particularly to the theory of Nordic racial superiority. He might equally have referred to lapses into scholasticism in another quarter. ' Good results in scientific research depend upon their correct orientation—upon the acquisition by the investigator of the sole truly scientific methodology—that of dialectic materialism. A merciless combat against any sort of alterations in Marxism-Leninism is a particularly urgent necessity,' ran the leader in the first number of *Sovietskaya Archeologiya*.

Let us not complaisantly exaggerate our neighbours motes ! Some of the latest German works on prehistory are just as objective as works on mathematics. The second number of *Sovietskaya Archeologiya* denounced the scholasticism of the first as sabotage and called for ' an intensive, methodical and objective study of the primary sources ' to replace dogmatic schematism. And if any archæological dogma were officially imposed by a totalitarian British State, it would I fear be more sterile than Nordicism or pseudo-Marxism. Its character can be forecast all too well from the talks broadcast just a year ago and subsequently printed in the *Listener*. That sort of farrago is what an insular bureaucracy might make canonical had it the chance ! Would the bureaucrats be alone to blame ? Do professional archæologists always keep so closely within the bounds of definite evidence ?

The prehistorian's aim is to reduce to an ordered and intelligible system the scattered and isolated splinters of evidence collected through surveys, excavations and chance discoveries. But only a few regions and short periods have as yet been so thoroughly explored and investigated that the facts of themselves make an intelligible pattern. We have to fill up the gaps with guesses and assumptions. In constructing his synthesis, said Dr. Randall MacIver at York, 'the general writer is often carried far beyond the possibilities of strictly logical proof. But so long as the author keeps his fancies and his facts distinct, he can remain perfectly scientific.' Speculations outrunning the ascertained facts are indeed necessary for the strictly scientific purpose of ascertaining fresh facts and guiding research—so long as fact is kept distinct from hypothesis. But in London Prof. Radcliffe-Browne complained that in ethnology 'generalizations are the postulates with which the subject starts, not the conclusions which it aims to attain as the result of the investigations undertaken. The procedure is often that of disciples of a cult rather than of students of science.' Can a like complaint be levelled against archæology ?

The title of my address is intended to recall an assumption which has exercised a profound formative influence on archæological studies, which is indeed held by many as an axiom above discussion. In 1899 Montelius stated this faith in the book, entitled, like my address, 'The Orient and Europe.' 'At a time when the peoples of Europe were so to speak without any civilisation whatsoever, the Orient and particularly the Euphrates region and the Nile valley were already in enjoyment of a flourishing culture. The civilisation which gradually dawned on our Continent was for long only a pale reflection of Oriental culture.'

In 1899 such a statement was very much more an affirmation of faith than a deduction from accumulated data. When our spiritual ancestors first turned for light to the East, they gazed on an uncharted plain, its limitless horizon broken only by the Oriental mirage or the dust-clad ruins of pyramids and ziggurats. In 1899 the Palace of Minos was still a mound where olives grew and Sargon of Agade still reigned placidly in the empty firmament of the fourth millennium B.C. To-day the dust stirred up by excavating spades settles to disclose a landscape no longer uncharted. Beneath the ziggurats and behind the pyramids we can descry Tel Halaf villages and Badarian cemeteries. Sargon has been dragged down from his remote pinnacle and set among mortal men a thousand years later. Exploration has left no *terram incognitam* wherein to picture the sun recuperating when forest obscured his light on the Dordogne. We can no longer plead ignorance as a pretext for treating a successful working hypothesis as an axiomatic truth. We must instead make the hypothesis explicit and scrutinise it anew in a light that is no longer mythical.

But even before we begin to apply the touchstone of experiment to it, we find that Montelius' statement is itself a complex of postulates. His hypothesis rests upon other assumptions and has given birth to corollaries, which, treated as facts, have been used to re-enforce it. These too must be first made explicit and tested by experience.

Montelius tacitly assumes diffusion. Dr. Harrison at Bristol expounded convincingly the logical justification for the general diffusionist assumption implicit in the passages I quoted. But in general terms diffusion must remain a postulate incapable of rigorous proof. That does not justify us in treating it as an axiom applicable to every special case; if we are scientists rather than devotees, each case of alleged diffusion must be examined on its merits. At York Dr. Randall MacIver reminded the Section of a rigorously objective criterion by which the opportunity for diffusion may be scientifically established. 'When the natural distribution, as known to geologists, of rocks, ores, or other natural products is artificially changed, there can be no doubt that man has been at work.' In other words, if we find in one region, in this case say Central Europe, substances naturally occurring only in another, e.g., the Mediterranean basin, intercourse between the communities inhabiting the two regions is unambiguously proved. And intercourse implies the opportunity for diffusion of ideas. Graebner and Schmidt have formulated criteria for enhancing the probability of diffusion between two regions—the number of traits common to the suspected areas and the continuity of their distribution. How far have fresh discoveries and the co-operation of petrologists and conchologists demonstrated intercourse between Europe and the Near East and enhanced the likelihood of diffusion by multiplying the traits common to both areas and filling in gaps in their distribution?

Montelius' phraseology implies another assumption that is really a corollary of the first. He is implicitly comparing Oriental cultures with contemporary cultures in illiterate Europe. But how compare cultural phases, dated by written records, with those where *ex hypothesi* no such records survive? You all know the postulate by which Montelius and his colleagues resolved the antinomy and set out to frame a general time-scale applicable alike to Europe and Asia. He notes that type-fossils, used to distinguish typological periods in Europe, recur in historical periods in Mesopotamia, Egypt and the Ægean. In accordance with his general assumption he assumes that the age of the type-fossils in the Orient provides a *terminus post quem* for dating the European periods which they serve to define. The rigorous application of this principle by the present and former occupants of this chair has had results little expected by Montelius. The neolithic period, for instance, then a hoary giant reckoned in millennia, has been squeezed to the stature of a slim Minoan youth. And yet, precisely at this moment, geologists and palæobotanists have come along with chronological systems of their own for dating in terms of solar years the earlier periods of European prehistory. These tend to enhance the antiquity of the Old Stone Age. The gap between the geological and historico-archæological record is widening. The old hiatus is becoming more distended. When Montelius wrote, the mesolithic had just been created to bridge it. To-day it will take a lot of microliths to fill the chasm!

I have unmasked a formidable conspiracy of assumptions masquerading as facts. Let us critically examine the evidence put in for their defence. Restated in simpler, but still not altogether unambiguous terms, the statement quoted from Montelius resolves itself into the following propositions, treated as axioms: (1) Civilisation in the Orient is extremely ancient; (2) Civilisation can be diffused; (3) Elements of civilisation were in fact diffused from the Orient to Europe; (4) The diffusion of historically dated Oriental types provides a basis for bringing prehistoric Europe within the framework of historical chronology; (5) Prehistoric

European cultures are poorer than contemporary Oriental cultures, i.e., civilisation is later in Europe than in the East. To-day, none of these propositions except No. 2 need be treated as 'postulates rather than as conclusions from the results of investigations.' For the excavations published during the last five years have provided abundant data by which to test the axioms' validity.

The high absolute antiquity of Oriental civilisation has been very dramatically confirmed by excavations in Mesopotamia and Syria and Anatolia. Opportunities for applying the criteria, already enumerated, to the possibility of diffusion between the Orient and Europe have been multiplied. Discoveries in Anatolia at Alişar, Alaca, Kusura, Thermi and Troy have in fact revealed long missing links between Mesopotamia and the Ægean. Heurtley's work in Macedonia, taken in conjunction with the publication of the relics from Vinča, has established the continuity of neolithic culture from the Ægean to the Danube. We no longer have to compare two remote areas separated by an ambiguous tract of unexplored territories, but can survey a continuous province over which cultural phenomena interlock from the Tigris to the Rhine. The opportunities for the diffusion, assumed in axiom 3, can be estimated in the light of the phenomena observed herein.

The validity of the chronological axiom 4 is to some extent confirmed by the enhanced likelihood of diffusion revealed by the exploration of intermediate regions and by the discovery in Mesopotamia and in Anatolia of an imposing number of the type fossils, long familiar to European prehistorians. But these have turned up in such unexpectedly early contexts that the conclusions Montelius drew from them forty years ago need drastic revision. Only when European chronology has been thus revised, can the earliest cultures of the Orient and Europe, as concretely revealed by the latest excavations, be compared. The result will be to transform the fifth axiom from a postulate into a conclusion.

Let me first summarise the results of excavations in Hither Asia that tend to establish the first axiom—the antiquity of Oriental culture. The beginning of the historical or Dynastic period in Egypt and Sumer now constitutes a fairly accurately dated horizon. The coincidence of Egyptian and Mesopotamian sources is now close enough to permit of this horizon being dated with general consent about 3100 ± 100 B.C. The latest additions to knowledge resulting from Frankfort's masterly operations at Tel Agrab, Tel Asmer and Khafaje, have not only to be mentioned as enhancing the likelihood of diffusion and providing fresh data for European chronology, but intensify our appreciation of the high level of Oriental civilisation and emphasise the long duration of the Early Dynastic Age. The Sin Temple at Khafaje was rebuilt five times. In the same period the Temple of Abu at Tel Asmer underwent four reconstructions.

And the Early Dynastic period itself was far from the beginning of urban life. In the Tigris-Euphrates delta it is preceded by two periods, termed respectively the Jemdet Nasr and Uruk phases, during which monumental buildings were already being erected. At Erech, below the earliest dynastic temple ruins, the German excavators uncovered the wall stumps of a gigantic edifice that had been reconstructed once or twice in the Jemdet Nasr period. These walls in turn rested on ruins of a no less imposing building, the Red Temple ; a veritable cathedral adorned with a mosaic of clay nails and with friezes of stucco beasts. The Red Temple itself was twice remodelled and was after all only the successor of a still earlier, but no less monumental, cathedral, termed in view of its unusual stone foundations the Limestone Temple. Now you do not build a cathedral every fifty years, even if it be built only of mud brick. This series of three prehistoric temples with their several reconstructions must cover a period of several centuries. (Incidentally writing was invented during that period.)

But even in the Limestone Temple we are dealing with a highly-organised urban civilisation presupposing centuries of experimentation and development. Some aspects of that development are explicitly revealed in the archæological record. From the floor level of the Limestone Temple the Germans sank a shaft, 17 m. or just under 60 ft. deep to virgin soil. It was dug entirely through the debris of prehistoric dwellings. As one winds down the ramp into that dizzy abyss one can distinguish in the pit wall 18 layers marked by hearths, floors, stumps of walls and heaps of sherds and artifacts. As Dr. Randall MacIver has insisted, nothing could be more perilous than an attempt to estimate in years the time taken for such an accumulation to form. But I must confess that nothing has driven home so vividly the antiquity of settled life in the Tigris-Euphrates delta as the descent of that great shaft. Admitting that I am now guessing perhaps rashly, I cannot believe that the al'Ubaid culture represented in the lower levels at Erech is later than 4500 B.C.

But no one has ever suggested that the geologically very recent delta of Lower Mesopotamia was the cradle of food-production. It is in fact evident that the al'Ubaid farmers who settled on the freshly emerged land-surface there, brought with them from older regions a culture already mature. And in the last five years the excavations of Mallowan and Speiser in Assyria and Syria have given us glimpses of what preceded al'Ubaid in the Fertile Crescent. It is true that history does not fully dawn there till relatively late—till the time of the Dynasty of Akkad indeed. But relations with Lower Mesopotamia were so close and so continuous that the archæological record provided by the prehistoric levels of Gawra, Nineveh and Chagar Bazar can be proved parallel to that from the protohistoric levels of Sumer. Imported Assyrian pottery actually found at Tel Asmer thus shows that Gawra VI is at least Early Dynastic and Gawra VIIIa not later than Jemdet Nasr in Babylonia. Below the last-named level come four or five architectural periods, Gawra VIIIb to XII, presumably parallel to the Uruk period of Sumer. So when we find in Gawra XIII pottery and other relics typical of the earliest or al'Ubaid phase of Sumer's prehistory, we have no reason to doubt that al'Ubaid in Assyria is virtually contemporary with al'Ubaid in Sumer. But Gawra XIII already boasted a cluster of three handsome and monumental temples, decorated with painted buttresses and niches and grouped round a court 20 m. by 14 m. in area.

And the al'Ubaid temples at Gawra are perched upon a tell, formed from the ruins of older settlements and rising already 25 to 30 m. above the plain. Below the al'Ubaid foundations come settlements belonging to the Tel Halaf culture. Mallowan found the same culture beneath, and therefore older than, al'Ubaid remains at Arpachiya, 38 ft. below the historical horizon at Nineveh and in deep layers at Chagar Bazar. The Tel Halaf culture is accordingly older than the al'Ubaid—if you want a guess, I would hazard 5000 B.C. as a moderate date—but it is no less sophisticated. Monumental circular buildings, cobbled streets, delicate and beautifully painted vases, ingeniously carved stone beads and stamps already used for sealing property attest already a well-organised society, an advanced economy, highly developed craftsmanship. If the collection of pit-dwellings and wattle-and-daub huts sheltering under the gigantic ramparts of Maiden Castle be termed a city, can we deny that name to the Tel Halaf settlements at Arpachiya ? Its cobbled streets disclose a community as well organised for works of public utility as were Iron Age Britons for defence preparations. Even the economic aspect of city life is represented. The richest house at Arpachiya would seem to have belonged to an artist-craftsman presumably producing for sale, not merely for the satisfaction of domestic needs. And even long distance trade is dramatically attested by a shell of *Cypraea vitellus* imported from the Persian Gulf to the Tel Halaf village at Chagar Bazar on the Khabur.

The Tel Halaf culture must have flourished for several generations. Mallowan uncovered at least five building levels at Arpachiya and seven at Chagar Bazar. And yet at Gawra, Nineveh and Chagar Bazar, the oldest Tel Halaf foundations rest upon the ruins of villages characterised by painted pottery of the Samarra style. Guessing frankly once more these might take us well back into the sixth millennium B.C.

Yet the culture revealed even in these remote depths resembles the European neolithic only in the most formal sense—in the continued use of polished stone adzes and some other tools. The earliest cultures of the Fertile Crescent, like its Early Dynastic cities, are so unlike anything we know in Cis-alpine Europe before Roman times, are economically so far ahead of Köln-Lindenthal or Skara Brae or even Tószeg as to seem almost incommensurable. Yet some comparison is inevitable if Montelius' fifth postulate is to be objectively criticised.

The abruptness of the contrast may to-day be softened by reference to a region that is more than spatially intermediate between Mesopotamia and Europe. During the last five years a promising beginning has been made in reducing to a system Anatolian prehistory. The results are relevant not only to the antiquity of Oriental culture, but also to the probability of the diffusion postulated in axiom 3.

The results of the long campaign conducted at Alişar Hüyük by the Oriental Institute of Chicago which were published this year have given the first definite clue to the culture-sequence on the plateau. In particular they provide the skeleton of a chronology. Recorded history began relatively late in the Halys basin ; continuous records disclosing names and dates do not go back beyond the foundation of the First Hittite Empire in the twentieth century B.C. But intercourse between Anatolia and Mesopotamia is attested by business documents several centuries earlier and by tradition as far back as the reign of Sargon of Agade. It is faithfully reflected in the archæological record.

Below the Hittite foundations on the acropolis at Alişar (but not on the terrace) came a deposit with Cappadocian painted ware now termed Early Bronze Age or Alişar C. Below that, five building layers, accounting for 11 m. of deposit, represent the Copper Age or Alişar B. This must end by 2000 B.C. A beginning towards 3000 B.C. might be inferred from an imported Mesopotamian cylinder of Jemdet Nasr style, stone figurines like those regarded as Anatolian intruders in the Early Dynastic layers of Gawra and Tel Asmer, and animal pendants of stone remarkably like those from the Early Dynastic temple of Sin at Khafaje. To this same Copper Age belong the ruins and burials at Ahlatlibel near Ankara. It was a period when commerce was sufficiently organised for metal to be common and seals to be useful.

But beneath the lowest Copper Age floors von der Osten's shaft pierced 8·5 m. of debris, divisible into seven building levels, before reaching virgin soil. The earliest Anatolian culture, represented by Alisar A, is already so advanced that it is accurately termed Chalcolithic. However sparingly used, copper, silver and lead were common enough to indicate well-established commercial channels of distribution and specialised producers. Stamp-seals were already employed. But certain pot-forms and fabrics are already comparable to the Central European; two-handled tankards, like those of the Hungarian Copper Age, occur in the topmost layers only (Alişar A2); for the rest lugs take the place of handles, but a distinctive shape is a high-pedestalled bowl, at first with a remarkably Danubian profile. The fabric is self-coloured, black to red but generally muddy and sometimes particoloured—black inside and round the rim, but brownish below on the exterior. The Anatolian Chalcolithic seems rooted in the fourth millennium B.C., but how far back remains quite uncertain.

Despite conspicuous divergences the Copper Age and Chalcolithic cultures of Central Anatolia are patently related to, and continuous with, those of north-western Anatolia, long known from Schliemann's excavations at Troy. And there re-excavation under Blegen has substantially enhanced the impression of the antiquity of Anatolian culture. If the Americans have not yet provided unimpeachable data for determining the absolute age of the earlier ' cities,' they have at least filled in and expanded the scheme propounded by Schliemann and Dörpfeld. The Troy that the Achaeans might have sacked about 1200 B.C., did Lord Raglan allow us to believe in a Trojan War, was not VI but VIIa. Troy VI goes back on the strength of Helladic imports to 1500 B.C. Cities V, IV and III turn out to be quite important settlements, divisible into several architectural levels and making up together a formidable accumulation 4 m. deep. Troy II, thus separated from the Mycenaean horizon, can no longer be brought down to the Shaft Grave epoch, however neat Åberg's typological comparisons may look. It is firmly anchored in the third millennium whatever its precise limits may be. And Troy I below it was already a city girt by an imposing wall. Its citizens were executing monumental sculptures that provide a new limiting date, on Montelius' assumption, for the statue-menhirs of Atlantic Europe. And by this time, as Miss Lamb has shown at the contemporary Lesbian township of Thermi, copper and even bronze were already being worked, celts might have hammered flanges, battle-axes were used in war, while trade brought marble vases from the Ægean Islands. And remains of a still earlier phase of culture may be discerned at Kum Tepe. Soundings there produced pedestalled bowls like those from the earliest Chalcolithic of Alişar that seem still missing in Troy I and the contemporary Lesbian site.

The experiments in Anatolia thus go far to re-enforce with objective facts the antiquity and relatively high level of Oriental culture assumed in axiom 1. Moreover, taken in conjunction with Heurtley's excavations in Macedonia, they concretely demonstrate connections between Asia and Europe that are the precondition for admitting axiom 3 and provide a crucial instance for testing axiom 5, i.e. for comparing demonstrably contemporary cultures in Europe and Asia. Heurtley has convincingly demonstrated the Anatolian ancestry of the Early Macedonian Bronze Age culture; it begins with fully developed horned tubular lugs growing from the bowls' rims. The evolution of this odd type that appears fully formed in Europe can be traced stratigraphically on the Asiatic side. It emerges as a finished product first in phase B at Thermi; its earlier stages are illustrated in phase A. For once we have, fully documented, a cultural spread which is irreversible; in this concrete instance axiom 3 becomes a conclusion from ascertained facts.

But, implanted in Europe, Anatolian culture appears poorer than its Asiatic parents. Even in phase A Thermi was quite a township, the contemporary Troy I a fenced city. Their economy was so far advanced that copper and even bronze could be used for tools as well as weapons; metal was so plentiful that quite a lot was left lying about for Miss Lamb to find. The Early Macedonian settlements which are not older than Troy I give the impression of rustic villages. For all the metal collected among their ruins, they might be neolithic. Macedonia was still veiled in mists which the Oriental sun must pierce before an economic system comparable even to the Anatolian could function.

But if the Early Bronze Age culture of Macedonia is unambiguously rooted in Asia, the later neolithic culture which it supersedes is no less securely linked with that of Vinča and Tordos in the Middle Danube basin beyond the Balkan ranges. Comparison of the Macedonian relics with those from the Morava-Middle Danube-Maros sites shows that we are dealing not with two cultures but with different facies of one and the same culture. Common to both regions are stone adzes of shoe-last form, bone combs, bracelets of *Spondylus* shell, clay figurines, clay altars, carinated bowls and chalices on solid pedestals in dark-faced, particoloured and red-slipped wares decorated by incision, fluting, stripe-burnishing and painting in black or white on red sometimes with spiral motives and embellished with lugs modelled as animal heads. A veritable cultural continuum traversing the Balkans connects the Ægean coasts with the Danube basin. We may reasonably speak of a Vardar-Morava culture extending from the coasts to the Maros.

How such a continuum was constituted remains a question for debate elsewhere. Its absolute antiquity in Macedonia cannot be defined with precision owing to the difficulties of applying the Minoan-Helladic systems to what may have been a cultural backwater and the uncertainties in the systems themselves. Even the position of the Vardar-Morava culture in the Danubian sequence remains ambiguous. Though the deposit at Vinča is 10 m. thick and comprises type-fossils of Danubian II, the methods of excavation and publication do not permit of the distribution of the relics between stratigraphically defined periods. For our purpose the supreme importance of the Vardar-Morava complex is that it establishes at least once a continuity of culture from the Ægean to the Danube basin. Whatever be the chronological horizon of that continuity, its existence enhances enormously the significance of the south-eastern analogies to cultural phenomena in Central Europe. It provides a justification for admitting axiom 3—diffusion from Asia to Central Europe is likely.

Fortified by this conclusion let us turn to axiom 4—the prehistoric chronology of Central Europe. There the cultural sequence is reasonably clear at least north of the Bakony and the Little Carpathians. The divisions which I tentatively suggested ten years ago have on the whole been fully justified by recent research. A reference to the comprehensive survey of the Danubian and Western Cultures in Germany published by Buttler last year will show how well my scheme works. Thanks particularly to the work of Banner round Szeged it can even be extended to the Hungarian plain more fully than I could do. The Copper Age Bodrogkeresztur culture there is plainly the counterpart of the so-called Nordic and Bell-beaker cultures of my Danubian III in the Sudeten lands, and Banner's Körös culture may well fill up my period I. But to what Oriental cultures shall these several phases be compared ? Encouraged by the newly-revealed proofs of intercourse, let us apply Montelius' fourth axiom to dating the Danubian sequence.

The earliest bronze objects found in Central Europe (in graves and hoards of the Aunjetitz culture) include a whole constellation of specialised and arbitrary forms of ornament that are now known also in historically dated horizons. Ingot-torques have been found in Early Dynastic levels at Tel Agrab and recur in North Syria and in the Copper Age graves of Ahlatlibel in Turkey. Earrings and lock-rings with flattened ends are common in Early Dynastic Sumerian graves and in the ' treasures ' of Troy II ; racquet pins are found in the Royal Tombs of Ur ; the knot-headed pin goes back to Gerzean times in Egypt and appears at Troy II ; its principle was applied to Sumerian toilet sets in Early Dynastic times. By then tin bronze was already known to the Sumerians as to the Lesbians in the time of Thermi I. In a word all the type-fossils of the Early Bronze Age in Central Europe, and the technical discovery that defines the period, can be traced back to somewhere about 3000 B.C. in the Orient. On the strictest application of Montelius' axiom the beginnings of the Continental Bronze Age should be nearer 2800 B.C. than 1800 !

And as far as Central Europe is concerned that chronology would involve no glaring contradiction. Oriental parallels can be found to the types that define earlier periods, while Mediterranean shells, imported even to the Rhine Valley, prove intercourse with the south-east right back to Danubian I. Stone battle-axes such as characterise period III are found already at Thermi I. The Early Dynastic levels of Tel Agrab have yielded rather degenerate specimens ; better battle-axes come from the al'Ubaid settlement at Arpachiya and from Gawra VIII–IX, that is equivalent to Uruk in Sumer. Hence Danubian III could be equated with the Uruk period.

Clay stamps, generally called *pintaderas*, appear in Danubian II (and in Körös sites that may be older). In form they closely resemble Asiatic stamp seals of stone and, like the latter, often bear a filled cross design. In Europe, such stamps, nowhere very numerous, are common only in the extreme south-east—Bulgaria, Wallachia, Transylvania, the Middle Danube plain ; stray examples reach Moravia ; still fewer the Upper Elbe and Oder basins. Such a distribution justifies their interpretation as copies of Asiatic stone seals. But in Asia prototypes can be found as early as Tel Halaf times and in the Chalcolithic layers of Alişar. And there there are pedestalled bowls remarkably like those characteristic of Danubian II. The upper limits for that period could accordingly be pushed back to Alişar Chalcolithic or even Tel Halaf.

And that is not the end of our comparisons. As *Spondylus* shells were being imported from the Mediterranean even in Danubian I times, so some Danubian I vases are decorated with patterns in which Neustupny rightly sees a representation of a double-axe. For the models he looked to Minoan Crete. But double-axes were used in Assyria as amulets even in Tel Halaf times. So the *terminus post quem* provided by that motive can be relegated to a remote Tel Halaf period.

Testing this long chronology in the other direction, it can still be made to work. Åberg and Reinecke have indeed insisted on Middle Helladic and Shaft Grave parallels to Aunjetitz bronzes of period IV. But on the whole Middle Ægean armament—rapiers, ogival daggers, socketed spear-heads—is typologically parallel rather to that proper to the Middle Bronze Age or period V, in Central Europe. A halberd from Shaft Grave IV is admittedly an Early Bronze Age type, but Forssander has plausibly compared its contours with those of a Middle Age sword from Hajdu Samsón. The pottery from Middle Age Bronze graves at Vattina and from south-eastern Hungary includes many tankards and goblets with crinkled rims and grooved handles that might be copies of well-known Middle Minoan silver vessels. In a word a limiting date about 1700 B.C. for the Middle Bronze Age is defensible.

And with the fall of the Mycenaean culture we have admittedly reached the Late Bronze Age or period VI of Central Europe. The barbarian invaders who sacked late Mycenaean Vardaroftsa, in the twelfth or eleventh century, brought ceramic traditions proper to the Late Bronze Age urnfields like Knovíz and Hötting. And this date is for once a *terminus ante quem* for the continental period. An even higher limit might be deduced from the fibulae and flange-tanged swords that appear in Greece during the thirteenth century. Accordingly the following scheme of European chronology might be defended :—

Danubian VI	
(urnfield cultures fibulae and slashing swords)	1200 B.C.
Danubian V	
(Vattina ware, rapiers, ogival daggers, socketed spear-heads)	1700 B.C.

Danubian IV
(bronze, ingot torques, knot-headed pins, lock-rings) . . . . . . 3000 B.C.

If geologists and botanists can show good grounds for demanding an enlargement and prolongation backward of the neolithic age, archæological chronology can be adjusted to meet theirs without violating Montelius' axioms. Danubian I, admittedly the earliest neolithic culture in continental Europe, would still be limited by Tel Halaf. If the former have to be dated to the sixth millennium, the latter can just as reasonably be assigned a like antiquity.

The foregoing dates are advanced only as extreme possibilities. The Oriental analogies cited provide under axiom 4 only upper limits for the corresponding period. It is not till the Late Bronze Age that we get a *terminus ante quem* from our comparisons. But for the moment let us adopt the maximal dates as a framework for comparing Asiatic and European cultures. How would Montelius' general view of the relations between Europe and the Orient be affected by adopting the long chronology outlined here ? What happens to his fifth axiom if the Central European Bronze Age began about 2800 B.C. ?

By that date we should have the following picture of the tract we have been surveying. We should see in Egypt and Lower Mesopotamia populous cities, covering like Erech perhaps two square miles of area, governed by a well-established organisation, emancipated from immediate dependence on environmental conditions by extensive public works and a rich technical equipment and regular far-flung commerce and all fully literate. Then in Assyria and Syria come smaller cities, only slightly less richly equipped and still at least semi-literate. Further afield in Anatolia and peninsular Greece are fortified townships whose walls protect a variety of specialised craftsmen so well served by regular commerce that metal at least could be freely used for tools ; their citizens may already need and use seals, but seem to be illiterate. Next, in the Balkans and on the Hungarian plain, we find rustic townships occupied principally by farmers. Their rural economy is advanced enough to support a truly sedentary population, but virtually the sole outlet in industry for the surplus is offered by metallurgical employments, and trade is so imperfectly organised that metal has to be reserved mainly for armaments. The same picture would apply to Bohemia and southern Germany with the important reservation that agriculture seems not to have advanced so far as to allow the population to be really stable. Denmark and southern Sweden are still frankly in the Stone Age. And still further north food-gathering is the sole economy.

Look back as many thousand years as may be necessary to reach Danubian I times, which have been for this purpose equated with the Tel Halaf period in the Fertile Crescent. In the Orient we see already little townships permanently occupied by experienced farmers, comprising already expert craftsmen and supplied by trade at least with obsidian. In Crete and Thessaly too perhaps more self-sufficient farmers are still applying sufficient science to their fields to be able to live permanently on the same site. But beyond the Balkans nomadism reigns. The Körös herdsmen are roaming over the Alföld, tilling and grazing patches for a few seasons and then moving on. And Danubian I peasants are spreading over the löss, shifting their little hamlets of twenty or so households to new virgin fields every few years. And beyond the frontiers of the löss are only food-gatherers, fishing and fowling along streams in the forest or collecting shell-fish on the coasts.

Yet earlier still beneath Tel Halaf villages we have glimpses of settled cultivators who, judging by the few items of equipment so far recovered, were at least as far advanced as the Danubians.

Even on this extreme chronology Montelius' fifth axiom is justified. Oriental cultures are richer than the contemporary European. Moreover the first picture discloses a very significant cultural zoning. As we pass north-westward from the Orient we descend through regular gradations from the many-sided richness of urban civilisation to the stark poverty and immediate dependence on external nature of food-gathering hordes. Such a grading is exactly what would be deduced from Montelius' third axiom. Its discovery in the archæological record is the best demonstration of diffusion that I can imagine. I take it as confirming the diffusion of bronze-working with all its economic implications.

But on the extreme chronology this demonstration could not be applied to food-production, to the more important discovery-complex that made possible what I term the neolithic revolution. The Vardar-Morava culture, that as yet alone establishes concretely continuity across the Balkans, could hardly be put so early in relation to Oriental cultures, however it may be related chronologically to Danubian I. Objective proof of cultural continuity, giving effective opportunity for diffusion between the Near East and Central Europe, would be still lacking. The belief that agriculture and stock-breeding, the foundations of any neolithic culture, were introduced into Europe from the Orient would remain only a probable hypothesis which, however much its plausibility has been enhanced, must await final confirmation or refutation in the observed facts of excavation. The Balkans are still but little known. Till the crucial experiments have been made there, it would be permissible to hope for confirmatory evidence in that quarter.

Montelius' thesis has come unscathed through the severest test. Even on a chronology based on geological rather than archæological premises and designed to meet the demands of an extraneous discipline, his axioms 4 and 5 prove workable. If geologists demand dates of the order just outlined, archæologists can meet them without sacrificing any essential principles, but preserving intact their own proper methods and all the historically vital deductions therefrom. But these high dates for Central European prehistory have been advanced provisionally simply and solely to test the applicability of Montelius' method, and not as proven or even probable. To justify them archæologically we have had to sacrifice many tempting comparisons and to explain away observed facts that must be admitted as relevant.

Remember that down to 1200 B.C. no date in European prehistory could be justified archæologically by an actual object of Oriental manufacture found in Central Europe, still less by an admittedly European product in a historically dated context. We have had to rely exclusively on copies of Oriental models made in Central Europe. Remember further that all the types on which we have relied enjoyed a long popularity in the Orient: seals that could serve as models for Danubian II 'pintaderas' were current in Crete and Asia Minor throughout the third millennium and later. Battle-axes for comparison to those of Danubian III were brandished equally long in central Anatolia and first appear in peninsular Greece in Middle Helladic times. The type-fossils of Period IV only came into fashion in the East in the third millennium and fashions did not change abruptly. Knot-headed pins were still being worn in the third (Hittite) settlement at Kusura during the second millennium. Ingot-torques, racquet pins, lock-rings and earrings with flattened ends are common in Caucasian graves well after 1500 B.C. The archæological 'synchronisms' so far considered are really just upper limits.

Accordingly till geologists present their demands with more unanimity and confidence, it is permissible to remind you of other comparisons between Central European and south-eastern phenomena that entail substantially lower dates for our prehistoric periods. Characteristic of Danubian II are cubical blocks of clay, with one, or rarely two, cups hollowed out in them and perforated at the corners. These have been convincingly explained as clay copies of Early Minoan block vases of stone. Thus interpreted, they would bring the limits of Danubian II down into the third millennium under axiom 4.

Found allegedly in an Aunjetitz grave of period IV at Nienhagen in Central Germany was a clay cup; its curious handle is strikingly like those of the metal Vapheio cups of Late Minoan I, most popular between 1600 and 1500 B.C. Parallels between Aunjetitz weapons and those of the Mycenaean Shaft Graves of roughly similar age have already been mentioned—and explained away. Still the amber beads from these and later Mycenaean graves should re-enforce the arguments for a parallelism between Central European Aunjetitz and Late Helladic Greece. The amber trade was a mainspring of the Aunjetitz commercial system. Did it involve nothing more than barter between barbarians in Denmark, Bohemia and Upper Italy? The brilliance of the Early Bronze Age in Bohemia would become much more intelligible if that region were already connected by the amber trade with civilised Greece. The probability of such a connection is enhanced by Piggott's recognition among the amber beads from Kakovatos (Nestor's Pylos) of massive forms and space-plates in the Danish style such as often occur in graves contemporary with Aunjetitz. All these pointers converge upon a date for the beginning of the Central European Bronze Age a full thousand years later than the upper limits deduced from the metal ornaments.

Such considerations are, however, frankly speculative and can if needful be dismissed. It is less easy to explain away certain actual Ægean or Egyptian imports found in an apparently Early Bronze Age context in Central Europe. Segmented fayence beads occur in four graves near Szeged associated with pottery of the Perjamos type and in two Moravian graves with Aunjetitz pottery. Though the blue glaze is generally less well preserved, these beads, Dr. Stone assures me, agree perfectly in form and technique with those from Wiltshire and from Grave 1808A at Abydos, dated about 1400 B.C. Now admittedly the coincidence of Perjamos and Aunjetitz may not be altogether exact, and Aunjetitz and Perjamos ceramic forms and even knot-headed pins and ingot-torques outlast the bounds of the Early Bronze Age or Danubian IV as defined by hoards. But even if the relevant graves be transferred to the beginning of the Middle Bronze Age (Reinecke B), it is difficult to admit that Perjamos jugs and Aunjetitz mugs persisted virtually unchanged for 1400 years or to spread over so long a period even the 180 graves of the Szöreg cemetery from which some of our beads come.

And the foregoing are not quite the earliest imports recognised in Central Europe. Willvonseder found a very small blue segmented bead of the sort made in Egypt from 1600 to 1300 B.C. at Leopoldsdorf near Vienna in a grave with a Bell-beaker. Of course, it is now recognised that Bell-beakers are not confined to period III ; some are contemporary with early Aunjetitz. Still taken altogether these undoubted imports provide really cogent arguments for the limiting date of 1500 B.C. proposed by Åberg for Aunjetitz. Clearly that would fit in beautifully with the more speculative considerations adduced above for earlier periods. And notoriously it is difficult to make a Bronze Age of two thousand years look credible outside the Central European and Britannico-Hibernian economic systems—in south France for instance.

Perhaps then it may be legitimate to consider a short chronology such as I have previously advanced on several occasions as a still plausible alternative to the long one outlined to-day. Till incontrovertible evidence from the geological or botanical side makes it obsolete, it is still permissible to consider in conclusion how the low dating endorsed by the fresh data just adduced affects the general credibility of Montelius' hypotheses.

In our previous pictures of the Tigris-Rhine tract we shall have to transpose individual items to fit a Central European chronology based on synchronisms through Greece with Egypt and altogether independent of Asia. We then get two scenes both disclosing the cultural continuity and gradation recognisable only in the first picture on a long chronology. At the beginning of the Central European Bronze Age towards the middle of the third millennium B.C., the following zones could be distinguished:

(1) The metropolitan civilisations of Egypt and Babylonia.

(2) Relatively provincial civilisations in Crete, Syria and Hittite Asia Minor, but all fully literate and truly urban.

(3) Bronze Age towns in western Anatolia and peninsular Greece whose walls may enclose from 4 to 11 acres and defend not only smiths but also specialised potters and many other craftsmen. Most are illiterate, but literate urban civilisation is already dawning at Mycenae.

(4) In Macedonia and the Balkans and on the Middle Danube stable villages exist ; their size can be estimated from the cemeteries comprising a maximum number of 180 graves. Besides farming the only specialised industry is metallurgy, and commercial organisation is too rudimentary to make metal generally available for tools.

(5) In Czechoslovakia and South Germany a similar economy reigns, but the settlements are less permanent and the maximum number of graves so far reported from a cemetery is 100.

(6) In North Germany, Denmark and South Sweden are bands of

herdsmen and small hamlets of self-sufficing peasants still equipped with only stone weapons.

(7) In the extreme north the sole source of livelihood is food gathering.

Fifteen hundred years or so earlier the gradations would be similar but the zones would have contracted. We should see :—

(1) In Egypt and Mesopotamia true cities whose walls may already enclose nearly 2 sq. miles, relieved from immediate dependence on environmental accidents by public works and organised commerce, comprising a variety of artisans and officials including scribes.

(2) Smaller cities in Syria less richly equipped and only partially literate.

(3) Copper Age townships in Anatolia and peninsular Greece with a walled area of 2 to 4 acres and a population comprising specialised smiths and some other craftsmen adequately provided by trade with metal and other raw materials.

(4) In Thessaly, Macedonia and the Morava-Maros region beyond the Balkans neolithic villages are permanently occupied by experienced farmers who are content to do without metal.

(5) North of the Maros Körös herdsmen and Bükkian troglodytes are grazing and tilling patches of löss and then moving on ; still further north Danubian I hoe-cultivators are shifting their hamlets of twenty odd huts every few years to fresh fields till they reach the confines of the löss.

(6) Beyond these on the North European plain are only scattered bands of food-gatherers hunting, fowling and fishing and collecting nuts or shell-fish.

In each picture we see within a continuous area of interlocking cultures gradations such as would be deduced from the diffusionist postulate. But a comparison of the second picture with the first reveals just that expansion of the zones affected by the neolithic revolution that would be anticipated were its effects being indeed diffused. The acceptance of axiom 4, the rigorous application of his chronological method alone, would virtually allow the graphic demonstration of Montelius' remaining assumptions.

SECTION I.—PHYSIOLOGY.

Owing to the coincidence of the International Physiological Congress (Zürich, August 14–19, 1938), no separate meetings were arranged for this Section.

SECTION J.—PSYCHOLOGY.

EYE AND BRAIN AS FACTORS IN VISUAL PERCEPTION

ADDRESS BY

R. H. THOULESS, M.A., Ph.D.,

PRESIDENT OF THE SECTION.

THE title that I have chosen for my address describes a larger field than it is possible to deal with adequately in a single hour. My aim to-day is to consider only some aspects of this question, illustrating my remarks by reference to those problems of visual perception with which I happen to have been most closely concerned.

That we see with our eyes is known to everyone and has been known for a long time. That we see also with our brains is less generally realised, and the implications of this fact are relatively recent importations into the theory of vision. The full statement of the physiological mechanism of vision would include not only the sensitive retinal surface and the visual areas of the cortex but the whole system which includes retina, optic nerve, visual area of the cerebral cortex, and other sensory areas of the brain as well.

I. THE TRANSMISSION THEORY OF VISION.

It is possible, of course, to study vision in such a way that everything except the activity of the retina is neglected altogether or relegated to a secondary position, and it was in this way that the scientific study of vision began. This is the point of view which we find in the work of Helmholtz and in much of the experimental research into vision which has followed his deservedly great authority. The basic assumption is that the essential process of vision is the formation of an optical image on the retina and its transmission to the visual centres of the brain by means of the optic nerve. Differences between the sensations transmitted to the brain and the finished perception which appears in experience were attributed to the action of the higher processes of judgment and the influence of past experience.

This theory of vision, which we may call the 'transmission theory,' has behind it not only the weight of the authority of the great originators of the experimental study of vision. It has also the advantage of being the view of the man in the street. Its truth seems to many to be so axiomatic that its denial may have the appearance of wilful paradox.

It is, nevertheless, now clear that the transmission theory is wrong, and that a wholly different way of approaching the problems of visual perception is necessary if we are not to be led astray. To say this is not to deny the greatness of the achievements of those investigators in the past whose work on vision was guided by this theory. Within a certain limited field, it proved itself a fruitful guide to research. This field was that of the sensory physiology of the retina. If we wish to discover what is happening on the retina we must arrange conditions of experiment so as to cut out, as far as possible, the complicating effects of the cerebral components of the visual part of the nervous system. This was what was done when the early experimenters made observations through tubes or on black backgrounds. So such workers as Helmholtz, König, Abney and a host of others made a firm foundation for a science of vision in the sensory physiology of the retina. The error, however, has sometimes been made of mistaking the foundations for the completed building. When we get rid of tubes and black backgrounds and open both eyes to look at objects surrounded by other objects we find that what we see follows other and far more complicated principles than the laws of sensory physiology.

Against the successes of the transmission theory of vision in originating fruitful lines of research, we must set its failures. Fruitful in the field of sensory physiology, it left most of the field of perception a barren waste. Its underlying assumption was that any visual experience which could be exactly correlated with an event in the sense-organ was a true element of experience (a 'sensation'), while those that could not were regarded as due to the action of higher processes on these elements or were relegated to the class of 'illusions' or mistaken judgments. There were many investigations of the perceptual experiences (such as that of depth) which were attributed to correct judgments about sensations, although the successes in this field were much less striking than in that of sensory physiology; the 'illusions,' however, were almost wholly abandoned to anecdotal report.

Fruitful experimental work on perception began when psychologists began to doubt the validity of the distinction between true perceptions and illusions. Katz, for example, in 1911, devoted a considerable part of a book to a description of such differences between colours as are not due to differences between the local retinal stimulations. Thus the difference between the appearance of yellow and of red is accompanied by a difference in the bands of wave-lengths of light stimulating the eye, and is regarded on any theory as a real psychological difference. There is, however, also a difference between the appearance of a red book (a surface colour) and a red patch seen in the spectrum of the same brightness and the same composition of wave-lengths (a film colour). This difference is accompanied by no difference in the conditions of local retinal stimulation and is, therefore, from the point of view of the transmission theory, an illusory difference. It was only when psychologists adopted what has been called the 'phenomenological' point of view of regarding all differences in appearance as equally reputable objects of experimental study, whether or not they were accompanied by differences in local physiological stimulation, that the serious experimental study of such things as the modes of appearance of colours became possible.

II. Experimental Objections to the Transmission Theory.

Although the way was undoubtedly paved for criticism of the transmission theory by earlier researches, the main attack on it developed from Wertheimer's investigation (published in 1912) of the so-called 'phi-movement' which results from successive stimulation at certain intervals of two retinal points. This is one of many known examples of the appearance in perception of something which does not exist in the pattern of stimulation. On the retina there is only intermittent stimulation of stationary points; in the perceived world there is movement. Wertheimer argued that this movement must be regarded as a genuine element in the total system of physiological events which determine the perception, and that it is not to be explained as an illusion of judgment. These experiments formed also the starting point from which Köhler launched his more general attack on the 'constancy hypothesis' (i.e. the transmission theory of perception).

Criticism of the transmission theory and exploration of the implications of the inclusion of the brain as a factor in vision might obviously have started from other investigations. It is not my purpose to discuss Wertheimer's work or the arguments that have taken place about it, but instead to invite you to consider these problems in connection with a field of experimental study with which I happen to be more familiar.

We may begin by considering a simple experiment which can be performed by anyone with no more complicated apparatus than an oval table-mat or a sheet of cardboard and a pair of scissors.

We place on a table an elliptical object with its long axis pointing directly to and from the observer. If his head is directly above the object, it will, of course, look elliptical. If now he moves his head from the position directly above, but still keeping it in the vertical plane passing through the long axis, the object will at first still look elliptical, but with a smaller apparent elongation than when it is viewed from directly above. If the head is now lowered, but still kept in the same plane, the apparent shape of the object becomes nearer and nearer to a circle. It then becomes truly circular and, if the head is still further lowered, the object appears elliptical again only now with the really longer axis apparently the shorter.

So far everything appears to be as one would predict on the transmission theory by the elementary principles of perspective. Measurement of the actual angles at which these various appearances are found reveals, however, a considerable discrepancy from the expectations aroused by the transmission theory. At the height, for example, at which the ellipse looks circular, it is found that the retinal image is not of a circle but of an ellipse with the vertical axis much shorter than the horizontal, that is, an ellipse flattened in the opposite direction. It is as if the shape that is seen (the phenomenal shape) is in between the real physical shape of the ellipse and the shape that is projected on the retina (which we may call the stimulus shape).

The natural expectation on the transmission theory would be that the stimulus shape and the phenomenal shape would be identical. Plainly they are not, and the discrepancy is large enough to show clearly without any great refinement of measurement. Can we save the transmission theory by making some supplementary hypothesis to explain how the phenomenal shape gets changed from its expected identity with the stimulus shape? A suggestion that has been made is that the flattened elliptical shape as projected on the retina does produce its own 'sensation' of shape, but that this sensation becomes changed by a process of judgment. Plainly it is not a judgment in the ordinary sense of reflective judgment since the subject is carrying out no conscious process of inference, but it is claimed that there is a more primitive unconscious process of perceptual judgment which is always modifying our sensations in accordance with the knowledge we have of the nature of the objects producing them.

There are, I think, many objections to this explanation. First, it does not explain the fact which has to be explained. This is not that the subject *judges* the ellipse he looks at to be circular, but that, at that inclination, he *sees* it as circular. He may quite correctly judge it to be really an elongated ellipse, and if he is well informed about such experiments (but not otherwise) he may judge it to be making a retinal image which is a flattened ellipse. He may or may not make such judgments, but his immediate experience is of a circular shape.

Secondly, a process of judgment is affected by the knowledge that the subject has about the facts relevant to the thing which is being judged. This, however, is not the case here. A subject who has not done these experiments before will think that his retinal image is circular when the inclined ellipse looks circular, whereas one informed about the nature of the experiment will know that it is not so. The angle of inclination of the line of vision at which it looks circular will be found to be in no wise affected by the presence or absence of such knowledge. We can, however, make the subject see the inclined ellipse in its retinal shape by giving it a dark structureless background and making him look at it with one eye through a blackened cylinder. Now it will look like a circle only when the retinal image is circular, and under these conditions it will look circular whether or not he is told that he is really looking at an inclined elongated ellipse and not at a circle normal to the line of vision. Again knowledge of what he is looking at does not make any difference; what is necessary for the apparent shape to be intermediate between the stimulus shape and the real shape is the actual presence of perceptual cues which indicate that the real object is an elongated ellipse with its long axis away from the observer.

A last and most fatal objection is that this hypothesis requires at least that a sensation corresponding to the retinal image should have been transmitted to the brain in order that a judgment about it may take place. There is, however, no indication that this supposed sensation has any existence as an element of experience. The subject doing this experiment under ordinary conditions—with both eyes fully open—is quite unable to see the flattened ellipse which is the shape of the retinal image. He can do so, as we have seen, by altering the conditions of perception, as, for example, by monocular observation through a tube. This, however, is no reason for saying that in any sense he sees this retinal shape under ordinary conditions of vision. Obviously we cannot discover what is seen under one condition of perception by finding out what is seen under another condition of perception. Looking with one eye through a tube, we see a flattened ellipse ; looking with both eyes and no tube we see a circular shape, and the flattened ellipse which is the retinal shape is not an element in our experience at all. If it is not an element in our experience, we cannot make a judgment about it of any kind, and the hypothesis of perceptual judgment is in no better position than that of reflective judgment.

I know that it has sometimes been stated (e.g. by Gelb) that we can see an object in its stimulus character by adopting a special ' critical ' attitude. I think this, however, is a mistaken observation based on the undoubted fact that some subjects can, by adopting a special attitude, see the object more nearly to its stimulus shape ; they can reduce the effect of its real shape on the apparent shape, but I do not find that they can reduce it to zero, nor do I know of any evidence that anyone else has found this.

We can test this question by asking our subject in the above experiment to try to place his head, not at the height at which the ellipse looks circular, but at that height at which he thinks it makes a circular retinal image (prohibiting him, of course, from partially closing his eyes or otherwise altering the conditions of perception). Now, if the retinal shape (the ' sensation ' of the transmission theory) were itself an element of experience, this should be an easy task. In fact, the subject does not find it easy. He has no immediate knowledge of when the retinal image is circular. He has immediate experience of the phenomenal or apparent shape, and on this he must base a judgment. If he knows nothing about effects of this kind, he will generally judge that the ellipse is making a circular retinal image when it looks circular to him. If he is better informed, he will judge that the retinal image is circular at some angle of inclination at which the ellipse looks elongated along its vertical axis, but any adjustment he makes is merely a guess and generally wildly inaccurate. Even if he happens to guess more or less correctly, he will say : ' I think this must be about the position, but the ellipse doesn't look circular here.' How could this be, if the retinal image were itself transmitted to the brain ?

We are led from consideration of this experiment to the same conclusion as was arrived at by Wertheimer as a result of his experiment on phi-movement, that the ' sensation ' corresponding to the conditions of local retinal stimulation, as an element in a complex perception, is a mere fiction. Although it is clear that the conditions of local retinal stimulation affect the resultant perception, we can find no trace of evidence that they do so by being transmitted to the brain as ' sensations.'

III. An Alternative way of treating Visual Perception.

I have examined this experiment in detail because it seemed as good a text as any on which to hang a homily against the transmission theory of vision. Other, more familiar, psychological facts might have been used. Indeed it may be argued that even the familiar fact of the perception of depth cannot be explained on the transmission theory without doing violence to obvious facts of experience. Yet, for many reasons, our minds tend to cling to the transmission theory. Most of all, I think, because it is simple and easily intelligible. It is indeed the explanation of visual perception which any person informed of the nature of the retinal image and of neural transmission of impulses would come to by his own reflection. The objections to it he would be unlikely to realise by everyday experience, since these depend on laboratory experiments and observations of which those who do not work in laboratories have, in general, no knowledge.

The transmission theory is easily intelligible because it can without difficulty be explained by a physical analogy. Photographs might be transmitted telegraphically by forming an image on a plate made up of a large number of small photo-electric cells each of which was connected by a wire with a corresponding reproducing cell at the other end. This is not, of course, the method actually used for the telegraphic transmission of photographs, but it is physically a possible one. If the receiving electric cells are replaced by the retinal organs, the transmitting wires by the fibres of the optic nerve, and the reproducing cells by the nerve cells of the visual centres of the cerebral cortex, we have a perfect analogy to the physiological process of vision on the transmission theory.

Yet this advantage of simplicity and easy intelligibility must be given up if the transmission theory does not fit the facts. We have so far criticised it only in connection with one experiment. Perhaps this will be a convenient place to summarise the whole case against it.

First, there is a physiological difficulty as to the mechanism of transmission. Such a method of transmission as is suggested by the above analogy would require a number of wires equal to that of the receiving cells. This condition is not fulfilled by the visual system since the number of retinal end-organs is two hundred times as great as the number of fibres in the optic nerve.

Secondly, a breach in the transmitting part of such a system would lead to a corresponding gap in the received picture. This expectation is not fulfilled in vision. We might explain away on the transmission theory the fact that we do not see a gap in the part of the monocular visual field corresponding to the blind spot, but Fuchs has shown that similar completion may take place over a blind area of the retina caused by an acquired destruction of part of the optic nerve.

Thirdly, if this theory were true it would be necessary that differences in the picture at the sending and at the transmitting end should always accompany one another. The experiment already discussed has given one example of that not being the case, since the impression of a circular shape may be given either by the circular retinal image given by a circular object at right angles to the line of vision, or by a retinal image which is a flattened ellipse if this is made by an object which is itself an elongated ellipse viewed at a suitable angle of inclination.

There are plenty of other examples of this in visual perception ; indeed, except in those conditions of simplified perception which were characteristic of the early investigation of visual ' sensations,' exact correspondence between the details of the retinal image and of what is perceived is the exception rather than the rule. In Rubin's reversible figures, for example, we may have a pattern which is seen either as a row of black **T**'s on a white ground or as a row of white fleurs-de-lys on a black background. Thus we have a single stimulus pattern on the retina giving rise to two wholly different perceptions. The after-image of a circle, moreover, will look large or small as it is projected on to a far or a near object respectively, although the area of retinal activity remains unchanged. And if a subject seated below the object glass of a projection lantern looks at a picture projected on to an inclined screen, he sees the picture as distorted although it is easy to demonstrate that his retinal image is identical with that which he would have received if the screen had been at right-angles to his line of vision.

Such facts as these are not easily reconcilable with the theory of simple transmission of a retinal picture to the brain. That there is a close relationship between the condition of physiological stimulation of the retina and of the resulting pattern of visual perception is, of course, obvious and is denied by nobody, but the relationship may not be of the kind suggested by the analogy with telegraphic transmission.

A better analogy for the modern view of perception is, I suggest, the construction of one of the charts published with weather forecasts. The lines of equal pressure on the charts are constructed from information received from various land stations and ships, just as the perceptual picture constructed by central activity depends on information received from the sense organs. If no information as to barometric pressure is received from a certain area, this does not mean that the corresponding area must be left blank, but that the person constructing the chart must fill it up by guess-work, which he generally does by constructing smooth curves consistent with the other information. In the same way, in Fuchs's experiments, it was found that central perceptual activity tended to fill in areas from which no information was received from the retina by simple completions providing ' good continuation ' with the figure received on the rest of the retina.

This view of visual perception may be expressed in various ways. We may follow the Gestalt psychologists in speaking of perception as being due to the combined effect of ' external forces ' belonging to the pattern of retinal stimulation and of ' internal forces ' belonging to the central factor in perception. Or we may speak of the processes of retinal stimulation as ' cues ' for the resulting perception. These are different ways of expressing the same fact that perception is regarded as a central activity of which sensory stimulation is generally a determining cause, but not a necessary condition.

The analogy of the construction of a weather chart suggests a possible way of looking at the process of visual perception which is alternative to the transmission theory and which, I think, gives a much better account of the experimental facts. It regards the mind (or the brain acting to some extent as a unitary whole) as active in perception, responding to information given by the sense organs and not merely reproducing a pattern of stimulation from the sense organs.

We have shown that seen shape is a product not only of the shape of retinal stimulation but also of the perceived inclination in space of the object looked at. Other experiments (to be mentioned later) show that the seen size of an object is similarly a product not only of size of retinal stimulation but also of the perceived distance of the object, and that seen brightness is not a product only of the brightness of the retinal image but also of the perceived illumination of the object (being greater if the object is seen to be shadowed and less if it is seen to be strongly illuminated). Such facts as these show that a visual characteristic of an object is not (as we should expect on the transmission theory) a product only of the corresponding local stimulation of the retina or of a projection of this local stimulation on a corresponding area of the visual cortex. We must rather regard it as the product of the combined action of different activities of the visual cortex which also may make their contributions to other characteristics of the perceptual field.

Thus apparent size is not the product only of size of retinal stimulation but is determined also by those cortical activities which give us the perception of depth (such as those aroused by binocular disparity of retinal images). If we do not accept the theory of 'perceptual judgments,' we must conclude that such factors as binocular cues to distance affect seen size as directly as they affect seen distance. In the same way, we must suppose that sensory cues indicating illumination and illumination gradients affect the seen brightnesses of objects as directly as do the brightnesses of their retinal images.

We are led then to think of the visual cortex to some extent as acting as a whole in determining the properties of parts of the visual perceptual field. This whole activity is not, however, confined to the visual cortex since we perceive visually characteristics of objects whose sensory origin is not visual. Thus we see the surface of the table as smooth and hard, that of a carpet as rough and soft, a slug as slimy, and so on. These appearances certainly form part of our visual world and no effort of ours can get rid of them, although they are appearances referring to properties which come from other sense organs than the eye. Their appearance in the visual world can be best explained by supposing that not only do various parts of the visual cortex contribute to the visual appearance of a particular object, but also sensory areas of the cerebral cortex other than the visual areas.

The converse fact that visual cues may contribute to other sense modalities than the visual is shown by the size-weight illusion. If we ask a subject to compare the weights of two canisters, a small and a large one, both weighing fifty grams, he reports that the smaller one has the greater apparent weight. The sense of resistance has, therefore, been determined not only by the sensory impulses from the muscles and joints but also by visual cues. Again we must remind ourselves (as in all the appearances which are here being discussed) that we are dealing with the relative phenomenal weights of the two canisters and not with judgments about their actual weights. If the subject puts the two canisters on opposite sides of a balance he will see that their weights are equal, so he will no longer judge that they are unequal. They will, however, still feel unequal to him. The phenomenal difference in weight contributed by the visual factor does not disappear when he has correct knowledge of the actual weights. This contribution from vision is a genuine determinant of phenomenal weight.

IV. Individual Differences in Visual Perception.

Let us now return to the experiment with the inclined ellipse to note a particular feature in it which is, I think, a characteristic of the perceptual processes that has often been ignored. This feature is the wide range of individual differences. Apart from such obvious differences as errors of refraction, colour-blindness, etc., the optical system of different individuals' eyes and consequently the conditions of local physiological stimulation on the retina for a given arrangement of external objects is very much the same. The perceptual responses of different individuals are, however, widely different, so that any two of us in the same physical surroundings may create from them a very different phenomenal world.

If two or three people perform the experiment I have just described, we shall find that the height at which they say the apparent shape of the inclined ellipse is circular is different to an almost incredible extent. One may see the ellipse as circular when his head is only a few inches from the table so that his retinal image is of a very much flattened ellipse, while another sees the ellipse as circular when he is looking well down on it so that his retinal image is itself not very far from circularity. The first individual shows a very great effect of the real shape of the ellipse in determining its apparent shape, the second shows a relatively smaller effect of the real shape on apparent shape.

It is true that the exact height at which each subject reports the ellipse as looking circular is somewhat variable and may depend to some extent on his mental attitude, but the limits within which variation occurs in any one individual are small compared with the differences between different individuals. If a subject showing little effect of the real shape on apparent shape look at the ellipse from the height at which one with small effect sees it as circular, he will report that by no effort of imagination can he make the ellipse look circular at that inclination, and he will generally add that he does not believe that any one else can.

That these are real individual differences and not merely accidental variations in measurement is shown by the fact that they show great consistency from one time to another. I once retested, after an interval of two years, a group of twenty-five subjects for each of whom I had measured the apparent shape of an inclined object. They differed widely amongst themselves at each test, but the agreement between the two sets of tests was extraordinarily high. The coefficient of correlation was $0 \cdot 92$, which is as high as one expects to get in psychological measurements.

There are, then, genuine and large individual differences between different persons in the apparent shapes of inclined objects. We may add that there are similar individual differences in the apparent sizes of objects at different distances and in the apparent whiteness of objects under different illuminations. In both of these cases, the same general law holds. If an object is moved to twice its previous distance from our eyes, it does not look half its previous size. It may, for different individuals, look three-quarters of its previous size or nineteen-twentieths. With rare exceptions (which I shall mention later) the law holds that the apparent size is in between the retinal size and the real size. In the same way, if a piece of white paper is put into shadow so that it reflects less light to the eyes than a brightly lighted piece of black paper, it does not necessarily look less white than the black paper, although it may do so if the shadow is very deep. The seen whiteness is in between the 'real' whiteness and the stimulus intensity of the retinal image. Again, in this tendency to see objects in their real whiteness irrespective of illumination, we find wide individual differences.

These effects have been generally described under the name 'the constancy tendency.' I do not much like this name since the distinctive feature of these effects is not that phenomena (or appearances) tend to remain constant while stimuli change. It is easy to arrange an experiment (such as that of the inclined lantern screen already mentioned) in which the stimulus remains constant and the phenomenon changes. A more fundamental feature of the effects seems to be that phenomena are determined not only by local stimuli but also by the perceived 'real' characters of the objects causing the stimulus. I have, therefore, suggested that we should call these effects 'the tendency to phenomenal regression to the "real" characters of objects.' I have no wish to quarrel with those who prefer the term 'phenomenal constancy,' but it is convenient to stick to one name, so for the purpose of the present address, I shall speak of 'phenomenal regression,' and I shall call the three effects above described: phenomenal regression for shape, for size, and for colour, respectively.

When we include under one name (whether 'constancy' or 'phenomenal regression') these three tendencies to see objects more or less in their real shapes, sizes, and colour, irrespective of their inclination, distance and illumination respectively, we are implying that these three effects are all of the same nature. For this assumption, we need better evidence than the mere fact that all three can be described in similar terms. The direction in which to look for this evidence is suggested by the fact that in all of them there are wide individual differences. If now the individual who shows a large tendency to see things in their real sizes tends also to see inclined objects near to their real shapes and objects in different illuminations near to their real albedos, we have positive evidence to justify the natural suspicion that these are simply different aspects of one general tendency.

We can easily determine by experiment whether or not this is the case. If we test a group of subjects in their tendency to phenomenal regression for shape, for size, and for whiteness, we find that those who have a large tendency to see the 'real' size of an object tend also to have a large tendency to see the 'real' shape and the 'real' whiteness. The correlations between these tendencies are about 0·6, which shows that they have a considerable factor in common. We can thus speak of individuals as having high phenomenal regression if their perceptions of apparent shape, size and whiteness are largely determined by the 'real' characters of the objects looked at, while those whose perceptions are determined relatively more by the conditions of retinal stimulation (i.e. who see objects getting much smaller as they go farther away, and so on) we shall describe as those of low phenomenal regression.

It may seem fantastic to suggest that there are such large individual differences in the way the world looks to different people since certainly most people are unaware of these differences until they have been shown to them by experiment. This, however, should not surprise us when we remember that most psychological individual differences remain unsuspected until revealed by measurement. The enormous individual differences in imagery, for example, are not generally known, and most people imagine that others have much the same equipment of imagery as themselves. In the same way, a colour-blind person is rarely aware of the difference between his own and other people's colour perception unless his attention is drawn to it by his inability to perform some task such as that of recognising the difference between red-covered and green-covered wires in a cable.

So it is with phenomenal regression. The shapes and sizes of objects in the phenomenal world differ widely for two people from the same view-point, but they are not aware of this difference since neither can see the world through the other's eyes and they have generally no occasion to discuss apparent shapes and sizes. One can, however, easily start a dispute between a group of people when driving through the country by asking them whether the cows in a distant field look larger or smaller than a sparrow perched on the hedge. The answers will be very different and each will think that he is giving the only reasonable answer and that others must have misunderstood the question. They will not easily understand that the origin of the different answers is that the phenomenal world does really look different to different persons.

We may ask the question : Are there any laws governing these changes of phenomena with changing distance, inclination, and illumination, other than the fact that they differ from one individual to another (a fact which suggests lawlessness rather than law) and that their amounts tend to be approximately the same for any one individual ? It might be possible that there were no invariant relationships within these individual variations, and, however unsatisfactory that situation might appear to the scientific mind, it might be necessary to accept it. It does not appear, however, that things are as bad as this. I have found one invariant relationship, within a certain range of distances, for phenomenal size.

If we measure, for a given individual, the apparent size of a disc at different distances we find that its linear dimensions decrease as the distance becomes greater, this decrease becoming slower as the distance is increased (as, of course, do the linear dimensions of the retinal image). The decrease of apparent size is always much slower than the decrease of retinal size (in accordance with the principle of phenomenal regression), and with those individuals who have high phenomenal regression it is much slower than with others. If apparent size is plotted on a graph against distance, therefore, we have a curved line, which, so far, gives us no law.

If we now plot on a graph the apparent linear size of the disc at various distances for any one individual against the stimulus size (that is, against a size proportional to the retinal projection), a law emerges. All the points plotted fall on a straight line inclined upwards from the P axis. If projected backwards towards the P axis it would not pass through the origin but would intersect the P axis above the origin. This means that the relation between phenomenal and stimulus linear dimensions of the object at different distances can be expressed by the equation $P = a + b \, . \, S$.

Expressing this in words, we may say that for any one individual under uniform conditions of observation, the apparent linear dimensions of a disc at different distances change as if they were made up of the sum of two parts, one of which remains constant at all distances while the other is inversely proportional to the distance.[1]

If this were true for all distances it would lead to the odd conclusion that at no distance, however great, would the apparent size of an object be reduced to less than a certain amount (that represented by a in the above equation). This appears to contradict common experience, since we know that if an object is far enough away its apparent size can be reduced to zero.

The above relationship was found for objects at distances ranging from 30 to 400 cm. from the observer. The next step was to investigate the effect of still further increasing the distance. The law appeared to hold up to a distance of 10 m. One subject was tested beyond this point, and it was found that at greater distances than about 10 m., apparent size decreased with distance more rapidly than the law would indicate. No great reliance can be placed on this limit of 10 m., since it was determined for one subject only, but accepting this provisionally as the limit of operation of the law, we must restate it in the form that $P = a + b \,.\, S$ for distances of an object not exceeding 10 m.

There are, no doubt, other regularities of phenomenal regression which it will be possible to express in the form of laws. I think it is encouraging to discover that, in spite of individual differences, the relationships within phenomenal space are not so chaotic as might at first sight appear.

It seems most likely that the tendency to see the real characters of objects is one that increases through life, being least with young children. That seems to be indicated by common experience. Many of us may have noticed that young children are disappointed in the size of large objects when seen at a distance. I vividly remember my own disappointment when about forty years ago I first saw lions and elephants going along the street to a Barnum and Bailey's circus and found them contemptibly small. I think now that this was because I was at such a distance that their retinal images really were small. If I had seen them close up I think they would have appeared satisfactorily large. Since then I have noticed similar disappointments in other children. A small boy of about six seeing wild red deer for the first time at a distance of about 400 yd. said: 'Are those deer? They only look as big as rabbits.' On another occasion he was taken to see the *Queen Mary* at the other side of the Clyde. He maintained that it was not big, not so big as a tug which was passing near his side of the river.

This is only anecdotal evidence of no scientific value. What experimental evidence is there on the subject? On the whole, the experimental evidence seems to support the expectations aroused by common observation. Working with adult male subjects, I found a tendency for phenomenal regression to increase with age. The group used was small (36) and the significance of the result was not sufficient for strong conviction

[1] An exception to this law is to be found in the rare cases of 'anomalous phenomenal regression' who, over a certain range of distances, see objects as larger when their distance is increased.

of its validity. Other evidence, however, points in the same direction. Beyrl found that children showed a greater tendency to see objects in their real sizes from two to ten years and that this tendency was even larger in an adult group. Brunswik found an increase in the tendency to see real whiteness through childhood at any rate up to the age of fourteen years.

Against these findings, we have Katz's denial of an increase with age based on experiments of Burzlaff who found that it was possible to devise an experiment on size perception in which objects were seen as their real sizes at very early years. I do not think this experiment is relevant to the question. If the size experiment can be so arranged as to give complete phenomenal regression for early years, this way of doing the experiment is unsuitable for determining whether or not the effect increases with age. A real increase may be masked by an unsuitably designed experiment.

On the whole, it looks as if the tendency to phenomenal regression does increase with age. This suggests that the tendency is to some extent plastic to experience. This suggestion is considerably strengthened by the observation that although the tendency to see things in their real shapes and sizes was not absent in a group of artists, it was significantly less than in a control group of corresponding age. This might be explained not as due to the artists' acquired habit of reacting to the stimulus characters of objects but as the result of selection, those of high phenomenal regression not being likely to become artists since this characteristic handicaps them in learning to draw in perspective. The second explanation seems, however, to be ruled out by the further observation that there is no such difference to be observed between a group of art students and a control group of university students. The decisive factor in lowering the tendency to phenomenal regression in the artist group must, therefore, be the greater length of time during which they have formed the habit of reacting to the stimulus characters of objects.

It is, nevertheless, very improbable that the tendency to phenomenal regression is wholly the product of experience. Köhler has shown that it is, at any rate, found amongst animals with less highly organised nervous systems than our own, since both chimpanzees and hens could be trained to take food from the whiter of two greys even when it was so much less illuminated than the other that its stimulus intensity was lower than that of the darker grey. Phenomenal regression to real colour is also reported to have been observed in fishes and in chicks of three months old.

These facts have been claimed to disprove an empiricist theory of the origin of those properties of perceived space which determine phenomenal regression (e.g. by Koffka). This claim can hardly be maintained, since the empiricist may retort that we know nothing of the possible speed of learning of spatial relations or of the level of nervous development at which this learning is possible. We can, however, agree that these facts render the empiricist theory somewhat improbable. They do not, however, even offer an argument against the view that the tendency to phenomenal regression may be influenced though not originally produced by experience. The hypothesis that I would suggest is that a phenomenal space so organised in its properties as to produce the phenomenal regression effects is born with us, but that individual differences in the amount of phenomenal regression are largely or wholly determined later by individual experience. If phenomenal regression is to be regarded as a product of the organisation of individual phenomenal space, there may be other aspects of spatial organisation whose changes run parallel to the changes in phenomenal regression. It would, for example, be interesting to know whether there is a parallel change in size contrast effects with increasing years.

V. Practical Consequences.

It may be asked whether the kind of thing we have been talking about has any practical importance. It certainly may have. We test for such differences in the sensory physiology of the eye as colour-blindness because they may lead to practically important incapacities, and it is very likely that individual differences in the cerebral side of perception may also affect an individual's practical capacities. Some years ago I suggested that a person of high phenomenal regression might be expected to drive a car more easily through traffic than one with low. He sees a gap in the traffic in something near its real size before he drives up to it, whereas the person with low phenomenal regression sees it as smaller than it really is when it is at a distance. Neither, of course, adjusts his driving to the apparent size of the gap ; both must make a judgment as to its real size. The person with low phenomenal regression has, however, a much larger gulf between appearance and reality to bridge by means of judgment. Judgment being a slower and more uncertain process than perception, he may be expected to drive through gaps with more difficulty and less certainty than the individual who can trust to his immediate impression of size. The individual with high phenomenal regression may therefore be expected to drive more easily and better through traffic. This prediction appears to have been justified by a research in motor driving by the National Institute of Industrial Psychology, who found that a test of phenomenal regression showed a correlation with driving ability.

It has already been mentioned that some individuals show the odd peculiarity of seeing objects as larger when they go farther from the eyes. This may be called 'anomalous phenomenal regression' or 'superconstancy.' It seems to be rare. I have found two cases in the course of testing over two hundred subjects. These showed the peculiarity (each over a certain range of distances) repeatedly and under different conditions of testing, so there is no doubt of its reality. One subject who was presbyopic was tested with and without glasses and gave identical results, so it is not due to any peripheral defect of vision. Theoretically this condition is interesting. It seems strongly to indicate that the explanation of phenomenal regression is not to be found wholly in experience, since it is inconceivable that any experience should have taught the subject that, of two objects casting equal retinal images, the nearer is the larger. I can suggest no explanation. All that we can say at present is that the organisation of these individuals' phenomenal space is such that there is a reversal at some distances of the ordinary condition that an object producing a retinal image of a certain size is seen as larger when it is located at greater distance from the observer.

The rarity of this condition might seem to make it of little practical interest. It may, however, be of considerable importance in motor driving. Following the same argument as before, we may see that the probable effect of this condition will be that the subject driving a car will see a gap at a distance as big enough to get through when, in fact, it is too small. Unless he has learned to make correct judgments to counteract this perceptual peculiarity, this would be as dangerous a condition for the motor driver as one can imagine. Further research is necessary, but at present there are indications that the condition may be dangerous. One of my subjects did drive a car but admitted having smashed a wing in getting through a gate. The other had found himself unable to learn to drive because he drove into gaps where he had no room to pass, and I have had one other case like the last reported to me. A condition which may be lethal to the motor driver is not unimportant even if it is found in only about 1 % of individuals.

The effect of drugs on individual organisation of phenomenal space is an interesting problem. I have made only preliminary experiments on one subject in the hope that someone better equipped to experiment on drugs will take the enquiry further. The indication I obtained was that (as might be expected) alcohol decreased phenomenal regression while caffeine increased it. I think that it might be worth while for those investigating the effect of alcohol on motor driving to consider the possibility of disturbance of spatial perception as well as of speed of motor responses. That a change of spatial organisation can affect driving I am sure from personal experience. I was driving one night towards Buxton suffering from the effects not of alcohol but of fatigue (which probably affects spatial organisation in the same way as alcohol). At one point, I found my perception of the road so much disturbed that I had to stop my car and get out. The road seemed to narrow almost to a point in front of me; I seemed to be driving not on a parallel-sided track but into a funnel. I recognise the condition now as one of extreme reduction of phenomenal regression. One result of this condition was an almost irresistible impulse to drive in the centre of the road. A persistent tendency to drive on the crown of the road is a common fault of many drivers. I suggest that it may be a fault characteristic of an individual with low phenomenal regression, and that if this were proved to be its origin, an understanding by the driver of the cause of his fault would put him into the way of correcting it.

Mist and fog may disturb the perceptual world even of the motorist who is wise enough to avoid the effects of alcohol and fatigue. It is a matter of common experience that the apparent sizes and distances of objects undergo strange changes even in a slight mist. Exact measurement of the effects of fog and of such veiling glare as that of headlights shining through mist have been made by Martin and Pickford.

I have one last indication of a possible practical importance in individual differences in phenomenal regression for which I am indebted to Dr. S. Bernfeld. He had a patient suffering from anxiety. One of her causes of fear was the change that took place in the sizes of objects as they approached her. An object at a distance looked very small, but became terrifyingly big as it approached her. Dr. Bernfeld was not able to make any experimental measurements of the variation for this patient of seen size with distance, but the condition is clearly recognisable from her description as one of extreme loss of the tendency to phenomenal regression. As to why approaching objects should look terrifying if phenomenal regression is very small, we can only guess. There is one observation in biology which suggests a clue. It has been pointed out to me by Dr. Cott that many creatures protect themselves from their enemies by sudden increases of size. It looks as if sudden increase of size of an object may be one of the situations innately provocative of fear. We might even be tempted to speculate that one reason for the development of phenomenal regression might be as a protection of the individual against the fear-provoking situation of approaching objects increasing in apparent size. There seem to me to be other more likely explanations of the biological function of phenomenal regression to real size. I mention this only because it seems interesting to explore all possibilities, the improbable as well as the probable.

Conclusion.

The change that has taken place in the psychological study of vision during the last twenty-five years may be expressed in a summary way as a change from the time when it was treated as if vision were a function of the eye alone to a time when the eye and higher centres are regarded as co-operating in visual perception. The psychology of vision is not and cannot be merely the sensory physiology of the eye. At the present time, these wider aspects of visual perception offer a more fruitful field of research than do those of sensory physiology which have been so adequately dealt with in the past. Particularly, I should like to suggest that individual differences in visual perception and the statistical study of these differences is a field whose surface has hardly yet been scratched. Let us hope that, in the next twenty-five years, psychologists may be as successful in resolving the many remaining problems of visual perception as were the great Helmholtz and his contemporaries in making a scientific study of the sensory physiology of the eye.

SECTION K.—BOTANY.

THE GENERAL PHYSIOLOGY OF THE PLANT CELL AND ITS IMPORTANCE IN PURE AND APPLIED BOTANY

ADDRESS BY

PROF. W. STILES, F.R.S.

PRESIDENT OF THE SECTION.

ALTHOUGH few subjects are of more importance to the life of man than the physiology of plants, the investigation of this physiology is a field of endeavour which has attracted attention only comparatively recently. A few men, it is true, working in the eighteenth century with the scanty equipment of physics and chemistry then available, did indeed lay the foundations of the science of plant nutrition, but any deep insight into the mode of working of the plant was impossible until, on the one hand, the development of microscopical technique had rendered possible the determination of the internal structure of the organism, and, on the other, the physical and chemical sciences had so far developed as to provide the botanist with information which was fundamental for any sort of understanding of what took place in the plant. While now the structure of the plant is largely known, the work of the physiologist still waits on the work of the physicist and chemist, and must, of necessity, lag behind development in physics and chemistry. Occasionally, indeed, the botanist, impatient of this state of affairs, has taken matters into his own hands and has advanced into the field of the physicist and chemist; perhaps the outstanding example of this is afforded by the work of the botanist Pfeffer in attacking the problems of osmotic pressure, but this is not an isolated case. It is thus no accident that any depth of knowledge of the physiology of plants has been acquired in comparatively recent times.

The first development in knowledge of the physiology of plants was, as I have already indicated, in the field of plant nutrition. It was made clear during the eighteenth and first half of the nineteenth centuries that the plant absorbed certain substances from its environment and that from these substances the plant body was built up. That the different organs of plants had different functions in this respect also became clear. But up to the middle of the nineteenth century the actual processes taking place in the building up of the plant body from the materials absorbed from the environment were not understood in the least. Nor is this to be wondered at. We know very well now that the complex activities of the plant are related to the protoplasm, but although the cellular structure of plants had been recognised with the work of Hooke, Grew and Malpighi in the seventeenth century, the discovery of protoplasm and appreciation of the supreme importance of this substance only dates from 1835, when Dujardin described the protoplasm, or sarcode, as he called it, of animal cells. The importance of protoplasm in the plant appears not to have been recognised until 1846. The credit for this is due to von Mohl, although he cannot, as Sachs asserted, be credited either with the discovery of protoplasm or with the invention of its name.

In spite of the fact that by the end of the first half of the nineteenth century the connection between protoplasm and life must have been evident, little attempt was made for many years towards a serious investigation of the properties of this substance, or to determine the general activities of the protoplasm as manifested by every living cell. This may have been due partly to the traditional outlook on plant physiology, which had emphasised the nutritional relationships, and partly to the impetus given to other aspects of botany by Hofmeister's work on the life history of plants and by the publication of the *Origin of Species*, which diverted thought on botanical matters to problems of descent and comparative morphology. And yet even as far back as 1828 Turpin had put forward the idea that the cell is the elementary primary organism, and the plant built of these cells a sort of community or colony of such elementary organisms. This view appears to have enjoyed considerable popularity for a time. It is found in Meyen's *Neues System der Pflanzenphysiologie* published in Berlin in 1837, and was held in no uncertain fashion by as distinguished a botanist as Schleiden, and even appears as late as 1861 in the fourth edition of his *Grundzüge der wissenschaftlichen Botanik*, although it should be pointed out that this was no more than an unaltered reprint of the third edition of 1849. Such a view is, of course, as wrong as one which takes no account of individual cell activities. Phenomena of nutrition, development and irritability combine to refute such an idea.

There are, nevertheless, activities which are characteristic of all living cells. They all exhibit respiration in the sense of a release of energy from substances present in the cell, a release which in the very great majority of plants is brought about by the oxidation of carbohydrate or fat. Again, all living cells are capable of absorbing water and dissolved substances, and of giving out these materials under certain conditions. For an understanding of these activities a knowledge of the system in which they take place is important, so that the study of the cell system, and in particular of the protoplasm, comes within the sphere of inquiry of the general cell physiologist. This includes the study of enzymes, which undoubtedly plays an extremely large part in protoplasmic activity.

Thus investigations in general cell physiology fall for the most part into four groups, namely (1) those concerned with the chemical and physical constitution of the protoplasm and other cell constituents; (2) the study of enzyme action; (3) those dealing with absorption and excretion of water and dissolved substances which have, for the sake of convenience, generally in the past been referred to as problems of cell permeability; and (4) those concerned with respiration. The first two are largely biochemical studies, and it is with the more purely physiological problems of respiration and salt and water relations that I propose mainly to deal in this address.

Inasmuch as investigations into these various aspects of vital activity involve different techniques, it is not surprising that our knowledge of them has developed to a large extent independently. They are, of course, not independent, for it must be realised that the functioning of the plant machine depends on the harmonious working together of all plant processes. And that the various aspects of our subject mentioned above are closely connected is now, for the most part, fairly clear. It is obvious that, since all vital activity depends on the presence of protoplasm, the chemical and physical constitution of that substance must determine that activity, while, coming to details, many investigators have sought to connect the passage into and out from the cell of water, and particularly of dissolved substances, with the existence of limiting plasmatic layers of a constitution different from the inner part of the protoplasm. The composition of the vacuole determines without doubt to a very great extent the absorption of water by the vacuolated cell. This absorption is, however, also dependent on the presence of protoplasm, since when this is destroyed, the water relations of the cell are quite altered. Enzymes are only produced by living tissues and their production presumably depends on the presence of protoplasm. Respiration, again, is a function of living cells, and, although exact proof may still be wanting, it is almost certain that enzymes are intimately concerned in the respiratory process. That the solute relations of cells are intimately connected with respiration is not so obvious, but that this is probably the case I indicated first in 1927, and later work has left this connection in little doubt, although the nature of the connection is not so evident.

With regard to the protoplasm itself, it is now generally recognised that it forms a colloidal system of probably a number of phases in which the chemical constituents have as their basis water, proteins and complex fatty substances. Carbohydrates are also constantly present in protoplasm, but whether they are to be regarded as dead inclusions playing no essential part in vital behaviour, or whether they are necessary for the maintenance of protoplasm in its living condition, is not clear. Nor is it at all clear how these various substances are distributed among the various phases. It is sufficient indication of the doubt that surrounds the problem of the constitution of protoplasm that one authority on it emphasises its generally low viscosity, while another lays stress on its slimy character. One regards it as having the character of a suspensoid, another that of an emulsoid. If we accept the reports of observations on the physical qualities of protoplasm, and there is no reason for supposing that the majority of such observations are incorrect, we must inevitably conclude that protoplasm varies very considerably in its constitution from one object to another, and probably in the same material at different times. In general, however, there is little difference to be observed under the microscope, or even the ultramicroscope, between samples of protoplasm from widely different materials, yet there must be fundamental differences between the protoplasm of different species. What these differences are, whether they are subtle chemical differences or differences in arrangement of molecules or molecular aggregates, is a matter on which our ignorance is exceptionally complete. Indeed, it looks very much as if some altogether new technique is required for any advance to be made in this direction, and in this connection I may refer to some remarks made by Sir William Bragg last November in his Anniversary Address to the Royal Society. He pointed out that there is to-day a very considerable interest in magnitudes which are too small to be examined under the microscope and too large to be studied conveniently by X-ray methods. While with the microscope it is possible to observe the presence of particles with a diameter of about $0 \cdot 15\mu$ (1,500 Å.U.) and while the ultramicroscope can reveal the presence of particles as small as $5m\mu$ (50 Å.U.), neither the microscope nor the ultramicroscope can reveal any details of structure in objects as small as this. On the other hand, X-ray methods have enabled the arrangement of atoms and molecules to be determined with great accuracy, but they do not enable the details of larger structures to be determined. The invention of a method for determining the structural details of particles larger than those with which X-ray analysis can deal, and which are yet too small for the microscope to resolve, could not fail to provide the general physiologist with a powerful weapon with which to attack the problem of protoplasmic constitution.

At present, then, we must be content with recognising in the protoplasm a system in which an essential feature is the possession of a large internal surface, with all that this involves, in which there are various phases of different chemical composition, a composition roughly but by no means accurately known. One of the characteristics of this system is that, in so far as it can be regarded as a system in equilibrium, it is in a state of dynamic, not static, equilibrium, for all the time it is absorbing oxygen and giving out carbon dioxide. The process does not end in this, for it involves the loss of material, if not from the protoplasm itself, from material held in the protoplasmic complex, and this material must therefore sooner or later be replaced, so that the respiration process must in any case be linked up with a movement of material into the cell, from the outside environment, either directly from this or through the medium of some other cell or cells of the plant body, most frequently indeed also after profound chemical changes in the material so absorbed.

That plants, like animals, absorb oxygen and give out carbon dioxide was recognised by Ingen-Housz and de Saussure towards the end of the eighteenth century, but for long the greatest confusion of ideas prevailed on this question, a confusion which was only dispersed by Sachs with the publication in 1865 of his book on the *Experimental Physiology of Plants*. Sachs not only made clear the parts played by the respective gaseous exchanges involved in photosynthesis and respiration, but he laid stress on the universality of the respiration process, and emphasised the fact that it is a property of every living cell. Thus, in his text-book of Botany published in 1868 he wrote: 'The respiration of plants consists, as in animals, in the continual absorption of atmospheric oxygen into the tissues, where it causes oxidation of the assimilated substances and other chemical changes resulting from this.' And further: 'But in all the other organs also—in every individual cell—respiration is constantly going on; and it is not merely the chemical changes connected with growth that are dependent on the presence of free oxygen in the tissues; the movements of the protoplasm also cease if the surrounding air is deprived of this gas; and the power of motion possessed by periodically motile and irritable organs is lost if oxygen is withheld from them; but if this happens only for a short time the motility returns when the oxygen is again restored.' This is, as far as I am aware, the first reference to the fact that respiration is a constant function of cell activity. Although this doctrine has been accepted without adequate proof, it must be admitted that no evidence has been adduced against it and it has never been disputed, and it is clear that respiration is as much part of the field of general cell physiology as the absorption and excretion of water and dissolved substances. Although Sachs is thus to be credited with establishing the view of the constant incidence of respiration in cell activity, neither he himself nor his pupils contributed a great deal towards the elucidation of the respiratory mechanism nor to the part played by it in cell activity, although a few important investigations were made in his laboratory, and later in that of his pupil, Pfeffer. The greatest activity in this field was displayed by Russian investigators, and, commencing in 1875, there has issued a constant stream of records of researches on plant respiration from Russian workers, among whom Palladin and Kostytschev were particularly conspicuous.

As work on respiration proceeded it came to be more and more supposed that enzyme activity played a leading part in the respiratory process. But it must be admitted that whereas during recent years considerable advances have been made in elucidating enzyme systems in animal cells and relating them to the processes of respiration, the part played by such systems in plant respiration is still rather a matter of speculation than of indisputable fact. Indeed, in spite of much careful and painstaking work, our knowledge of the enzyme systems themselves is still very chaotic.

If experimental observations have been interpreted aright, and if it is a fact that every living cell respires, then we must conclude that respiration is something most inextricably connected with life. Yet it seems to me that explanations of the function of respiration in the plant are not altogether satisfying. Let us examine the views of the great authorities of the past on this question. Sachs wrote: 'The loss of assimilated substance caused by respiration would appear purposeless if we had only to do with the accumulation of assimilated products; but these are themselves produced only for the purposes of growth and of all the changes connected with life; the whole of the plant consists in complicated movements of the molecules and atoms; and the forces necessary for these movements are set free by respiration. The oxygen, while decomposing part of the assimilated substance, sets up important chemical changes in the remaining portion, which on their part give rise to diffusion currents, and these bring into contact substances which again act chemically on one another, and so on. The dependence on respiration of the movements in protoplasm and motile leaves is very evident, since, as has been mentioned, they lose their motility when oxygen is withheld from them. These considerations lead to the conclusion that the respiration of plants has the same essential significance as that of animals; the chemical equilibrium of the substances is being constantly disturbed by it, and the internal movements maintained which make up the life of the plant.' That is, the function of respiration is to provide the energy for plant movements and for the building up of materials of higher energy content than the assimilates synthesised in photosynthesis. Pfeffer's statement is less precise. 'A continual supply of energy is necessary for the maintenance of vital activity, and hence the possibility of aerobic or anaerobic respiration is a primary essential for all vital processes, including those which do not involve any direct consumption of kinetic energy.' This is simply a statement that for a plant to remain alive a continuous supply of energy is necessary, and that this is provided in respiration. Palladin's view was much the same as Sachs's. 'Plants grow, and in growing they produce various metabolic changes and movements of materials. It thus comes about that work of various kinds is performed in living plants, and this necessitates the consumption of energy. . . . The processes of living plants in which organic reserve substances are oxidised by oxygen are quite analogous to combustion, and this vital oxidation is known as respiration.' Kostychev regarded respiration as yielding the necessary release of energy for important vital processes, and he pointed out that 'most syntheses of organic substances, such as the synthesis of proteins,' and 'the various types of architectural processes of tissue differentiation' require a supply of energy, although it must be admitted that what exactly he meant by the latter group of processes which, as far as energy requirements were concerned, were not included in the former, is not obvious. At any rate it is quite clear that the general view of respiration, put in as precise terms as possible, is that it provides energy for certain plant movements, and for the building up of substances of higher energy content than the products of photosynthesis which serve as the substrate.

While it is not clear that all plant movements obtain the necessary energy for their occurrence from respiratory activity, no doubt some do, and there is every reason to believe that the energy required for the production of various constituents of the plant arises from the same process. But having agreed to this, can we really be satisfied that we have obtained a complete explanation of the function of respiration? In the case of germinating seeds, growing organs, the formation of flowers and fruit, this view seems completely adequate, but we must remember that storage tissues such as potato tubers and carrot roots respire at a by no means negligible rate, and that the same is true of senescent organs such as mature fruit. Indeed, such tissues, notably those of the apple, have provided some of the most interesting data of plant respiration. With what movement, or with what synthesis of materials, is respiration of the cells of a mature apple concerned? Such considerations lead one to wonder whether respiration is not concerned in some much more subtle way with the maintenance of life. It does look as if the mere maintenance of the protoplasm in a living condition depends on the continuous occurrence of these processes which manifest themselves in the oxidation of organic material to carbon dioxide and water by means of absorbed oxygen. The only exception to this rule is found in certain so-called 'resting' organs, such as seeds, in which the amount of water present is very low, and in which, presumably, the protoplasm is in some very different state from that of active cells.

If we cannot answer this question we can, at any rate, attempt an examination of the functions of respiration of which we feel more certain. The most universal of these, as we have seen, is the provision of energy for the building up of materials of higher energy content. A problem which awaits solution here is the mechanism by which the energy released in the oxidation of the substrate is transferred to the actions bringing about the synthesis of proteins and other complex plant constituents. The solution of this problem no doubt involves that of what is generally known as the mechanism of respiration, that is, the stages in the process, the enzymes involved, the conditions of the process : in fact, the general course of the breakdown of substrate into carbon dioxide and water. The assumption is usually made that the breakdown of sugar follows the same course in its earlier stages as in its fermentation by yeast, in which, according to the theories of Neuberg and of Embden and Meyerhof, pyruvic acid, $CH_3CO.COOH$, is an intermediate product, and in which enzymes of the zymase complex play a leading part. Both in presence and absence of oxygen the course of the breakdown is supposed to be the same up to the splitting of pyruvic acid under the action of the enzyme carboxylase to acetaldehyde and carbon dioxide. The carbon dioxide evolved in this action accounts for the whole of this gas evolved in the absence of oxygen, and for one-third of the carbon in the sugar broken down, the other two-thirds, contained in the acetaldehyde, finally appearing as ethyl alcohol. If the rate of sugar breakdown, or glycolysis, remains the same in both presence and absence of oxygen, then the ratio of anaerobic to aerobic respiration depends on the fate of the acetaldehyde in air. If none of this appears as carbon dioxide the ratio is unity, if all the carbon contained in it appears as carbon dioxide the ratio is 1 : 3, and it must be supposed that if the ratio exceeds this value some of the acetaldehyde is built either into some fresh product or back into the system. Indeed, this was realised as long ago as 1880 by Wortmann, who actually found with seedlings of *Vicia faba* that the rate of carbon dioxide evolution was the same in presence and absence of oxygen. To explain this finding he put forward a theory that in air the alcohol produced in the first stage of the process is converted back to sugar, so that the whole of the respiratory process can be summarised by two equations which he wrote as follows :

$$1.\quad 3(C_6H_{12}O_6) = 6(C_2H_5OH) + 6CO_2.$$
$$2.\quad 6(C_2H_5OH) + 12\ O = 2(C_6H_{12}O_6) + 6H_2O.$$

The second equation he also wrote :

$$3.\quad 6(C_2H_5OH) + 12\ O = 2\ \underbrace{\begin{pmatrix} C_2H_4O_2 \\ C_2H_4O_2 \\ C_2H_4O_2 \end{pmatrix}}_{2(C_6H_{12}O_6)} + 6H_2O.$$

In short, he accounted for the fact that the same amount of carbon dioxide was released under anaerobic conditions as under aerobic conditions, by supposing that two-thirds of the sugar broken down was re-synthesised in presence of oxygen into sugar.

But there is also the possibility that the rate of sugar breakdown is affected by oxygen. According to F. F. Blackman's analysis of the observed data of respiratory activity of apples in air and pure oxygen, there is evidence that the rate of production of the active substrate for glycolysis from stable sugar is increased with increase in oxygen concentration. J. K. Scott and I have obtained what may be further evidence of the possibility of this from a consideration of the relationships of respiration to surface and volume in bulky tissues. Here, owing no doubt to the low oxygen tension in the middle region of such tissues, there is a low rate of respiration, although there may actually be no indication of anaerobic respiration. The observed results can be explained on the view that the low oxygen tension induces a minimum rate of production of the active substrate and so limits the rate of respiration. It may be noted, as a corollary to this view, that the anaerobic respiration usually observed after a period of aerobic respiration should gradually fall to a lower level owing to oxygen shortage bringing about a lessening in the rate of production of active substrate. Investigations on the quantitative relations between aerobic respiration and anaerobic respiration have made it clear that this is what generally happens, so that it is frequently difficult, or indeed impossible, to fix a value for the rate of anaerobic respiration, since this undergoes changes with time, sometimes rising at first, but always subsequently falling. Researches carried out in my laboratory by J. K. Choudhury indicate, however, that there may be exceptions to this rule.

From the course of anaerobic respiration it is, however, often possible to calculate the initial rate of anaerobic respiration at the moment when the material is first transferred to an atmosphere of nitrogen before secondary effects have come about. This was done by F. F. Blackman and P. Parija in their well-known work on the respiration of apples, and they showed that the output of carbon dioxide at the beginning of a period in nitrogen was actually greater in this case than the output of carbon dioxide in air immediately before transference to nitrogen. From a very careful analysis of the experimental data, Blackman concluded that in respiration in air a large amount of some substance is formed along with the carbon dioxide and water and that this substance does not accumulate but is built back into the stream of katabolites. There is no evidence yet to show whether this substance is actually built back into sugar as Wortmann supposed to be the case in the broad bean, or whether some intermediate substance of the breakdown is formed. It is important to note, however, that here also evidence of an anabolic process linked with the breakdown is obtained, a process called by Blackman oxidative anabolism since it is dependent on the presence of oxygen. Similar evidence for the existence of oxidative anabolism in storage tissues such as potato tuber and carrot root has been obtained in long series of experiments carried out in my laboratory by W. Leach, J. K. Choudhury and J. K. Scott.

It is more than likely that the investigation of the organic acid metabolism of plants may shed light on our problem. It is well known that in many plants, notably in succulents, but in many non-succulents as well, organic acids such as malic, citric and oxalic, are present in considerable quantities. In the succulents the output of carbon dioxide is usually very small and acid, particularly malic acid, accumulates in the leaves and stems during the night and disappears during the day. It is generally held that the malic acid arises as a product of respiration, but two main views have been put forward to explain the actual part played by this acid in the sequence of actions following the breakdown of carbohydrate. Ruhland and his collaborators hold that this breakdown follows the same course as in normal respiration as far as the pyruvic acid stage. Owing to the inhibiting action of high concentration of acetaldehyde the normal action of carboxylase is inhibited and the pyruvic acid, instead of breaking down under the action of this enzyme to acetaldehyde and carbon dioxide, undergoes synthesis to diketoadipic acid, which then breaks down to succinic acid and formic acid, from the former of which malic acid arises.

$$
\begin{array}{ccccccc}
\mathrm{COOH} & & \mathrm{COOH} & & \mathrm{H.COOH} & & \\
 & & & & + & & \\
\dot{\mathrm{C}}\mathrm{O} & & \dot{\mathrm{C}}\mathrm{O} & & \mathrm{COOH} & & \mathrm{COOH} \\
\dot{\mathrm{C}}\mathrm{H_3} & & \dot{\mathrm{C}}\mathrm{H_2} & & \dot{\mathrm{C}}\mathrm{H_2} & & \dot{\mathrm{C}}\mathrm{HOH} \\
+ & \longrightarrow & \cdot & \longrightarrow & \cdot & \longrightarrow & \cdot \\
\mathrm{CH_3} & & \mathrm{CH_2} & & \mathrm{CH_2} & & \mathrm{CH_2} \\
\dot{\mathrm{C}}\mathrm{O} & & \dot{\mathrm{C}}\mathrm{O} & & \dot{\mathrm{C}}\mathrm{OOH} & & \dot{\mathrm{C}}\mathrm{OOH} \\
 & & & & + & & \\
\dot{\mathrm{C}}\mathrm{OOH} & & \dot{\mathrm{C}}\mathrm{OOH} & & \mathrm{H.COOH} & & \\
\text{pyruvic} & & \text{diketoadipic} & & \text{succinic} & & \text{malic} \\
 & & & & + & & \\
 & & & & \text{formic} & &
\end{array}
$$

Disappearance of malic acid is ascribed to its oxidation to oxalacetic acid, the conversion of this to pyruvic acid, and the breakdown of the latter on removal of the inhibitor of carboxylase.

Bennet-Clark has pointed out a number of objections to this theory, among which perhaps the most important are that formic acid does not accumulate in the tissues, while, so far from succulent plants containing a high concentration of acetaldehyde, the concentration of this substance in succulents is very low (0·01 to 0·001 %), and is, in fact, too low to have any appreciable inhibiting effect on carboxylase.

From his own researches and a critical consideration of the work of others, Bennet-Clark has shown that for each molecule of sugar which disappears from succulents not more than one molecule of malic acid is formed, so that for every molecule of sugar which is lost by glycolysis at least two atoms of carbon must be involved in the formation of some other material. This is not carbon dioxide, and in fact, no carbon compound with one, two, or even three carbon atoms in the molecule accumulates in the tissues, and Bennet-Clark therefore concludes that the carbon compound formed from glycolysis along with malic acid must be built back to polysaccharide. The carbon dioxide evolved by succulents does not come from malic acid, for the rate of carbon dioxide evolution is not proportional to the concentration of malic acid. Bennet-Clark's view of the breakdown of sugar by succulents is thus represented by the scheme:

$$\begin{array}{ccccc} \text{sugar} & & & & \\ \downarrow & & & & \\ \text{intermediate products of glycolysis} & \rightarrow & \text{malic acid} & \rightarrow & \text{polysaccharide} \\ \downarrow & & & & \\ CO_2 & & & & \end{array}$$

and the malic acid is thus an intermediate product of anabolism.

Lack of time prevents a further discussion of this interesting subject of the part played by organic acids in plant metabolism: it must suffice to say that in other plants besides succulents evidence is accumulating which indicates that the part played by these acids in oxidative anabolism may be quite a general phenomenon.

While then data are accumulating which indicate the linkage of anabolic processes with those of the breakdown of sugar, it is important to note that there is no evidence of the formation of products other than carbohydrates. Is it possible, however, that syntheses of more complex substances are indeed involved, and that we have here a dim glimpse of the mechanism for the production of these substances, and that along with the formation of sugar or some intermediate there may be also the formation of protein or other complex substances; that, indeed, we have here the mechanism by which the carbohydrate is brought into a suitable form for combination with nitrogenous and other compounds? If this is so we should expect to find the strongest evidence of oxidative anabolism in actively growing material. It is therefore disappointing that in tissues such as those of germinating seeds the indication of oxidative anabolism is often wanting. In work by Leach on the respiration of germinating seeds of different types it was found that in those seeds storing carbohydrate as their chief food reserve the ratio of anaerobic to aerobic respiration was about 1 : 3 or less, so that in these the experimental data suggest that the same amount of carbohydrate is broken down to carbon dioxide and water in presence of air as is broken down to carbon dioxide and alcohol in absence of oxygen. In seeds which contain a considerable amount of fat the ratio of the initial rate of anaerobic to the previous rate of aerobic respiration was found to be greater than 0·33, and in these, therefore, some oxidative anabolism might take place. On the other hand, a high rate of anaerobic respiration has been observed in other fruits besides apples, and it is curious that the indications of anabolism should appear in just those materials where it would seem to have least meaning. However, many seeds contain a considerable reserve of protein which suffers breakdown, at least in part, during germination. Thus Isaac has shown that in the seeds of the same variety of sweet pea in which Leach found a ratio of anaerobic to aerobic respiration of only about 0·2, there is a continuous breakdown of reserve protein during the first ten days of germination, over a third of the protein disappearing in this period.

While a synthesis of what may be called protoplasmic proteins and other substances is taking place in the growth centres, this synthesis is much less than the breakdown of protein reserves, and it would therefore appear that in such material there is a source of energy available apart from that provided by the breakdown of carbohydrate. Before we can hope to present a picture of the relations between respiration and vital syntheses in germinating seeds, and perhaps in all other material as well, it seems to me that we need not only many more data regarding respiration rates under both aerobic and anaerobic conditions throughout the whole germination period, but also a detailed biochemical analysis of the carbohydrate and various nitrogenous materials present in the seedlings. So expressed, this may sound and look a simple enough matter, but actually, as anyone who has attempted to tackle such problems knows, it is one that abounds in difficulties.

As far, then, as the mechanism by which respiration provides the energy for the formation of compounds of higher energy content is concerned, we are still very much in the dark. There is even the possibility that we are completely wrong in assuming a connection between aerobic and anaerobic respiration. While there is very strong evidence that anaerobic respiration in plants is often similar to fermentation, inasmuch as the substrate and the end products are the same, there are so many exceptions, or apparent exceptions, to the production of ethyl alcohol in the correct quantity demanded by the equation for fermentation, that one may well hesitate in accepting this view as of universal applicability. On the other hand, the opinion of Müller and Lundsgaardh that anaerobic respiration is a process quite distinct from aerobic respiration, in which different enzymes function and in which the course of the breakdown is different from the beginning, has found little support from more recent work. The view of anaerobic respiration as the effect of deprivation of oxygen on the normal aerobic process, appears to me by far the more reasonable one. For if the two processes were completely independent we should expect anaerobic respiration to proceed at all times, in both presence or absence of oxygen, or we should have to assume that oxygen inhibits the breakdown of carbohydrate to carbon dioxide and ethyl alcohol. Now the first hypothesis is untenable because it would mean that in air aerobic respiration took place in addition to anaerobic respiration, so that the output of carbon dioxide under such conditions should always be greater than in absence of oxygen, which is not always the case. Nor do the products of anaerobic respiration normally appear in presence of air. On the other hand, the breakdown of carbohydrate to carbon dioxide and alcohol by the enzyme complex zymase does not appear to be inhibited by oxygen.

While it has generally been assumed that respiration is linked in some unknown way with the synthesis of proteins and other substances, its connection with those other processes, the absorption and excretion of materials which are characteristic of cells, has only come to be appreciated more recently. The absorption and excretion of water and dissolved substances was generally more or less tacitly assumed to be determined by the physical laws of osmosis and diffusion. Water was supposed to diffuse into or out of the vacuole according to the difference between the osmotic pressure of the cell sap and the sum of the osmotic pressure of the external solution and the inwardly directed pressure of the stretched and elastic cell wall. Dissolved substances were supposed to enter the vacuole according to the laws of diffusion expounded by Graham and Fick more than eighty years ago. The method of measuring the rate of entry of dissolved substances by observing the rate of deplasmolysis of plasmolysed cells placed in a solution of a penetrating substance assumes that this substance diffuses unchanged through the protoplasm into the vacuole, where it still remains unchanged and so increases the osmotic pressure of the vacuole approximately in proportion to the amount of it which has entered the cell.

Although Collander's work on the absorption of a number of non-electrolytes indicates that this assumption may, in the case of such substances, be quite justified, it has been known now for thirty years that the entry of electrolytes into cells cannot be explained as the simple diffusion of a substance through a membrane (cell wall and protoplasm) from a region of higher concentration to one of lower concentration. In the first place it was shown that the two ions of a salt could be absorbed at different rates by living cells as long ago as 1909. In that year observations of this kind were published by Meurer and by Ruhland on the absorption of salts by storage tissue (carrot and beetroot) and by Pantanelli, using a great variety of plant material. These observations have since been extended by many others, and it has been established beyond a doubt that, at any rate under certain conditions, the two ions of a salt are absorbed by living cells at different rates. Since the total of positive and negative electrical charges must remain equal in the external solution, it follows that either some other ion must accompany the excess of the more rapidly absorbed ion into the cells, or that some ion of the same sign as the more absorbed one must diffuse out into the external solution to balance the excess of the less absorbed ion remaining. If the former is the case and the external solution is one of a single salt, the solution must become acid or alkaline, since an excess absorption of kation would involve some absorption of the hydroxyl ions of water, leaving some of the anion balanced by hydrogen ions ; similarly, if there is an excess absorption of anion the solution will become alkaline. It was suggested by Pantanelli that this might be the reason why culture solutions sometimes become acid or alkaline. My own experience has been that all plant tissues absorb hydrogen and hydroxyl ions with considerable rapidity, and that solutions containing plant tissue tend to become less, and not more, acid or alkaline. It would be unwise, however, to assume that such is the case under all conditions, and, as far as I am aware, there is no evidence regarding the range of hydrogen-ion and hydroxyl-ion concentrations over which absorption of these ions takes place. W. J. Rees and I found, however, that organic acids of the formic acid series are absorbed until the *p*H of the external liquid is as high as 6·55, a value little removed from that of pure water.

At any rate, the few experiments I have made myself on this point indicate that the excess of sodium absorbed by carrot tissue from a solution of sodium chloride is replaced by ions of calcium, potassium and magnesium which diffuse out of the tissue. Although to be regarded as only preliminary in character, they indicate that an exchange of ions between tissue and external solution can occur in connection with excess absorption of one ion of a salt.

Even more strikingly at variance with the earlier view of solute absorption by plant cells is the phenomenon which is now generally described, not altogether happily, I think, as accumulation. In 1919 F. Kidd and I showed that when thin slices of storage tissues, carrot and potato, were placed in solutions of various salts in different concentrations, absorption took place towards a condition of equilibrium which is not that of equality of concentration inside and outside the cell, but which depends on the concentration of the salt. With dilute solutions the concentration attained inside the cell may be many times that of the solution outside, while in concentrated solutions the reverse is the case and the concentration of the salt inside, even after 48 hours' immersion of the tissue in the solution, may be very much less than that outside. Thus, while more salt is actually absorbed from a stronger solution than from a weaker one, the absorption relative to the concentration is less, both as regards rate and total amount, from a stronger than from a weaker solution.

These observations by Kidd and myself, though definitely establishing on broad lines the relationship between concentration of salt and absorption, did not pretend to provide more than approximate quantitative data. Thus we found that the relationship between concentration of salt and absorption was much the same as it would have been if the salt were adsorbed by an adsorbent within the cell. It is easy to suggest that a first stage in the absorption of salts by plant cells is the adsorption of the ions of the salt by some constituent or constituents of the protoplasm. While I have pointed out the similarity of the absorption of salts by plant cells with an adsorption phenomenon, I have more than once stressed the point that this similarity is in itself not sufficient to justify the advocacy of an adsorption theory of salt absorption. Yet it must be admitted that later work by more exact methods has only served to confirm the approximate similarity of the relationship between salt absorption and adsorption. Reference in this connection may be made to the work of Laine on the absorption of manganese and thallium by roots of *Phaseolus multiflorus*, as well as to observations of my own on the absorption of sodium chloride by carrot root. Further, if the protoplasm contains adsorbents of the ions presented to it, then adsorption must take place if conditions demand it.

Before leaving this question for the moment I would like to point out that it is obvious that if the similarity between the relationship of salt absorption to concentration and the adsorption equation is more than a coincidence, then adsorption can only be the first stage in this absorption, at any rate by actively growing tissues in which the absorbed ions must be transferred elsewhere. Again, the adsorbing material one would expect to be present in the protoplasm, whereas a number of more recent observations by various investigators indicate that there is actually an increase in concentration of the absorbed ion in the vacuole. The adsorption would then have to be followed by elution of the salt at the surface of the vacuole. In this connection it is interesting to note that S. C. Brooks has obtained some evidence that *Valonia*, immersed in sea-water containing rubidium chloride, accumulates rubidium in the protoplasm for two days, after which this kation passes from the protoplasm to both vacuole and external solution. The same worker has also found that when cells of *Nitella* are placed in 0·01M. solutions of radio-active potassium chloride there is an accumulation of potassium in the protoplasm after 6 hours before any appreciable amount of potassium appears in the vacuole. Previously M. M. Brooks had found that when *Valonia* is immersed in a solution of methylene blue the cell wall and protoplasm become deeply stained by the dye before any appreciable coloration of the vacuole is observable.

If adsorption is indeed operative in the absorption of salts, one would expect it to be partly mechanical and partly electrical, and the unequal absorption of the two ions of a salt could be related to the electrical charges on adsorbents in the protoplasm. Further, the occurrence of electrical adsorption would render the conformity of salt absorption with the equation for mechanical adsorption only approximate.

Another mechanism which has been suggested as possibly operative in the absorption of salts is one of interchange between ions within and without the cell under conditions which give rise to the ionic distribution between the cell interior and exterior characteristic of what is called Donnan equilibrium. If the solution exterior to the cell contains a salt both ions of which can penetrate the cell membranes, while the interior of the cell contains an electrolyte one ion of which can penetrate the membrane while the other is immobile, then at equilibrium there will generally be inequality of concentration of any ion on the two sides of the cell membrane. There are probably in the protoplasm protein salts which provide the necessary conditions for Donnan equilibrium. A difficulty is that in a condition of Donnan equilibrium the products of the concentration of any pair of oppositely charged ions should be the same on the two sides of a membrane, so that if one ion of a salt is absorbed to such an extent that its concentration is higher inside the cell than outside, the other ion can only be absorbed to a concentration inside the cell which is lower than its concentration outside. But actually this is not necessarily the case. Thus I showed in 1924 that storage tissue can absorb both ions of sodium chloride until the concentration of both is higher than that of the same ion outside the tissue, it being assumed that the ion remains active inside, an assumption for which there is good evidence. Briggs has shown that this does not present an insuperable difficulty to the view that absorption may be conditioned by Donnan equilibrium if the two ions are absorbed by different phases in the cell, and he shows that actual observations of salt absorption can be so explained if the kation is absorbed by the protoplasm and the anion by the vacuole. And in this connection it must be emphasised that just as adsorption must take place if the cell contains adsorbents of ions capable of reaching the adsorbent, so, if the cell system involves the conditions giving rise to Donnan equilibrium, it is inevitable that the movement of ions demanded by these conditions must result.

The possibility that respiration has a direct effect in bringing about the absorption of ions has been pointed out by several workers, notably by Briggs and S. C. Brooks. The production of carbon dioxide in the cell leads to the appearance of carbonic acid and hence of its ions hydrogen and bicarbonate, H and HCO_3. The interchange of ions required by the Donnan equilibrium will lead to the diffusion out of hydrogen ions which are replaced by kations from the external medium, while the bicarbonate ions will be exchanged for anions from the external medium. As the tissue continually respires the production of hydrogen ions continues to replace those which diffuse into the external solution, and so the absorption of ions continues as part of an interchange between tissue and external medium.

An interesting theory of salt absorption which hypothesises some sort of combination of the absorbed ions with constituents of the protoplasm followed by passage of the ions into the vacuole through exchange with hydrogen and bicarbonate ions, has recently been proposed by S. C. Brooks. According to this theory, the substances in the protoplasm responsible for the initial absorption are the proteins. In the protoplasm are proteins of various kinds, which are differently ionised, some with the protein group carrying a positive charge, others with the protein ion carrying a negative charge and thus constituting a proteinate ion. When a salt such as potassium chloride is absorbed the potassium ion unites with a H-proteinate and the chloride ion with a protein-OH. The potassium proteinate and protein chloride thus produced unite with the basic and acidic groups of adjacent molecules and so move through the protoplasm until they reach molecules adjacent to the vacuole. Here exchange with H and HCO_3 ions produced as a result of respiration is supposed to take place.

Against the view of a direct effect of respiration on salt intake by ionic exchange it has been urged by Hoagland and Steward that accumulation of ions is negligible or slight when tissue is deprived of oxygen, although there may be a considerable anaerobic production of carbon dioxide. But as regards this objection it must be noted that under conditions of anaerobiosis the rate of carbon dioxide production usually falls rapidly with time, so that it is doubtful whether a considerable production of carbon dioxide anaerobically generally continues for any length of time. The question is obviously one requiring further experimental investigation.

That the absorption of salts by tissues is related to a supply of oxygen, and probably in some way to respiration, there can, however, be no doubt. As long ago as 1913 Hall, Brenchley and Underwood showed that barley and other plants in aqueous culture solutions grew more rapidly in aerated solutions than in non-aerated ones, an observation which was confirmed by Jorgensen and myself in 1917 in regard to barley and balsam and by Knight in 1924 with wallflower, *Chenopodium album* and *Elodea*. The conclusion could be drawn that in these experiments the augmentation of the oxygen supply to the roots brings about an increase in the rate of absorption of the nutrients necessary for metabolism and growth, but the problem is complex, for the effect of carbon dioxide accumulation in poorly aerated solutions may be a factor, and there is a marked difference in the reaction of different species, for both Free in America and Jørgensen and myself in this country found that buckwheat cultures did not react to differences in the oxygen supply to the roots, while as regards maize, whereas Andrews and Beal found that aeration of the culture solution very greatly increased the yield, Knight found that this was the case with soil cultures, but not with water cultures. Whether this divergence in behaviour is to be related to varietal differences or to some undefined factor in the experiments is not clear.

More direct evidence of the effect of oxygen on the salt relations of the cells has been obtained in work with storage tissues. In 1927, as a result of observations on the behaviour of such tissues when placed in water either kept still or agitated, I called attention to the importance of respiration in regard to the salt relations of the cells. I pointed out the importance of maintaining the supply of oxygen to such tissues for the maintenance of their vitality, and that in the absence of an adequate oxygen supply exosmosis of electrolytes took place, leading to the speedy death of the tissues, whereas with maintenance of a supply of oxygen absorption of electrolytes continued, in the case of beetroot, for example, for periods of about three weeks. Towards the end of this time a condition of equilibrium was reached or approached, in which the content of electrolytes in the external liquid was very low. During this period conditions leading to lower oxygen and higher carbon dioxide concentration led to increase in the electrolyte content of the liquid, while addition of fresh oxygen led to a decrease. In similar experiments carried out by Briggs and Petrie in 1931 in which a continuous stream of air was passed through the liquid, these workers examined the course of respiration along with the changes in electrolyte content of the external solution, and established the fact that there was a general parallelism between the rate of respiration of the tissue and the electrolyte concentration of the external liquid. If the stream of air was replaced by nitrogen the respiration rate increased, and so did the concentration of electrolytes in the solution, while replacement of the nitrogen by air brought back the original distribution of electrolytes between tissue and external liquid. Steward and collaborators have shown that reduction of the oxygen supply to storage tissue of potato, carrot and artichoke below a certain value limits the accumulation of both the ions of potassium bromide by the tissues, while Hoagland and Broyer have obtained a similar result with barley root systems. In attempting to explain this effect of oxygen one must bear in mind that the relationship between respiration and salt accumulation may not be a direct one. The maintenance of an adequate supply of oxygen is necessary to maintain the vitality of the tissue, possibly on account of the deleterious effects of the products of anaerobic respiration. Thus the fact that accumulation depends on oxygen supply may be regarded as an expression of the fact that under conditions of partial or complete anaerobiosis the functioning of all or many vital processes dependent on the protoplasm is adversely affected, and along with them that of salt accumulation. From this point of view the effect of conditions leading to poor oxygen supply may be related not only to oxygen concentration but also to accumulation of carbon dioxide and other products of anaerobic respiration. Hoagland's observations on the absorption of potassium bromide by cells of Nitella may, perhaps, be of significance in regard to the part played by oxygen in salt absorption. He found that absorption of bromide only took place if the cells were exposed to light, or if they have been previously exposed to adequate illumination. If for some time previously they had been growing in weak light no accumulation of the salt or its ions took place. From a consideration of all the data it seems to me that the following conclusion can be drawn regarding the relationship of respiration to the absorption of salts by plant cells, namely, that accumulation of salt depends on the vitality of the cells and that the maintenance of this vitality depends, as has been long recognised, on the presence of oxygen, either because aerobic respiration or some other process requiring oxygen is essential for this maintenance of vitality, or because in the absence of oxygen the accumulation of carbon dioxide and other products of anaerobic respiration adversely affects the functioning of the protoplasm. This dependence of absorption of salts on the vitality or healthiness of the tissue was clearly shown by my experiments of 1927 and the later ones of Steward in which stress was laid on the effect of aeration of the tissues. I think Hoagland's observations fall into line with these. Nitella kept for some time in low light is probably somewhat unhealthy, just as is tissue that is deprived of an adequate supply of oxygen. In other words, most of the work published on the relationship between respiration and salt accumulation does no more than show that this accumulation is a vital process, depending on the normal functioning of the protoplasm. Any general relationship between respiration and salt accumulation, as regards the linkage of reactions involved or the transfer of the energy required for the entry of a salt against its own diffusion gradient, may thus be very indirect.

This view of the necessity of oxygen for salt accumulation does not rule out the possibilities of adsorption, chemical combination and ionic interchange as playing a part in salt absorption, and indeed, my experiments of 1927 and those of Briggs and Petrie of 1931, to which I have earlier referred, are most readily explicable in terms of ionic interchange. Apart from the more obvious physico-chemical relationships already mentioned, what is called decline in vitality, health or activity is associated with changes in the protoplasm, which may involve changes in the state of aggregation of the protoplasmic colloids and in the distribution of their various constituents, which will profoundly alter their capacity for adsorption or chemical combination and the nature of ionic exchanges. I am certain that in the present state of our knowledge there is no justification for putting aside any of these processes as possibly playing a part in determining the salt relations of cells. What is required for the clarification of the problem I have emphasised for many years, namely, the accumulation of experimental data regarding these relations, and it should help greatly if data are obtained for different kinds of cells and with different kinds of solutes. With the development of both chemical and physical methods for the measurement of small quantities, such data can now be obtained which were impossible to acquire twenty or even ten years ago. The katharometer, spectrograph and polarograph are three physical instruments which in particular will prove of the greatest aid to such work.

One significant fact does, at least, emerge from the information so far acquired, namely, the absorption of dissolved substances by plant cells is as much a vital process as the respiratory function and, like it, depends on the presence of the living substance. On what does this dependence consist ? On the presence of a protoplasmic membrane, which is broken down when the protoplasm changes in the direction of loss of vitality ? On the state of aggregation of the particles in the colloidal complex which constitutes the system, and which certainly changes as the cell becomes moribund ? On the respiratory process itself ? On the presence of certain enzymes or other substances which are contained in the protoplasm ? I have indicated how certain suggestions have been made in regard to these various possibilities, but only further research will provide the answer.

It is a remarkable fact that with the continued application of the principles of physical chemistry to the investigation of vital plant activities, it has gradually become more and more evident that simple explanations of these activities in terms of physical chemistry are not forthcoming. Even the usually accepted simple explanation of the water relations of the plant cell is now suspect. Ever since the classical investigations of De Vries and Pfeffer it has been supposed that these relations, at any rate for vacuolated cells, were explained with complete satisfaction on what I have called the ' simple osmotic view,' the assumption being made that the protoplast, or the limiting layers of it, functioned as a semi-permeable membrane permeable to water but impermeable to many solutes. Now Bennet-Clark, Greenwood and Barker have found that this explanation is not always valid. They have measured the osmotic pressure of the cell sap of a number of plant cells by the plasmolytic method, and also cryoscopically after extraction of the sap from the tissues. In some cases (petioles of *Caladium* and *Rheum*) the values obtained by the two methods are the same, and hence in these cases the simple osmotic view affords a satisfactory explanation of the observed facts, but in other cases (petioles of *Begonia* and roots of beet and swede) the osmotic value of the sap determined plasmolytically was found to be markedly greater than the value obtained for the expressed sap by cryoscopic determination. This means that in the latter cases the pressure sending water into the vacuole is greater than can be accounted for by the actual osmotic pressure of the sap as determined physico-chemically, and hence such cells possess a power of active secretion of water analogous to the capacity for accumulating salts which I have already discussed. That this is so is confirmed by the fact that cells of such tissues are not plasmolysed by their own sap, whereas in the case of those tissues which do not exhibit this phenomenon approximately half the cells of the tissue are plasmolysed by sap expressed from the tissue. So here also the vital activity of the protoplasm is operative, and it may be presumed that the energy required for this active secretion of water from the external medium is ultimately provided by respiration, but how the transfer of the energy is brought about is as obscure as in the case of salt accumulation.

Thirty years ago, when the importance of the principles of chemical dynamics in life processes was coming to be fully realised, it looked as if the solution of many of the problems of plant physiology in terms of physical chemistry was fairly imminent. But with the application of these principles to our investigations into living processes we find that in every one of them the protoplasm introduces a factor which renders these processes not readily explicable in this way. Clearly we must seek an explanation in the apparent divergence of vital processes from physical or chemical laws in the constitution of the protoplasmic system, and hence a fuller analysis of this system now appears to be a requisite for further advance in our understanding of physiological processes in general. There is at present no reason to suppose that with further advance in knowledge of the protoplasmic system we shall not ultimately be able to explain physiological processes in physico-chemical terms, and I would re-affirm what F. F. Blackman emphasised in his Presidential Address to this Section thirty years ago, namely, 'the inevitableness of physical-chemical principles in the cell.'

It is scarcely necessary to emphasise how the principles of general cell physiology must be of fundamental importance in plant metabolism, for inasmuch as this depends on the activity of specialised cells and tissues, these, wherever they are alive, must also exhibit the normal features characteristic of protoplasmic activity. The process of photosynthesis involves the absorption of substances by the assimilating cells, and, like those more general cell processes we have considered, depends on the protoplasm in some way not clearly understood, although there is a probability that at least an enzyme is concerned. The passage of the products of photosynthesis from the assimilating cells to the phloem must take place according to the laws governing the movement of dissolved substances into and out of living cells in general. The importance of general cell physiology to absorption by roots is obvious, and here it may be pointed out how the relatively rapid absorption of nutrient salts from soils in which the soil solution is known to be very dilute, is explained by the relationship between concentration and rate of absorption of solutes: the diluter the solution the more rapid the uptake of solute in relation to the concentration. Other physiological problems such as winter hardiness of plants and the effects of extreme conditions in general are also problems of general cell physiology. But in spheres of botanical science outside the range of pure physiology the general physiology of the cell is just as important. This applies in particular to ecology. This study, in so far as its aim is the determination of the relationship of plants to their environment, is indeed nothing else than physiology, a fact which was clearly recognised by Clements more than thirty years ago. Of the two groups of factors which determine the distribution of vegetation, the climatic and edaphic, the mode of action of the latter in particular can only be studied with any hope of success by those with an adequately deep knowledge and appreciation of cell physiology. It does not need a knowledge of physiology, it is true, to determine plant distribution, but such knowledge is essential for what Tansley, in a paper read to this Section in this place thirty-four years ago, called 'the higher branch of ecology, i.e. the detailed investigation of the functional relations of plant associations to their surroundings.' However desirable and necessary the collation of existing knowledge of plant distribution may be, I am certain that the solution of the fundamental problems of ecology will only be achieved by the use of physiological methods, and particularly by the application of our knowledge of the general physiology of the cell. For edaphic factors must act through the root and by the absorption of materials from the soil, or the exchange of material between the soil and root; in fact the processes of respiration and salt absorption would appear to be of the first importance.

Certain aspects of mycology have much in common with physiology; indeed, that part of mycology which concerns pathogenic organisms is inevitably closely linked with problems of the relation of host and parasite, problems which are, in their very essence, physiological. Years ago it was questioned whether the physiology of the plant physiologists was not half pathology. Certainly the reverse question can be answered with more assurance; pathology is at least partly physiology, and therefore the principles of general cell physiology must here also be of immense importance, and an intimate acquaintance with these principles should be an important part of the equipment of the experimental plant pathologist.

Perhaps no branch of botany has made such spectacular advances in recent years as that of cyto-genetics. At least it has produced a nomenclature which rivals or excels the early efforts of the descriptive ecologists. And just as descriptive ecology can do little more than correlate certain types of vegetation with certain environments, so cytology can do little more than correlate visible structures in the cell with genetical behaviour. I cannot help thinking that a real insight into these problems also will only come with the interpretation of cytological observations in physiological terms, and that the greatest advance in the study of cytology will come with the linking up of the knowledge of the cell acquired by these two lines of investigation, the cytological and physiological. And it is surely a rather remarkable fact, one indicating how far away we are at present from the achievement of this end, that the physiologist tends to think of the cytoplasm as the essential factor in determining vital activities, while the cytologist almost exclusively concerns himself with the nucleus. Neither the physiologist nor the cytologist appears at present to have anything but the vaguest ideas of the relationship between the two, a relationship which, however, we may feel sure is most intimate and fundamental to life.

I would now like to pass on to the economic importance of cell physiology and say a few words about its importance in applied botany. We all know, but it cannot be too strongly emphasised, that botany is the pure science of a great part of the most important industry of the world, agriculture, and that, like every other industry, it can only be carried on wisely if its practice is based on scientific principles. Almost all branches of botany are important for agriculture, but mycology, genetics, and physiology are particularly so, and certainly physiology is not the least of these.

Absorption of water and nutrients from the soil, assimilation of carbon, water relations of the plant, vegetative development, flowering and fruiting are all problems of agriculture which are essentially physiological, and in many of which the principles of general cell physiology are of importance. Similarly in forestry physiology must play as equally important a part. But besides these more obvious economic applications there are numerous industries in which the principles of general cell physiology are no less fundamental. There are all those industries, ever increasing in number and importance, which are based on some particular plant product, such as cotton, linen, jute, rubber, tea, sugar and tobacco, to mention only a few of the more important. Apart from the growing of the plants themselves, which like any other form of agricultural practice should be based on sound physiological principles, a knowledge of these principles may be equally important in the subsequent treatment of the plant material. In particular a knowledge of cell organisation, the action of enzymes contained in the cell, its behaviour towards various reagents, all aspects of general physiology, are essential. Finally the great food storage industry depends greatly on the application of knowledge of cell physiology. As an example of this I may refer to pioneer work on the scientific principles of cold storage by Jorgensen and myself carried out some twenty years ago. From a consideration of what was then known of the constitution of the cell we concluded that the satisfactory preservation of certain tissues in the frozen condition depended on rapidly freezing the tissues, a method which was subsequently put into practice in certain branches of the food storage industry. It was indeed encouraging to read in the daily press last December of what was described as the scientific discovery of the week, which turned out to be none other than the rapid freezing method for the preservation of fruit, a method that had been examined and advocated by Jorgensen and myself nearly twenty years previously. This is, of course, only one instance of the bearing of general cell physiology on the subject of food preservation. The effect of the conditions of storage on enzymes and other cell constituents, and on the vitality of different kinds of cells, tissues and organisms are among the problems which a knowledge of the facts and methods of general cell physiology will help to solve.

With the ever-increasing mass of knowledge in the various branches of botany, an increase which is especially noticeable to-day in those aspects of our subject which are undergoing rapid development, physiology, mycology and genetics with cytology, it is impossible for anyone to be an active worker in more than a relatively very small field of botanical endeavour. We sometimes meet with reference to a mysterious gentleman called the ' general botanist ' who is expert in general botany, as someone distinct from the morphologist, physiologist, mycologist or other worker in a defined field. But in these days, when to make any contribution to knowledge necessitates specialisation, there can indeed be no such person as the expert in ' general botany,' for there is, indeed, no such subject. But in whatever part of our subject our own special interests may lie, we can still appreciate the efforts and aims of workers in other fields, and realise the bearing of work in these fields on our own problems, and in this sense we are all general botanists ; that is, just botanists.

For if ' general botany ' as something distinct from ' botany ' is a myth, there is no doubt that the various branches of our subject are related in the whole. In this address I have tried to indicate not only the scope and present position of our knowledge of the general physiology of the cell, but where this particular part of the science of plants comes into contact with other branches of botany, and how the application of a knowledge of the facts, principles and methods of cell physiology may be expected to lead to an increase in knowledge, not only of the physiology of the plant, but of other aspects of botanical science and of its industrial applications.

SECTION L.—EDUCATIONAL SCIENCE.

THE FUNCTION OF ADMINISTRATION IN PUBLIC EDUCATION

ADDRESS BY

J. SARGENT,

PRESIDENT OF THE SECTION.

THE British Association in general, and this Section of it in particular, have long been accustomed to Presidential Addresses which, with less than the usual compromise between truth and politeness, have generally been described as brilliant and provocative. Certainly there would be no exaggeration in applying these epithets to those addresses to which I myself have had the privilege of listening.

This year I can at any rate promise the Section a change, but it will not be a change for the better. Even if, in my undergraduate days, I occasionally staggered College societies with visions of things to come, I can only say that, after twenty-five years in the service of local government, the instinct of self-preservation if nothing else has taught me to confine myself to things as they are.

At the same time I am proud to be old enough, or young enough, to have been at school and college at a period when young men looked for a new book by my immediate predecessor with something of the same spirit of hope and excitement as the Christians of Macedonia may have awaited a communication from St. Paul. There was a memorable evening in our Senior Common Room when I laboured, not with entire success, to persuade our venerable Dean that, in spite of a certain similarity in title, Kipps, the book I was commending to his notice, was not identical with another modern work called Kim, which had earned his disapprobation and was in fact by quite a different author.

I will not at any rate blame my subject, even if at first sight it may appear a dull one, for the shortcomings of this address. The reasons I chose it are twofold ; in the first place it is the only serious topic I know enough about to justify my discussing it in the presence of an audience of such various distinction, and in the second I am rapidly approaching a state of suspended animation so far as my association with local government is concerned, so that without aspiring to brilliance or even provocation I can air my views with greater freedom and possibly less offence than any of my colleagues who are still bound to the wheel of official discretion.

At the same time I am not unmindful that this address is being delivered to the Educational Science section of the British Association, and that to some the connection between educational science and practical problems which to a large extent are common to local government as a whole rather than peculiar to educational administration may well appear remote. I am not quite sure what educational science connotes but I imagine it may comprehend not only the philosophical principles upon which educational practice is or ought to be based but also experiment and research into method. The administrative machine, particularly in the public education service, is an instrument which, if improperly employed, may well distort the first and hamper the second. For that reason alone it deserves an occasional inspection by the educational scientist whatever his particular interest may be. Moreover, in recent years the British Association has attached special importance to the impact of science on society. For the great majority of teachers, pupils and parents in this country the medium through which this impact is felt so far as education is concerned is the Local Education Authority,

Furthermore this question of local administration, uninspiring as it may appear, may not be without its significance in relation to current issues of world-wide importance. Only the other day I heard a prominent member of a local education authority quoting, or as I believe misquoting, a still more eminent personage to the effect that 'local government is the last bulwark of democracy.' Exactly what he meant by the word 'last' is obscure, and as nautical metaphors are notoriously tricky things there is a possibility that he may have meant bulkhead rather than bulwark. I take it, however, that his meaning was that, if democracy is going to founder, the immediate cause will probably be found not so much in the legislative eccentricities of Parliament as in the inefficiency of local administration. It is when men begin to feel miserable that the value of political liberty begins to slump, and it is when intelligent men feel the pinch worst that revolutions begin to happen. It may be a hasty and inadequate generalisation, but there seems to me to be much in the view that the totalitarian state has arisen from the economic and spiritual destitution of the professional classes. I must, however, resist the temptation to platitudinise on this popular problem and try to confine myself to certain tendencies in the administration of local government, and of education in particular, which can have at most only an indirect bearing on the much wider question of the relation of the State to the individual.

Political thinkers throughout the ages have frequently defined or described the function of administration. Of all their attempts the one which appeals most to an harassed official is the late Lord Fisher's cynical aphorism that it consists in the intelligent anticipation of agitation. From a somewhat less negative point of view it may be regarded as compounded of deliberation and execution, of which the latter should but does not always follow the former. In very simple terms, administration is neither more nor less than a method of transacting business, and particularly public business, as cheaply and as quickly as is compatible with doing it reasonably well. Even this lacks precision and is by no means free from ambiguity. Where for instance is the standard to be found by which from time to time ‘ reasonably well ’ shall be measured ? It may be argued that the practical administrator will in fact know at any given time the standard he has to aim at in order to satisfy public opinion, just as the craftsman may point to contemporary taste as the criterion of production.

Whether it is possible or not to find an acceptable definition of administration, it will probably be agreed that it expresses itself through two functions, the legislative and the executive. Most of the administrative problems which come within the purview of local government fall in the latter category.

By a process which is at once historical and natural, the legislative side of administrative activity has remained largely in the hands of the central Government, though it would be to fall into an error which professed experts have not always avoided if the fact were overlooked that in many instances experiments legitimately conducted by local authorities within the powers conferred upon them by Acts of Parliament have often led to new ideas and consequent legislation. Side by side with this distribution of legislative and executive activities, and to a large extent determining it, there has proceeded a fundamental change in the conception of the function of the State in relation to the individual citizen which has marked the last century and, with increasing emphasis, the last quarter of it. The change to which I refer is one from a negative to a positive conception of legislative objectives and has profoundly modified the scope and character of local administration. Until a hundred years ago the main interest of government was to restrain men from living evil lives ; since then the intention, however mysterious in operation, has been to help them to live good ones.

This change has coincided and is no doubt connected with another conception widely developed if not created during the same period, viz. the idea of human progress or the infinite perfectibility of man. A social order designed by those who believe that every day and in every way men are getting better and better may be expected to exhibit fundamental differences from one the main object of which is to postpone as long as possible the coming of inevitable decay.

The obvious result of this evolution from a negative to a positive view of the function of government has been a vastly increased interference by the State in the goings and comings of the ordinary citizen ; and the problems which form the subject of this paper arise from the steps which have been and are being taken to make this interference effective. The growth in this business of government, as in other businesses, has forced home the need for administrative devolution, with the consequent rise of local government as the machinery through which much of the will of Parliament must be implemented.

It is no part of this paper to try to trace the process of this devolution, but it is relevant to point out that there have been occasions when the need for defining satisfactorily the respective spheres of the central and the local government has presented itself as an extremely urgent problem, at any rate to the minds of many local administrators. There is little, however, for me to say on this point because the rules according to which the game is to be played are now generally accepted, and the players, in my experience, are observing them in an increasingly friendly and harmonious spirit. We all think and may even speak unkindly about Whitehall from time to time, but on calm reflection cannot but admit that we are treated on the whole with delicacy and consideration.

There is one aspect of this relationship, however, which is important, and that is the financial one. I shall have something to say a little later on the question of the adequacy or otherwise of exchequer grants so far as Local Education Authorities are concerned. On the wider issue we may rest content with the fact that, whatever arguments may be adduced or principles invoked, so long as there are local administrators they will continue to pursue the laudable object of getting as much money and as little interference from the central authority as they possibly can. But if devolution is to remain a necessity, and granted the continuance both of a democratic system and of the parental interest of the State, there seems no alternative. The really disconcerting problems for the future seem to me to arise from the present nature of the local government bodies themselves. The first difficulty would appear to lie in the unit, i.e. in the size and geographical distribution of local government areas. Recognised authorities, who are mostly foreigners and seem to regard our political institutions with greater enthusiasm than we do ourselves, tend to congratulate us on our ingenuity in adjusting them to meet new social and economic needs as they arise. It would be difficult to detect this evolutionary process at work so far as local government boundaries are concerned. It is true that towns have grown and encroached on county areas and that there has been a distinction in the degree of autonomy conferred on authorities of different sizes by successive Acts of Parliament, but substantially it remains true that our local government boundaries derive mainly from Saxon times when the problems of modern administration can hardly have been foreseen.

When the present Local Education Authorities were established by the Act of 1902, there was an opportunity to devise areas with regard to administrative convenience rather than historical association, but it is significant that there does not appear to have been any serious suggestion to do other than to allocate the new powers and duties among the existing local units. Consequently we find the control of public education, under the benevolent supervision of the Board of Education, distributed among 318 different bodies varying from London with 4,396,821 inhabitants down to Tiverton (Devonshire) with 9,610.

These Local Education Authorities inherited the property of the School Boards and Technical Instruction Committees, including a number of buildings in various states of repair, and of officials in much the same condition, together with some strange and embarrassing residuary legacies, like the Cockerton Judgment and Dual Control.

It is very much to their credit that within three and a half decades, with a great war intervening, they have not only introduced some kind of order into this confusion but have also built up a great system of secondary education, put the salaries of teachers on a more satisfactory basis, and undertaken the task of reorganising the whole system of so-called elementary education, the full effect of which it is too early to appreciate. It is significant of the success they have achieved that those pioneers in public education, the Scots, should recently have reconstructed their administrative machine on the English model and so driven another nail in the coffin of the *ad hoc* education authority. And yet we must confess that we are still very far from that adjustment of opportunity to ability which is, I suppose, the fundamental aim of any democratic system of public education.

If I appear to be devoting most of my time to pointing out the defects in our local education system, I should like to make it clear that my object is to contribute my mite towards smoothing out the long road which has yet to be travelled and in no way to belittle the efforts of a by no means ignoble army of public servants.

Apart from questions of size and population, Local Education Authorities also vary greatly in their financial resources as regards both their own rateable value and the contributions which they receive from the Exchequer towards their net expenditure. Neither the money they raise themselves nor the grants they receive from Government are in any arithmetical proportion to their respective areas or populations, and, although the formula by which the grant is calculated was no doubt intended to take account of local circumstances affecting expenditure, the conditions which it was designed to meet in many cases no longer obtain. The resultant anomalies are a fruitful cause of dissatisfaction in many areas and of acute embarrassment in some; in fact the whole question of the financial relationship between the central government and the local authority is one which calls for an immediate and comprehensive review.

Then again Authorities vary very much in character, some being purely rural, many purely urban, while others contain a mixture of the two, or are in process of transition from the former to the latter. A further and ever-present difficulty so far as many of them are concerned is the fact that while some of them are empowered to deal with all forms of education in their area (Counties and County Boroughs, technically known as Part II Authorities), others are only empowered to deal with elementary education (Part III Authorities). Part III Authorities, and particularly the smaller ones, are naturally jealous of their prerogatives and one cannot but admire the courage with which many of them are facing the strain on their resources, financial and otherwise, which the provision of elementary education on reorganised lines must entail. At the same time, when it is realised that 'higher' education usually starts at the age of 11 or even earlier, while 'elementary' education will shortly extend to 15 or even 16, and that most of the larger Part III Authorities have exercised the right of establishing selective central schools, which in many cases approximate in standard and aim to the other forms of selective post-primary institution provided by the Part II Authority in the same area, the possibilities of confusion, overlapping and friction will need no emphasis.

It is true that many of these difficulties can be and are in fact being overcome by co-operation between the Authorities concerned, but it should be pointed out that, while co-operation ranks high among the blessed words in the educational vocabulary, it usually involves a compromise and is never the ideal method of administrative procedure. No departmental chief, I imagine, would set two typists to type the same letter or two office boys to lick the same stamp simply in order that they might have the advantage of co-operating.

The next problem is concerned with the personnel of the Local Education Authorities. The personnel is divided into the amateur and the professional elements, or the unpaid and the underpaid as I have heard it expressed. The amateur element is again divided between persons co-opted for their knowledge of and interest in education, and others elected by the people not solely, experience suggests, because they are known to possess either or both of these qualifications. The co-opted members for obvious reasons are generally among the most valuable members of an Education Authority, but the fact that they are not members of the County or Borough Council, and so have no direct responsibility to the electorate, is usually regarded as disqualifying them for occupying really responsible positions, e.g. chairmanships of committees.

The most serious aspect of the problem to my mind is the steady and even accelerating deterioration in the amateur personnel which has taken place since the War. This is particularly marked in the case of the elected representatives of the people. The reasons are as plain as the fact. The most obvious of course is the gap caused by the War itself in the ranks of those who, if they had survived, would probably have been the first to offer themselves for public service. But this is by no means the whole or even the main explanation. The vast increase in the responsibilities laid upon local authorities by legislation since the same period makes it necessary that any member who is to become really *au fait* with the business of education should be able to devote a considerable amount of his weekly time to it, whereas before the War it was possible for a person of average intelligence to grasp not only the general lines of policy but also day-to-day happenings by occasional attendance at committee meetings. Outside tendencies have also been at work during the same period to make such extra attention increasingly onerous and difficult; the business of making a living has also become more strenuous, and people, who might have been able to devote before the War the amount of time which was necessary to grasp the business of administration, now find themselves, so far from being able to give the additional time which the increasing duties demand, in a position to give much less time than before. Consequently local administration is being progressively denuded of persons actively engaged and occupying positions of responsibility in industry and commerce.

There seems no sign whatever that either of these tendencies is likely to lose its effect. Everything in fact points in the other direction, and the result is already apparent in the increasing tendency of Education Authorities to consist of people who have retired from work, or have never had work, or who are in fact professionals rather than amateurs because, as officials of political or other associations, it is expedient for them to become members of Local Education Authorities from the point of view of promoting the objects which their associations have at heart. It is no reflection on the personal integrity of these last to express the opinion that they constitute a serious danger to the system on the ground that if there is a bureaucratic habit of mind, and if as some people believe it is inimical to good government, these people possess it and bring it to bear on their consideration of educational problems without the saving grace of the professional educationist's training in and knowledge of the particular branch of administration with which he is dealing.

There remain, of course, many splendid people who give their services to educational administration, and I must safeguard myself against appearing to suggest by the use of the word deterioration that graft or other forms of dishonesty are on the increase. That, I am glad to say, has not been my experience. There is the risk, however, which is more than theoretical, of intellectual dishonesty creeping into the discussion of educational affairs when the Authority contains any substantial number of members who are pledged to a set of opinions which may have a cross-bearing on purely educational considerations.

As I have pointed out the difficulties—I will not say the defects—in our local government system at considerable length, I suppose I am under some obligation to attempt to indicate possible remedies. So far as the numbers, sizes and financial arrangements are concerned, it is not difficult either to indicate the general lines which reform in theory should follow or to envisage the practical difficulties which will confront the reformer when he sets out to tamper with the traditional boundaries of English local government. It would be a bold man who would underrate the strength of that local feeling which in its nobler aspects is not unworthy of being termed local patriotism, but at other times merely vocalises the parish pump. It is, however, possible for practical experience and even *a priori* reasoning to suggest certain of the attributes which the ideal local government unit should possess. It should be large enough to be able to provide the variety of services which a modern community requires, but not so large that the day-to-day discharge of routine administration necessitates a rigid or bureaucratic attitude towards the problems presented for solution. In education in particular it is important that the area should contain sufficient children or students to justify the provision of the various types of educational institution which modern needs demand. It is difficult, for instance, for a small area to face the cost of modern schools, particularly of the most expensive form of them, the technical college, and although a solution may be found in co-operation between neighbouring Authorities, it does not always follow that Authorities who are contiguous geographically have similar needs, and there is also the risk that the standard of co-operative effort may come to approximate to the lowest common multiple among the Authorities concerned.

Another important consideration from the economic point of view is that the Authority should be sufficiently large to be able to obtain good contracts for the supply of the various materials which it requires. Modern methods of mechanisation and rationalisation have been slowly but surely invading the province of local government, but their advocates have not always been ready to recognise the fact that, while centralisation under the control of one committee or one officer makes for efficiency and economy up to a point, the stage can easily be reached when the activities and responsibilities both of the committee and the officer become so large that neither they nor he are able to keep the threads comfortably within their grasp. When this stage is reached the question of devolution becomes just as important as that of centralisation at the earlier stage.

I have come to the conclusion that for Education Authorities, and I believe for other Authorities also, the minimum size of any local government unit should be an area with a population of 250,000 ; the ideal size would be between 500,000 and 750,000, and the maximum size 1,000,000. The establishment of areas of this size would, of course, pre-suppose the total abolition of Part III Authorities, by conferring complete autonomy on the largest, or on the amalgamation of others where they are geographically contiguous, and by abolishing the rest.

There is one other matter in this connection which is worth some consideration, and that is the question of so redistributing areas that none of them may in future be exclusively rural or exclusively urban. This is a proposition which has commended itself widely to many social reformers who have advocated a regional organisation for local government. I am not sure that it is quite as important as some of its advocates have supposed, partly because with the development of modern transport and of town and country planning the difference in outlook and needs between the town and country dweller is tending to disappear. I would, however, admit that in such matters as technical education a purely rural area tends to be penalised, at any rate where agriculturists have still to realise that their industry is just as much in need of technical instruction as any other.

To some extent the establishment of geographical units of a more uniform and rational size would contribute towards the solution of the major difficulty of personnel because, while it is true that some small Authorities enjoy admirable committees and officials whereas some of the larger ones are notoriously below standard in these respects, it will remain true on the basis of probability that within reason the larger the area the wider the choice it will have among people for its members of committees, and the larger salary it will be able to afford and consequently the wider field it will be able to draw upon for its administrative appointments. Larger areas and higher salaries will not, however, by themselves overcome the personnel difficulties which have bulked so largely in this paper. Unless people who are competent to govern can be made to realise that the preservation of liberty must depend on the capacity of those who voluntarily serve the community, that is, unless people are moved in greater numbers to offer themselves for public service by the Socratic urge, namely, fear of being governed by worse people than themselves, the prospect of arresting the deterioration in the amateur personnel of local authorities is small.

Something of course may be done by so easing the burden falling upon committees that members may be freed from the tedium of what are at present known as ' dustbin ' debates and enabled to devote themselves to the wider issues of policy and the supervision of their officials. The trouble is that the present type of member often prefers the 'dustbin' debate to any other kind because its subject is a matter with which he is familiar; it is common experience that memoranda embodying recommendations of high policy are much easier to get through committees than those which deal with comparatively trivial issues.

It may be a pessimistic opinion, but my own view is that local government will have in future to counteract the deterioration in its amateur element by a corresponding improvement in the professional element; that is, it will have to look to recruiting better officials in the future than it has recruited in the past. This is not simply a matter of higher salaries, it is more a question of placing the training and status of the local government officer on a basis at least equal to that of the central civil servant. I am not shutting my eyes to the fact that there has been a steady improvement in the conditions of service for local government officers during the last twenty-five years and, as a natural consequence, in the type of officer who is now coming forward. In the education service, for instance, the Associations of Local Authorities have recently approved proposals affecting the status, emoluments and recruitment of entrants to the higher ranks of the service. Other people thinking along other lines have played with the idea of the City or County Manager. There may be possibilities in this idea provided that areas do not exceed the limits to which I have already referred, and provided that the traditional idea that the chief officer of an Authority should be a lawyer can be finally laid to rest. The legal mind has many virtues and administration would become chaotic without its restraining influence, but it is by temperament and training a restraining influence and is consequently unfitted to take quick decisions or give prompt effect to them when taken.

But if there is any validity in my contention that the salvation of democracy as exemplified in our local government is to be sought in an improved type of official, I must in conclusion try to give some answer to the question, 'Who is the happy warrior?' The Association of Directors and Secretaries for Education, of which body I am proud to be a member, answered this question more adequately than I can hope to do so a few years ago when they gave evidence to a Royal Commission on Local Government, and I can only refer those interested to a document which is almost lyrical in its fervour. Speaking in more mundane terms, I would say that the educational administrator should have had a university training and some experience as a teacher in one branch or other of the education service. It is essential that he should possess the qualities of a sound administrator, that he should know how to initiate, when to delegate, when and where to advance, how to endure setbacks—above all, how to handle men. If he can retain a genuine enthusiasm for the science of education, it will not be so necessary for him to have a profound knowledge of educational theory.

Finally, he must beware of the hardening effects of custom and precedent. The needs of society are changing rapidly and it is the function of all educators to study these needs and to consider how best they can be met. At its highest this demands from him a philosophy of life in which he is compelled to study continually the philosophical basis of education and the principles on which this great human science has developed ; at the worst he can fall back on Pope for comfort and inspiration :

'Whate'er is best administered is best.'

There is a story that there was once a subaltern in a famous cavalry regiment who was so stupid that his brother officers noticed it. There is an equally apocryphal incident of an educationist who was so platitudinous that an educational conference noticed it. I wonder whether I have emulated him.

SECTION M.—AGRICULTURE.

LEY-FARMING AND A LONG-TERM AGRICULTURAL POLICY

ADDRESS BY

PROF. R. G. STAPLEDON, C.B.E., M.A.,

PRESIDENT OF THE SECTION.

My own leaning is towards the word 'ley,' although according to the Oxford Dictionary this word is obsolete, but in adopting ley I follow the best agricultural precedent.

It is not my intention to talk about farming for laymen, for in my opinion ley-farming properly understood is the most highly scientific farming that it is possible to practise. The ley farmer must be a proficient stock-master and a proficient cultivator, versed alike in the arts of animal and crop husbandry. 'To be a farmer' is 'to till the soil,' and in 'till' is implied the bringing of the soil into a fit condition for the production of crops—the care of the soil. A farmer in the true and proper meaning of the word is a man who has ever before him two purposes: the one to put all his fields to optimum use in respect of commodity production, and the other, and of even greater ultimate importance, to attend to the maximum need of all his fields in respect of soil fertility. Thus judged, my thesis is that the ley farmer is a farmer *in excelsis*.

My address has to do with the most honourable, and what should be the most venerated, aspect of the whole of agriculture—the rotation, for upon the rotation I claim everything depends. So I at least respond to the honour that has been done me in placing me in the position in which I find myself to-day in the selection of my subject. It is a neglected subject. I am the first President of Section M to do homage to the rotation. I have researched amongst the utterances of my distinguished predecessors; incidentally, although only of interest to myself, I find that the first Presidential Address to Section M was given by Sir Thomas Middleton in the year that I came into Wales and began my researches on grassland—that was in 1912. The only mention of the rotation in the total of twenty-four addresses that have been given was by Sir John Russell, who in 1916 started off promisingly with winter corn: spring corn: fallow, but to my intense disappointment followed the rotation no further.

In view of the immense amount that has been published during the present century it is not without significance that the leading agricultural journals contain but few articles dealing primarily, or even remotely, with the rotation, and next to nothing relative to the basal philosophy of the rotation. The truth is that agricultural thought in recent decades has turned ever more exclusively towards the narrow, too narrow as I think, path of commodities, each considered as such. Excessive concentration on commodities leads inevitably towards monoculture, and to what we too lightly please to call specialisation, and leads away from the rotation and ultimately to disaster. Greatly daring, then, I have set myself to combat this modern fetish of over-concentration on commodities, a fetish that has revealed itself not only in the trends of agricultural science, but in a very great deal of what the State has endeavoured to achieve for agriculture and which daily reveals itself in the actions and utterances of the leaders of the agricultural industry.

I think that everybody will be agreed that such is the precarious state of the world to-day, and of this country in particular, that there can be only one approach to the problems of agriculture, and that is the national approach. We must not so much consider what is good for the farmer as what is good for the State: then what is good for the State must be made good for the farmer. That is the only possible approach towards a stable and long-term agricultural policy. A long-term agricultural policy, if it is to be enduring and adequate, must envisage both present and future needs of the State. The success of the policy must be judged in the main by one overriding consideration, namely, the sureness and rapidity with which the farmers of the country (all the farmers of the country) in order to meet any emergency prove themselves able either to pass from the production of one series of commodities to the production of another, or, radically to alter the proportions of the several commodities produced.

It so happens, at least it appears to me, that the present needs of the State, and also the more menacing of the foreseeable contingencies, unite to demand one and the same essential contribution from our agriculture. It is not for me to attempt to decide whether war danger, or the danger of our about-rapidly-to-dwindle population is the greater peril; little less disconcerting are the effects of soil erosion and soil depletion in those countries from which we are wont to obtain abundant and cheap supplies of food. I am concerned with a long-term agricultural policy, the kind of policy that would take at least ten years to put into full operation, and consequently we have to consider not so much immediate war danger as war danger as such, a danger that owing to our island position would seem to be something from which it is now hard to see how we shall ever escape. I believe the extent of the influences of soil erosion and depletion are not even yet fully realised. All methods of countering this must in the last resort react against the British housewife, and must tend to increase the cost of overseas production, while taking soil erosion, soil depletion and land deterioration together a vaster area of the globe is undoubtedly affected than is generally supposed.

Our own rough and hill grazings have manifestly deteriorated: witness the spread of bracken, to quote only the most obvious but by no means the most serious example. They have become increasingly depleted of lime and phosphates in recent decades, and the same thing must be happening to a greater or lesser extent—and sometimes accompanied by actual erosion—in all the great ranching areas of the world. In framing our own long-term agricultural policy heed must be taken of every shred of evidence on land deterioration that is available all the world over, for it is patent that when the sum is totted up the total will far exceed what is already only glaringly manifest.

The immediate, and on all hands generally admitted, need of our peoples is an abundance of fresh food. An abundance of fresh food is not compatible with a superabundance of permanent grass. Since permanent grass flows like the sea right up to the very doors of some of our largest centres of population, such centres of population are automatically denied an abundance of really fresh vegetables.

I make no apology for this somewhat long, and in a sense non-agricultural and at all events non-technical introduction, for it seems to me imperative to stress our national needs, for it is these needs which should govern our whole agricultural outlook and, therefore, should determine all our systems of farming. To sum up so far, and on the strength of the various considerations I have brought forward, I would say this. What is demanded of our agriculture is, *firstly*, to maintain as large a rural population as possible, for probably on a large and contented rural population depends to a marked degree the increase of our population as a whole. *Secondly*, to maintain as large an acreage as possible in a highly fertile and always ploughable condition, and *thirdly*, so to conduct our farming as to allow at all times, and in all places, for the absolute maximum of flexibility in commodity production.

Before further developing my argument I must endeavour to put ley-farming in its proper perspective in relation to other systems of farming. I must therefore, and as a further preliminary, attempt to define the systems of farming as conducted in this country.

My concern is to define the systems not in terms of commodity production, but in terms (*a*) of their flexibility, (*b*) of their indebtedness to imported feeding stuffs, (*c*) of their relation to the maximum needs of the soil in the matter of maintenance and enhancement of soil fertility, and (*d*) as to the amount of labour demanded. For if my major premises are anything approaching to correct, these are the matters of supreme national importance. My classification is, of course, amenable alike to amplification and simplification, and I put it forward to-day quite tentatively, and primarily to illustrate the principles which I consider absolutely basic to any rational consideration of a long-term agricultural policy for this country. Here is my classification.[1]

Arable Farming.—A small acreage of permanent grass—a few odd corners, a couple of fields—may be conceded to even the arable farmer. For the rest he must be presumed to take the plough around his whole farm, and

(*a*) work on a rotation of crops without any resort to the ley,[2] or

[1] I first put forward this classification in an article, 'Agricultural Policy,' appearing in *The Fortnightly* for March 1938.

[2] A ley is a field sown down to grass and/or clovers, and is such that it is designed to take a definite place in the rotation of crops. Leys are of two main types: the one-year, or 'arable' ley, and the ley of two or more years' duration. Implicit in the idea of the ley is, however, the conception of 'due date': after an appropriate, and within fairly narrow limitations, pre-defined, period it becomes due to be ploughed up.

(*b*) adopt a rotation which involves the use of the one-year ley only.

The arable farmer as thus defined is never a grazier. When the one-year ley is employed this is for the primary purpose of producing hay for horses or stall-fed animals, and contributing to the muck heap, while the clover sod as such contributes to the fertility of the farm. The major function of the ley is here the maintenance of soil fertility. The chief concern of the arable farmer is the production of cash crops. His system is capable of extreme flexibility within the sphere of crop husbandry, it is capable of employing much labour—market gardening, and relatively little labour—mechanised wheat growing. It is a system which from the point of view of soil fertility is easily abused, and which in some of its forms, *e.g.* market gardening, makes excessive claims on farm and stable manure (when obtainable) from sources outside the boundaries of the farm. The robbing of 'Peter' ('Peter' in this case being the hay and straw producing fields of other, and often remote, farms) to pay 'Paul' (the truck crop fields) is an aspect of large-scale market gardening which has from the national point of view, I think, never been fully appreciated.[3]

It is likely that the market gardener in his own interest will be driven increasingly to adopt a system of alternate husbandry as presently to be defined—town stable manure being a rapidly waning commodity.

Alternate Husbandry, or, as I prefer to call this system, *Ley-Farming*.—A couple or so fields of permanent grass can be conceded to the ley as to the arable farmer, but for the rest the ley-farmer takes the plough in ordered sequence around the whole farm. Ley-farming is of two main types, but always the majority of the leys employed will be of two or more years' duration, and always in any particular year the area of the farm in leys (and therefore in grass) will be not less than one-third of the ploughable acreage; will frequently be over three-quarters of that acreage, and in extreme cases, and at unusual periods, the whole of the farm may be in leys. The main points to be emphasised are these. The ley-farmer is of necessity, and essentially, a grazier and a crop husbandryman; he may also be a feeder. He must, therefore, be equipped for crop and animal husbandry, and, as I have already said, to be successful he must be proficient in both arts of farming. His system, his mental stock-in-trade, and his equipment on the farm all bear the same hall-mark, and the hall-mark above all others of value to the nation, to wit, FLEXIBILITY.

The ley to the ley-farmer has two equally important functions to perform: the sward, or animal ration function, and the sod, or soil fertility function; of this duality, which to my mind is at the root of successful farming in all the moderate to high rainfall areas of the temperate regions of the world, I shall in a moment have much more to say.

The two main types of ley-farming I will define as follows:

The Arable-Grass Rotation.—In the arable-grass rotation most usually the leys are of two or three years' duration. The area in grass at any time will not exceed 50 per cent. of the farm, and may be somewhat less:

[3] A good many acres near London once devoted almost entirely to the production of hay for the City horse, and therefore also of manure for the market gardener, still show the mal-influence of that type of monoculture.

Good examples of this system are the arable dairy farming of Denmark, and the rotations practised in Aberdeenshire in connection with beef production. In both cases animal products are the chief concern of the farmers, and the holdings produce at least a good proportion of the winter rations. The mechanised cereal grower may also adopt the arable-grass rotation, primarily with a view to maintaining soil fertility and to making it easier to get on his land during periods of sketchy weather. A typical rotation would be wheat : grass : grass : wheat.

Grass-Arable Rotation.—In these rotations the majority of the leys are left down for long periods, from four to as many as twelve, or in some cases even more, years. Most usually as much as three-quarters, or even more, of the farm will be in leys at any one time. Ordinary animal products are the major concern of those following the grass-arable rotation, and it is on these farms that dairy bailing, poultry and pig folding are often such important and telling features of the system. Grass-arable farms at a moment's notice can be turned over to cereal production on a grand scale and hence, if for no other reason, the enormous importance of the system and of farms conducted on this system to our national welfare. What is achieved by this system properly conducted is to farm without wasting a gallon of urine or a blade of grass ; it marries the animal to the soil as can no other system, and ensures that the sod performs its maximum function in respect of soil fertility and crop production, and the sward its maximum function in respect of animal production. The nation is under an incalculable debt to Mr. Hosier and his followers, and this will eventually be realised, for it is not so much what the Hosierites do on their own acres as the principles which underlie their activities.

To the credit of ley-farming as a whole is to be placed the fact that it makes heavy, or at least reasonable, demands upon labour ; it is less dependent upon imported feeding stuffs than most other systems, and it maintains its acres and its practitioners in a condition of maximum flexibility and ready for anything.

Nondescript.—In so far as acres are concerned the nondescript system is the one I should imagine most generally practised in England and Wales. I mean when a man practises ley-farming or arable-farming on one corner of his farm, and maintains the rest in permanent grass. Such a system is not incompatible with reasonably high production, but it is under this system that we see some of the worst examples of slovenly, negligent and deplorable husbandry. Our nondescript farms stand as a token of the fact that a system of farming by which under present conditions a farmer may contrive just to keep body and soul together is likely to be a system completely out of harmony with the needs of the nation. Many nondescript farms are family farms, and the amount of tillage is a function of the size of the family, or of the number of sons willing to stay at home—both dwindling in number.

Permanent Grass.—The permanent grass farms are those upon which there is no cultivation of any kind : on some it is still possible to find a plough, but only as a museum specimen. The number of permanent grass farms has demonstrably increased ; such farms are apt to be run together, when generally fences will be more than ever neglected and the whole (and too large) unit operated as a ranch. In the national interest, as I have defined and envisaged that interest, this system suffers from every conceivable defect. In the first place, speaking quite generally, the permanent grass farms contribute nothing more valuable than inferior hay to the winter ration; they afford the minimum of flexibility, and maintain the minimum of acreage in a ploughable condition. Permanent grass farms serve as an excuse for an immense amount of national and private laxity, because in brief, however bad they are they generally have some slight earning capacity, and that with the minimum of trouble to anybody—landlord, agent or farmer. Thus these farms frequently stand on land in urgent need of drainage and of lime, and so in the main they continue to stand.[4] It is perhaps the greatest tragedy of British agriculture that even the poorest of poor grass has some earning capacity. Milk production on permanent grass farms, and especially on those deficient in lime and phosphates—and they are many—and particularly where the stationary night paddock figures prominently in the management, stand as the best example I know of ultra-dependence on imported feeding stuffs and exaggerated waste of the manurial residues from such feeding stuffs: waste as such down the drain, and waste because of extraordinarily inept grassland management (on this latter point I will enlarge in a moment); waste also of the potential fertility tied up in the sods of the night and other more heavily dunged and urinated paddocks.

At this point I would urge that unless we know the number of farms and the gross acreage of such farms operating on each of the four systems I have enumerated we know next to nothing as to how this country stands relative to potential food production. Furthermore, schemes for helping the farmer *via* commodity subsidisation and by planned marketing cannot be assessed in their influence on the maintenance and enhancement of soil fertility—and that is what matters above all things—unless we know the systems of farming under which the assisted commodities are being predominantly produced. How much quota wheat, for example, is being produced respectively on arable farms, nondescript farms, or on ley farms? Where is most of the milk being produced—and this is a matter of fundamental national importance in the interest alike of the health of the cattle and of the children of this country—on nondescript farms, permanent grass farms, or on ley farms? Where is most of the permanent grass of the country, and where is the best and where the worst—on nondescript farms, or on permanent grass farms? These are all essential facts to be known in the formulation of a long-term national policy for agriculture. The facts are only on the land, the agricultural statistics cannot give anything approaching a full answer to any one of these questions. The answer to these questions, and to equally important questions connected with facilities at the farmstead and over the fields (watering, drainage, and the condition of fences), can only be given by

[4] Rice Williams (see 'The Growing Danger of Lime Depletion in Welsh Soils,' *Welsh J. Agric.*, 1937) has estimated that the permanent grass and arable land of Wales alone require at least 1½ million tons of lime to bring the lime status to a satisfactory level. The distribution of lime for England and Wales together under the Land Fertility Scheme has not, up to date, been materially in excess of one million tons.

a properly conducted survey carried out over the whole country and on a uniform plan. Map also the type or class of all the rough grazings and permanent grass (in a manner broadly similar to the survey of Wales recently undertaken by my department), and map the ploughability of the several fields: then, and only then, should we know where we stand. To conduct such a survey would be a relatively simple matter. To my mind, until such a survey is put in hand, and the lessons of the same—cruel and bitter the lessons will be—duly digested, there is little hope that the country at large will realise either the deplorable condition of our acres or their immense potentialities. The first necessity from all points of view—that of the statesman, the townsman, farmer and countryman, in short, that of the nation—is literally and in fact to put rural Britain on the map.

Only when rural Britain is on the map shall we be able amongst other matters to decide where in the national interest it is desirable to extend arable farming, and where ley-farming, and where it may be necessary or permissible to tolerate nondescript and permanent grass farming.

Having discussed systems of farming and levelled certain well-founded criticisms against nondescript and permanent grass farming, I am now in a position to unloose a whole barrage of criticism against permanent grass as such: and note this, the case for ley-farming is implicit in almost every word of just criticism that can be levelled against permanent grass.

My criticisms of permanent grass are general and particular; here are my general criticisms. The psychological influences of permanent grass go much further than I have already indicated; of course there are clever managers of permanent grass, but I doubt if even the best practitioners are on a par with the most proficient arable and ley farmers; while speaking generally, the standard of management of permanent grass, I should say, stands to the management of arable land, taking the country as a whole, as certainly not more than 60 (and probably hardly as much as 40) to 100. Leys as long as they continue to be managed as such are almost invariably managed better than permanent grass; they are both easier to manage properly and the inducement so to manage them is greater.

My next general criticism is that of the veterinarians who are telling us with a voice that becomes daily louder and more united that permanent grass harbours many of the organisms of disease.

My next, because as I have already said an enormous proportion of our permanent grass is in urgent need of lime, a need that becomes ever more serious in view on the one hand of extended milk production, and on the other of the movement in the direction of rearing and slaughtering increasing numbers of young animals. There is only one correct and entirely satisfactory way to apply lime, and that is under the plough, and I think this fact alone is sufficient to condemn not thousands, but at the very least three million acres of ploughable permanent grass, mostly quondam arable, in England; in Wales to my own certain knowledge it is enough to condemn something over 700,000 acres.

My last general criticism of permanent grass is that good young grass properly conserved can be made of immense value to help out the winter ration. Grass silage (and probably dried grass also) is bound eventually to come into its own. Bad grass cannot, however, make good silage or good dried grass, while everything is to extend the season over which it is possible to dry grass and make silage—special purpose leys can help enormously to this end.

My particular criticisms of permanent grass, considered as grass, are these. Even the best permanent grass is far too weedy and much more weedy than first-class leys, and the best permanent grass has a shorter growing season than can be arranged for by a sequence of good leys. Exceedingly productive leys can be maintained on soils incapable of holding and incapable of being made to hold good permanent grass.

I want first to say a little about weediness, and this will lead naturally to the considerations around which the strongest case for ley-farming on grounds of pure husbandry is to be made.

Weediness makes for uneven grazing—witness, for example, the effect of buttercups ; it therefore makes for a waste of valuable material ; it also makes for an uneven spread of urine which cannot be mechanically rectified. Because of this, and for another reason now to be explained, weediness or any tuftedness in a pasture reacts against the enhancement of soil fertility, as well as causing the waste of edible material.

My ' other reason ' is that herbage returned to the soil through the animal, provided the lime and phosphate status of the soil is maintained at a proper level, leads to greater soil enrichment and productivity than when such herbage is allowed to rot back, a fact which has been shown by numerous experiments conducted at Aberystwyth,[5] and which tends to add emphasis to the teaching of our own and other experiments, as, for example, those of Mr. Martin Jones, on the profound influence of night paddocking and of any even slight robbing of Peter to pay Paul. These experiments, coupled with observations over a great number of years, particularly striking phenomena now presenting themselves on the lands where we are conducting our Cahn Hill experiments, force the conclusion upon me that urine has a virtue greater than is fully appreciated, and a virtue that reveals itself on land no matter how generously manured with what have come to be regarded as standard dressings of CaPKN. Consequently any system of grassland management, or for that matter of farming, that does not make the best use of what Mr. Bruce Levy of New Zealand has so aptly, but possibly one-sidedly, described as stock nitrogen, is open to grave criticism.

Because of weediness, tuftedness and uneven grazing, and of herbage never converted, and because of night paddock and quasi-night paddock effects, stock nitrogen is wasted, or uneconomically distributed, to a far greater extent on permanent grass than on leys ; it is so wasted, and often to an exaggerated extent, on even the best fatting pastures, and particularly so when watering arrangements are ill arranged. The matter, however, goes much further ; the fertility accumulating under the best grassland (permanent grass and leys alike) becomes in excess of what can be cashed from the grass-clover covering. All very old

[5] Experiments now in progress at the Welsh Plant Breeding Station, and see R. G. Stapledon, ' The Improvement of Grassland,' *Journal of the Bath and West and Southern Counties Society*, 1937–38.

sods become in effect, and to a greater or lesser extent, pot-bound, with the result that the plant covering is incapable of reacting in full measure to the inherent fertility of the soil, while to plough, aerate and lime (where necessary) is to give life to favourable biochemical changes and further to enhance the productivity of the soil. The best grassland holds within itself an immense store of arable potentiality, while the soil rejuvenated by ploughing and aeration, even after yielding several white straw or other crops, can be put back to ever better and better grass. That is the experience of every competent ley-farmer, and ley-farming is creeping into ever better and better permanent grassland.

To plough up an old sod full of white clover, and one that has carried an abundance of stock, and therefore which has been well impregnated with stock nitrogen, and to harrow lime into such upturned sod, is to make and spread a compost at one operation. This, in short, is to mix with the soil three essential ingredients, vegetable and animal residues, and lime, and under conditions most conducive to favourable biochemical activity. It is the arable or crop-producing attributes of sod that I maintain constitute the strongest case for ley-farming, for without the intervention of cropping the full fertility value of superb sods can never be cashed.[6]

At the other extreme—the poorest soils—there is nothing to match the continued ploughing down of sod, accompanied by adequate liming and phosphating, to build up fertility. In my own experiences of land improvement gained on what must be some of the poorest soils in Britain, as well as on soils of great inherent virtue, I have been astonished at the progressive improvement in sward and carrying capacity attained when three or four four-year leys have been ploughed down in succession (each sown on the upturned sod of its predecessor) without the intervention of a removed nurse crop or of a hay crop. The sequence here is all grass, all grazing and stock nitrogen the whole way, the plough being called in only to assist in compost-making and to ensure adequate admixture of lime, phosphates, organic residues and soil, and to prepare the way for the sowing of the sequential leys. By the adoption of this procedure over a sufficient run of years it is possible to bring land of a most unpromising character into a condition capable of maintaining a rotation balanced between leys and white straw and other crops.

There is nothing new in the idea of sowing down immediately on the upturned sod, just as there is nothing new in the idea of ploughing up grassland as a means of improving it. Marshall as long ago as 1789 remarked, 'Old pasture lands overrun with ant-hills and coarser grasses are not easily reclaimed without the powerful assistance of the plough.' The idea of the all-grass rotation perhaps, however, has an air of novelty about it; wild white clover as a commercial commodity is comparatively novel; cheap phosphatic manures are comparatively novel; the tractor and modern implements are a recent novelty, and more recent are the

[6] It is true that it is sometimes difficult to utilise the richest sod to the best arable advantage because of wireworm and the lodging of cereal crops. Much remains, however, to be achieved in the direction of the breeding of short stiff-strawed cereal varieties, while in so far as cereals are concerned wireworm is not so destructive after properly managed leys as after permanent grass.

improved and leafy strains of grasses—all these taken together, if they are to be used to best advantage, must inevitably spell novel rotations. One of the greatest merits of improved technique based on modern facilities for putting down leys on upturned sods, and without resort to covering crops, is that by the periodic adoption of this method (that is to say, as and when necessary) the farmer is enabled to take his leys around the farm sufficiently quickly and before there is any sward deterioration, and in sympathy with the lime demands of his animals and the lime requirements of his soil.

It is somewhat remarkable that so little exact experimental or statistical evidence exists for comparing the yield of leys, either in grass, milk or meat, with permanent pastures on similar soils and under precisely comparable conditions. We have Mr. Roberts's evidence from Bangor,[7] which is in favour of the ley, and not a little evidence from Aberystwyth, also in favour of the ley.[8] Evidence from grass less favourable to the ley has also been brought forward by various authors. The most convincing evidence, however, is the performance and experiences of competent practitioners in the art of ley-farming, and thus the results of investigations and inquiries conducted by Mr. John Orr, lately of Manchester University, are particularly informative and are wholly in favour of the ley.[9]

At present I am engaged upon collecting the material for writing a book on ley-farming. As a preliminary I sent out a questionnaire and have had a most helpful and gratifying response from farmers. The evidence from the replies received is overwhelmingly in favour of the ley, great stress being laid on the improved quality and stock-carrying capacity of the ley grass compared to the quondam permanent pasture, and the extended grazing season provided by the leys. The leys would seem, however, to have justified themselves not only in an extended grazing season, but by virtue of giving grass at periods within the grazing season proper when owing to weather or other conditions grass is liable to go short. Thus Major Dugdale of Llwyn, Montgomeryshire, who is rapidly and methodically (at the rate of about fifty acres per annum) converting the permanent grass of his farm into a sequence of leys by the methods I have discussed, informs me that during the early and unprecedented drought of this year the leys were invaluable, ' and thanks to them my ewes and lambs which had a turn at them all have done better than usual and have not suffered from the drought.' Mr. R. L. Muirhead, of Borsdane Farm, Westhoughton, Lancashire, who is well known for his enterprise in ley-farming, speaks equally highly of the value and performance of his leys during the past critical months, and particularly interesting is his remark that ' the younger fields stood up to the dry conditions better than the others, and the youngest of all (sown

[7] E. J. Roberts, ' I. The effect of wild white clover on the live weight increments from a temporary pasture. II. A comparison of temporary and permanent pasture,' *Welsh J. Agric.*, Vol. 8, pp. 84–93 (1932). E. J. Roberts, ' Comparison of (*a*) an old with a temporary pasture and (*b*) two temporary pastures, from one of which wild white clover had been omitted at seeding down,' *Welsh J. Agric.*, Vol. 11, pp. 132–9 (1935).

[8] See R. G. Stapledon, *The Hill Lands of Britain*, London, 1937.

[9] See John Orr, ' Grass and Money,' *Scot. J. Agric.*, Vol. 20, pp. 31–40 (1937).

last August) with Italian rye-grass has done best of all.' [10] Mr. Wilks, of Whartons Park, Bewdley, Worcestershire, who after prolonged attempts at improving the poor permanent grass on his farm is now rapidly getting into the ley system, says that during last back end (1937) the whole of his grazing came from leys and newly grassed areas. The old permanent pastures did not recover from the late summer and early autumn drought of that year, and the leys carried all the stock from July onwards. During the drought of this spring his position was never difficult, the maiden leys providing an abundance of good pasture, and these after being grazed into May will be mown for hay.

In a recent letter to me Mr. Wilks concludes with this peculiarly significant statement: 'An interesting sidelight is that the arable crops on land recently ploughed out have stood the drought much better than those on the stale old arable . . . the whole thing is complementary and balanced.'

The experiences of Colonel Pollitt, of Harnage Grange, Cressage, Shropshire, are in keeping with those of Mr. Muirhead and Mr. Wilks. Colonel Pollitt has also sown out early in May without a nurse crop and has been able to start serious grazing (ewes and lambs) in the first week of July, thus obtaining valuable young grass at what is often a critical time of the year. On a field thus treated Colonel Pollitt also wintered cattle continuously from November 1 to May 1, and he informs me that there was no poaching except at the gate.

The ley, furthermore, affords great scope for special treatment with a view to providing grass when it will be most wanted. Ley grass put up for the winter carries green and protein-efficient into February, March and April altogether more effectively than does permanent grass, and this is perhaps one of the greatest merits of the ley, and a merit which by virtue of further research in plant breeding in the direction of producing winter green and winter growing strains is likely to become increasingly pronounced.[11]

The employment of different seed mixtures with a view to giving grass more particularly at different and explicit periods of the year affords additional scope to the ley-farmer. Thus at Aberystwyth we have found that a mixture consisting predominantly of Danish meadow fescue and Aberystwyth S. 48 timothy gives exceptionally good grazing during July and August. On this and similar points there is, however, need for greatly extended investigation.

I have now made my case for ley-farming, but I am not at present claiming that all permanent grass should be brought under the plough; before that claim could be substantiated we want a proper survey and a great deal more experimenting. Apart from steepness, boulders and such like, low rainfall and heavy clays present their special problems. As to the clays, the fact that it is a perfectly sound procedure to re-grass

[10] This performance of Italian rye-grass is on all fours with results obtained for the past four years with Italian rye-grass at the farm of the Cahn Hill Improvement Scheme.

[11] See R. G. Stapledon, 'Immature Grass and Young Swards.' Part I, *J. Minist. Agric.*, Vol. 44, pp. 317–29, July 1937; Part II, *J. Minist. Agric.*, Vol. 44, pp. 442–9, Aug. 1937.

straight away on an upturned sod makes a lot of difference, as does the soundness and feasibility of the all-ley rotation, while we have the tractor and modern implements. To make it possible to establish leys without undue risk of failure on the heaviest soils is to-day, I feel convinced, only a matter of sufficient experimenting as to ways and means. The same is, I am sure, largely true of establishing leys in regions of low rainfall. Mr. Mansfield seems to have no difficulty in establishing excellent leys in this district not remarkable for its high rainfall, while everybody who farms on something akin to the four-course rotation after all establishes leys. What is wanted in order to establish a foolproof and almost weather-proof technique is much more experimenting. There is a right date to sow for every district, while in the driest areas I doubt the wisdom of sowing under a nurse crop, for the quicker growing cover crop must compete exaggeratedly with the slower growing seedlings for what little moisture there may be. It may be unwise under such conditions to include even Italian rye-grass in the mixture, for this is always by far the quickest grass seedling to get off the mark, while it would seem to be of supreme importance to obtain a scrupulously clean seed bed, and to bring in the mower at the first sign of weeds gaining dominance. The successful grassing of new golf courses in regions of low rainfall, I think, holds valuable lessons for the would-be ley-farmer—' put as little as possible to compete with the grasses you ultimately want ' would seem to be the teaching. I would again emphasise that it is not sufficiently realised that a ley sown without a nurse crop very soon starts earning money on its own account, and where 4–6–8–10 year leys are at stake it is poor economy to jeopardise the whole for the sake of a preliminary cash crop.

I cannot conclude my address without a little more detailed reference to the ley itself. The chief points at issue are how to establish it, what to sow and how long to leave it down. Not one of these questions can be answered in general terms, but there are in each case fundamental principles at stake. The fundamental principle relative to duration is the fertility attributes of the sod. From that point of view, and considering alike soil condition and manurial residues, my friend Prof. Robinson (1937) in the informative letters he has so kindly, and if I may say so, attractively, written for my major enlightenment, would seem to agree with me that there is everything to be said for the four-year ley, ending, as I would wish to insist, with at least two years of honest hard grazing, with urination and spread of white clover. The general principle here is ' to plough down the sod before it has by one jot deteriorated.' It has, however, to be remembered that grazed swards do not leave behind them a sod with a deep-going root system; hayed swards develop a deep-going root system. In the interest of general fertility and soil condition I hold that it is sound practice, ever and anon, to plough down sod with a deeply penetrating root system. Now from the point of view of hay production, the highest yields are obtained from leys in their first and second harvest year—that is to say, as long as late-flowering red clover lasts. In general my view is this, that the best practice founded on scientific principles would be to employ 1–2 year leys for hay and

4–6 year leys for grazing only. The three-year ley is rather like the dual-purpose animal. Although it is a brave southerner who would criticise Scottish practice, I am inclined to criticise excessive dependence on dual-purpose (hay-grazing) three-year leys. I would rather have a sequence of 1–2 year deep-rooting-hay leys following after four-year-white-clover-replete-shallow-rooting-grazing leys. This procedure would give more hay, more grazing and more fertility. With apologies to Aberdeenshire, that is my considered opinion. In any event my criticism of the very best practitioners of ley-farming is that they do not use leys of different kinds for different purposes, and do not rotate all the different sorts of leys after each other all round the farm to anything like a sufficient extent, for it is thus, and only thus, that all-the-year-round grazing is to be obtained. This is too large a subject to discuss in detail here, but it is one demanding much thought and much agronomical research.

In passing I might say that in my view no problems so much as those of grassland demand prolonged and large-scale agronomical investigation. I would wish to distinguish between, on the one hand, agronomical research, and on the other, scientific research as normally understood and conducted. The major aim of agronomical research, which is essentially field research, is to study all the factors which are operative at once and together, and in their natural interplay, for 'nature is a theatre for the inter-relations of activities.' Such a procedure, it may be said, is impossible, or at least unscientific. It is certainly not impossible, and if it is unscientific it will yet remain agronomical, and many of the problems of agriculture are more likely to be solved, shall I say, by agronomical investigation than by scientific research, while nearly all the results of scientific research have to pass through the sieve of an immense amount of agronomical investigation before they can be made useful, and in some cases perhaps before they can be other than positively dangerous to the practitioner. The technique of agronomical research entails a great deal more than blindly following all the elaborate rules and regulations laid down by the statisticians; indeed, such rules and regulations are of no fundamental significance in the proper planning of an elaborate series of field experiments. They are sometimes, but by no means always, useful in the actual placing of plots on the ground, and they are sometimes essential, but are by no means always necessary, in the examination of quantitative data. One effect of the modern glorification of statistical methods has undoubtedly been a tendency to obscure the wood for the trees, to concentrate on the part, often an isolated part (yield, for example), instead of the whole ; and, worse still, to fill the agronomist with a medley of complexes and inhibitions which have reacted adversely on the development of a technique adequate to solve a large number of the problems that can only be solved by highly complicated field experiments. Many agronomists are almost too frightened to set up the sort of experiments their experiences teach should be set up, because they are timorous lest the data could be made amenable to statistical analyses. Agriculture would have been the gainer if the agronomist had never been taught to be timorous, and if he had plodded away undeterred and undismayed at the details of his own technique, when by now perhaps he would have been able to justify his claim that what is primarily wanted to-day is enormously increased facilities for the conduct of field experiments in contra-distinction to field trials and demonstrations. That at least is my claim, for I claim to be an agronomist, and in that capacity one who has been responsible for the setting up of hundreds of weird little field experiments involving in all literally thousands of plots.

As always, however, the greatest and the final hope is the farmer himself, for he at least is untrammelled by the technique of science, and is not a slave to the fashions current in science, while his major training is not in collecting data, but in the gentle art of unadulterated observation. Just because, therefore, of the immense accumulation of scientific knowledge, so much of it but half digested in the practical sphere, never so urgently as at present has there been such a necessity for an abundance of well-informed, originally-minded and affluent pioneers, men willing and eager to transgress against every canon of good husbandry, and to explore, and almost *de novo*, the whole field of rotation of crops, and the whole idea of rotation of pastures of different types and of stock over the surface of the farm.

This has been a long digression ; it has, however, been relevant to my theme, and it has been on a question of undeniable importance and about which I think I am entitled to express opinions. I will now return to the ley.

Grazing management affects the permissible duration of the grazing ley to a marked degree. Thus he who bails cattle or folds poultry can keep his leys down much longer than the ordinary farmer who thinks he is grazing intensively, but in fact is doing nothing of the sort ; only the close folder, or the tetherer, really grazes intensively, and by intensively I mean without waste of any sort. But even under the cleverest management sooner or later the sod will begin to become pot-bound, and according to soil type, bent, soft brome, Yorkshire fog, weeds or moss will proclaim the need of the plough and a new start.

What to sow and how to establish are in the main twin problems—twin to this extent, that what to sow is determined much more by every shade of after-management that it is proposed to follow than by soil type ; the trouble here is that agricultural chemistry has such a terribly long start of agricultural biology. Grassland, like every crop the farmer handles, is the plaything of soil, climate and the biotic factor ; with grassland the master factor is the biotic—that is to say, what man himself does with his animals. One, and the most obvious, example will suffice—the use and abuse of Italian rye-grass. Italian rye-grass is essentially a grazing grass ; if allowed to grow away in a hay mixture it will smother and depress other and higher yielding hay grasses. It should therefore only be included in hay mixtures when such mixtures will be grazed long into the spring or early summer, and when after a small and herby hay crop aftermath is of prime importance. Italian rye-grass is of its greatest value for sowing with grazing mixtures put down on an upturned sod. The aim here is two-fold ; firstly, to bring treading feet and urine on to the developing sward as soon as possible—this is the function of the Italian rye-grass ; and secondly, to encourage the spread of wild white clover as rapidly as possible—this is the combined function of light (keeping the Italian rye-grass in reasonable subjection), the treading feet and the urine.

The so-called indigenous strains! Badly called, and I am afraid that I have been largely responsible. In the few words I have to say on this subject I will confine myself to the Aberystwyth bred strains, for here at least I am talking about something definite and about which I myself at all events may be supposed to know something. For the sake of brevity I will lump the findings of all our experiments, and of all my own experiences, and those of my colleagues, into a single short paragraph.

For the ordinary three-year hay-pasture ley on medium-good soil, postulating the inclusion of wild white clover and good urination, the Aberystwyth pasture and pasture-hay strains are by no means an absolute necessity, but in reasonable amount (say up to about one-fifth to one-third of the rye-grass, cocksfoot and timothy contribution) I recommend their inclusion for the sake of the extra back-end grazing they will give, and to add leafiness to the hay crop. For leys of four years and longer duration, I believe a contribution of Aberystwyth pasture or pasture-hay strains of not less than one-third of the contribution of rye-grass, cocksfoot and timothy always to be justified. On really poor soils and for re-grassing derelict grasslands there can be no question as to the absolute necessity of including the pasture and pasture-hay strains. On our Cahn Hill lands, and elsewhere, we have made quite remarkable swards by using such strains wholly, or up to two-thirds of the mixture, where with the non-pedigree bred strains it has been impossible to establish a sward capable of maintaining itself for more than twelve months. You will note I have talked explicitly of the Aberystwyth pasture and pasture-hay strains. We have now early hay strains coming on such as Dr. Jenkin's S. 24 perennial rye-grass, his S. 51 timothy, and my own somewhat modified S. 37 cocksfoot, which will I think vie with the ordinary seed of commerce in earliness and bulk during the first and second harvest years, and which are much more leafy. The matter here will turn almost wholly on the relative cost of the pedigree and non-pedigree seed, for manifestly an expenditure on seed that would be abundantly justified for a four- to twelve-year ley might not be an economic proposition for a one-, two-, or three-year ley. If, however, the hay strains ultimately prove themselves to have sufficient virtue they are bound in due time to replace the ordinary commercial strains, and in fact by a process of substitution to become in effect the ordinary commercial product. This I think will be the destiny anyway of Dr. Jenkin's S. 24 rye-grass, for as well as being early and relatively leafy it gives much better July-August grazing than the ordinary Irish and Ayrshire rye-grass.

In this matter of the Aberystwyth strains, however—such is the deeply penetrating influence of psychological factors—I can have no cause for complaint if you deem it well to regard me as a prejudiced witness, but if you so regard me, please yourselves be sufficiently broad-minded to come and see our trials, or go and have a look at one of those which with the help of the Royal Agricultural Society we are setting up in various

English counties ; or better still, experiment for yourselves under your own, your very own, *scheme of management.* It may be that management in some cases is so superbly good that it hardly matters what a man sows, while in others it may be so supremely bad that no proper use can be made of a good thing when a man has got it.

I am afraid I have adopted an unusual course in my approach to my subject ; I have not followed normal practice, for instead of reviewing the data and evidence available I have in effect reviewed my own reactions to the implications of the work with which I have been connected for the past twenty-five years and more. Perhaps I need not apologise for this, for after all facts and data are of no practical use until people grapple with the practical implications. Instead of my ' facts '—and scientific ' facts ' are not always correct—I have put my grapplings before you, that is all, and if justification is necessary I think sufficient justification is the admittedly deplorable condition of a huge acreage of this country, the dilapidated condition of many of our farms and farmsteads, and the therefore necessarily backward state of much of our farming. Two needs seem to me to be crystal clear : first, the conduct of a survey on the land—and I believe every agricultural scientist, though perhaps not every farmer and every economist, would agree to ' on the land ' somewhat on the lines I have suggested—and then the ways and means of getting the plough into the grasslands that the survey conclusively proves ought to be ripped up. Working capital, and the correct expenditure of that working capital, is in the last resort the only solution for our derelict and quasi-derelict acres.

I like the American idea of loans with a working plan ; of loans with advice. I do not believe that the history of the years since about 1894 show that the spasmodic periods of agricultural prosperity that have on occasion intervened have been responsible for a great deal of land improvement, or for a proportionate improvement in the equipment necessary for productive farming. Prosperity as such in agriculture, as in industry, is to a large degree a function of equipment, for without the necessary equipment it is impossible to farm economically, just as it is impossible to manufacture economically.

Again, it is unreasonable to expect that a man devoid of working capital, and probably the son of a man similarly devoid, should have all the knowledge of how best to farm, and particularly of how best to improve land (in which art he will necessarily have had no sort of experience), in sympathy with adequate working capital suddenly provided for the purpose. Advice, and some measure of control, must necessarily go with credit facilities, and in so far as breaking up grassland is concerned I like still better the American idea of group loans, and of a ' master borrower.' The ' master borrower ' in this case would be set up as a contractor with tractor and necessary equipment to break up the grasslands, for it is important to remember that ploughing up of this sort is essentially tractor work, that it interferes with the normal routine of an ill-equipped farm, while tractors are to all intents and purposes non-existent in many of the districts where wholesale ploughing up is most necessary. My own experiences are interesting in this connection. We tested the desire for contracting last year, and had three times as many applications as we could fit into the acreage we could do, while now, and because of the demand our work has created locally, a lorry contractor in the neighbouring village has acquired a tractor, and is fully engaged on contract ploughing.

I like also the American idea of being boldly eclectic and scheduling particular districts as being eligible for their rehabilitation loans; indeed, I was foolhardy enough to make a suggestion very much on these lines in my book *The Land Now and To-morrow.* There are innumerable districts that should be similarly scheduled and similarly helped in this country, but always through financial help *cum* technical advice terminating in an agreed working plan; and here again my own experience comes to support my contention, for in those cases where we contracted we only did so when the farmer agreed to follow all our advice as to subsequent operations, manures and seeds, to the letter, and in all cases the farmer has done so, and demonstrably to his own advantage.

The breaking up of derelict grassland is to be helped forward not only by loans, but by a reorientation of such working capital as the farming community possesses, and also, I think, by a reorientation of the monetary and other arrangements existing between landlord and tenant.

Ley-farming in my view affords great scope for such reorientation, for it would make possible, and on a general scale, a variety of methods of share farming. For example, one might conceive of a mechanised wheat grower operating over a large number of neighbouring ley farms on a share basis; another man on a share basis might be running the poultry, the proprietors themselves being primarily interested in the adequacy of the rotation and farming operations, and possibly in one major product—milk, shall we say? By this means farmers should achieve a better return on such working capital as is available, and also the nation should achieve a more balanced specialisation between farming *qua* farming and commodity production and disposal. Landlords themselves with advantage could often think out methods of sharing-in with their tenants, and ley-farming opens many avenues of approach to such sharing-in, but in any event it behoves the landlords of many districts to be alive to changing times, and to be ready for the day—not, I think, far distant—when better tenants will be found for farms which are going concerns on the ley-farming basis than for those which are nondescript or permanent grass. It may thus prove to be a wise policy to adjust leases, and even financially to assist purposeful tenants towards that system of farming which will accord best with the trend of national and international events.

Let me insist, in conclusion, that the affairs of agriculture, slowly moving as they necessarily must be, are ill adapted to respond to the dictates of any immediate expediency, for expediency is ever shifting, and at the best 'is the mere shadow of what is right and true.' To be ever prepared for change in a world that is ever changing can be the only possible basis for a sound agricultural policy for this country, since we are so peculiarly liable to be crucially affected by happenings beyond our own control, beyond our own jurisdiction and beyond our own borders.

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REPORTS ON THE STATE OF SCIENCE,

Etc.

SEISMOLOGICAL INVESTIGATIONS.

Forty-third Report of the Committee of Seismological Investigations (Dr. F. J. W. Whipple, *Chairman*; Mr. J. J. Shaw, C.B.E., *Secretary*; Miss E. F. Bellamy, Prof. P. G. H. Boswell, O.B.E., F.R.S., Dr. E. C. Bullard, Dr. A. T. J. Dollar, Sir Frank Dyson, K.B.E., F.R.S., Dr. A. E. M. Geddes, O.B.E., Prof. G. R. Goldsbrough, F.R.S., Dr. Wilfred Hall, Mr. J. S. Hughes, Dr. H. Jeffreys, F.R.S., Mr. Cosmo Johns, Dr. A. W. Lee, Prof. E. A. Milne, M.B.E., F.R.S., Prof. H. H. Plaskett, F.R.S., Prof. H. C. Plummer, F.R.S., Prof. J. Proudman, F.R.S., Prof. A. O. Rankine, O.B.E., F.R.S., Rev. C. Rey, S.J., Rev. J. P. Rowland, S.J., Prof. R. A. Sampson, F.R.S., Mr. F. J. Scrase, Dr. H. Shaw, Sir Frank Smith, K.C.B., C.B.E., Sec.R.S., Dr. R. Stoneley, F.R.S., Mr. E. Tillotson, Sir G. T. Walker, C.S.I., F.R.S.)

Meeting of the Committee.

The Committee met once during the year, on October 29. The annual grant of £100 from the Caird Fund was allocated to the University Observatory, Oxford, for work on the International Seismological Summary.

Expenditure on various objects from the Gray-Milne Fund was authorised. Dr. E. C. Bullard gave a short account of the methods adopted in America in the application of seismological methods to the investigation of the thickness of the strata overlying the continental shelf. In view of the fact that research on these lines was likely to be undertaken with the support of the Royal Society, it was decided that no action on the part of the British Association was necessary. Dr. Dollar gave the Committee an account of the British Earthquake Inquiry, which he was organising, and it was decided to give some financial support to the organisation.

The Gray-Milne Fund.

The accounts for the year are reproduced below. The income of the fund has again improved owing to an increase in the dividend paid by the Canadian Pacific Railway. Expenditure on the Milne Library includes the purchase of Dr. Davison's book, *Great Earthquakes*.

Gray-Milne Fund.

	£	s.	d.		£	s.	d.
Balance, July 1, 1937	187	4	2	Milne Library	3	2	6
Trust Income	65	4	10	Insurance		15	0
Bank Interest	1	1	10	Printing (Bullen's Conversion Tables)	14	5	6
				Jaggar Shock Recorder	21	0	0
				British Earthquake Inquiry	10	0	0
				Balance, June 30, 1938	204	7	10
	£253	10	10		£253	10	10

Seismographs.

The six Milne-Shaw seismographs belonging to the British Association have remained on loan to the seismological stations at Oxford (2), Edinburgh, Perth (W. Australia), and Cape Town (2).

During the year a Jaggar shock recorder has been made for the Committee at Bristol under the supervision of Dr. C. F. Powell. This instrument is to be set up at Dunira, near Comrie, the village in Perthshire which is famous for the prolonged series of minor earthquakes in the last century. It may be recalled that a Committee appointed by the British Association set up seismometers, in Comrie and near by, with which to measure the amplitude of earthquake waves. Eight of the seismometers were inverted pendulums, designed by Prof. J. D. Forbes and 'working on the principle of the watchmaker's noddy.' These instruments were affected by the local earthquakes on two or three occasions, but they were not sensitive enough to be disturbed by the majority of the shocks. After an interval from 1844 to 1867 the Committee for registering earthquakes in Scotland was reappointed. Only one of the original seismometers was then in use, the 10 ft. inverted pendulum in the church tower at Comrie. The Committee decided to adopt a suggestion of Mallet's and provide a set of small cylinders which were to topple over when an earthquake occurred. A special hut, which still stands in the grounds of 'Dunearn,' was allotted to the cylinders, but it is believed that no earthquake ever bowled them over. As far as is known none of these primitive seismometers survives in Perthshire, but there is in the Royal Scottish Museum at Edinburgh one of Forbes's inverted pendulums. It is hoped that the new shock recorder will eventually provide some evidence as to the nature of the earth-movements in the Comrie region. The Committee is indebted to Mr. W. G. Macbeth of Dunira for allowing the installation of the instrument, and to Mr. White who is undertaking to operate it.

Geocentric Co-ordinates.

Owing to the progress in the precision of seismological observations, and in the accuracy of the tables with which the observations can be compared, it has now become desirable to take into account the ellipticity of the earth, both in locating the epicentres of earthquakes and in discussing the behaviour of seismic waves of different types. It was pointed out by Gutenberg and Richter, in 1933, that this could be done most readily by using geocentric co-ordinates instead of the ordinary geographical co-ordinates. Investigations by Dr. Jeffreys and by Dr. Bullen have confirmed the desirability of this refinement. Tables giving for each observatory rectangular co-ordinates on the new system, or rather the direction cosines of the radius from the centre of the earth, are required. The Committee enlisted the help of Dr. L. J. Comrie, who has had the necessary calculations made and is seeing the resulting tables through the press. These tables will be published in the autumn.

Geographical angular distances have been employed hitherto in the International Seismological Summary, as in almost all other work on earthquakes; *i.e.* the angle between the verticals has been regarded as giving the distance between two points on the globe. A method of utilising the data without recomputing the distances *ab initio* has been devised by Dr. Bullen. Tables computed by Dr. Bullen for use in the application of this method have been published by the Committee, with an Introduction by Dr. Jeffreys.

British Earthquake Inquiry.

The Organisation for the Collection of Seismic Data.

Through the agency of an organisation developed since October 1935 to collect detailed non-instrumental data about earthquakes disturbing the British Isles, twenty-one undoubted earth tremors, thirteen doubtful tremors, six land-subsidences and the seismic effects of three explosions have been investigated to date, and material gathered for a catalogue of quakes noticed between January 1, 1916 and October 1, 1935.

At present the personnel of the organisation involves 287 permanent voluntary reporters, recruited from forty-four counties in Great Britain and the Irish Free State, and others in the Channel Islands, each of whom notifies Dr. Dollar at Emmanuel College, Cambridge, immediately any earth tremor disturbs the locality of the reporter concerned. Often these reporters assist in the subsequent distribution and re-collection of questionnaires relating to effects of such a tremor. Their number has been increased by 221 since July 1, 1937, and additional help has been obtained from officials in Government, University and private seismological observatories, the British Broadcasting Corporation, the Trinity House Corporation, meteorological observatories and schools, as well as from the Press Association and daily newspapers.

The greatest part of the information is gathered by questionnaires, more than 95 per cent. of which are dispatched from Emmanuel College. In June 1938 a third and abbreviated edition of the questionnaire was issued; this has proved of greater general utility than previous more elaborate editions.

The Seismic Data gathered between July 1, 1937, *and June* 30, 1938.

Since July 1, 1937 details have been collected about the following tremors felt in the British Isles :

Earthquakes :	
July 9, 1937 . . .	Walsall, Staffordshire.
July 20, 1937 . . .	Perthshire.
September 8, 1937 . .	Horsham, Sussex.
December 4, 1937. . .	Comrie, Perthshire.
March 21, 1938 . . .	South-East Edinburgh, Edinburgh.
June 11, 1938 . . .	Ghent, Belgium.
Subsidences and Mine-shakes :	
September 13, 1937 . .	Cudworth, Yorkshire.
December 14, 1937 . .	Nelson, Glamorganshire.
December 30, 1937 . .	Norwich, Norfolk.
January 1, 1938 . . .	New Tredegar, Monmouthshire.
Explosions :	
November 20, 1937 . .	Thrapston, Northamptonshire.
December 1, 1937 . .	Waltham Abbey, Essex.
Origins not Established :	
November 21, 1937 . .	Worthing, Sussex.
December 6, 1937 . .	Tenby, Pembrokeshire.
January 29, 1938 . . .	Great Missenden, Buckinghamshire.
April 13, 1938 . . .	Stepney, London.
April 20, 1938 . . .	Golders Green, Middlesex.
June 18, 1938 . . .	Gilfach Goch, Glamorganshire. (Now being investigated.)

The Belgian Earthquake of June 11, 1938.

During the 12-month period the most important earthquakes originating beneath the British Isles were those of Walsall, Horsham and South-East Edinburgh. These were insignificant, however, in comparison with the earthquake that was centered below Belgium and shook more than 20,000 square miles of country in twenty-nine English counties on June 11, 1938.

As a result of appeals for information through the daily press, three wireless broadcasts and the distribution of numerous questionnaires, 856 reports have been gathered about this tremor from 278 towns in England, the Channel Islands, France, and Belgium. Eight reports have been obtained from seismological observatories in Britain and North-west Europe.

The tremor was noticed mainly by people at rest indoors. Positions in the upper stories of high buildings were especially favourable. Particularly in the east of the disturbed area at least two phases were distinguished, and the motion was described as being a succession of smooth undulations in an approximately east-west direction, conspicuously free from jerks. The numerous accounts of apparent giddiness may be related to the smooth wave-motion experienced.

The only damage on this side of the Channel appears to have been a single fall of a few tiles at Herne Bay, Kent. Appropriately-oriented pendulum clocks were stopped in some cases, and in others, liquids were agitated or spilled. Dogs, cats, and birds showed signs of alarm, and two reports suggest that bees in open out-apiaries were so disturbed by the shock as to have been unmanageable for a time.

The area over which a sound was heard is ill defined, but does not seem to extend far west of the longitude of London. Generally it was likened to a rumble such as might be produced by the passage of a heavily-laden lorry or train.

After-shocks of the Belgian earthquake were recorded at Kew Observatory on the same day at 12.10 and 13.9, and a much larger one on the next day, June 12, at 13.26. Only the last of these was felt in England. It was reported by nine observers. Mutually inconsistent reports of supposed foreshocks and aftershocks were received from about a score of correspondents. It is understood that Belgian seismologists place the epicentre of the main shock near Ghent. The best precedent for tremors affecting approximately the same area is the earthquake of April 6, 1580, which caused considerable damage in Kent. The epicentre of that earthquake is thought by R. E. Ockendon, the editor of the recent reprint of Thomas Twyne's *Discourse on the Earthquake*, to have been near the Straits of Dover.

Seismology at Kew Observatory.

During the year the installation of the seismographs in a new underground house was completed. The three Galitzin seismographs record on one electrically driven clock drum, the two Wood-Anderson instruments on another. A description of the installation is being published in a Memoir written by Dr. A. W. Lee. It is satisfactory to be able to note that the disturbances which affected so seriously the utility of the Galitzin seismograms in windy weather, and which were attributed to the rocking of the observatory, have no counterparts in the records obtained in the new seismological building which is mostly below ground level. A number of technical points with regard to Galitzin seismographs had to be investigated on account of the introduction of a new way of operating these instruments. Details will be found in Lee's Memoir.

The Wood-Anderson seismographs, which are adjusted with a period of $2\frac{1}{2}$ seconds, record the horizontal components of the earth's movement. An instrument with about the same period to record the vertical component is required. An experimental seismograph of this type was constructed in the Observatory workshop and has been in operation for some months. The special feature is the introduction of ' viscous coupling ' (by means of a plunger working in a cup filled with liquid) between pendulum and mirror. Some promising records have been obtained from recent earthquakes, but modifications to the instrument will be required before operation is entirely satisfactory.

A paper by Dr. Lee, ' The travel-times of the seismic waves P and S, a study of data from the International Seismological Summary, 1930 and 1931,' is being published shortly as a Geophysical Memoir of the Meteorological Office.

Seismology in the West Indies.

The series of earthquakes which occurred in 1934 and 1935 in Montserrat led to the despatch of an expedition to that island. Valuable reports on the geological structure of the island and on the distribution of the earthquake centres were written by Mr. A. G. Macgregor and Dr. C. F. Powell. The Wiechert seismograph and eight Jaggar shock recorders are still in operation in the care of Mr. Kelsick, who is making regular reports on the seismic activity in that island and is also collecting information about shocks which are felt in other islands. From August to November 1937 about forty earthquakes were reported by observers in Dominica. The Royal Society has nominated a West Indies Seismological Committee, and this Committee has under consideration the despatch of an expedition to Dominica. The earthquakes in that island have been less frequent, however, in recent months, and the proposal is therefore in abeyance at present.

The International Seismological Summary.

A Note by J. S. Hughes.

The International Seismological Summary has now been prepared in manuscript as far as July 1933 ; January, February and March are in the press, while April, May and June are in process of being finally checked through.

The number of earthquakes dealt with in a given period of time remains roughly constant but with a fluctuation which is mainly dependent on the presence or absence of cases in which a long sequence of after-shocks to an earthquake of great intensity occurs in a region well equipped with recording stations. Such a case was provided by the Sunriku earthquake of March 2, 1933, origin 39·1° N., 144·7° E., off the east coast of Japan. This earthquake, which is notorious for the devastating tunami it produced, was followed by a large number of shocks from the same neighbourhood, but apparently not from a single focus. This interesting series of shocks was worked out in as much detail as possible and a number of different epicentres were determined. It is not claimed, however, that finality has been attained, and the observations, extending over many days, would afford a good subject for special study. Of the earthquakes listed for the month of March 1933, 142 were after-shocks of the series in question.

In the portion of the Summary dealt with during the past year, there are many large earthquakes and numerous cases of deep focus. Notable among the latter are the earthquakes of October 14, 1932, 31·6° N., 13·8° E., where distant records are completely absent, and January 9, 1933, 36·5° N., 70·5° E., where there is a wealth of observations over a range varying in epicentral distance from 4° to 80°. In the former case, the epicentre being in the Pacific to the south of Japan, there were excellent observations of P and S at 40 stations, all within a distance of 11·7°, but there were no observations outside Japan. The focal depth (determined at Tokyo) was 300 km. The other epicentre, which is in Kafiristan near the north-west frontier of India, is one to which 10 deep-focus earthquakes were assigned in the years 1921 to 1930.

From January 1933 onwards an attempt has been made to distinguish in the Summary between compressional and dilatational longitudinal waves. For a compressional wave, where the initial motion of P, PKP or PKKP is away from the epicentre, the letter 'a' (anaseism) is entered after the reading. If the wave is dilatational, or towards the epicentre, the letter 'k' (kataseism) is used. This notation was adopted by the International Seismological Association at Edinburgh in 1936, the use of the adjectives anaseismic and kataseismic having been proposed by the Rev. E. Gherzi, S.J., as long ago as 1924.

If the components of displacement in the onset of P are recorded by the observing station, the direction of initial motion is known, and the discrimination between 'a' and 'k' can be made after the epicentre has been determined. Particularly useful is the Z component, as an upward initial motion always indicates an 'anaseism' and knowledge of the position of the epicentre is not required. A good many observatories are already providing in their bulletins the necessary information with regard to the initial movement of each earthquake. It is hoped that the practice will be adopted generally.

Work on Transmission Times and on Periodicity.

By Dr. Harold Jeffreys, F.R.S.

The work on southern earthquakes and the core waves, which was in progress at the time of the last *Report*, has been completed. For PKP only readings at the most reliable stations with vertical component instruments were used and the result was a symmetrical distribution of residuals with a standard error of about 2 sec., nearly the same as for P. Accordingly there is a high probability that the dangers of systematic error in PKP have been removed. The summaries have a standard error of about 0·4 sec., about the same as for P at most distances. The times of SKS have also been rendered somewhat more accurate. Some of the earthquakes used were found to show signs of multiplicity. There appeared to have been two or three shocks at the same place, separated by intervals up to 10 sec., and P had been read for the first, and S and SKS for a mixture, usually with a preference for the later ones. This explains most of the 'Z' phenomenon, leaving no more than can be reasonably attributed to variations of focal depth within the upper layers. Cases where the separation is larger have already been considered by Stoneley and Tillotson and appear to provide an explanation of most of the recorded cases of apparent 'high focus.'

A study has been made of the frequency of after-shocks of the Tango (Japan) earthquake of March 7, 1927. They were found to agree with a law, such that the chance of an earthquake in an interval dt is proportional to $dt/(t - a)$, where a is near the time of the main shock. Apart from this the after-shocks appeared to be independent. Search was made for periodicities of the solar and lunar days and half-days, a fortnight and a month, and for any evidence that returning waves tend to stimulate a new shock, but no such evidence was found in any case. It appears that, except within an interval very close to the main shock, after-shocks may be considered as related to the main shock and nothing else.

It appears, however, that if data used in testing suggested periodicities include after-shocks, the random amplitudes found would be greatly increased by the dependence of the after-shocks on the main shocks of their series. This makes the events occur in batches, and the usual tests for the significance of an amplitude found by Fourier analysis fail. No alleged periodicity can be trusted if it is based on data that include different series of after-shocks.

An analysis of deep focus earthquakes is in progress, in the hope of obtaining a test of the 20° discontinuity and improvements in the estimated thicknesses of the upper layers and in the times of S at short distances. It has been found that the times of P, adapted to a discontinuity and to a continuous time curve that would be consistent with the data of normal earthquakes, would differ by a maximum of about 1·6 sec. in deep ones. This is perhaps just within the range of observability if relevant data can be found.

Reappointment of the Committee.

The Committee asks for reappointment and for the renewal of the grant of £100 from the Caird Fund.

MATHEMATICAL TABLES.

Report of Committee on Calculation of Mathematical Tables (Prof. E. H. Neville, *Chairman*; Dr. A. J. Thompson, *Vice-Chairman*; Dr. J. Wishart, *Secretary*; Dr. W. G. Bickley, Prof. R. A. Fisher, F.R.S., Dr. J. Henderson, Dr. E. L. Ince, Dr. J. O. Irwin, Dr. J. C. P. Miller, Mr. Frank Robbins, Mr. D. H. Sadler, Mr. W. L. Stevens and Dr. J. F. Tocher).

General activity.—Eight meetings of the Committee have been held, in London.

The grant of £200 has been expended as follows:

	£	s.	d.
Wages and insurance for computer for forty-seven weeks .	142	9	5
Calculations for Bessel functions of order greater than one .	45	6	9
Calculations for Airy Integral, etc.	5	0	0
Secretarial and miscellaneous expenses	7	3	10

Personnel.—The Committee has been particularly unfortunate this year in losing by death two of its oldest members. Dr. J. R. Airey, who died on 16 September, 1937, joined the Committee in 1907, and remained associated with it until his death. During this period he was indefatigable as a computer, and was responsible for the production, single-handed, of a vast amount of tabulating work. He was Secretary from 1920 to 1929, and served as the clearing-house for tabulation work for the British Association until the time when regular meetings in London became the recognised procedure. The Committee desires to record its appreciation of the valuable work which Dr. Airey performed as a computer, of the generosity which placed his skill and experience at the service of his friends, and of the patience with which as Secretary he conducted the affairs of the Committee during the difficult period of reconstruction after the war.

Prof. Alfred Lodge, who died on 1 December, 1937, attended his first meeting of the British Association in 1883. He became a Life Member in 1886 and was a member of Council from 1913 to 1915. In 1888 he joined the first Committee set up by Section A ' for the purpose of considering the possibility of calculating certain mathematical functions and, if necessary, of taking steps to carry out the calculations, and to publish the results in an accessible form.' From that year Prof. Lodge was actively concerned with the tabulation work of the Association until the day of his death, and a very great deal of computation work lies to his credit, particularly in connection with Bessel functions. The Committee records with gratitude its appreciation of the patient and valuable work Prof. Lodge did as a computer, of the services which he rendered to the Committee in many capacities, and of the charm of character which made him the personal friend of every member.

Dr. Thompson has succeeded Prof. Lodge as Vice-Chairman.

Employment of Computers.—In the last Report mention was made of the employment of a full-time computer, to work mainly on the Committee's National Accounting Machine at the Galton Laboratory, by kind permission of Prof. Fisher. Mr. F. H. Cleaver, who was appointed to the post in January, 1937, remained fully employed under the personal direction of several members of the Committee, and under the immediate supervision of Mr. Stevens, until he resigned the post on 9 May, 1938. He has been succeeded by Mr. H. O. Hartley, who took up his duties on 13 June. The Committee has arranged for the demonstration of its machine at the Cambridge meeting, as part of a general demonstration of the possibilities of a number of modern calculating machines in scientific computing work.

The other machines belonging to the Committee have been in continuous use, and the Committee records with gratitude the voluntary services rendered in this connection by Mr. C. E. Gwyther. A number of part-time computers have been engaged from time to time under the direction of members of the Committee, and the Committee once more gratefully acknowledges the facilities offered by the Mathematical Laboratory of the University of Liverpool for the carrying out of computation under the supervision of Dr. Miller.

Bessel Functions.—The Committee's sixth volume, being the first volume devoted to Bessel functions and containing the four principal functions of orders 0 to 1, was published at the end of 1937. The volume was dedicated to Prof. Lodge, who, however, did not live to see the tables published.

The work of the Bessel Function Sub-committee on the preparation of a second volume has been to some extent exploratory, and good progress has been made in the calculations. During the year the following fundamental tables have been completed in readiness for sub-tabulation where necessary:

$y_n(x)$	$= x^n Y_n(x)$	$n = 0(1)20$	$x = 0\cdot0(0\cdot1)6\cdot0$	14 figures.
$Y_n(x)$		$n = 0, 1, 2,$	$x = 6\cdot0(0\cdot1)21\cdot0$	15 figures.
$i_n(x)$	$= x^{-n} I_n(x)$	$n = 0(1)22$	$x = 0\cdot0(0\cdot1)6\cdot0$	15 figures.
$\log i_n(x)$		$n = 20, 21$	$x = 6\cdot0(0\cdot1)20\cdot0$	15 figures.
$I_n(x)$		$n = 0(1)21$	$x = 0\cdot0(0\cdot1)6\cdot0$	18 figures.
$\log I_n(x)$		$n = 20, 21$	$x = 6\cdot0(0\cdot1)20\cdot0$	15 figures.
$I_n(x)$		$n = 0(1)21$	$x = 6\cdot0(0\cdot1)10\cdot0$	15 figures.

The calculation of $I_n(x)$ is being continued to $x = 20$. The computation of $K_n(x)$ has been performed for $x = 6\cdot0(0\cdot1)11\cdot5$ with 10 figure accuracy. The determination of the early zeros of $J_n(x)$ by inverse interpolation from the 12 decimal values of $J_n(x)$ already computed for $x = 0\cdot0(0\cdot1)25\cdot5$, $n = 2(1)20$ is in progress. An additional term in the asymptotic series for the zeros of $J_n(x)$ and $Y_n(x)$ has been determined and the coefficients computed.

Table of Powers.—The computation for this volume, and the preparation of copy are almost complete, but some checks have still to be applied.

Airy Integral.—This work, when complete, will form a part-volume of some 46 pages, including introduction. The greater part of the copy has already been prepared, and the remainder will be ready shortly. The Committee proposes to proceed as soon as possible with the separate publication of this table.

Legendre Functions.—Some delay has occurred in the production of these tables as a part-volume, for which authority for publication was obtained last year (see 1937 Report). The entire material has, however, been set up.

Sheppard Tables.—Authority has been obtained from Council for the separate publication of the tables handed to the Committee by the family of the late Dr. W. F. Sheppard, and referred to in last year's Report. The unfinished table, alluded to last year, has been completed. All the tables have now been checked and every entry verified. Sheppard's table of the common logarithm of the tail area of the normal curve, to 12 places of decimals at interval 0·1, has been sub-tabulated to form an 8 decimal table at interval 0·01. This small extension of the original scope of the volume is in response to a demand for a detailed table of this important function, which is much needed in statistical work. An introduction is being prepared, and the whole should be ready for press shortly.

Miscellaneous.—In response to an informal suggestion, the Committee is preparing a card-index of mathematical tables to supplement existing bibliographies. This should form a very valuable source of reference for members of the Committee, and be a means of enabling them to answer outside enquiries. It should be mentioned also that the Committee has been in touch with the Tables Committee of the National Research Council of America, which is engaged under the Works Progress Administration in the calculation of certain tables of mathematical functions.

Reappointment.—The Committee desires reappointment, with a grant of £200, which would be expended mainly on calculations for further volumes of Bessel functions.

THERMAL CONDUCTIVITIES OF ROCKS.

Report of Committee appointed to consider the direct determination of the Thermal Conductivities of Rocks in mines or borings where the temperature gradient has been, or is likely to be, measured (Dr. EZER GRIFFITHS, F.R.S., *Chairman*; Dr. D. W. PHILLIPS, *Secretary*; Dr. E. C. BULLARD, Dr. H. JEFFREYS, F.R.S., Dr. E. M. ANDERSON, Prof. W. G. FEARNSIDES, F.R.S., Prof. G. HICKLING, F.R.S., Prof. A. HOLMES, Dr. H. J. H. POOLE).

1. *Introduction.*—The heat flow at the surface of the earth is a measure of the heat being generated below; a knowledge of its variations from

place to place is therefore of fundamental geophysical interest. To estimate this heat flow it is necessary to know the vertical temperature gradient and the thermal conductivity of the rocks in which the gradient is measured. There exist numerous measurements of temperature in deep bores in various parts of the world, but almost no conductivity data except that collected by the former British Association Committee about fifty years ago. Thus there is no trustworthy data on the variation of heat flow from place to place, though it is believed by many that considerable variations occur.

In an attempt to remedy this state of affairs the Committee has pursued investigations along the following lines :

(1) An attempt has been made to get the necessary data from shallow holes. This investigation has met with difficulties through disturbances of the temperature by percolating water.
(2) Temperature measurements have been made in bores whenever they became available.
(3) An apparatus has been constructed for the measurement of thermal conductivities of rock specimens.

In the past it has been somewhat optimistically assumed that the conductivity measured in the laboratory was the proper quantity to use in the heat flow calculations. As the temperature gradient in the laboratory is of the order of 10° C./cm. and that in nature 0·0003° C./km., this seems a somewhat unsafe assumption. The investigations on heaters in shallow holes and on the annual temperature wave employ gradients of the order of 0·03° C./cm. Comparisons of these with the laboratory determinations therefore provide a most valuable check.

2. *Measurements in a Shallow Hole.*—If the temperature distribution is steady the flow of heat per cm² of the earth's surface should be independent of depth. It would therefore be supposed that the heat flow could be measured as well in a shallow hole as in a deep one, so long as the hole was deep enough to get below the effect of the annual temperature wave. This would avoid the troubles associated with the use of deep bores (see § 3) and would have the added advantage that the conductivity could be measured *in situ* by the temperature distribution round buried heaters.

Experiments briefly described in last year's report showed that temperatures could be measured with thermocouples in a shallow hole with an adequate accuracy. A heater was installed in a 15-ft. hole in gault. When it was turned on the temperature of the thermo-junctions changed in the expected way. Examples are shown in Fig. 1. The change due to the annual temperature wave was subtracted, and expressions of the form $A(1 - Erf\, B/t)$ fitted to the results. The constants A and B are functions of the conductivity, the specific heat per unit volume and of the positions of the thermo-junctions. The conductivity deduced from them and from laboratory measurements of the specific heat was 0·0027. The heat flow could not be deduced from these measurements since there was a large annual temperature change even at the bottom of the hole. The results, however, were taken as indicating that the method was sufficiently promising to try in a deeper hole. A 100-ft. hole was therefore drilled at a cost of £19 in a field near the Observatory at Cambridge. Three feet of water-bearing gravel were encountered on top of the gault and the top 20 ft. of the bore was cased to exclude this water. In spite of this water continued to enter the bore from lower levels. The casing was therefore continued to 60 ft., still without stopping the water; the water level was different inside and outside the casing, showing that the water was really derived from the gault and not from the surface gravel. As the hole showed signs of caving in, three heaters and fifteen thermo-junctions were installed as soon as the drilling was finished. As the casing was not excluding the water it was withdrawn.

The presence of water in the hole makes it impossible to make satisfactory thermal conductivity measurements with buried heaters as the water content of the clay around the hole has been completely altered. As specimens of the clay had been taken every 10 ft. with precautions to prevent them being affected by the water, laboratory measurements of conductivity can be made.

When attempts were made to measure temperatures with the thermo-junctions completely inconsistent results were obtained. This was traced to the leads having become damp from water condensed in the tube. This dampness caused the copper and constantan wires to act as a small battery.

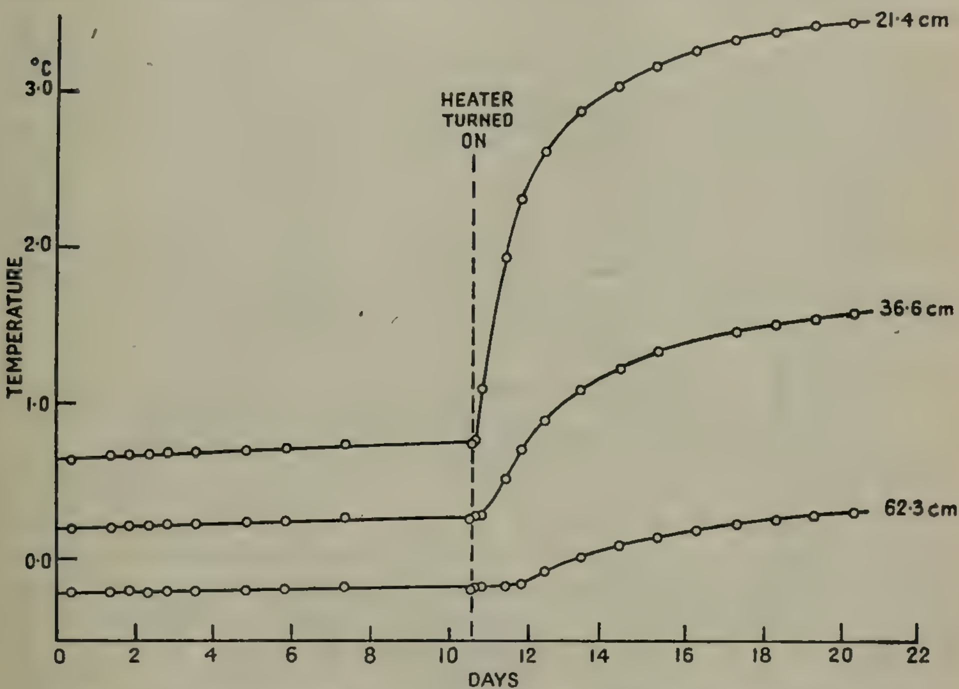

FIG. 1.—Temperature of junctions above that of the bottom of hole.

A 100-ohm copper resistance thermometer was therefore constructed in a water-tight steel case 8 mm. in diameter. Its behaviour was entirely satisfactory.

Temperature measurements were made in a $\frac{3}{4}$-in. steel pipe at every 10 ft. between the surface and the bottom of the hole (with the hole filled with water) and after it had been filled in with clay. For the lower 30 ft. of the hole the gradient was constant and equal to $33 \cdot 8 \pm 1 \cdot 5°$ C./km. for the filled hole and $33 \cdot 2°$ C./km. for the unfilled hole. Between 30 and 70 ft. from the surface the mean gradient was about the same as in the lower part of the hole but the individual points deviated by up to $0 \cdot 05°$ C. from a straight line (see Fig. 2). Above 30 ft. the annual temperature wave obscures the normal gradient. The measurements have been repeated several times and the departures from a straight line are reproducible and are certainly real. They are presumably due to the circulation of water in one or more of the porous bands that are shown to exist by the entry of water into the hole during the drilling. The presence of these irregularities throws considerable doubt on the value of the gradient derived from the observations. The site for this bore hole was chosen so that the bore would be entirely in gault as this is one of the most homogeneous and least porous formations. Thus if irregularities are found in the temperature curve even in this specially favourable case it is unlikely that useful measurements will often be possible in such shallow holes. The consistency of the measurements shows however that reliable measurements would be possible in dry holes if any can be found.

Observations of temperature have been made on 10 thermo-junctions in a 15-ft. hole at intervals over a period of a year. These observations should yield an excellent value for the diffusivity of gault. Since they refer to a large mass of undisturbed clay they provide a standard for checking

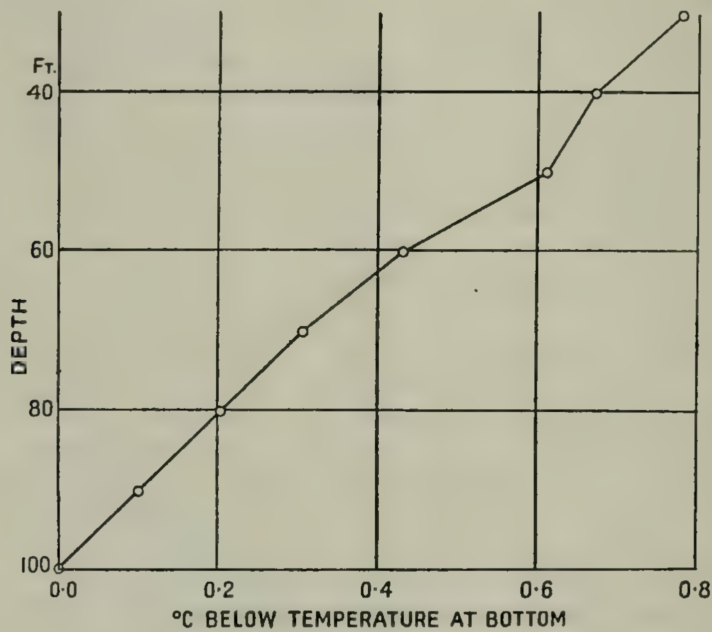

FIG. 2.

laboratory measurements, for if an apparatus will measure the conductivity of gault there is little doubt that it would deal satisfactorily with more consolidated rocks. A rough reduction of the observations suggests a value of 0·0037 $cm^2 sec^{-1}$ for the diffusivity, which combined with a specific heat of 0·39 cal/°C. grm. and a density of 2·00 gives a conductivity of 0·0029 cal/cm.°C. sec. To make a rigorous reduction involves a great deal of arithmetical labour, but it is hoped eventually to carry out the work.

3. *Temperature Measurements in Bore Holes.*—The Anglo-Iranian Oil Co. very kindly allowed specimens to be collected and temperatures to be measured in their bore at Kingsclere. As this bore had been made by rotary drilling with a continuous circulation of mud it was necessary to leave it for some days for the lower part to get into temperature equilibrium (see § 4). After the bore had been standing for three days an attempt was made to lower two maximum thermometers in a sealed case. It was found to be impossible to get the thermometers more than halfway down the hole owing to an obstruction. Attempts were continued for three days without success. As this work caused great delay and inconvenience to the Anglo-

Iranian Co. the work was then abandoned. Measurements in the higher parts of the hole would have been valueless as they had been cooled for months by the circulation of drilling mud. Similar difficulties were met with in some measurements made by the Anglo-Iranian at Portsdown.

This experience shows how difficult it is to obtain satisfactory measurements in rotary drilled holes. Even at the bottom of the hole measurements cannot be taken till some days after drilling has stopped ; and, since the hole is not lined, it is very likely to become blocked during this time and can only be cleared by lowering a drilling tool for which it is necessary to circulate mud which completely upsets the temperature equilibrium again. Even if the hole should remain clear the measurements cause much expense to the company drilling the hole, since the Mines Dept. insisted that such holes should be plugged with cement at various depths and as the drilling rig cannot be removed till this has been done, it must stand idle during the temperature measurements.

Measurements are therefore only possible if the hole is lined and the lining left in for some time after drilling. One such case has occurred. The Anglo-Iranian drilled a hole at Pevensey 842 ft. deep, cased it to 500 ft. and left it for some weeks. Temperature measurements were made at 765 ft. and 772 ft. (the hole was blocked below this). The temperatures found at these depths were 16·2° C. and 16·3° C. There was fluid in the hole only below about 700 ft. and in any case it had not been left long enough for satisfactory measurements to be made in the upper part. The gradient has therefore to be deduced by a comparison of the temperatures at the bottom with the surface temperature. The mean air temperature at Pevensey is stated by the Meteorological Office to be 10·3° C.[1] The mean temperature of the ground a few feet down is normally 0·8° C. above that of the air.[2] We therefore deduce a gradient of 5·1° C. in 668 ft. or 25° C./km.

In order that it should be possible to make temperature measurements in any bores that became available two inverted maximum thermometers and a winch for lowering them on piano wire have been purchased. As the thermometers used at Kingsclere showed a great tendency to shake down on raising them from the bottom of the hole, an investigation was made of this source of error with the new thermometers. At Pevensey the two thermometers were lowered opposite ways up so that shaking would affect them in opposite directions and their positions in the container were reversed for the second run. The results agreed to less than 0·1° C. As a further check a weight thermometer was constructed, but there has so far been no opportunity to use it in a bore hole. Messrs. Negretti and Zambra state that when maximum thermometers have been much used the constriction becomes worn and they tend to shake down more easily. The new thermometers are so difficult to shake down that it was necessary to construct a whirling case to do so.

4. *Laboratory Measurements of Conductivity.*—In order to measure the conductivity of specimens of rock from bores an apparatus has been constructed by Mr. Benfield of the Cambridge Department of Geodesy and Geophysics[3] with the advice of Dr. Ezer Griffiths. This apparatus consists of two brass bars one inch in diameter between the ends of which a disc-like specimen is placed. The top end of the upper brass bar is heated electrically and the bottom end of the lower is cooled by a stream of water.

[1] This is deduced from the mean air temperature at Eastbourne from 1888 to 1935.

[2] J. Koenigsberger and M. Mühlberg, *Neues. Jahrb. f. Min.*, beilage bd. 31, 115, 1911. Everett, *2nd Rep. Roy. Comm. on Coal Supplies*, 2, 292, 1904.

[3] Mr. Benfield also made the temperature measurements in the Pevensey bore.

The temperature at seven points along these bars is measured by thermo-junctions. From these the temperature drop across the specimen T and the temperature gradient p in the bars are determined. If there were no loss of heat by convection and the specimens were in perfect thermal contact with the bars, the conductivity of the specimen, k would be given by

$$k = CpS/T$$

where S is the thickness of the specimen and C is a constant that may be determined by measurements on a substance, such as fused silica, whose conductivity is known.

The loss of heat by convection may be reduced by enclosing the apparatus in a bell-jar across which run paper discs. The remaining loss may be estimated from the departure of the temperature distribution in the bars from linearity and allowed for.

The error from the lack of perfect thermal contact between the bars and the specimen may be eliminated by making measurements on specimens of two different thicknesses. For this elimination to be satisfactory it is desirable that the contact should be as good as possible. Experiments on the best method of joining the specimens to the bars are in progress. Painting with a thin layer of very thick cellulose varnish seems the most promising method. If the specimen is pressed on the bars while the varnish is still tacky the minute irregularities in surfaces of the specimen and the bars are filled with celluloid.

The preliminary tests of the apparatus are being made with specimens from Kingsclere, when it is working satisfactorily measurements will be made on the Pevensey specimens.

A specially careful investigation will be made on the Cambridge Gault for comparison with the values obtained by the methods described in § 1.

(5) *Theoretical Investigations.*—If the surface of the earth is not a plane of constant temperature there will be irregularities in heat flow that may mask those due to variations in conductivity and in the generation of heat. Dr. Jeffreys has devised a method of allowing for these ; his investigation, which has been published in Vol. 4 of the *Geophysical Supplement* to the *Monthly Notices of the Royal Astronomical Society*, shows that the disturbance δv of the temperature gradient at a depth Z below the surface is given by

$$\delta v = \int_0^\infty v \, \frac{r^2 - 2z^2}{(r^2 + z^2)^{\frac{5}{2}}} \, r \, dr \qquad . \quad . \quad . \qquad (1)$$

where v is difference between the mean temperature at sea level round a horizontal circle of radius r and that at sea level under the station ; that is v is the mean value of

$$(p - p')h$$

where p and p' are the temperature gradients in the earth and in the air and h is the difference between the height of the station and the mean height of the circle. From (1) expressions may be derived for the disturbance at the surface and for the mean disturbance down to any depth. Certain special cases may be evaluated analytically, for example the gradient at the bottom of a hemispherical cavity is three times that at a distance from the cavity, and the gradients at the crests and troughs of a series of parallel simple harmonic ridges and valleys of height $2A$ and wavelength λ differ by $4\pi A/\lambda$ times the normal gradient. Numerical solutions for a number of actual bores have been made by Dr. Bullard and published in the *Geophysical Supplement*; the biggest disturbance found is for the Simplon Tunnel, where the observed gradient requires to be increased by 14 %.

Dr. Jeffreys has investigated the disturbance of the temperature gradient produced by the casing in a bore. If a long rod of radius a and conductivity K is introduced into a solid of conductivity k with a temperature gradient p in a direction parallel to the rod, the temperature gradient within the rod at a distance s from the end differs from p by an amount δp given by

$$\delta p/p = \tfrac{1}{4}(K - k)\, a^2/k\, s^2$$

If $K = 100k$ the error is less than 1 % if $a/s = 50$, the effect of the casing is therefore always negligible except within a few feet of the end.

Future Programme.—Owing to the difficulty of obtaining satisfactory temperature measurements in rotary drilled holes it is desirable to make the best possible use of data obtained when other systems of drilling were in vogue. A great mass of temperature data exists from bores in the U.S.A., in Persia and elsewhere, much of which has been taken with every precaution; efforts should therefore be made to obtain specimens of the strata passed through by these bores in order that conductivity determinations may be made. Requests for specimens have been sent to various organisations who might be able to assist. It is easy to obtain odd specimens from bores but difficult to get a representative selection.

Temperature measurements should be made whenever bores are available. Any bore over 500 ft. deep is suitable, and any dry bore over 100 ft. is worth testing.

No grant will be required for this work.

QUANTITATIVE ESTIMATES OF SENSORY EVENTS.

Interim Report of the Committee appointed to consider and report upon the possibility of Quantitative Estimates of Sensory Events (Prof. A. Ferguson, *Chairman*; Dr. C. S. Myers, F.R.S., *Vice-Chairman*; Mr. R. J. Bartlett, *Secretary*; Dr. H. Banister, Prof. F. C. Bartlett, F.R.S., Dr. Wm. Brown, Dr. N. R. Campbell, Prof. J. Drever, Mr. J. Guild, Dr. R. A. Houstoun, Dr. J. C. Irwin, Dr. G. W. C. Kaye, O.B.E., Dr. S. J. F. Philpott, Dr. L. F. Richardson, F.R.S., Dr. J. H. Shaxby, Mr. T. Smith, F.R.S., Dr. R. H. Thouless, Dr. W. S. Tucker, O.B.E.).

Introduction.

This Committee, whose title indicates its terms of reference fairly accurately, was appointed at the York meeting in 1932, and has met since at irregular intervals, much of its work having been carried out by correspondence between members of different views and the circulation of statements made by members.

In the early stages of the Committee's existence, it seemed impossible to reach an agreement, and it soon became obvious that it was necessary to investigate the general implications of the term measurement and of the processes involved in the making of measurements, and that it would be profitable to study what was actually 'measured' in a number of experiments carried out in psycho-physical research. Further, it was evident that some of the experimental conditions under which these researches were carried out must come under review, and that considerable light would be thrown on the problem by a study of the historical order of development of the subject of mental measurement.

The Committee has been fortunate in securing the services of specialists who have collected and discussed, in detail, the evidence for the different views, and feels that it can best serve the advancement of this particular branch of knowledge by putting forward the evidence for these views without at present making any attempt to reconcile them.

To this end Mr. Guild has prepared a statement of the point of view of those who deny the possibility of quantitative estimates of sensory events, and Prof. Drever has dealt more briefly with the question from the opposite angle. Mr. Guild's statement has been circulated to the Committee, and notes and criticisms received from members are included in the Report. Prof. Drever, who had waited for Mr. Guild's paper before completing his own work, feels that a longer time is needed for a full presentation of a reply to Mr. Guild's position, but has sent in a short statement presenting the case for those who give the affirmative answer to the question whether sensation intensity is measurable.

Extensive experimental work has been carried out in the Cambridge Psychological Laboratory, and Mr. Craik has prepared a short summary of this work, to which Prof. Bartlett has added an introductory note.

Dr. Semeonoff, of the University of Edinburgh, has collated and studied critically the immense literature connected with the subject of the measurement of sensory magnitudes, and he has been kind enough to permit the Committee to include in its Report that portion of his work which refers to the measurement of sound sensation.

For consideration of the Sections the Committee therefore present the following :

I. An historical statement by Dr. B. Semeonoff.
II. A short summary of recent Cambridge experimental work by Prof. F. C. Bartlett, F.R.S., and Mr. K. J. W. Craik, M.A.
III. A statement by Mr. J. Guild.
IV. Notes thereon by :
  A. Dr. R. H. Thouless.
  B. Dr. L. F. Richardson, F.R.S.
  C. Mr. T. Smith, F.R.S.
  D. Dr. Wm. Brown.
  E. Dr. J. H. Shaxby.
V. A statement by Professor J. Drever.

I. *An historical statement by Dr. Semeonoff.*

The Measurement of Sound Sensation.

Early work on the measurement of sound sensation was closely bound up with two other studies : (i) the search for a simple method of measuring sound intensity, and (ii) the experimental verification of the validity of Weber's law and its derivatives.

Reviews of the early work on sound measurement were made in 1905 by Titchener (57), and in 1910 by Pillsbury (43). These naturally exclude the recent work using electrical methods, the development of which has revolutionised the whole field. A brief survey of these later methods was made by the present writer (49), who quotes references to fuller and more technical accounts.

The pioneer researches were marred both by imperfections and inadequacies of technique, and by theoretical misapprehensions. Of these drawbacks, those of the second type apply more particularly to methods based on the principle of falling bodies, those of the first both to these methods and to those based on the validity of the inverse square law.

The principle that the intensity of a stimulus which reaches the receptor from a distant source varies inversely as the square of the distance of the source from the receptor is still sometimes used, in the form of the 'watch test,' by practising aurists in the determination of hearing loss. This method, while suitable for rough estimates, is not to be recommended for accurate measurement, since reflection of sound waves is inevitable, and practically uncontrollable, even in the open air.

The method of falling bodies rests on the principle that the energy of a falling body is proportional to the weight of the body, and to the height of fall. Gravity being constant, it may be said that the product of height and weight gives a measure of the energy. Since, however, not all the energy is effectively transformed into sound, the simple ht. × wt. formula does not hold, and it was found by the early experimenters that in general a fractional power of the height had to be taken in calculating sound intensity. These conclusions were often reached as a result of equating for loudness the sounds produced by balls of varying size and weight, and it was usually noticed that differences of quality made such observations extremely difficult. Actually apparent equality of loudness under these conditions is no criterion of equality of intensity, and it is surprising how completely this rather obvious point was overlooked. Studies of the inter-relation of sensory attributes, which for sound may be said to date from the discovery in 1897 of the Broca phenomenon (6), have now established beyond dispute the fact that no point-to-point correspondence can be claimed between the psychological qualities of sensation and their physical correlates. Following Fletcher (13), we may say that while loudness is the subjective characteristic that is recognised as the magnitude of the sensation, and which changes most rapidly with changes in intensity, each of the three main subjective characteristics of sound—loudness, pitch and timbre—depend on all three of the physical characteristics—intensity, frequency and overtone structure.

Attempts to measure sound intensity were also made using such devices as singing flames, percussion systems with or without tuning-forks, phonograph records, blowing pressure, and direct microscopic examination of the amplitude of vibrations.

Some of these instruments were used in investigations of Weber's law, and apart from this approach little attempt was made to measure sensation as such, i.e. to express sensation magnitudes in relative or absolute units. At the time of the 1913 symposium in the *British Journal of Psychology* (40), the general consensus of opinion was that theoretically sensation was not measurable, but that Weber's law might hold, at least over a limited range, in most sense-departments. Fechner's 'fundamental assumptions,' first stated in 1860 in the *Elemente* (12), on which he based his mathematical development of the S (sensation) $= k \log R$ (stimulus) relation,[1] were taken

[1] Sometimes stated $S = k \log R/R_0$, to indicate that the stimulus is measured in terms of its absolute threshold (R_0) as unit. This is the law quoted in Warren's *Dictionary of Psychology* under the heading *Fechner's Law*. The definition adds : 'Fechner's law is frequently incorrectly called *Weber's Law*, and is now often referred to as the *Weber-Fechner law*.'

as the necessary conditions on which depended the measurability of sensation, so that this measurability stood or fell with the constancy of the 'Weber-Fechner fraction.'

This approach to the relation between stimulus and sensation has now been almost wholly superseded by other methods, though interest in the constancy of the difference threshold has not entirely died down. It is, of course, difficult to date this change of attitude with any degree of accuracy, but a useful turning-point may be arbitrarily fixed by the publication in 1920 of a review by Marx (35) of work on *die Unterschiedsschwelle bei Schallempfindungen.* Before this date work on the stimulus-sensation relation for sound was done exclusively by means of what we may term the 'Weber-Fechner approach,' the value of the just noticeable difference was usually found to be fairly constant, and the sounds studied were for the most part unpitched sounds (usually described as 'noises'). After 1920 developments in electrical apparatus made work with tones progressively easier and more accurate, the difference threshold was found to be much less constant than had previously been supposed, and new methods of investigation were evolved.

These new methods may be classified under two main heads : I. Attempts to assign numerical values to actual sensations, or to determine values of a stimulus whose subjective effect should bear a given numerical relation to the subjective effect of another value of the same stimulus ; II. Attempts to discover the relationship between measurable physiological events and the physical values of the stimulus. When the former can be shown to bear the same or a similar functional relationship to the stimulus as the subjective effects noted above, the hypothesis is commonly advanced that loudness is determined by some such variable as the rate of change of the physiological process or the number of nerve units activated. These are not discussed in the present summary, since their relevance to the measurability of sensation depends entirely on whether one is prepared to accept as valid the hypothesis mentioned.

In addition, the period under discussion has seen the development of noise-analysis methods, by which a complex sound can be reduced to a 'frequency spectrum,' and a better understanding obtained of the apparent anomalies of masking.

Table I contains a detailed summary of the principal work on Weber's law in its application to the intensity of sound ; the following descriptive notes may be of interest as giving a better indication of the actual experimental procedures employed. They also contain mention of a few researches not included in the table.

The material may be divided into five groups as follows : (i) the earliest work, 1856–79 ; (ii) work reported in a series of articles in Wundt's *Philosophische Studien*, 1883–1900, and performed for the most part in Wundt's Leipzig laboratory ; (iii) early work on tones, 1888–1905 ; (iv) later work on tones, using electrical apparatus, 1922–35 ; (v) miscellaneous work with a variety of instruments, and usually with some other end in view than a simple examination of the truth of Weber's law, 1930–37. The succeeding sections are numbered in accordance with this classification.

(i) The experiments of the first group gave inconclusive results, and are of interest chiefly for historical reasons. In particular, the work of Renz and Wolf (44) is significant in that it is 'pre-Fechner,' and seems to have anticipated the standardisation of the psychological methods. Renz and Wolf, medical undergraduates at Tübingen, experimented with a watch, intensity being measured in terms of distance. Various devices used to eliminate accidental errors showed a nice balance between the requirements of experimental accuracy and those of the comfort of the subject. Only one standard intensity was investigated, but both time-orders were used. The rather naïve conclusion was drawn that certainty of judgment grew with increasing difference of intensities. Individual differences between the results of the two experimenters, who acted as their own subjects, also received comment.

Fechner in the *Elemente* (12) derived his data on sound intensity from results obtained by Volkmann, who carried out two series of experiments. In the first, he used a simple improvised sound-pendulum, consisting of a strong knitting-needle as axis, and a wooden hammer which struck against a four-sided glass flask. Two heights were found such that in the majority of cases the observer could tell which gave the stronger sound, and observations were made at four distances, varying from $1\frac{1}{2}$ to 18 paces. It was found that judgment remained as sure and correct at all distances, and from this it was concluded that the difference threshold was independent of the absolute value of the stimulus. Experiments with freely-falling bodies gave for two subjects out of three a ratio of intensities 3 : 4 for which a difference could be accurately judged, while with a ratio 6 : 7 considerable uncertainty occurred.

A much fuller investigation was reported by Nörr (42), who introduced a number of refinements into his falling-bodies technique. A much wider range of intensities was used, and catch-experiments were introduced, in which the standard was presented with itself. Unfortunately, the numerical results are such as to make the calculation of difference thresholds by any of the ordinary procedures practically impossible. Nörr, however, concluded that differential sensitivity remained constant from the weakest to the strongest sounds.

(ii) Dissatisfaction with Nörr's results seems to have been one of the contributing causes of the work of the Leipzig group. This series of researches is of particular interest in that the writers had first-hand contacts with one another, even to the extent of opportunities of working with the same subjects and the same instruments. One of their main interests was the exact measurement of the sounds produced by the falling bodies, discussed above.

Tischer (56) and Lorenz (31) used Hipp's fall-apparatus. Tischer's results show wide individual differences among his five subjects, and considerable variation over a fairly small range, together with a progressive improvement of discrimination with practice. Nevertheless, the results are described as 'so gut wie constant.' Lorenz, on the other hand, was aware that his results were insufficient for generalisation. He characterises the constancy obtained as fairly satisfactory, and states that it might have been better with greater care.

Starke (50, 51), Merkel (36), and Mosch (39) used Wundt's improved fall-apparatus, and introduced further experimental refinements. Mosch laid particular stress on the 'error' aspect of variations in the difference threshold, and introduced further categories of judgment ('*much greater*,' '*much less*'). Kämpfe (23) and Ament (3) reverted to the use of the sound-pendulum, making considerable improvements on the model used by Volkmann. Ament's work shows increased recognition of individual differences, and also a decided drop in the value of the difference threshold after the weaker intensities had been passed. In both these respects Ament anticipates the results of later experiments.

A slightly different approach to the stimulus-sensation problem is seen in the work of Merkel (37) and Angell (5). Merkel and Angell used the method of 'Mean Gradation' to find an estimated mid-point between two

TABLE I

Date.	Authors.	1	2	3	4	5	6
1856	Renz and Wolf (44)	W	0	2	C	·38	
1860	Volkmann (12)	P	22		C?	*k*	
1860	,,	FA	wide		?	·33	
1879	Nörr (42)	FA	55	1	C	*k*?	
1883	Tischer (56)	FA	30	5	L	·4	indiv. diffs.
1885	Lorenz (31)	FA	20	1	L, C	·3	upper dev.
1886	Starke (50, 51)	FA	10	2	L	·3	
1888	Wien (59)	TF	110		L, C	·12, ·2	dev. at both ends.
1888	Merkel (36)	FA	40		L	·3	
1893	Kämpfe (23)	P	wide	3	C	*k*	
1900	Ament (3)	P	5	2	L	·3	lower dev.; indiv. diffs.
1904	Hoefer (22)	FA	small		C	not *k*	irreg. var.
1905	Deenik (11)	TF	23		L	·2, ·3	
1905	,,	OP			L	·1–·2	irreg. var.
1907	Keller (24)	FA	small	9	*, L	·1	
1922	Guernsey (19)	VO		6	L	·3	lower dev.
1922	Knudsen (29)	VO	wide	4	L	·1	lower dev.
1924	Halverson (20)	VO	23	3	C	·2	lower dev
1928	Riesz (46)	VO	100	12	L	·1–·3	lower dev.; reg. var.
1929	Kellogg (25)	AO	5		?	·1?	
1929	,, (26)	AO	wide?		C	·12	irreg. var.?
1930	Macdonald and Allen (32, 2)	V	10		L	not *k*	reg. var.
1930	Kenneth and Thouless (27)	VO	wide	2	L	·1	lower dev.; reg. var.
1934	Churcher, King and Davies (10)	VO	80	4	C	not *k*	reg. var.
1935	Gage (16)	VO	24		L	not *k*	lower dev.; reg. var.
1935	Telford and Denk (55)	VO	20	12	L	not *k*	lower dev.; reg. var.
1935	Montgomery (38)	VO	70	4	C	not *k*	reg. var.
1936	Semeonoff (49)	W	12	4	L	not *k*	irreg. var.
1936	,,	FA	1	3	C	·2?	
1936	,,	TF	80	4	L	not *k*	reg. var.
1937	Upton and Holway (58)	VO	28	1		not *k*	function of time.

Notes to columns :

1. Sources : W, watch ; P, sound-pendulum ; FA, fall-apparatus ; TF, tuning-fork(s) ; OP, organ-pipes ; VO, vacuum-tube oscillator ; AO, audio-oscillator ; V, variator.
2. Approximate range in db.
3. No. of subjects (when stated).
4. Methods : C, Constant method, or some variety thereof ; L, method of Limits ; *, see text.
5. Modal value of threshold, when constant (*k*).
6. Deviations from a constant value at low or high intensities ; course of the threshold over range studied, etc.

'terminal stimuli.' Merkel's results suggested that this mid-point lay nearer the arithmetic than the geometric mean of the terminal values, while Angell found the opposite to be true. Angell's result is in accordance with the requirements of Weber's law and Fechner's derivatives; Merkel's, on the other hand, would seem to support Plateau's 'quotient-hypothesis' described by Brown and Thomson (7) as the 'one-time chief rival of the Weber-Fechner law.' This theory postulated direct proportionality between the just noticeable difference of sensation and that of stimulus, and at one time had many supporters, though later Plateau himself repudiated it. The Merkel-Angell controversy attracted a good deal of attention, e.g., from Ament (3), who showed that the values of the mid-stimulus were a function of both the ratio between and the absolute values of the terminal stimuli.

The work of the Leipzig group was summed up by Wundt himself (60), who believed that hearing was the sense-department in which Weber's law was of the widest application. At the same time he admitted that the method of mean gradation seemed to yield mean values closer to the arithmetic than to the geometric mean.

Though not strictly belonging to the same group, the work of Hoefer (22) and Keller (24) may conveniently be included in the present section. Hoefer studied auditory differential sensitivity among individuals suffering from psychoses and functional neuroses. Subnormal sensitivity was found only in a few cases, though flagging of attention was often evident. Keller, using a modification of Mosch's method, found that Weber's law held good, with a mean value of about $\frac{1}{10}$, as against the figure of $\frac{1}{3}$ usually found by previous investigators. Keller also believed that the Gaussian law of error did not hold in psycho-physical experiments, so that methods involving its application were to be avoided.

(iii) All the work discussed so far was concerned with unpitched sounds. Wien (59) was the first, by nearly twenty years, to work with tones. These were produced by electrically-driven tuning-forks of three frequencies, and the intensity range was much the greatest of any in the early researches. A resonator, covered with the membrane of an aneroid barometer, served as an artificial ear-drum, by means of which the relative amplitudes of vibrations could be measured with great accuracy. Wien's work is remarkable not only in respect of its pioneer use of tones, but also in that on the basis of his results he drew up an empirical equation for the difference threshold, which, in its integrated form, gave a curve for the relation between stimulus and sensation surprisingly similar to that recently adopted as a standard by the American Standards Association (4).

Deenik (11) extended Wien's frequency range, and experimented with organ-pipes as well as with electrically-driven tuning-forks. In the case of the former the subject himself adjusted the intensity of the variable until a difference was noticeable. Unfortunately Deenik's intensities were not so conveniently graded as Wien's, and in general his interest was concentrated rather on differences of sensitivity as a function of frequency. It is worth mentioning, however, that Deenik found that the finest thresholds were in the region of 2,000 cycles; this corresponds quite well with the point at which recent work has shown that the widest range exists between the upper and lower thresholds of hearing.

(iv) The present section marks the beginning of work with vacuum-tube oscillators. Unless otherwise stated, it is to be assumed in sections (iv) and (v) that some form of oscillator was used in all the studies reported.

Like Deenik, Guernsey (19) was chiefly interested in the variation of the intensity threshold with pitch. Her point of maximum sensitivity, however, was located somewhat higher—between 3,000 and 7,000 cycles, results varying for different observers.

Mackenzie (33) made use of a principle similar to that of optical flicker. Mackenzie found that if two frequencies alternated in the ear at a fairly rapid rate, the interruptions of the louder were more conspicuous than those of the weaker. It was possible to adjust the intensities until the two tones appeared equally interrupted ; at this point the respective loudnesses were taken as equal. A comparison of the physical values of the tones thus balanced showed that except at weak intensities, where room-noise was held to have an interfering effect, the relative sensitivity of the ear was invariable over the whole hearing range.

Knudsen (29) also used an alternation method, whereby two intensities of the same tone were alternated at the rate of about fifty changes per minute, the difference between them being decreased or increased until the ' flutter ' ceased or began to be apparent. The value of the difference threshold was shown to decrease continuously with rise in intensity, until a constant value was reached. The point at which the curve flattened in this way was shown to vary for different frequencies, but on the whole the value of the threshold seemed to be independent of frequency. Knudsen also drew up a generalised equation intended to serve as a truer expression of differential sensitivity, and showed that computed values of the threshold based on this formula agreed fairly well with his experimental results.

Knudsen's results share with those of Riesz (46) the distinction of having been used by subsequent investigators as the basis of theoretical considerations and calculations. Riesz measured differential intensity sensitivity by determining for a tone of given frequency and intensity the minimal intensity to which a second tone, differing from the first by 3 cycles per second, had to be raised to make the beats just perceptible. Twelve observers each worked with seven tones ranging from 35 to 10,000 cycles ; the whole range of intensities, from the absolute threshold to near the threshold of feeling, was covered. The general conclusion as regards Weber's law was that it held at all frequencies for intensities above 10^6 times the absolute threshold value. Curves showing the difference threshold plotted against a logarithmic intensity scale seem to confirm this, unless the threshold axis is itself logarithmic, in which case the curves do not seem to flatten perceptibly at any point of the intensity scale. This observation, however, is based on replotting some of the data of smoothed curves, and as such is no doubt open to question.

Telford and Denk (55) confirmed Riesz's results rather closely for one frequency (800 cycles). The form of the curve obtained was almost identical with those of Riesz, but the apparatus used was such as to make measurement of intensities in terms of db level impossible. Further, for some reason not very apparent, thresholds were calculated from the formula $(I_2^2 - I_1^2)/I_1^2$, where I_1 and I_2 were the lower and higher intensities respectively.

(v) The work of Macdonald and Allen (32, 2) shows a departure from recent practice in that the sounds studied were variator tones, of which the intensities were measured in terms of blowing pressure. The authors follow Merkel and others in their recommendation that the reciprocal of the threshold be used as a measure of sensitivity, so that a higher numerical value would indicate heightened sensitivity. The main finding was that Weber's law did not hold, since a plot of the reciprocal of the threshold against intensity fell into two distinct parts. A new empirical equation was suggested as holding good (with appropriate changes of constants) for hearing not only under normal conditions, but also under conditions of depressed and enhanced sensitivity, obtained by previously stimulating the ear to be tested, or the other, with tones of the same frequency as the test tone and about 15 to 20 times its intensity.

Gage (16) investigated the variation of the uniaural difference threshold with simultaneous stimulation of the other ear by tones of the same frequency. Gage's detailed results are not relevant to the present purpose, but his general results suggest the occurrence of high values of the threshold at low intensities to a very marked degree.

An unusual approach to the continuity of the value of the difference threshold is seen in the work of Kenneth and Thouless (27), who claimed to show that this continuity extended to the absolute threshold. By starting at zero intensity and taking several intermediate values of the stimulus between this and the absolute threshold of hearing, they demonstrated that the just noticeable difference varied continuously until a point was reached at which it so happened that the requirements of Weber's law were fulfilled over a certain range.

The work of Churcher, King and Davies (10) is interesting (*a*) for its stress on the continuity of variation of the difference threshold, and (*b*) for the fact that the just noticeable increments and decrements of intensity found were not such as would occur if Weber's law held good. This suggests the importance of 'set,' a point more fully dealt with by Montgomery (38, see below). Differential sensitivity to cyclical changes of intensity was also studied, though the discrepancy noted under (*b*) above made this method, in the authors' opinion, not strictly valid. If this objection is a real one, it would seem to apply also to all the 'alternation' methods used in other investigations.

In addition to a direct investigation of differential sensitivity at four intensity levels, Montgomery (38) made a systematic survey of the effect of 'set' as determined by variations in experimental conditions. Table II shows thresholds, expressed in both db and fractional form, for the thermal noise from a high-gain amplifier at an intensity level of 40 db. These figures stress the difficulty, noted also in connection with loudness estimation, of comparing results obtained by different experimenters using different methods.

TABLE II. (Montgomery.)

Method.	Threshold db	Threshold $\Delta R/R$
Switch not controlled by subject; 1 comparison; ½-sec. between tones	0·8	0·20
Same, but no interval between tones	0·6	0·15
Repeated comparisons; no interval	0·4	0·096
As above, but switch controlled by subject	0·2	0·047
Sinusoidal (cyclical) variation	0·5	0·12

A further point was made by Upton and Holway (58), whose chief interest was to demonstrate that differential sensitivity to sound intensity was a specific reproducible function of exposure-time. Interpreted from the point of view of Weber's law, the results indicate that while relative constancy of the threshold was obtained with long exposure-times, with times less than 10 seconds considerable fluctuations (of an irregular character) were found within the range represented.

The present writer (49) carried out short series of experiments with various instruments. (*a*) Since watch-tick experiments had apparently not been performed since the time of Renz and Wolf, it was decided to see what improvements could be made on their procedure. Four intensities were studied, upper and lower binaural thresholds being obtained for each of four subjects. A stop-watch which could be operated noiselessly was used. Marked individual differences were found, and only a very crude approximation to constancy over a limited range. (*b*) Three subjects participated in some experiments with Wundt's fall-phonometer; a lower threshold was found for two heights, using both time-orders. Individual differences were again noted, but an overall average of about 0·2 seemed to be indicated, this value falling between those obtained by the Leipzig group on the one hand, and Keller (24) on the other. (*c*) Lower thresholds for four subjects were found by the method of Limits, using a valve-maintained tuning-fork (frequency 512 cycles), devised by K. J. W. Craik. The sound was suitably amplified, and heard in a moving-coil loudspeaker. The following are some of the conclusions reached: (1) Weber's law did not hold, even for a limited stimulus-range; the value of the threshold increased at both extremes, and varied continuously throughout; (2) Individual differences with respect both to fineness of discrimination and to shape of curve were found; (3) Although curves for each subject remained fairly constant in shape, marked day-to-day variations in sensitivity seemed to occur.

The psycho-physical method commonly known as 'Mean Gradation' acts as a convenient link between the Weber's law investigations and the recent work on the construction of sensation scales, since it can conveniently be used in either connection. The present writer carried out a series of experiments with this method, the purpose being to repeat, with variations, the work of Merkel and Angell. The instrument used was the tuning-fork described above. The stimulus ratios were 1 : 10 and 1 : 100—rather higher than those studied by Merkel and Angell. Each ratio was judged by two subjects at three intensity levels, spaced at intervals of 20 db. Some difficulty was experienced in finding a criterion of 'nearness' to either mean, but taken all over, the results seemed to favour a closer correspondence of the estimated with the geometric mean.

More systematic work using the same method was carried out by Gage (17), who was interested in the following problem: If a mid-stimulus value X_3 be found between extremes X_1 and X_5, and further mid-values X_2 and X_4 be then found between X_1 and X_3 and X_3 and X_5, then a mid-value X_3' between X_2 and X_4 should, if a sensation scale is to mean anything, coincide with X_3. (This would hold irrespective of whether the mid-values coincided with the geometric or the arithmetic mean.) Gage found that the required correspondence did not occur, discrepancies of 5 and 6 db being obtained over a range of 40 db. Newman, Volkmann and Stevens (41), on the other hand, repeated Gage's work, introducing certain refinements in the procedure, and reduced the discrepancies between X_3 and X_3' to 0·18 to 0·36 db over a range of 20 db, which actually, in terms of loudness values, was a greater interval than Gage's. The authors therefore concluded that the method of 'bisection' (as it is now generally called) could after all be used as the basis of a loudness scale.

Other work on loudness scales has been done by a variety of methods, which, grouped together, may be said to constitute the most satisfactory and satisfying approach to the stimulus-sensation relationship. In the following paragraphs the work is discussed in chronological order, but a classification may be attempted as follows :

(*a*) The observer matches for loudness sounds of different frequencies or frequency spectra, and the results are compared with those of physical measurement methods.
(*b*) The observer selects from a given range a sound which he believes to bear a given numerical relation to a standard sound.
(*c*) The observer estimates in numerical terms the 'loudness-value' of a given sound, as compared with a standard sound to which the value unity is assigned.

The pioneer work, apart from that of Wien (59), was that of Sabine (48), who performed an experiment in which organ notes of different frequencies at octave intervals were balanced for loudness. The results, described as 'surprisingly concordant,' can be expressed in the form of a 'loudness contour,' which, except for a sharp drop at the two upper frequencies, is not unlike those of Fletcher and Munson (14, see below).

Fletcher and Steinberg (15, 52), investigating the estimation of overall loudness of a complex sound, showed that a total loudness could be obtained by summing a fractional power of the weighted energy of each frequency region. Calculated and observed values were found to be in good agreement, and a rather complicated empirical equation determined. This formula has not found universal acceptance.

Kingsbury (28) made a direct comparison of the loudness of eleven pure tones within a frequency range of 60 to 4,000 cycles, with a 700-cycle tone as reference. A series of curves relating sensation-level and loudness has been much quoted by subsequent investigators.

Richardson and Ross (45) were the first to use what have sometimes been called 'intuited' loudness units. A tone of pleasant loudness was chosen as standard, and assigned the value 1·00. This (S) was presented along with variables (V) in the form $SVSV$, and the observer wrote down his estimates of the numerical value of the variable. Of the eleven observers, all were able to perform the task with some measure of success, although many found it difficult, or complained that they were only guessing. Different forms of relation were obtained for different subjects, but in no case was it found that the estimates conformed to the formula $S = k \log R$.

Marvin (34) applied the loudness-balance method to the measurement of 'noises' of various kinds, these being matched against a 1,000-cycle reference tone. It is not quite clear whether Marvin was testing aural balancing or the meter which he used, but good agreement between the two was obtained.

Laird, Taylor and Wille (30) were the first to study 'fractional' and 'multiple' loudness. An audiometer buzz was presented along with another of lower intensity, and the observer was asked to say whether the latter was half the previous loudness, or whether it had to be raised or lowered to give half the loudness of the original. The same procedure was carried out for reductions of one-fourth and three-fourths. The estimates of half loudness were checked by 'doubling,' i.e. asking for a loudness judged to be twice that of a standard. Curves drawn on the basis of the results show a fair degree of consistency, and the authors drew up a 'tentative law' to express the results. No marked individual differences were revealed.

Ham and Parkinson (21) carried out experiments similar to those both of Richardson and Ross, and of Laird, Taylor and Wille. In the first group the observer was asked what percentage of an original (i.e. reference) loudness was ' left in ' a comparison loudness. In the second, the observer was required to select from a range of seven or eight variables a value which appeared nearest to a given fraction ($\frac{1}{2}, \frac{1}{3}, \frac{1}{5}$) or a given multiple (2, 3, 5) of the standard. The stimuli used consisted of warble tones, single frequency tones, and room noise recordings, all reproduced on special records. The frequencies covered were 250 to 2,500 cycles, and the intensity levels varied from 34 to 84 db. Plotting multiple increase of original loudness or reciprocal of remaining fraction of original loudness (y) against energy change in db, it was found that an equation of the form

$$y = a + be^{nx}$$

gave the best fit to the data. The best results were obtained with the second of the methods noted. In all, 175 subjects were tested. Each individual's judgments were consistent over a wide range, though they might differ from those of other observers. The authors proposed a noise measurement scale on the basis of their results—a straight line relation between multiple loudness units on a logarithmic scale and db above threshold.

Geiger and Firestone (18) worked on rather similar lines to those of the researches just described. In this experiment the observer himself set the variable loudness to a value bearing the required relation to the standard. The fractions required were $\frac{1}{2}$, $\frac{1}{4}$, $\frac{1}{10}$, $\frac{1}{100}$; the multiples were 2, 4, 10, 100. Tones of 60 and 1,000 cycles, and a complex noise of over forty components were studied at three intensity levels: 30, 35, and 80 db. Results similar in some respects to those of Kingsbury (28), and showing a good degree of self-consistency were obtained; on the other hand, they seemed to be at variance with those of more recent experimenters. The general conclusion was that loudness judgments are made on the basis of actual sensation.

Riesz (47) advanced the hypothesis that two tones of different frequencies would sound equally loud when their intensities were such that the ratios of the number of distinguishable steps above the absolute threshold to the number of such steps above the threshold for a reference tone of the same frequency were the same for both tones. This was put to the test using as reference one of Munson's equal loudness contours, and a good correspondence between observed and theoretical values was obtained, except at the two highest intensities, at which the influence of the threshold of feeling was probably operative.

The most authoritative work to date on the measurement of loudness is that of Fletcher and Munson (14), whose results have been adopted by the American Standards Association (4). The intensity levels at which pure tones of frequencies from 62 to 16,000 cycles sound equally loud was determined by comparison with a 1,000-cycle reference tone. Both ears of eleven observers were tested, at all intensities. The results are summarised in two sets of curves. The first set shows equal loudness contours relating frequency to sensation level (i.e. db above the threshold for that frequency). The second shows a similar set of contours, but with intensity levels (i.e. db above a uniform reference level) as ordinates. The greater part of the authors' paper is devoted to the calculation of the loudness level of a steady complex tone; the empirical formula derived is of rather a complicated character.

A new loudness scale designed for free-space listening, was devised by Churcher, King and Davies (9). To begin with, a scale based on a number of just perceptible increments above the absolute threshold of an 800-cycle reference tone was constructed. This relation was found to be more satisfactory than simply adopting the decibel scale as a loudness scale, but it still conflicted with introspectional evidence as regards loudness. A method whereby observers adjusted tones to half an original loudness was therefore substituted; the procedure was progressively repeated six times, and estimates of quarter loudness gave a fairly good check; thirty-four subjects took part. The authors go on to describe the application of their scale to the assessment of total noise by an aural balance method. This was held to give better estimates of loudness than methods of frequency analysis, the use of which, it was said, might give discrepancies of anything up to 40 per cent. when compared with direct noise-meter readings.

At the discussion following the original reading of the paper just discussed, various numerical relations approximating to the authors' scale were proposed. A friend of the present writer, however, has suggested an exponential equation: L (loudness) $= 100\ e^{\cdot 05x-5}$ (where x stands for sensation level in db); this, though not accurate, especially at the lower end, corresponds as well as any of the other suggested formulæ, and is more in line with the results of Ham and Parkinson, and of the present writer (49), who experimented on loudness estimation with the tuning-fork apparatus already referred to in two places. The procedure most closely resembled that of Richardson and Ross. Estimates of fractional and multiple loudness were made separately. For the former the unit intensities were -30 and -20 db attenuation on the instrument; for the latter -30 and -40 db. Three subjects took part, and while considerable fluctuations were seen in the results of all three, those of each individual always showed more resemblance to his other estimates than to those of the others. Accordingly it was possible and necessary to draw distinct smoothed curves for each observer, so that real individual differences seemed to have been established. A full mathematical analysis of the curves was not attempted, but it was apparent that some at least were of exponential form. Checks by the '(*b*)' method (see p. 286) gave rather inconclusive results. One further point, however, is of greater interest, namely, that fractional estimation or judgment did not seem to give curves of the same form as multiple estimation. This may have been due to the limited number of estimations made, but it bears out the remarks on the non-comparability of results of different methods stressed by Riesz (47), Abbott (1), and others. The fullest single study of the effects of subjective conditions on loudness judgments is that of Steinberg and Munson (53), whose general conclusion was that 'when sounds of different tonal character are compared by small groups of observers, we must expect appreciable differences of judgment to occur.'

Stevens (54) states the same point in still more general terms when he says that 'we do not measure the magnitude of a sensation, but only of a particular dimension or aspect of sensation within a single sensory modality.' Any auditory attribute is a function of both dimensions of the stimulus (frequency and intensity), and 'loudness is a name which we give to a class of discriminatory responses on the part of an organism under certain conditions of "set" and stimulation.' Stevens goes on to propose a new unit of loudness, the *sone*—the loudness of a 1,000-cycle tone listened to with both ears at an intensity level of 40 db. This corresponds closely to Churcher's (8) value 1, and is also said to be of the order of magnitude of just noticeable differences of moderately intense tones of the musical scale.

The sone has not as yet been at all widely accepted, but it must be recognised as the first real unit of psychological magnitude. The *phon*

(*B.S.*)[2] is defined as a unit of equivalent loudness, a sound being said to have a loudness of n phons when it is judged to be equally loud with a 1,000-cycle reference tone at a level of n db above a reference pressure level of 0·0002 dynes per sq. cm. Thus, on the basis of phon values one can arrange sounds in order of loudness, but it does not follow that a sound of mn phons is m times as loud as one of n phons. On the other hand, loudness values expressed in sones will presumably be true numerical magnitudes which will conform to mathematical requirements such as that just stated. Stevens also formulates an equation relating loudness to a power of the number of just noticeable differences above threshold, and derives a method of determining the subjective magnitude of a difference threshold.

The claims of the various loudness scales described above, and those of certain 'physical' scales, were examined by Abbott (1), who concluded that both were necessary, for different purposes. That of Fletcher and Munson was recommended for psychological purposes, as using the best available data for high and low levels, for which accurate information is often more important than at intermediate levels.

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[2] I.e. British Standard, as stated in Publication No. 661—1936 of the British Standards Institution. The qualification is necessary since the term *phon* has already been used in Germany in the same sense, but with different defining conditions.

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II. *Short summary of recent Cambridge experimental work.*

Introduction by Prof. F. C. Bartlett, F.R.S.

The following is a brief memorandum written by Mr. Craik on work relevant to problems of sensory measurement done mainly in the Psychological Laboratory, Cambridge, during the last few years. Nearly all the methods involved rest on a use of the principle, or method, of just perceptible differences. They are consequently subject to whatever assumptions as to measurability may be involved in that method. A considerable amount of work on supraliminal differences has also been carried out, but the difficulties encountered have not been satisfactorily surmounted, and in consequence this work is not dealt with in the present report.

The general upshot of the whole of the work has been to show as clearly and definitely as possible that all the formulæ which have ever been proposed correlating physical measurements of intensity of stimuli with just perceptible difference series are valid only within wide limits. Some of the limits have now been thoroughly studied : these are,

(*a*) rate of application of the stimulus ;
(*b*) degree of practice of the subject ;
(*c*) knowledge by the subject of the accuracy or inaccuracy of his reactions ;
(*d*) state of adaptation
  (i) of the peripheral organs,
  (ii) of the central nervous system mechanism concerned.

The experiments also show, though at present less thoroughly, where and how inhibitions, in the strict and proper sense, are likely to be set up, and how they can be dispelled and lead to sudden anomalous results.

It still looks as if, given control of all of the determinants indicated above, and very likely of some others which we have not yet investigated in detail, human judgments, or perceptions, of equality or of difference in the case of sensory reactions set up by stimuli of constant physical magnitude do remain remarkably constant also. But it is certain that these constancies of reaction cannot be stated in terms of general laws which refer only to the physical magnitudes compared. Given, for instance, differential adaptation between the two eyes, it is the case that two simultaneously presented visual fields, one of which is 500 times as intense as the other—in terms of physical magnitude—may regularly and constantly be equated. But that amount of physical difference of intensities will, under other circumstances, produce reactions which vary widely from one another. It seems as if, provided the leading groups of physiological and psychological determining conditions are stabilised, human sensory reactions can be equated and differentiated (at least so far as liminal differences are concerned) with a high degree of constancy, and further that under these conditions the equations and differentiations remain relatively remarkably constant for constant values of physical magnitude. We conclude that probably within the human sensory reaction itself there is, or there are, some quality or qualities which enable remarkably consistent comparisons to be made in terms of equality and liminal difference. But what that quality is or those qualities are we cannot at present say.

Factors Affecting Sensory Thresholds.

By Mr. K. J. W. Craik.

A considerable amount of work has been done in Cambridge, during the last four years, on the influence of various factors on the absolute and differential thresholds of the visual, auditory and tactile senses. It has had two purposes—the discovery of the various conditions which must be controlled, in perceptual experiments, in order that reliable results may be obtained, and the investigation of the mechanism of sensory processes, by finding the variables on which they depend.

These factors are of various kinds. First, there are such as the subject's state of health, the amount of sleep he has had, the criteria used, the number of readings taken, and the amount of practice allowed. These may be considered as extrinsic and non-specific influences, though they introduce possible sources of error and misinterpretation, if insufficiently controlled.

Secondly, there are factors such as inhibition, the organisation or *gestalt* of the field presented, mutual interaction between two similar or dissimilar sense organs, time error, incentives, and knowledge of results, which presumably originate centrally or at some high level. In some cases these have been investigated on their own account, in the hope that their mode of operation might be discovered; in other cases, they have been kept constant, so far as possible, in order that the elementary responses of the sense organs to simple forms of stimulation might be studied.

Thirdly, there are factors which affect either the stimulus presented or the sensitivity of the action of the sense organ itself. The spatial accompaniments and temporal precursors of the stimulus, and its rate of application, fall under this category; they may have direct influence on the stimulus, or on the state of adaptation, and therefore on the sensitivity, of the sense organ. It is important to be familiar with these influences, so as to avoid erroneous or anomalous results in sensory experiments; they also supply many interesting ways of observing the behaviour of the senses in unusual situations or under slightly abnormal conditions.

It is principally the last two classes of factors which have been studied in Cambridge; the results, and their theoretical implications, will be briefly summarised, and reference will be made to some of the relevant work done elsewhere.

Rawdon-Smith (1934, 1) found that the absolute threshold for a pure tone of 1,000 cycles might be raised as much as 30 db. by exposing the ear for some minutes to a similar tone at 100 db. above threshold. A similar rise, though less marked, was found in the ear which had not been stimulated; this could not be explained as direct fatigue by bone conducted or air conducted sound, and appeared therefore to be due to central inhibition. The rise of threshold in the stimulated ear was apparently in great part central, since it could be 'disinhibited' by unexpectedly turning off the light in the room where the subject was seated. A considerable degree of variability and irregularity in the rise of threshold also suggested its central nature, as did the absence of any known peripheral mechanism which could produce so marked an effect. (The tensor tympani and stapedius muscles were shown in the cat to produce a fall of responses not exceeding 6 db. (Rawdon-Smith and Hallpike, 1934, 1).) Here, then, there appeared to be two central processes which could affect the auditory threshold—inhibition following exposure to a loud tone, and disinhibition by changing the stimulation of a different sense organ, the eye. A similar rise of the differential threshold, using a loud 'adapting' tone and a much softer testing one, has been found by Rawdon-Smith and Sturdy (unpublished). There was a tendency for the differential threshold to be lowest when the adapting and testing tones were of equal loudness. Thus, after several minutes' exposure to a tone at 100 db. above threshold, the differential threshold at this intensity was often lower than after a period of silence.

Zangwill, and after him Jones (unpublished), measured the differential brightness threshold for a small patch within a field, and subsequently gave large numbers of exposures in which the patch might or might not be present; the subject was required to state whether or not it was there. The number of correct judgments was markedly increased day by day if the subject was informed after each judgment whether he was right or wrong, in comparison with a control group who were not given this information.

It was found by Gelb and Granit (1923) that the differential brightness threshold was raised inside a figure drawn on a background; they concluded that the threshold is affected by factors of configuration and organisation in the visual field. As certain simpler effects of contrast and adaptation did not seem to have been fully controlled, some experiments were undertaken in Cambridge by Craik and Zangwill (1938, in press) which showed a similar rise for figures having the same degree of contrast but much less strong 'figural character.' Certain other results, to be mentioned below, suggest that visual brightness discrimination is very largely peripheral, and in this case higher processes might be expected to have little influence on it. It is probable, however, that central factors are at work in visual localisation, after-effect of seen movement, and binocular fusion. An ingenious proof of the peripheral nature of flicker-fusion was given by Sherrington (1906), who showed that the critical flicker frequency is practically the same for both eyes as for one, though the phase relations of the flicker to the two eyes might be so arranged that one or other was illuminated all the time. It seemed, then, that the basis of the judgment ' flickering ' or ' steady ' was given by each eye independently, and was not subsequent to central fusion of the two fields.

Vernon (1934) found the critical flicker frequency to be affected by conditions which caused binocular rivalry.

There remains the third group of factors—those which affect the sense organ itself, by changing either the nature of the stimulus or the sensitivity of the sense organ.

It was found that the absolute threshold for touch and the differential threshold for tactile pressure (Grindley, 1936, 1 and 2) and for light intensity (Drew, 1936) were raised if the increase was made more slowly; no rise was found for tactile pain, however. Experiments by Rawdon-Smith (1935) showed indirectly that the differential loudness threshold is higher for slow than for rapid change. Thus a sound which was made alternately to increase slowly and decrease rapidly appeared to become steadily less loud, as the slower increases were not noticed. Direct proof of the same effect is given by Sturdy's work, at present in progress here.

The interpretation of these results is uncertain, and the mechanism may not be peripheral. Explanations might be proposed in terms of a trace-theory or of adaptation; at least it seemed desirable to consider these factors here, since they are more specific and more closely connected with the stimulus than those considered previously.

The peripheral aspects of auditory adaptation—the action of the tensor tympani and stapedius—have been investigated by Hallpike and Rawdon-Smith (1934, 1). The masking of one sound by another is a further example of the interaction of stimuli, and was studied by Lane and Wegel (1929) in the Bell Telephone Laboratories.

In visual perception, the presence of black masses or surrounds, or of glare spots, can affect the absolute and differential thresholds, critical flicker frequency, and acuity, in neighbouring areas, as shown by Lythgoe (1935), Vernon (1934) and Stiles (1929). Though similar in its effects to the ' masking ' of one sound by another, the mechanism is probably different. Unlike hearing, visual perception is multi-dimensional, so that simultaneously presented stimuli are not necessarily superimposed in sensation. It appears more likely that interaction, and contrast effects between different stimuli presented together, are mainly due to the power of these stimuli to change the state of adaptation of the retina in neighbouring areas (Lythgoe and Tansley, 1929). This conclusion is supported by the results of Craik (1938), showing a close similarity between the effects of previous dark or bright adaptation upon intensity discrimination or acuity (unpublished) and those of dark surrounds or glare spots as studied by Lythgoe and Stiles. There is evidence of other interactive processes in the retina; Adrian has facilitated neural summation by strychnine (1928). But whether or not all cases of mutual influence between two simultaneous visual stimuli can be explained in terms of adaptation, spatially regarded, it is certain that previous adaptation of the eye to an illumination different from that at which its brightness discrimination is measured, can cause a marked deterioration in such discrimination (Rawdon-Smith and Mellone, 1935, unpublished; Craik, 1938). The regularity of these effects, their restriction to the stimulated eye, and the failure of attempts to disinhibit them, suggest a retinal origin.

Throughout these experiments on hearing and vision, an endeavour has been made to find the main processes at work, to attribute them to their correct ' levels,' peripheral or central, and to correlate those events which appear to be peripheral with anatomical and electrophysiological findings.

Following Sherrington, interaction between two similar or dissimilar sense organs has been employed as a diagnostic sign of the level at which any perceptual process was taking place. Irregularity and disinhibition also provide clues, as noted above. Peripheral factors influencing the frequency and intensity range of the ear have also been investigated by electrical recording of auditory nerve-impulses (Hallpike and Rawdon-Smith, 1934, 1; Pumphrey and Rawdon-Smith, 1936; Hallpike, Hartridge and Rawdon-Smith, 1937; Adrian, Craik and Sturdy, 1938), and localisation of pitch discrimination in the cochlea assisted by the same method (Hallpike and Rawdon-Smith, 1934, 2).

In the eye, the effects of adaptation are so marked and regular as to give ground for analysing the behaviour of the eye (in regard to brightness discrimination and acuity) into two parts—its behaviour at different test illuminations when maintained in a constant state of adaptation throughout, and its power of adaptation to different illuminations. To borrow an analogy from Lythgoe (1935), the eye behaves rather like an ammeter which can be set to various ranges (adaptation to different intensities) and gives readings over a certain scale of currents when set to any one range (the brightness sensitivity of the eye at various test intensities when adapted to a fixed illumination throughout). It then appears that the eye sets itself automatically to its most efficient range, for any given illumination, if allowed sufficient time to adapt; for it is found that the differential threshold is lowest when the adapting and test illuminations are equal.

It has further proved possible, in some work at present in progress, to make monocular comparisons between fields simultaneously exposed to the two eyes, when one eye is bright adapted and the other adapted to some lower illumination or to darkness. Under these conditions, judgment of equality to within a scatter of plus or minus 10 % may be obtained although, owing to the two eyes being differently adapted, the physical intensities may differ five hundred-fold. That so constant a judgment of equality can be made between sensations whose physical stimuli are very widely different indeed, raises once again the problem as to whether sensations may not be in some sense measurable, that is to say whether there may not be some quality of the sensory responses themselves that enables them to be equated, or differentiated in equal appearing steps. It is, at any rate, clear that what value of physical intensity will be constantly equated to another, or just differentiated from another, depends upon many other conditions than the physical intensities themselves.

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III. *Statement by Mr. J. Guild.*

Are Sensation Intensities Measurable?

I have been invited to present the case of those members of this Committee whose answer to the question: Are sensation intensities measurable? is in the negative. To write a report truly representative of the views and outlook of those members would have involved an amount of collaboration which for geographical and other reasons has been quite impracticable. I have therefore made no attempt at such collaboration and this section of the Report is simply an exposition of my own views. But though I am aware that some of my colleagues would approach the problem from quite different angles I am not aware of any significant respect in which their main conclusions would differ from mine. In respect of these main conclusions I am confident that this section of the Report is representative of the considered opinions of the majority of the physicists on the Committee.

It is necessary in dealing with a subject of this kind to discuss not only physical but psychological matters. I cannot claim that in dealing with the latter the terminology is always employed in exactly the same sense as it would be used in the literature of psychology. I must plead with psychologists for the same tolerance in this respect that physicists have to extend to them when perusing their writings on psycho-physical problems. I have tried as far as possible to ensure that my meaning shall be clear from the context despite probable inaccuracies in psychological terminology. I am also dealing with broad principles, and to avoid confusion it has been necessary to abstain from frequent digressions to mention and explain away minor matters of detail which may not be in exact accord with some general statement. There are practically no physical or psycho-physical phenomena which are accurately described by any general statement. For example, I describe a 'permanent object' as a relation structure in which all the relations are found to be the same at all times. Of course there is not, strictly speaking, any such thing: temperature variations and other causes produce minor variations of relation-structure in any so-called permanent object. Points of this kind have no relevance to our present discussion and it would merely cloud the issue to bother about them. I trust this will be borne in mind by anyone who may consider that any statement or definition made hereafter is not *quite* right. I have also been confronted with the difficulty, in writing for readers whose experience is mainly derived from two different fields of study, of deciding what may be taken for granted. I trust this will be sufficient justification if I appear to any reader, or section of readers, to indulge in over-elaboration of obvious points. The same points may not be familiar to all.

Measurement.

Before proceeding to consider the problem of measurement as applied to psycho-physical problems it is desirable to consider some of the general principles of measurement.

Measurement is primarily a device which enables us to use the laws of arithmetic to solve problems relating to phenomenal events. The laws of arithmetic pertain to *numbers* and to nothing else : there is nothing inherently numerical in the structure of the phenomenal world. We are, however, so familiar with the description of phenomena in numerical terms (or their formal mathematical equivalents) that the association has become instinctive, and we are apt to imagine that we directly perceive the metrical aspects of nature as inherent constituents of phenomena, existing in their own right, so to speak, and merely observed by us. This induces us to overlook the essentially arbitrary and man-made nature of the association. When this is of an unfamiliar character, as, for instance, when we associate the arithmetical operation $\times \sqrt{-1}$ with the physical events constituting a phase change of $\pi/2$ in alternating current problems and others of a like nature, we recognise it at once as a mere device of the mathematician. In principle it is no more artificial than the more familiar associations of events with arithmetical concepts which underlie all metrical processes.

The phenomenal world presents itself to us as a complex relation structure. We need not here enter into metaphysical questions concerning the parts played by our sense organs and by things external to us in determining the kind of relations exhibited by phenomena. We will take the phenomenal world as we find it : a structure of related events, which we find it convenient to describe and classify in terms of various concepts.

We have discovered—and this discovery is the foundation stone of physical science—that by employing a simple but ingenious device some aspects of phenomena can be classified so that certain phenomenal relations existing between members of any such class are ' similar ' to the relations between members of the class of numbers on which the laws of arithmetic are based. In arithmetic, these relations are implicit in the meaning assigned by two important symbols, namely $=$, the symbol of numerical equality, and $+$, the symbol of the operation of adding one number to another. All other arithmetical operations, subtraction, multiplication or division, involution or evolution, etc., ultimately derive their significance from the operation of addition.

In any class of phenomenal aspects of the kind we are considering we can perceive various relations. Further, by performing experimental operations on the things which exhibit the aspects in question we can change the actual relations exhibited. But there is nothing inherently numerical in these phenomenal relations : in order to establish a connection we must arbitrarily associate some unique symmetrical transitive phenomenal relation from among those which may have perceptual significance with the arithmetical relation of equality ; and, further, we must associate some suitable experimental operation with the arithmetical operation of addition. If, now, we base our phenomenal classification entirely in terms of this symmetrical relation and this experimental operation we obtain a phenomenal series whose members are related to each other in a similar manner to the members of the series of numbers. We must not confuse ' similarity ' as here used with identity. Relations are themselves things which can be classified in virtue of certain characteristics irrespective of the kind of things related by them. A relation may be symmetrical or unsymmetrical, transitive or intransitive, etc., and it is these properties of relations themselves, and not any specific properties of the things related by them, which confer relational similarity or dissimilarity on classes defined by relations. However, it is not here necessary to discuss the theory of similar relations, or go into the conditions which must be imposed on our selected criteria of ' equality ' and ' addition ' in order that phenomenal and numerical relations may be similar. The important point to be noted is simply that there is no *a priori* connection between phenomenal structure and number, and that to make a connection we must artificially associate a phenomenal criterion with numerical equality and a phenomenal operation with numerical addition. When we have done this, but not before, we can predict by arithmetical calculation those phenomenal relations which involve only the stipulated practical criteria of equality and addition. A phenomenal class defined by two such practical criteria constitutes a measurable magnitude of the type which Dr. Norman Campbell, in his well-known text-book on the principles of measurement, has termed an *A* magnitude. Any such magnitude can be measured by processes which do not imply the measurability of any other magnitude. It is true that the practical criterion of equality for any *A* magnitude will always involve the observation of some phenomenal state or condition which may (and usually does) involve other magnitudes ; but the observational criterion is always of the null type—no difference, or no observable change, in the prescribed state or condition : no numerical relations have to be determined or even be assumed to exist for these other magnitudes. Familiar examples of *A* magnitudes are length, volume, mass, electrical resistance, and many others which need not be detailed. The practical criteria of equality and addition which define these magnitudes for purposes of measurement are sufficiently familiar to require no description. It is their significance which is not so widely recognised as it might be. It is probably usual to regard the experimental processes of determining equality and of adding as something which we have just found to be a convenient method of determining quantitative relations inherent in the nature of the magnitudes, whereas these processes are the necessary connecting links between phenomena and number without which there would be no basis of comparison between the laws of the former and those of the latter. The experimental criteria do not merely enable us to measure a magnitude, they *create* the magnitude by defining the fundamental relations which are to be the basis of classification.

In Physics the general term measurement is not confined to *A* magnitudes. By suitable experimental processes we affix numerals to many aspects of phenomena to which no operation can be performed having any similarity in relation structure to the operation of addition. Familiar examples are density, specific heat, electrical resistivity, etc. All those things which we ordinarily regard as properties of substances are magnitudes of this type. They are the *B* magnitudes of Campbell's classification. We can usually formulate a practical criterion of equality for a *B* magnitude, *but not of addition.* Nevertheless the numerals affixed to these magnitudes by our experimental processes associate the members of the magnitude series with the members of the series of numbers in such a way that predictions based on arithmetic will always come out right. This does not, however, mean that measurement without a practical criterion of addition is possible. It results from the fact that *B* magnitudes are evaluated simply as a function of the measured quantities of two or more *A* magnitudes. The density of a substance, for example, is nothing more nor less than the ratio of the numbers which measure the two *A* magnitudes, volume and mass, associated with any lump of the substance. Its importance is simply that this ratio is found to be approximately constant for all lumps of what we call the 'same' substance and so is worth noting as a property of the substance. But it would be impossible to do what, for brevity, we call measuring density unless we were already able to measure volume and mass. The associations between phenomena and number required for the measurement of density are supplied, not in practical criteria applicable to density as such, but in the practical criteria of equality and addition on which the scales of volume and mass are based.

Strictly speaking, therefore, the only measurable magnitudes are *A* magnitudes. Density is only ' measurable ' in the sense that we can arrive at an evaluation of it by processes of measurement—measurement of the *A* magnitudes volume and mass ; and any relation involving density is primarily a relation involving volume and mass. Similarly what we call the ' measurement ' of any other *B* magnitude is really only the measurement of two or more *A* magnitudes and the combination of the results by some appropriate formula to give a single number which we term the ' value ' of the *B* magnitude in question. Confusion is sometimes caused by the fact that in practice we frequently evaluate *A* magnitudes by processes which are more appropriate to *B* magnitudes. After scales of measurement have been established for many magnitudes, and instruments of various kinds developed for their practical evaluation, it is often easier with the apparatus at one's disposal to evaluate an *A* magnitude indirectly from measured amounts of other magnitudes than to measure it directly. For example, electrical resistance, which is an *A* magnitude, can be measured as such with a Wheatstone bridge system which does not involve measurement of any other magnitude. It can also be determined indirectly from measurement of the potential difference required to drive a measured current through it, and this quite often is the most convenient method. Similarly length, which is pre-eminently an *A* magnitude, is often measured indirectly by methods involving measurements of the angles of a triangle. But these indirect methods are only possible because we have previously established the scales of measurement of the *A* magnitudes by direct methods involving only the criteria of equality and addition appropriate to each, and have, then, from measurements made possible by the existence of these scales, determined the various quantitative relations among phenomena which we must know before we can deduce the value of an *A* magnitude from measurements of other magnitudes.

We must clearly distinguish between indirect methods of measuring *A* magnitudes which we may adopt as a matter of choice *after* we have accumulated various experimental data involving previous direct measurement of the magnitudes, and the indirect methods that are inevitable in the case of *B* magnitudes, which, because there is no criterion of addition applicable to them, cannot be measured except as the numerical constants in laws relating the quantities of two or more *A* magnitudes which are found to be associated together in some important class of circumstances. Density, for example, is simply the constant in the experimental law that for any lump of a given substance under specified conditions if we determine the number which is the measure of its mass on the scale appropriate to mass and the number which is the measure of its volume on the scale appropriate to volume the ratio of the former number to the latter is constant.

The significance of all measurement is therefore derived from the principles of measurement of *A* magnitudes. These, very briefly, are as follows : We prepare a large number of samples of some physical entity which exhibits the magnitude in question. In the case of length or mass, for example, these samples will be material rods or lumps. In the case of a magnitude like intensity of radiant energy the samples of the physical entity exhibiting the magnitude may consist of lamps or other sources of radiation operating under constant conditions.

By some method appropriate to the particular case we adjust these samples until they fulfil our experimental criterion of equality. In this way we obtain any required number of ' equal ' quantities of the magnitude. We attach the same numeral to each of these quantities, thereby associating them with one number. We then produce other quantities in increasing order of magnitude by the successive performance of our practical operation of addition on the equal quantities at our disposal. In this way we obtain a series of discrete samples of the magnitude having a relation structure similar to that of an arithmetical progression. Though not essential, it is convenient for identification purposes that the same numerals should be used as names for the corresponding members of the phenomenal and numerical series. The particular arithmetical progression which fulfils this condition is that in which the first term and constant difference is the number which we have arbitrarily associated with each of the equal samples used to build up the series of magnitudes. If for convenience we take unity for this number, our series of magnitudes consists of members, associated by virtue of our practical criteria of equality and addition, with the cardinal numbers 1, 2, 3, 4, . . . etc., the number associated with each member being simply the number of our original equal samples of the magnitude which have been added together to obtain it. This series constitutes a scale of measurement of the magnitude in terms of the original quantity as the 'unit.'

A scale of measurement would, however, be of little use if its significance were confined to the members of the initial series built up in this way. We want to be able to use the scale to 'measure' any sample of the magnitude which we chance to encounter. We do this by comparing the unknown sample with the various members of our built-up series to find if it fulfils our practical criterion of equality with any one of them. If we find it to be equal to the member associated with the number n, we say that n is the measure of the unknown sample. There is an obvious difficulty if we cannot find a member in our standard series to which the unknown magnitude is equal, for we have not defined a practical criterion for any numerical relation other than equality. All we can say in such a case is that the sample measures more than n and less than $n + 1$. In principle no scale of measurement can be continuous because it involves an association with number, which is essentially discontinuous. A scale of measurement can only define and identify a discrete series of quantities. In practice of course we can reduce the gap between successive members of the measurable series by taking smaller samples of the magnitude for our initial collection of equal quantities. If we take them so small that the gaps in our scale are of the same order of magnitude as the uncertainty in determining whether or not our practical test of equality is fulfilled we shall always be able to find some member which seems to be equal to any given sample of the magnitude. In other words we can measure any sample to within the accuracy of our practical tests, though in principle any scale we can construct, however fine-grained, has exactly the same kind of discontinuity as one constructed with large steps. It would clearly be tedious to build up a complete scale of any magnitude from very small samples of the magnitude, and in practice various short cuts based on the numerical relations imposed by our initial definitions of equality and addition are employed, but the fundamental principle is not affected.

It is obvious that to state the number which measures any given sample of a magnitude tells us nothing unless we know what quantity has been taken as the 'unit.' This is not necessarily the quantity of the members of the initial collection of equal samples used to build up the scale: as already mentioned we are free to associate any number we like with this quantity, though in the absence of any reason to the contrary it would be natural to associate it with the number 1. Usually, however, there are reasons to the contrary, but these are extraneous to the principles of measurement and are mere reasons of convenience which we need not go into here. There is no reason inherent in the nature of things why the number 1 should be associated with any particular quantity of any magnitude. What is essential to measurement is that this number shall be uniquely associated with *some* quantity, and that standard samples shall be available by means of which we can reproduce this association at any time. These standard samples need not be, though they often are, of the exact quantity constituting the unit. They may have any value on the scale defined by the unit. Their function is to make possible the co-ordination of the scales of the various measuring instruments which have to be used in practice. Every measuring instrument has a scale of its own. If it is constructed on correct principles its scale will be similar to the standard scale but will not in general be identical with it. By measurement on the scale of the instrument of a standard sample whose value on the standard scale is specified we ' calibrate ' the instrument so that values on the standard scale can be deduced from its readings.

It is clear that standards are only necessary for A magnitudes. No standards are required for B magnitudes, whose numerical values are entirely determined by the scales used for measuring A magnitudes.

It is obvious that the practical criterion of equality and the practical operation of addition which together define an A magnitude must be applicable at all parts of the scale of the magnitude. In arithmetic it is not only true that $1 + 1 + 1 + 1 + 1 = 5$, but also that $1 + 4 = 5$ and that $2 + 3 = 5$ and also that $5 + 5 = 10$, $5 + 8 = 13$, and so on. Once we set up a scale of measurement for a magnitude, and begin to apply arithmetic to the results of measurement, the laws of arithmetic will predict that phenomenal relations ' similar ' to the numerical relations just quoted will exist among the members of the magnitude series, and these predictions are meaningless unless the practical criteria of equality and addition are applicable irrespective of the ' size ' of the samples to be compared or combined. Thus it is not enough to have a practical criterion of equality for, say, length which can only be applied to samples of 1 mm., or a criterion of addition of lengths which can only be applied to samples 1 mm. long. From such limited criteria we could construct any number of equal samples 1 mm. long and could add them to form a series in ascending order of magnitude, but the process would be of no use whatever for measurement. The laws of arithmetic would predict that one group of five of our equal samples, added by our criterion of addition, should be equal to any other group of five similarly added ; but this arithmetical prediction has no phenomenal equivalent if the practical criterion of equality by which our 1 mm. samples are selected is for some reason inapplicable to the comparison of 5 mm. samples. Further, arithmetic predicts that one 5 mm. sample added to another 5 mm. sample is equal to a sample obtained by adding ten of our 1 mm. samples. This prediction is again meaningless if our process for adding the 1 mm. samples is not also available for adding 5 mm. samples. There is, in fact, an important principle of measurement, which as far as I am aware has never been explicitly stated because it is so obviously fulfilled in most of the measurements carried out by physicists that to state it seems superfluous. This principle is that the phenomenal significance of equality and addition as applied to any magnitude must remain the same to whatever samples of the magnitude they are applied.

If this principle is violated by changing the significance either of addition or equality as we ascend the series of magnitudes, we destroy the cross-relation between the series of magnitudes and the series of numbers on which their ' similarity ' depends, and the result, whatever it may be, is not measurement.

The foregoing considerations obviously apply to the majority of the magnitudes measured by physicists. There are some cases in which the application is less obvious, for example the measurement of intervals of time and the measurement of temperature. To discuss satisfactorily the measurement of time would, at the present stage in the development of physical theory, involve a long excursion into Relativity problems ; so as no one has tried to base sensory measurements on any analogy with time measurements, there is no justification for devoting space to it here. Suffice it to say that there is no method of measuring time which is not in accordance with the general principles already laid down. More consideration must be given to the measurement of temperature, for attempts are frequently made to justify the use of certain so-called scales of sensation intensity by analogy with arbitrary scales of temperature. There is no real analogy ; but before this can be demonstrated we must examine the case of temperature.

When we observe physical objects by the sense of touch the sensations produced contain various constituents which we interpret as indicating distinctive properties of the objects. Objects may be rough or smooth, hard or soft, *hot* or *cold*, etc. The condition of an object in virtue of which it may feel hot or cold to the touch is called its temperature. Experiment has shown many observable relations of a general kind between the temperatures of bodies and their measuable properties. The length and electrical resistance of a given rod, for example, are usually greater when the rod feels hot than when it feels cold. In fact, nearly all the properties of a sample of any substance are appreciably different in these two conditions, and in particular, what is called the amount of heat in the sample, which has been identified with the energy of the relative movements of the ultra-microscopic particles of which bodies are known to be composed, is greater when the body feels hot than when it feels cold. When two bodies, of which one is hotter than the other, are brought into close contact, there is a transfer of energy from the hotter body to the cooler, which goes on until the molecular movements in each body are (on the average) equally energetic. We are entitled to use the term ' equally energetic ' here, because kinetic energy is measurable quite apart from this property of hotness or coldness of bodies, so that equality, as applied to quantities of energy, has a definite significance. When the transfer is complete, the hotter body has been cooled and the cooler body warmed until neither is hotter than the other. The bodies are then said to be in thermal equilibrium. This is a symmetrical transitive relation between bodies, and so provides a practical criterion of equality for the condition called temperature. But there is no practical operation similar to addition which can be applied to temperature. Obviously there is no method of combining bodies of equal temperature which will provide a series of different temperatures. In this respect temperature is analogous to density : any combination of bodies of equal density results in no change in the value of density. Similarly, any combination of bodies of equal temperature results in no change of temperature. Temperature must therefore be treated as a B magnitude. Our scale of temperature must depend on the measurement of something else for which a scale of measurement is already established. Since the temperature of a body depends on the energy of molecular movement, we may produce our arbitrary association between a series of temperatures and the series of numbers by considering the temperature of a body to increase by equal amounts for equal increments of the energy of molecular movement. If we assume a scale of temperature to be established on this basis it can be shown theoretically that certain physical relations—called thermodynamical relations—are of a very simple form. This is of great convenience to the mathematician, but is of no special importance to anybody else. We must not make the mistake of assuming that the simplicity of thermodynamic formulæ on this scale of temperature indicates that we have hit upon a ' true ' measure of temperature. The thermodynamic relations will obviously be simple since nothing but dynamical quantities are taken into account in establishing our scale of temperature, which is purely conventional and made to give the simplest possible relation between temperature and energy. Unfortunately this scale is of little use to the practical physicist as there is no practicable method of measuring molecular energies, which have to be deduced from measurements of still other things, such, for example, as the properties which would be exhibited by a so-called perfect gas or an ideally perfect heat engine. But as there is no perfect gas and no ideally perfect heat engine practical measurements can only be made with imperfect gases or imperfect heat engines, and corrections which are difficult to determine and in some cases involve assumptions of doubtful validity have to be made. The thermodynamic basis does not, therefore, afford a practical means of measuring temperature. It cannot be over-emphasised that measurement is a practical process for obtaining experimental information, and it is not sufficient to be able to formulate the necessary kind of association between a series of magnitudes and numbers ; it must also be possible to carry out all the operations involved in defining the basis of the association. Otherwise we have a theoretical scale of measurement with which we cannot measure anything. The thermodynamic scale of temperature is in this category. For the practical measurement of temperature we must seek some other basis. We seek it in some measurable physical property of substances or bodies which varies continuously with temperature. We have an embarrassing choice of such properties : the length of a rod, the volume of a given mass of a gas or liquid, the electrical resistance of a wire, the e.m.f. of a ' thermocouple ' of two wires of dissimilar metals, the intensity of radiation from a ' black body,' etc., etc. All these are measurable by practical processes and all vary with temperature. We may choose any one of them to provide a scale of temperature by *deeming* to be equal those steps in temperature which accompany equal increments in the measurable property, or by *postulating* any other convenient law relating the property with temperature. The mercury thermometer scale is based on this principle, the practically measurable property utilised being the apparent volume of mercury in a glass container. It is as ' true ' a scale of temperature as any other, that is to say, there is nothing either true or false about it. Physicists, however, soon wanted to pursue their investigations of thermal phenomena to temperatures both higher and lower than those for which mercury thermometers can be used, and various forms of gas thermometer were evolved. Each of these involves a new definition of a temperature scale based on deeming to be equal the increments of temperature associated with equal increments in the pressure, at constant volume, or the volume at constant pressure, of a constant mass of whichever gas is used, usually hydrogen or nitrogen. When the pressure is low, and the temperature high, the properties of hydrogen approximate very closely to those of the mathematician's idea of a perfect gas. The scale of the hydrogen thermometer, except at low temperatures, therefore agrees closely with the thermodynamic scale.

But there are disadvantages in using a gas thermometer for everyday thermometry, and in practice other types of thermometer are used. Conspicuous among these are the platinum resistance thermometer and various types of thermocouple. All of these provide scales of temperature which are essentially independent of each other, though they may, of course, be empirically related by intercomparison of thermometers of the various types. There is not, however, any *a priori* formal relation between the scales, and it is necessary to make an arbitrary choice of some one of them as a standard scale for defining the magnitude temperature. Unfortunately, no single type of thermometer can be used over the whole range of temperature in which physicists are interested, and the international temperature scale, adopted in 1927, is a patchwork arrangement involving three essentially different scales. From $-190°$ C. to $660°$ C. the standard scale is defined by the variation of resistance of pure platinum wire ; from $660°$ C. to $1063°$ C. by the variation of the e.m.f. of a thermocouple of platinum and a specified alloy of platinum and rhodium ; and above $1063°$ C. by the variation in intensity of monochromatic radiation emitted by a ' black body ' radiator.

In each of these ranges the temperature scale is defined by *postulating* a formal law relating the measurable variable (resistance of platinum thermometer ; e.m.f. of platinum, platinum-rhodium thermocouple, or intensity of monochromatic radiation) with temperature. The constants in these laws are determined by means of appropriate ' fixed points,' e.g. the temperatures of melting ice, boiling water, boiling sulphur, melting gold, etc. In order to preserve the simplicity of thermodynamic equations and the gas laws, the various scales are brought as closely into accordance with the thermodynamic scale as possible by suitable choice of the values to be assigned to the ' fixed point ' temperatures and by suitable choice of the law of variation postulated for the standard type of thermometer applicable to each range. This does not mean that we succeed in setting up a thermodynamic scale. The scale is still in fact made up of several independent parts, each of which is completely defined by the law of variation postulated for the prescribed type of thermometer together with the values assigned to those of the fixed points utilised in determining the constants of the law. All we can succeed in doing by suitable choice of laws and constants is to get a scale which is sufficiently close to the theoretical thermodynamic scale that the laws of thermodynamics based on the theoretical scale are approximately true for measurements made on the real scale.

It is of great convenience to do this ; but it is not an essential requirement of measurement that it should be done, and the scale arrived at in this way is not in any fundamental respect a ' truer ' scale of temperature than one based on the postulation of simple proportionality of, say, the resistance of a platinum thermometer and temperature.

The fact that there is no operation of addition applicable to temperature *qua* temperature, prevents it from being measurable in the true sense of the term. All we are able to do, however we may disguise it by theoretical considerations, is to assign numerals to temperatures in accordance with an arbitrary postulated relation to some measurable property of some specified substance or piece of apparatus. When once we have defined some such scale of temperature, temperature becomes ' measurable ' in the broad sense in which this word is generally used ; and the laws relating other physical variables with *temperature as so defined* become open to empirical investigation.

It will be clear that the measurement of temperature is only possible because the relations between temperature and those properties of substances or bodies which we utilise in defining the scale are constant.

But how do we know they are constant ? It may be thought that confirmation of the constancy of the relations defining our scale is to be sought in the constancy of the various other relations between phenomena and temperature which we determine by means of our scale once we have established it. This is not so, for such a principle is based on an *a priori* assumption of the constancy of natural laws. Any such assumption belongs to metaphysics and has no place in physical science, in which no statement about the behaviour of the universe can be made except on the basis of experimentally determined facts ; and no facts about the relations between phenomena and temperature can be determined *until* a metrologically sound method of measuring temperature is available. We must therefore be satisfied that our method of measurement is sound without reference to any natural laws involving temperature. The point is that the constancy of the law defining our scale does not require confirmation. It is not an *assumption*, which may or may not be true, it is a *postulate* forming part of the conventional framework of physical measurement. The postulated law is necessarily always true for the simple reason that it serves the purpose of defining temperature as ' the thing for which this law is true.' There is no criterion of the magnitude of a temperature (nor of any B magnitude) other than the law by which we choose to define it. It would therefore be meaningless to ask whether the temperature to which our scale assigns the numeral n is in fact the *same* temperature at all times and places.

Temperature only enters into the physicist's experience *indirectly*, in virtue of its effect on the measurable properties of bodies. He is therefore never concerned with it as a thing having existence in its own right which may on occasion assert that existence in ways which are inconsistent with his arbitrary definitions. He is perfectly free to define it by any postulated relation to some one physical property, and so long as he keeps to this definition and makes his measurements in accordance with it no contradiction between the results of measurement and any other kind of experience can ever arise.

Psycho-physical Measurements.

With the foregoing introduction in mind, the application of measurement to psycho-physical problems may now be considered. The title of this Committee is wide enough and vague enough to include many matters which are not in dispute, and some misapprehension has been evident during the deliberations of the Committee as to precisely what is in dispute. Those who have contended that methods hitherto supposed by some to lead to quantitative scales of sensation intensity are invalid have been supposed to contend that no quantitative experiments bearing on ' sensory events ' are possible. Such a contention would of course be absurd, and I will first consider some important types of psycho-physical measurement which can be, and have been, effected. These may briefly be classed as measurements of relative stimulus efficiencies. An important and well-known example of this kind of measurement is found in the so-called relative visibility function of the eye, which tells us the relative visual efficiency of unit intensity of monochromatic radiant energy of different wavelengths. Briefly, these measurements consist in determining the relative amounts, measured in physical units, of two samples of monochromatic radiation of different wavelengths which are required to make the two sides of a photometric field appear equal with respect to the attribute of brightness. By repeating this for different pairs of stimuli we obtain measures of the relative physical intensities of stimuli of various wavelengths throughout the spectrum which are required to produce equal intensities of the sensation of brightness. The relative efficiencies of radiation of different wavelengths for exciting the sensation of brightness are inversely proportional to the amounts required to produce the same brightness, and it is these relative efficiencies which are usually tabulated or plotted as the ' relative visibility function ' of the eye.

Now what is meant by the physical intensity of a stimulus in these experiments ? We should usually define it as the rate at which radiant energy is incident on unit area of the photometer screen. This is what we intend our physical intensities to mean, but there is a difficulty. Intensity of radiation can be measured as an A magnitude for all samples of radiation of the same spectral quality, because in comparing such samples the properties of the measuring instrument are completely eliminated and equality in its response implies equality of radiation intensity. It is a different matter when we wish to measure samples of radiation of different spectral qualities ; for example, monochromatic samples of different wavelengths. A physical detector of radiation operates in virtue of some interaction between radiation and matter, and in general the efficiency of radiation in producing the response characteristic of the detector varies with wavelength. In other words the sensitivity of the detector is a function of wavelength. It is evident that when we compare samples of radiation of different wavelengths λ_1 and λ_2 for which our detecting apparatus gives equal response we have not necessarily got equality in the rates of energy flow E_{λ_1} and E_{λ_2}, but in the products $\sigma_{\lambda_1}E_{\lambda_1}$ and $\sigma_{\lambda_2}E_{\lambda_2}$, where σ_{λ_1} and σ_{λ_2} are the sensitivities of the detector for these wavelengths. Our experimental criterion of equality is not equality of E_λ but equality of $E_\lambda\ \sigma_\lambda$, where σ_λ is some function of wavelength. This is true whatever type of detector we use to provide a practical criterion of equality in radiation measurements. In principle we cannot get beyond it.

We cannot formulate a practical criterion of equality for E_λ alone, but only for the product of E_λ and another function of wavelength characteristic of the particular instrument used to provide the criterion of equality. Now we have seen that in the measurement of an A magnitude quantitative knowledge of the properties of the instrument used to establish equality must not be assumed. This requirement is fulfilled in the present case if we regard as our measureable magnitude not the quantity E_λ but the quantity $E_\lambda\ \sigma_\lambda$. This is, in fact, the quantity measured by any radiometric operation, and it is evident that different types of detecting instrument for which σ_λ is not the same function of wavelength will measure different magnitudes. It so happens that some types of detector can be constructed for which σ_λ is nearly independent of wavelength. Thermopiles and other instruments with lamp-black receiving surfaces or with nearly-closed radiation traps are of this class. In a well-blackened thermopile, for example, σ_λ is so nearly constant over large ranges of wavelength that for practical purposes it may be regarded as constant. With such an instrument the magnitude $E_\lambda\ \sigma_\lambda$ may, to a close approximation, be written $k\,E_\lambda$ and it is usual to leave the constant out altogether and regard the magnitude as E_λ. For practical purposes this is quite legitimate. Our results are the same to within the errors of observation as they would be if we really did measure E_λ ; but for an understanding of the significance of a criterion of equality in any metrical process, it is fatal to ignore the factor σ_λ even when it is constant. This is the same kind of philosophical error as we make when we regard the arithmetical ratio $n/1$ as identical with n, ignoring the division by unity because it does not affect numerical results. The two *n*s are, however, quite different things ; one may be the cardinal number n, the kind of number used for counting eggs, whereas the other denotes a *relation* between two numbers, one of which just happens to be unity. In the same way there is a fundamental difference in significance between the magnitude E_λ and a magnitude consisting of the product of E_λ with a function of wavelength, and this difference in significance is not eliminated because in some particular case the function of wavelength is a constant and may be assigned the value unity. The essential point is that whatever apparatus we use in an attempt to measure radiation intensity we in fact measure a magnitude of which radiation intensity is only one factor, the other factor being a property of the particular kind of physical system on which radiation produces the detectable effect utilised in our measuring apparatus.

In the case of a thermopile or similar instrument this second factor is practically independent of wavelength, and for practical purposes we treat the magnitude as though it were E_λ alone. With detectors of the photo-electric or photo-chemical type, the instrumental factor varies considerably with wavelength, but any such detector furnishes a practical criterion of equality for a magnitude $E_\lambda\ \sigma_\lambda$ where σ_λ is its sensitivity factor at wavelength λ.

Now this is exactly what the human eye does in photometric measurements involving differences in spectral composition. The part of the eye which interacts with radiation is, of course, a physical instrument which converts some of the radiant energy into other forms, photo-chemical and photo-electric effects being produced which in turn cause stimulation of the optic nerve. We are not at this stage of events concerned with the physics or physiology of the nerve system or with how the physical phenomena occurring in the nerves ultimately produce the sensation of light. The peripheral organ is simply a physical detector of radiation, and when used in conjunction with a photometer provides a criterion of equality for a magnitude $E_\lambda\ \sigma_\lambda$ where σ_λ has the same kind of significance as in the other cases referred to above.

Now in the case of the eye σ_λ is the efficiency of radiation of wavelength λ in stimulating the sensation of brightness, so the magnitude $E_\lambda\ \sigma_\lambda$ has the dimensions of sensation intensity, as of course it must have since it is equality of sensation intensity which provides our criterion of equality for $E_\lambda\ \sigma_\lambda$. If we could measure $E_\lambda\ \sigma_\lambda$ we should be measuring sensation intensities ; but while we have a practical criterion of equality for this magnitude we have no operation of addition. All the means employed to vary the intensity of the light reaching the eye from the photometer are applied solely to the beams of radiation and depend ultimately on the operation of addition applicable to E_λ alone. There is no operation we can perform which would correspond to the addition of quantities of the product $E_\lambda\ \sigma_\lambda$. It is therefore impossible to deduce any quantitative information about unequal intensities of sensation from photometric measurements such as those embodied in the visibility curve. We can predict that certain relative quantities of energy of different wavelengths will produce equal sensation intensities under prescribed conditions, but that is all, and the whole science of heterochromatic photometry is devoted to establishing this kind of equivalence between stimuli of different physical qualities. The variation of relative sensitivity with wavelength as exhibited by the visibility curve is of course an important constituent of ‘ sensory events ’ and can, as we have seen, be quantitatively described by the results of measurements.

The kind of measurement denoted by the term ‘ Colorimetry ’ involves only an extension of the same principle.

To discuss colorimetry would carry us beyond the limits of available space. Suffice it to say that in all such measurements the only criterion provided by the eye is one of equality, in this case not only equality in the one attribute of brightness but simultaneous equality in colour and brightness. As in ordinary photometry all quantitative operations performed in the measurements are performed on beams of radiation, and do not provide any experimental operation which can be identified with addition of sensations. The results of colorimetric measurements can therefore only be used to predict equality of colour (under conditions of equality of brightness) for various combinations of stimuli. They cannot be used to obtain a quantitative measure of the difference between two different colour sensations. The sets of numerals assigned to any stimulus as a measure of its colour are in no sense a measure of the colour sensation evoked by that stimulus: they are simply a measure of the relative quantities of three standard stimuli which, if combined, would evoke the *same* colour sensation.

The sensory properties which can be quantitatively described by the results of photometric or colorimetric measurements are typical of all those in which controllable stimuli are compared with respect to the relative amounts of them which evoke the *same* intensity or quality of sensation, or which, more generally, produce the *same* psychological effect. The psychological criterion of equality which we impose defines, qualitatively, some psycho-physical magnitude, a B magnitude, defined completely by the ratio of two or more stimulus quantities which fulfil the psycho-physical criterion of equality under prescribed conditions. Such magnitudes can be measured, and their measurement gives important information about 'sensory events,' but such measurements do not serve to place dissimilar sensations on a quantitative scale either of intensity or quality.

This of course has always been recognised by leading psychologists. Thus Fechner, as quoted by Titchener (*Experimental Psychology*, Vol. II, 1905, p. xxiii) says, '. . . the measure of sensitivity, as a measure of mere capacity of sensation, is not to be confused with a measure of sensation itself. Nor does it presuppose any such measure, but only the observation of instances of equal sensations, under like or different conditions of stimulation.'

A type of measurement which has been supposed to provide a measure of sensation intensity is that based on the determination of just noticeable differences of stimulus intensity, usually designated j.n.ds. for brevity. The principle of such measurements is sufficiently familiar to anyone who can be interested in this Report that it is needless to describe it here. The relation between j.n.ds. and stimulus intensity has been determined for various senses and in itself provides valuable information about the operation of the various sensory mechanisms. The ratio of the j.n.ds. at any intensity to the intensity tells us the minimum fractional change in that intensity which is perceptible. We may term it the fractional sensitivity, and its determination under properly specified conditions is not only of interest to the psycho-physicist, but is of great practical importance. Fractional sensitivity is a B magnitude defined as the ratio of two stimulus quantities associated in a specified manner—namely, that the smaller of them is just noticeable when added to the larger. Its evaluation involves only measurement of stimulus intensities, and the association between phenomena and numbers if entirely provided by the scales of measurement established for stimulus intensities.

Fechner, however, believed that a scale of sensation intensity could be based on j.n.d. measurements. His contention has received much criticism from the beginning, both from psychologists and physicists, and I find it impossible to say that psychologists on the whole have been more or less ready to accept its implications than physicists. Despite the criticism which has been lavished on it, however, Fechner's general principle is still believed by many to afford a basis for measurement of sensation intensities. This is probably due to the fact that much of the criticism has been on questions of detail or on questions of principle which are not really relevant and the real objections have been lost like a needle in a haystack of discussion. I will endeavour to confine attention here to the essential principles of Fechner's theory and examine these in the light of the general principles of measurement.

Let us assume a stimulus to be increased from zero intensity by steps each of which is a j.n.d. Let us denote the j.n.d. at intensity I by ΔI. The total stimulus intensity at any stage is, of course, the sum of all the ΔIs which have been added up to that stage. From our experiments we can express ΔI as some function of I, say $\Delta I = f(I)$.

When the j.n.d. ΔI_1 is added to the stimulus I_1 there is an increase in intensity of sensation which we denote by ΔS_1. Similarly when the j.n.d. ΔI_2 is added to the stimulus I_2 there is an increment of sensation ΔS_2, and in general, the stimulus increment ΔI when added to the stimulus I produces an increment ΔS in the sensation intensity. Fechner's first principle is that the total sensation S corresponding to the stimulus I may be regarded as the sum of all the ΔSs corresponding to all the ΔIs which have been added to produce the stimulus. If we know the relative magnitudes of the various ΔSs we then, by this principle, have a scale of magnitude for S. Of course we have no *a priori* knowledge of the relative magnitudes of the sensation increments corresponding to j.n.ds. at different intensity levels, and Fechner's second principle is that we are free to postulate an arbitrary quantitative relation between ΔS, the liminal increment of sensation, and S, the total sensation, say $\Delta S = \varphi(S)$, where $\varphi(S)$ is some arbitrary function.

We then have, by postulate, $\Delta S = \varphi(S)$, and from j.n.d. experiments $\Delta I = f(I)$.

$$\text{Whence} \qquad \frac{\Delta S}{\varphi(S)} = \frac{\Delta I}{f(I)}$$

treating the small quantities ΔS and ΔI as differentials we get the differential equation

$$\frac{dS}{\varphi(S)} = \frac{dI}{f(I)}$$

and by solving this equation we obtain the relation between S and I, that is to say, the relation between sensation intensity and stimulus intensity.

I have put Fechner's second principle in its most general form. Fechner himself propounded it in the special form in which $\varphi(S) = k$: that is to say, he postulated that all the ΔSs are equal. Plateau and others have suggested the form $\varphi(S) = kS$, equivalent to the postulate that $\Delta S/S$ is constant at all parts of the scale. Much discussion has centred round which of these forms of $\varphi(S)$ is most in accordance with facts. It does not appear to have been noticed that the very possibility of a factual criterion being applied to discriminate between the two functions is inconsistent with either of them forming a true basis of measurement, for, as we have seen, a scale of measurement is independent of any facts other than those created by the necessary and sufficient conventions postulated in defining the required association between number and magnitude for the scale in question. If Fechner's second principle is to be accepted it is immaterial what form is given to the arbitrary function $\varphi(S)$, and Fechner was quite justified in adopting the simplest one.

Now we note that Fechner aimed at measuring sensation intensity as an A magnitude in terms of units of its own kind: his two principles imply both a criterion of addition and a criterion of equality. In Fechner's own form of the second principle the criterion of equality is stated explicitly, but in the general case it is implicitly defined by the form postulated for $\varphi(S)$ taken in conjunction with the criterion of addition.

We see at once, without examining either of these criteria in detail, that something must be wrong somewhere. The scale purports to measure an A magnitude, yet its defining relations involve measurable quantities of another magnitude—stimulus intensity. We have already seen that the relations defining the scale of an A magnitude must be independent of any quantitative relation (other than equality) for other magnitudes. Fechner's principles do not lead to a scale of this kind for sensation, and so do not measure sensation as an A magnitude. Nor do they measure it as a B magnitude. The only way to treat sensation as a B magnitude is to *define* S by a postulated relation to I. We shall return to this later ; at present we are only concerned to note that Fechner's principles do not do this. They introduce criteria of equality and of addition of sensation magnitudes, forming the basis of some association of sensation with number independent of the association established for stimulus magnitudes.

The S obtained by Fechner's principles is therefore neither an A magnitude nor a B magnitude, but has some of the properties of both, which means it has not the necessary and sufficient properties of either. Fechner's principles do not therefore enable us to measure any magnitude. It may be useful to examine in more detail why this is so. First consider the criterion of equality as applied to some pair of ΔSs, say ΔS_1 and ΔS_2. ΔS_1 is the sensation increment associated with a j.n.d. at intensity I_1, while ΔS_2 is the sensation increment associated with a j.n.d. at some other intensity I_2. These two statements taken together form the only specified relation between ΔS_1 and ΔS_2. The relation is not symmetrical : it ceases to be true if ΔS_1 and ΔS_2 are interchanged. It is therefore not a relation of the kind necessary for providing the practical criterion of equality in a system of measurement. This one consideration alone renders superfluous all the semi-metaphysical arguments which have centred round the question whether or not equal, in the sense of equally noticeable, necessarily means ' really ' equal. A symmetrical transitive relation is essential as a practical criterion of equality in measurement.

Further, the proposed criterion of equality, being defined in terms of j.n.ds., has no meaning when applied to quantities of sensation other than those associated with j.n.ds. Thus it is quite meaningless to say that $S = \Delta S_1 + \Delta S_2 + \ldots \Delta S_n$ by Fechner's criterion of equality. We cannot apply the same practical criterion to the comparison of S with $\Delta S_1 + \Delta S_2 + \ldots \Delta S_n$ as we use to establish the equality of the ΔSs. Fechner's definition of equality, in addition to its failure to fulfil the requirements of symmetry also fails to fulfil the requirement of applicability throughout the scale of magnitude. It is a gross logical error to use ' equal ' in one sense for liminal magnitudes and in some quite different sense for supraliminal magnitudes. In regard to sensation intensities we have the ordinary intuitive criterion of equality which we employ whenever we judge that of two stimuli neither is greater than the other. This is the only kind of equality which has any meaning at all sensation intensities, and as we must accept this criterion of equality for sensation intensities in the general field of sensory experience we cannot admit a different one in some special part of the field. We see therefore that Fechner's second principle, that we may arbitrarily postulate equality (or any other relation which implies an arbitrary definition of equality) for the ΔSs of a j.n.d. series is wrong. We may not do this because there is already, in our psychological constitution, a criterion of equality which we cannot ignore or modify. Fechner's postulate is not therefore a postulate but an *assumption* that the ΔSs are equal by this general criterion of equality. We have no right to make this assumption for there is no operation in the determination of a j.n.d. series which corresponds to the operation of judging equality of sensations in the ordinary way. In fact it is really meaningless to enquire if the ΔSs are equal on any scale applicable to sensation intensities, for the criterion of equality applicable to sensation intensities is not applicable to liminal increments at *different* intensities. Failure to realise this from the first is doubtless due to failure to realise that a practical operation for establishing equality is part of the *definition* of any magnitude. Equality of sensation intensities is established when we compare stimuli neither of which appears greater than the other. But ΔSs at different parts of the j.n.d. series cannot, from their nature, be experienced under the same degree of stimulation. The operation of establishing equality of sensation intensities is inapplicable, not merely in practice but in principle, to members of the ΔS series. From the point of view of measurement it is therefore impossible to regard the ΔSs as samples of the magnitude S. Thus Fechner's first principle, that we may regard S as the sum of a series of ΔSs, is invalid, and also, of course, his identification of the ratio $\Delta S/\Delta I$ with the differential coefficient of S with respect to I; for, of course, the small quantities constituting the numerator and denominator of a differential coefficient must differ in size only, and not in nature, from any other samples of the two variables. They must be measurable on the same scales as other and larger samples, which means that the phenomenal relation defining equality and the phenomenal operation defined as addition must be the same for the differentials as for all other samples of the variables. As this is not true of ΔS, it cannot be regarded as a small quantity of the magnitude S and $\Delta\ S/\Delta I$ has no relation to a differential coefficient.

This conclusion, derivable wholly from the principles of measurement, confirms those psychologists who have argued, from quite different premises, that we cannot regard a sensation S as analysable into a series of small quantities added together like the millimetres in a metre stick. A millimetre and a metre are samples of the same magnitude and differ only in size, but the ΔS of j.n.d. experiment and S are not: they differ in nature, as they cannot be defined by the same kind of relations.

We could go on much longer examining Fechner's principles in detail, but any one of the objections we have already discussed is sufficient to show that these principles cannot lead to the measurement of any magnitude at all, either of the A or B class.

Another type of psycho-physical experiment we must consider is that in which a series of three or more stimuli are graded by the method of 'mean gradations.' The principle may be illustrated by an example from vision. The observer is presented with a series of patches of light, all of the same colour, whose intensities are under his control, and is asked to adjust their brightnesses until they form a series so that the 'seeming disparity' between each one and the next in the series is the same. Brown (*The Essentials of Mental Measurement*, 1911, 2) calls this 'seeming disparity' a 'sense-distance.' When this experiment is performed a relation can be established between the grading so effected and the grading in terms of stimulus intensity (photometric units). The results of experiments of this kind for vision and other senses are usually interpreted as establishing a relation between a psychological magnitude—sense-distance—and stimulus intensities. The grading is supposed to consist of equal sense-distances, and the relation found between these equal sense-distances and the corresponding stimulus intervals is regarded by most psycho-physicists as providing a basis on which a quantitative relation between sensation intensity and stimulus intensity may be constructed. The underlying assumptions are that a sense-distance is the difference between the sensation intensities corresponding to its terminal stimuli, and that the difference between the sensation intensities corresponding to the lowest and highest of a series of unequal stimuli is the sum of the sense-distances between the adjacent pairs of the series. For this to be possible sense-distances must be samples of the same magnitude as sensation intensity. Let us examine the proposed criterion of equality. We define as equal the sense-distances between pairs of unequal stimuli which satisfy a certain subjective criterion. It is undesirable to attempt to define this criterion at the moment because the words we choose to define it tend to invest it with some one of a number of alternative interpretations. Suffice it to say that there is a condition which the observer attempts to satisfy in experiments of this kind. For our present purpose the nature of this condition is immaterial, the essential point is that the practical operation of producing equal samples of sense-distance necessarily involves at least three stimuli of *different* apparent intensities, whereas the operation of producing equal samples of sensation intensity involves only stimuli of the same apparent intensity. We see therefore that sense-distance, whatever it may be, is not the same kind of magnitude for the purposes of measurement as sensation intensity. The one cannot be expressed on any scale applicable to the other, and it is meaningless to regard them as quantities of the same measurable magnitude.

It may be objected that this applies with equal force to the difference of two samples of any magnitude. We cannot produce an example of equal differences of length, for instance, without at least three objects of unequal length while the operation of producing equal lengths involves only objects which appear equal by our criterion of equality for length. The analogy is illusory. Difference of lengths as something expressible on a quantitative scale derives its significance from the association of number and length established by the practical criteria of equality and addition which define length as a magnitude. It merely means the length which must be added to the smaller of two lengths in order to make a new length equal to the larger of the original pair. We cannot define a process of subtraction independently of a process of addition. We cannot construct a scale of length from units of difference-of-length defined by operations other than those involved in defining equality and addition for length. Similarly we cannot give any quantitative significance to difference-of-sensation-intensity unless we already have practical criteria both of equality and addition for sensation intensity; for all that difference-of-sensation-intensity means, if it means anything, is the sensation intensity which, when added to the smaller of two given sensation intensities, will produce a new intensity equal to the larger.

The mean-gradation series as a basis for determining a relation between sensation intensity and stimulus intensity has therefore the same defect as the j.n.d. series. The proposed criterion of equality is not the one applicable to sensation intensities. Thus if sense distance is a magnitude, it must be a different magnitude from sensation intensity.

Further, as with the j.n.d. series, the proposed criterion of equality is not a legitimate one for defining *any* magnitude. Each sense distance in the series is defined for practical purposes by a pair of dissimilar stimuli, a different pair being applicable in each case. No symmetrical transitive relation can be constructed from such material, therefore no criterion of equality appropriate to measurement can be formulated for sense-distances. So whatever sense-distance may be it is not a measurable magnitude! Thus if we examine by what right the word *equal* is applied to the phenomenal relations observed in this type of experiment or by what right we assumed that it is *differences* of sensation intensity which determine the grading, we find that there is no justification whatever in either case.

We have here an example of the mental suggestion produced by the repeated use of certain words in a loose and inaccurate manner. The instructions given to observers invariably beg the question of the nature of the operation to be performed. These instructions naturally vary, but they are certain to contain such phrases as 'bisect the interval,' or 'adjust . . . half-way between,' or 'adjust C to be as much brighter than B as B is brighter than A,' and so on. In all descriptions of the experiment which I have seen, the process is described in terms of this kind which have a definite significance only when applied to magnitudes which we already know how to measure. The observer is in fact instructed to perform a definite quantitative operation, *but is not told how to do it*. He does the only thing he can do; that is, adjust the stimuli until he detects some unique character in the relation exhibited by each pair. There is nothing in the operations he actually performs (adjustments of lamps or of sound-emitters, etc.) to tell us what kind of relation it is which he arrives at, and there are no *a priori* grounds for assuming that differences of sensation intensity, or equality, either of such differences or of any other magnitude, enter directly into that relation. Nevertheless the terms of the usual instructions, by their quantitative implications, suggest the idea of equality and difference as if the operation were exactly analogous to marking off a metre stick into a series of equal parts. If we are instructed to bisect a metre stick, or to arrange three points A, B and C, so that C is as much higher than B as B is higher than A, the instructions have a definite meaning in virtue of the phenomenal relations and operations defining length as a measurable magnitude; and we can obey the instructions by performing operations in accordance with these definitions; but if we had not already defined practical criteria for equality and addition of lengths there would be no operation corresponding to 'bisection,' or to the establishment of any other quantitative relation, for lengths. This is the position in the present experiments. Though we have a criterion of equality for sensation intensities we have no operation of addition, and until we have there is no meaning in associating any quantitative relation, equality or any other, with differences of sensation intensity.

It should be clear, therefore, that the description of this experiment should be couched in language which makes no assumptions about the nature of the relation which the observer is to establish, and that we should avoid all terms like *bisect*, *equal*-appearing, sense-*distance* or others which suggest, by their association with the properties of measurable magnitudes, an unjustifiable interpretation of the operations performed in the experiment.

Let us attempt to describe the experiment in terms of the operations performed.

We set up three [3] similar *objects* of the type which are perceptible by the sense—vision, hearing or whatever it may be—for which the experiment is to be made. *Object* is here used in the most general sense to denote anything external to ourselves which is perceptible in virtue of the fact that it is the origin or apparent origin of a stimulus affecting one of our senses. In the case of vision the objects may be lamps, or self-luminous surfaces, or surfaces seen by reflected light. It is necessary to emphasise the fact that the first requisite of the experiment is a set of perceptible objects. If we keep discussing stimuli without reference to their origin we may easily lose sight of the fact that all stimuli reach us from our environ-

[3] Three or more, but three is enough to illustrate the principles involved.

ment and that the normal function of perception is to provide information about that environment. So we set up three suitable objects to provide stimuli.

We must be able to adjust at least one of the objects with respect to the perceptible property in such a way that the stimulus we receive from it can be varied in magnitude without change of quality. In the visual case, to which I will confine the remainder of the description, we must be able to alter the brightness of the object without changing its colour. There are numerous practical methods of doing this. Let us denote the objects by A, B and C, and suppose that C is brighter than A, and that B is brighter than A but less bright than C. The pair of unequally bright objects A and B present us with a directly perceived phenomenal relation. So also does the pair B and C. We must not confuse these directly perceived phenomenal relations with the quantitative relations between the measured intensities of the stimuli. In order to know anything about the latter relations we have to measure the stimulus values by appropriate methods, but in order to perceive the phenomenal relations we have only to look at the objects. The perceivable phenomenal relations would still be just what they are if we had never formulated a scale of measurement for brightness as a photometric magnitude, in which case there would be no quantitative relations between the stimuli. So we cannot describe phenomenal relations, *as intuitively perceived*, in terms of quantitative concepts. They are simply those aspects of the objective world, as directly perceived, in virtue of which we differentiate objects and groupings of objects from each other. Returning to our experiment, we perceive the phenomenal relations between objects A and B and between B and C, which we will denote by $A.B$ and $B.C$, the notation conveying no implications about the nature of either relation. We also notice a relation *between these relations*. We find that as we vary the brightness of B between those of A and C not merely do the individual relations $A.B$ and $B.C$ change but so also does this cross relation. We are not required to describe (nor indeed are we able to describe) how it changes. To say that the perceived interval AB is greater, equal to, or less than the perceived interval BC is merely to use words before we know what they mean in the particular application. The relation changes in some respect which is directly apprehensible to us : that is all we can say. In general the relation is not one that we recognise as having any special significance, but as we go on adjusting the brightness of B to various values we find that there is one value, and only one, for which the relation we are now speaking of, the relation between the relations $A.B$ and $B.C$, is such that we *recognise* those relations as having *sameness* in some respect which has a unique significance in perceptual experience.

This is the criterion we have been trying to satisfy : the experiment is finished except for the measurement of the stimulus values, i.e. the actual brightnesses of A, B and C, by the appropriate photometric methods. The experimental psychologist may object that to look for a recognisable relation between undefined relations $A.B$ and $B.C$ is not what the observer is asked to do, and there is therefore no reason for supposing that this is what he does. It is the only thing he can do. The instructions usually given him, as already stated, are couched in terms of operations for which no meaning has been defined. Whether or not he thinks he understands them and believes himself to be carrying them out is of no consequence. He cannot in fact carry them out. All he really gathers from the instructions is that there is some unique perceptual criterion by which the stimuli may be arranged, and assumes when he has found such a criterion that it is the one the instructor means. Thus, although in many psycho-physical experiments the instructions are of vital importance because there are several different things the observer may do depending on his interpretation of them, in the present case the nature of the instructions has no influence on the experiment because there is only one thing the observer can do if he performs the experiment at all.

The foregoing is, in all essentials, an exact description of how experiments of this kind are carried out. The extension of the process to build up a series of stimulus intervals each of which is related to the next in order in the same way as $A.B$ is related to $B.C$ clearly implies no new principle. The application of equivalent processes to hearing or other senses only involves differences in practical details.

We observe that the experiment may be described without introducing the concepts of equality, difference, bisection, etc. The only concept required is that of ‘ recognisability ’ as something which confers on some relations a unique significance not possessed by other relations of the same general type. We also note that it has been unnecessary to make specific mention of sensation intensities. In our description sensations remain in the background, as they do in ordinary life, serving their normal purpose of making us aware of phenomenal relations among the objects in the perceptual field. Of course there must be psychological relations corresponding in some way to the observed phenomenal relations, but we pay no attention to these in performing the experiment ; we simply accept the information they afford us about the external objects. The mental attitude of the observer during these observations is the normal one of *perception*, not of apperception. He is observing the objects, not his own sensations : his criterion of the accomplishment of his task is not the deliberate identification of any unique relation among his sensations, but the recognition of a unique relation exhibited by the objects.

Recognition appears to me to be the key-word for the interpretation of this experiment. What does recognition mean ? We may confine attention to the intuitive recognition of *phenomena*, the kind of thing that happens when I know a friend as soon as I see him, or when I know my own house as soon as I come within sight of it, or know that a certain sound is the call of the cuckoo as soon as I hear it, or know tea by its taste, or lavender by its smell, and so on. What is my friend, what is my house, what is a cuckoo, or tea, or lavender ? Each of these is nothing more (at least as far as I am concerned) than permanent, or quasi-permanent, relation-structures perceivable against a background of ever-changing phenomenal relations. We look out (and hear out, feel out, taste out, and smell out) on a world consisting of a medley of directly perceivable phenomenal relations of a great variety of different kinds. Some of these relations keep changing and attract but fleeting attention. Others remain apparently unchanged for a long time, and these we group into relation structures which constitute enduring phenomena such as people, houses, cuckoos, tea and so on, the ‘ objects ’ or ‘ things ’ which endure. Now any such relation structure may contain many different types of relation. Not all of these involve magnitudes, but many of them do, and of these some will be extensive, like length, mass, brightness, etc. Of the relations involving such magnitudes, some will involve absolute extension while others will involve only relative extensions. Absolute is here used in the ordinary everyday sense. There is, of course, no criterion for absolute extension of any magnitude in the fundamental sense of absolute. Absolute extension in the ordinary sense is itself relative—relative to some universally applicable standard, such as the length of the standard metre, or the mass of the standard gram, or the brightness of a surface emitting a lumen per sq. cm., and so on, which is not necessarily a member of the group of phenomena we are perceiving at any given moment; whereas by relative extensions we mean relative not to an external fixed standard but to the other extensions of the same magnitude within the perceived group of phenomena. In a relation structure composed of lengths, those relations involving absolute extension determine what we vaguely call the *size* of the structure while those involving only relative extensions, in this case the ratios of the various lengths, determine what we call the *shape* of the structure. Of two such structures it may happen that the relations involving absolute lengths are different but that all the relations involving the ratios of lengths are identical. The two structures are of the same shape but of different sizes. The terms *size* and *shape* are so convenient for discriminating between properties depending on absolute extension and those depending on relative extension that it will be useful for our present discussion to extend them to apply to relation structures involving any extensive magnitude. We may use the term *relation-shape* to signify the sum total of those properties of a relation structure involving any extensive magnitude which depends only on the relative extensions of the samples of the magnitude comprising the structure, and *relation-size* to signify the sum total of those properties of the structure which depend on the absolute extensions. For instance, consider a simple relation structure involving two masses A and B of 20 and 30 gm. As this particular structure does not include the universal standard gram, the relations of the masses A and B to this standard are not part of the structure, so the masses of A and B are absolute extensions of the magnitude mass. Of course the symbols 20 gm. and 30 gm. used to denote these absolute masses are meaningless unless we determine the relation between each mass and the standard gram, but this is determined from the examination of other relation structures of which the standard gram is a part. So long as attention is confined to our present structure the masses are absolute. Relations exhibited by this structure include (1) $B - A = 10$ gm.; (2) $B + A = 50$ gm.; (3) $B/A = 1 \cdot 5$. If we have another structure involving masses C and D of, say, 40 and 60 gm., the corresponding relations are: (1) $D - C = 20$ gm.; (2) $D + C = 100$ gm.; (3) $D/C = 1 \cdot 5$. Relations (1) and (2) involve absolute masses; relation (3) only relative masses. These two structures have the same relation-shape but differ in relation-size. In the same way, two relation structures involving, say, brightnesses, may have the same relation-shape provided the ratios of the brightnesses comprising one structure are the same as the corresponding ratios in the other structure, but will differ in relation-size if the absolute brightnesses of the corresponding members of each structure are not equal.

Of the physical relations constituting a relation structure of the kind we call an object, the relation-sizes are properties of the body just as much as the relation-shapes; but they are not of equal importance for intuitive recognition. The complete relation structure of an object can never be perceived on any single occasion. It is a synthesis of many relations observed in different ways at different times, and usually involves several of our senses. We can never see a house; we can only see the visual relations exhibited by the particular bit of the outside or inside of it which comes within the field of view at one instant. In order to become acquainted with all the visible properties of a house we have to perform numerous acts of seeing at different times and in different conditions. Even then we have not apprehended the whole house: we have to feel all over it for such tactile relations as it may exhibit—hardness or softness of its various parts, roughness or smoothness, etc.—and perform all sorts of measurements on it to obtain the absolute extensions of the various extensive magnitudes which are involved in its relation structure. A relational construct built out of all this material is what we mean by ' the house.' From the moment when we first notice that some of these relations are repeated on different occasions of observation, we get the idea of a permanent physical entity firmly rooted in our minds ; and thereafter every sense impression which presents relations that may, without evident contradiction, be regarded as part of a self-consistent relation structure is linked in our minds, by the psychological process known as *association*, not merely with the other relations comprising the structure but also with this co-ordinating idea of a permanent physical entity—an object. Then, when at any time we perceive any reasonably important group of the relations for which such associative bonds have been established, it at once calls up the idea of the whole object, and we *recognise* the object as a house, or a cuckoo, or tea, or whatever it may happen to be.

Now the strength of an associative bond is reinforced by every repetition of an experience of the relations involved. It is only after many repetitions of similar experiences that the association becomes instantaneous and automatic as is the case with those associations by which we recognise the presence of a well-known object or well-known type of object. It is clear therefore that of all the relations pertaining to a relation structure, those will be most important for recognition which are most often observed, and will be those phenomenal relations of which our sense-impressions are least affected by the variable conditions in which our miscellaneous observations are made. Now if we consider the visual sense, it is at once obvious that those phenomenal relations involving absolute size will apparently vary every time we change our distance from the object. It appears bigger in the perceptual field from one distance than from another and those apparent sizes vary continuously as the distance changes and do not cluster round any norm. Any relation involving absolute size will be but rarely repeated in our miscellaneous observations of the object, and the associative bond will be too feeble for us to recognise the relation as part of the relation structure of the object. On the other hand, those phenomenal relations involving only the relative lengths of different parts of the object are apparently unchanged by distance, and the same relations will be observed every time we view the object from any given direction. Strong associative bonds between these relations and the idea of the permanent objects are established, and we regard the experiencing of any important group of them as ' seeing the object.'

In so far, therefore, as its distribution in the dimensions of space is concerned, an object is recognised by its apparent shape, not by its apparent size. Apart from possible differences in the relation structure of other magnitudes associated with the object, and which we are not considering at the moment, objects of the same apparent shape observed separately are absolutely indistinguishable from one another by direct perception whatever may be their sizes. A series of objects of the same shape, but in ascending order of size, seen in succession present merely a selection of the appearances which any one of them would present as we approach it from a distance. It is this which makes sameness of shape a uniquely important phenomenal relation for perception. If we are shown a large number of triangles of miscellaneous sizes and shapes, each is a relation structure of the same general character, involving the same number of samples of the same magnitude, length, arranged with respect to each other in the same manner, i.e. end to end and completely enclosing a space. In addition to perceiving the relation structure constituting each triangle we perceive various relations between these individual structures. Between any pair selected at random there will in general be an obvious difference both in size and shape. If we have a really large and miscellaneous selection to examine, we shall see between various pairs practically every possible kind of phenomenal relation that can exist between triangles. In general there will be nothing unique in any of these relations, nothing to which we can attach more importance in the case of one pair chosen at random than in the case of any other pair. There is only one relation which will strike us as unique : if we find that there are two or more triangles of the *same* shape we can pick them out at once as a group characterised by the unique relation of having a *recognisable* property common to all its members.

Sameness of relation-shape is a symmetrical and transitive relation between relations or relation structures, and it might seem that it is this property which makes it uniquely important to perception. I don't think there are grounds for this view. We do not in perceiving phenomenal relations analyse them into their logical classes. The unique significance we attach to a pair of triangles of the same shape when we pick them out from a medley of pairs exhibiting other relations between their shapes is instinctive. It does not depend on a conscious realisation that if (triangle) A is of the same shape as B, B is also of the same shape as A, and further that if B is the same shape as C then A is also of the same shape as C. It might be suggested that we do realise these facts almost simultaneously with the perception of the triangles, but even if this were so only a mathematician would also realise that the facts were of any unique importance as properties of a relation. I can see no reason why any phenomenal relation should have a unique significance for intuitive perception apart from association ; and if we find, as we actually do from experience, that some particular kind of relation has a unique significance it must be because it has become indelibly associated in our minds with some unique type of experience of outstanding frequency of occurrence. Broadly speaking, practically the whole of our perceptual experience consists of the observation of permanent objects and the ever-changing relations which these permanent objects, regarded as self-contained unchanging entities, may enter into with each other. The changing relations between permanent objects are ephemeral ; the relations between the moving motor-car and the mile posts are different every time we look : but the relation structures constituting any given aspect of the permanent objects, the motor-cars and mile posts themselves, do not change, and are perceived every time we observe the same aspect of the objects, whatever the circumstances of observation. Any particular relation between different objects is therefore observed very rarely as compared with the relations which characterise objects themselves, and which, for a given object, are the same at all times. This is why ' sameness ' of relation structure is perceived far more frequently than any other relation between relations. *It is the relation between the perceived relation structures of any object on different occasions.*

It may be objected that when we identify the same object on different occasions we are not really concerned with a relation between separate relation structures observed on these occasions, but are merely perceiving the one identical structure every time. This, however, is to confuse the relation structure which constitutes any phenomenon with the abstract relations involved in it. A phenomenal relation structure is not a structure of abstract relations, but of *instances* of relations. For example, suppose we arrange three billiard balls so that each ball is distant ten feet from each of the others and that we also arrange three other billiard balls in the same way. We have here two phenomenal relation structures in which all the relations, including those involving absolute length, are the same. But the two relation structures are not identical ; they are separate *instances* of the same relations. If we regard three balls arranged as above as a phenomenal unit—a ' thing '—the two relation structures are not the same thing but two different things, each with its own identity, even if there is no possible test by which we can distinguish one from the other, as there will not be if all the relations in the two structures are identical. In describing phenomena as relation structures we do not mean a conglomeration of abstract relations, but a grouping of actual instances of relations. Some modern philosophers are apt to forget this and regard the universe as built up of abstract mathematical material. But we cannot build reality with abstractions : there must be ' things ' to relate before we can have an example of any relation. It is true that these ' things ' are themselves resolvable into relation structures involving relations between more elementary ' things,' and so on indefinitely, but we never reach a stage at which we find relations without ' things ' to relate. This is not because methods of physical analysis do not go far enough. If our present ultimate things—electrons, positrons, etc.—were split into a million other things, and each of these into a million others, we should still be in the same position. Any actual instance of a relation must have things in it to be related. All the relations in the relation structures constituting two billiard balls may be identical, but the relation structures are not identical ; the ' things ' in them are different : it requires twice as many of them to make two balls as to make one. Therefore when we say that two objects have the same relation structure we do not imply identity. This is clear enough when we are considering two or more objects like billiard balls whose non-identity can be proved by their simultaneous existence in different places. It is not so obvious in the case of the relation structures of which the same object is composed at different times. Here the elementary ' things ' which enter into the various relations in the structure are the same things on all occasions, or at any rate we have no way of knowing that they are not. But we have no way of knowing that they are. Our idea of a permanent object is based on the inter-consistency of an enormous number of acts of perception involving different aspects of the object and different relations to other objects. In any single act of observation only a small part of this material is presented to us, and its perception is a self-contained experience. We perceive a phenomenal relation structure, that is all. On a subsequent occasion we also perceive a phenomenal relation structure. How do we correlate these experiences ? Neither of them in itself contains anything to indicate that it is a repetition of the ' same ' experience. As far as immediate perception is concerned we must regard the relation structures perceived on separate occasions as separate structures, and we can only correlate them by means of cross relations between these structures. The correlation of successive appearances of the same object therefore depends on the same principle as the correlation of the appearances of two or more objects simultaneously observed. In the latter case the relation structures are separated in the dimension of space and in the former in the dimension of time, but for the comparison of perceived relations this difference in the dimension of separation is of no importance. Perceptions separated in time are comparable in consequence of *memory* which reproduces the relation structure of a past observation for comparison with the relation structure of a present one. The smaller the gap in time between the observations, the more accurately is the memorised structure reproduced. As a matter of fact the comparison of the relation structures of simultaneously existing objects also depends, at least to a considerable extent, on memory. We cannot simultaneously apprehend all the material in a complex perceptual field. This is well known to workers in experimental psychology. Our attention fluctuates rapidly to and fro over the field and the composite impression of which we are conscious is really a blend of memories of features perceived at slightly different instants. Thus the difference between what we ordinarily regard as simultaneous perception of phenomenal relations and successive perceptions is only one of degree. The *recognition* of the successive appearances of an aspect of any permanent object does not depend on any mysterious power of *identifying* one relation structure on different occasions, but is simply an instance of the relation we call *sameness* between relation structures which are separate in perception but are brought together, by memory, for simultaneous comparison. We do not give the relation this name because it implies mathematical similarity or for any reason of that kind. It exists among phenomena as directly perceived, and implies no knowledge of quantitative relations which may or may not be establishable by the indirect processes of physical measurement. It is only in relation structures comprised of measurable magnitudes that mathematical relations have any meaning, and when our perceptual criterion of sameness is applied to relation structures of this kind we have to find empirically the mathematical relation to which it corresponds. We call our relation *sameness* simply because its unique importance in perception is derived from its continual presentation to us in association with the idea of 'same' objects; but it will, for the reasons already discussed, retain its unique significance whenever we encounter it, whether in successive appearances of the same object or in the successive or simultaneous appearances of different objects. The appearances of two or more objects which happen to exhibit this unique relation between their perceived relation structures will be recognised as the 'same' appearances just as if they were in fact successive appearances of some one object.

An important conclusion follows from these considerations which has a bearing on many problems of sensory experience. This is that any phenomenal relations that may be of special significance in cases of simultaneous perception will also be of special significance when the phenomena are separately observed, and vice versa.

We have already seen that in phenomenal relation structures involving lengths, owing to the varied conditions in which our everyday experience is obtained, the only relations in the structure which are significant for recognition of the object are those which determine the relation-shape of the perceived structure, while those which determine the relation-size play no part in recognition; and that for such structures the recognitive relation of sameness involves only relation-shape.

It is easy to see that this must also be the case for structures involving other perceptible extensions. The apparent brightnesses of the various parts of recognisable permanent objects depend on circumstances of observation just as much as their apparent sizes. The illumination by which objects are seen varies over an enormous range, and their perceived brightnesses vary accordingly. Obviously no relations involving absolute brightness will be recognised as uniquely associated with any object. Similarly with sound, any sound pattern, such as a musical chord or a piece of music which we recognise as the 'same thing' when we hear it on successive occasions may be heard on different occasions at different distances from the source. Its apparent loudness will vary enormously in our experience of it, and there will be no particular loudness in any way uniquely identified with the pattern.

It is needless to discuss other senses; in general the varying conditions of perception, coupled also with the variations which take place in our sensory sensitivities, make it impossible to form an associative bond between any perceptible extensive property and the 'thing' which possesses it. In all cases our perceptual criterion of sameness must involve only the relation-shape of the perceived relation structure and not its relation-size.

This much is obvious; but the psycho-physicist is concerned with establishing relations between our sensory criteria and the measurable physical properties of the world, and we must enquire what sameness of perceived relation structure implies with regard to the corresponding physical relations established by processes of measurement. The arbitrary criteria for associating phenomenal relations with numerial relations which, as we have seen, are the basis of any metrical scale, establish a correlation between phenomena and number of such a kind that whenever a given numerical relation between measured magnitudes is repeated, the corresponding phenomenal relation will also be repeated. It follows that if the relation shape of the relation structure comprising the metrical relations is repeated so also will the relation shape of the phenomenal relation structure, and vice versa. Therefore, since the recognitive relation of sameness involves only the relation shape of the phenomenal structure, the measurable property which is recognised must involve only the relation-shape of the metrical structure, in other words the *relative* quantities of any measurable magnitudes associated with objects. When we observe a miscellaneous collection of objects of the same type, those which appear to be characterised by the same ratios of the measurable magnitudes will be instinctively classed together in virtue of our perceptually unique relation of recognisability.

Returning now to our experiment on 'mean-gradation' we see that the criterion is nothing more than the recognition of the sameness of the relation structures consisting respectively of the bright objects A and B and the bright objects B and C, and that owing to the way in which the relation of sameness has come to acquire its unique importance (by association with appearances of 'same' objects of constant physical properties under varying conditions of perception) this perceptual criterion should only be satisfied if the measured brightnesses of A and B and of B and C are in the same ratio. Our example, and most of the discussion, has been in terms of visible phenomena, but the conclusions are applicable to all experiments of this kind. The grading of a series of stimuli in such experiments should result in steps such that the ratio of each stimulus to the next in order is constant. The measurement does not depend on equality of any sensory magnitude describable either as difference in sensation intensity, or as intensity difference regarded as a magnitude by itself. The concept of sense distances, assumed to be equal for the steps of such a series, is entirely illusory. Nothing is involved but the recognition of a relation *between the relations* which relate the physical intensities of adjacent pairs of stimuli, a relation which does not imply equality of any magnitude, either objective or subjective, but owes its special significance to association with the perceptible aspects of the various kinds of permanent objects or things in terms of which we are accustomed to interpret our experience. Our perceptions will always result in our assigning properties to objective things, and, as we have seen, should result in our grading these things, as far as extensions are concerned, in terms of relative magnitudes.

Magnitudes observed on separate occasions will be graded by perception in the same way as when observed simultaneously, because the only basis of comparison in such cases is memory, and the same relation determines the comparison as in simultaneous observation. The position we assign to

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any stimulus, experienced by itself, against the background of memorised experiences of other stimuli of the same kind will be the same as we would assign to it in a series of stimuli experienced simultaneously. If we mistakenly estimate sensation intensities on the unjustifiable assumption that the sense distances in a so-called mean-gradation series are equal, we are led to the mistaken conclusion that sensation intensity tends on the whole to increase by equal amounts for equal increments of the ratio of stimulus intensity (or that S varies as log I); then when we experience some stimulus by itself, as when we observe a single bright object or hear a single sound, we tend to estimate the sensation intensity on the same false (or rather meaningless) scale by comparison with memories of previous experiences of the sensation intensities corresponding to known stimulus intensities. Actually we are not comparing sensation intensities at all, but are attempting to place the stimulus in a series of its memorised predecessors, graded in terms of the recognitive relation of 'sameness' of relative intensity ratios.

It cannot be over-emphasised, because it is so consistently overlooked, that when we perceive a light or a sound or any other perceptible thing our resulting impression, and the judgment we base on it is not of our sensations but of those features of the environment from which we receive the stimulus. That is the normal function of perception: our judgment is about the objective intensity of the stimulus, and if we say of the members of a group of stimuli graded by the mean-gradation criterion that the intensities 'appear' to differ by equal amounts, when in fact they do not differ by equal amounts, we are merely asserting that our senses are misleading us and providing wrong information about the phenomena observed. But our senses do not grossly mislead us (except in unfamiliar circumstances). In general they inform us reasonably correctly of the relations exhibited by the phenomena: that is what they have been evolved for. It is only if we misinterpret these relations that we are led to false conclusions.

So, when we are trying to estimate the loudness of a sound, for example, it is not the subjective intensity of our sensation we are estimating on any scale, true or false, but the objective intensity of the sound. Whatever the actual relation may be between sound intensity and sensation intensity it merely serves the purpose of leading to an intuitive judgment about the objective sound, and if we so interpret this judgment as to grade the sound on any scale of magnitude but the right one—the scale of stimulus intensities—we are not discovering any fact about sensation intensities, but are simply making a mistake about the objective intensities. Thus the prevalent idea that the sensations of brightness, loudness, etc., vary approximately as the logarithm of the stimulus intensity is devoid of any basis and is neither proved by, nor even suggested by, mean-gradation experiments.

All these experiments can tell us is whether, on the whole, the operation of our sensory system is such as to provide us with reasonably accurate information about the relation structure of the objective world. If any single act of perception provided us with absolutely accurate information about the relations perceived, all mean-gradation series would consist of intensities each in the same ratio to the next for the reasons we have discussed. But we would scarcely expect any act of perception to provide absolutely accurate information about objective relation structure. As we have already remarked, what we mean by the true objective relation structure is a synthesis of an enormous number of relations observed in different ways at different times. For any individual act of perception to provide information absolutely consistent with this synthetic whole would require a uniqueness of relation between stimulus and response only obtainable from a mechanism of absolutely constant properties, deliberately designed and constructed for the purpose. We do not look for such machine-like behaviour from sensory mechanisms, made of living material necessarily influenced by changes in our bodily conditions, and not *designed*, in the usual sense of the term, but simply developed by the gradual processes of evolution to become more and more serviceable to us. Evolution is a gradual process. Our sensory mechanisms are doubtless much better suited for providing us with the kind of information we want from them than were those of our early forefathers, but are just as surely less well suited for that purpose than they might be, and presumably may be some day.

Although, as I have tried to emphasise, we do not consciously judge our sensation intensities in an act of perception but judge the objective intensities, the sensation intensities are nevertheless the only basis of the judgment; and there must be a relation-structure in the psychological content of the sense impression corresponding to the relation structure of the phenomena perceived. We cannot expect from the nature of our sensory machinery that exactly similar psychological relation structures will be produced by the same phenomenal relation structure on every occasion and in all conditions of observation. Repeated experience of the observation of any permanent phenomenal relation structure will, however, establish a *norm* among the various psychological structures produced by it at different times, and this *norm* will be the psychological structure which produces in us the reaction which we interpret as perceiving the objective structure. Now if on any individual occasion this same objective structure produces a psychological structure different from the norm we shall 'perceive' not the actual phenomenal structure, but another one for which the actual psychological structure is itself the associated norm. In other words our judgment of the external objects will be in error. Our judgments would, in fact, very frequently be in error from this cause if they depended entirely on the immediate sense impression; but in the ordinary observation of the world our mistaken impressions are corrected by another effect of association, known to psychologists as *regression towards the phenomenon*. When we receive a sense impression from a familiar object (or from one which we assume, correctly or otherwise, to be a familiar object) which is not quite consistent with the phenomenal structure that we normally associate with the object, associative reflexes originating in accumulated experience of the relation structure which *ought* to be perceived, come into operation and correct our impression either to what it *ought* to be or to something much nearer this than it would be in the absence of such associative reflexes. We are not concerned here with either the physiological or psychological machinery of phenomenal regression, but merely have to note that it is one of the most important agencies in preserving the correspondence between our judgments of phenomenal relation structures and the structures themselves despite a somewhat imperfect correspondence between our immediate sensory reactions and the objective conditions which evoke them.

Now it is evident that the effectiveness of this corrective agency—phenomenal regression—since it depends on correlative associations, will be most strongly developed for the observation of familiar phenomenal groupings encountered regularly in everyday life. Further, it cannot work miracles. Even a familiar phenomenal grouping, if observed under conditions for which the immediate sensory impression differs too much from the norm associated with it, will be misjudged and will seem different from what it really is.

From its nature, phenomenal regression will be inoperative in circumstances which do not call up the correlative associations on which it depends.

This is the case in the majority of experiments of the kind we are considering in which artificial groupings of a few bright objects isolated against a dark background, or a few isolated sounds produced in unfamiliar ways, are perceived. There is nothing in such systems to evoke the correlative associations involved in phenomenal regression, and judgment will depend almost entirely on the immediate sensory reaction. The psychological relation structure due to the direct effect of the stimuli on the sense organ, if for any reason it does not happen to be the norm associated with the particular phenomenal relation exhibited, will not be 'corrected' towards this norm: we shall simply judge, wrongly, that we perceive the phenomenal relation for which the impression we receive is the associated norm. Consequently if all the relevant properties of the sensory system are not constant throughout the range of intensity covered by the series of stimuli we shall not judge the stimulus relations correctly. Our criterion will still, however, be the perception of sameness of relation shape in the phenomenal relation structures consisting of adjacent pairs of stimuli, but our judgment when this is achieved will be in error.

The extent, therefore, to which any experiments of this kind fail to grade stimuli in a geometrical progression is simply an indication of the extent to which the sense organ under investigation fails to tell us the truth. The particular causes of this failure in any particular circumstances are the business of the physiologist and possibly, also, the psychologist; but neither compliance with the law of geometrical progression, nor the departures from it which may be observed, can lead to the discovery of any quantitative relation between sensation intensities and stimulus intensities.

The 'apparent' phenomenal structure will always be that for which the immediate psychological structure of any instance of perception has been related as a norm by the associative bond of integrated experience. In particular, any two phenomenal structures will be 'perceived' to have the unique relation of sameness, whether they really have or not, if in a given act of perception the psychological relation structures which they evoke are related to each other by the psychological relation which is the norm associated with sameness of objective relation structure. Thus, any difference which may ever be manifested between the 'apparent' relations of phenomena and the 'true' relations, do not involve any law of variation of sensation intensity with stimulus intensities, but only arise from the fact that from some cause, either adventitious or systematic, the phenomenal relation structure under observation is not producing the normal psychological relation structure which is associated with these phenomenal relations by the totality of our experience. The reason for it not doing so in any particular case may be physiological or psychological or both. We cannot expect any physiological mechanisms, such as those involved in our receptor organs and neural systems, to exhibit constant properties at all intensities of stimulation. We do know, however, as an empirical fact, that over the range of conditions typical of the bulk of our ordinary experience, our individual perceptions give us a fairly faithful account of phenomenal relations; so within this range the differences between apparent and true phenomenal relations cannot be great. We should, however, expect to find more important departures at intensities lower or higher than those for which the bulk of our associative experiences are obtained. We should therefore expect in mean-gradation experiments to find stimuli to be graded by perception in the ratio of their intensities, or nearly so, within the range of ordinary comfortable perception, but to exhibit departures from this relation at high and low intensities.

This is, in general, what is found when the experiments are carried out.

As the phenomenal relation involved in the grading is sameness of the relation between adjacent members of the series, it is independent of the size of the intervals, and the same criterion should be operative even if the intervals are made so small that we are just able to distinguish between the members of adjacent pairs. If the intervals are less than this, the perception of pairs, and therefore of any relation at all between pairs, is impossible. Thus so long as the members of a mean-gradation series are distinguishable from each other they are related by our criterion of sameness of interval. But we have now reached what is, in effect, a j.n.d. series, from which we see that the successive stimuli in such a series must be graded in intensity by the same law as the members of a mean-gradation series, i.e. in accordance with the apparent ratio of their intensities. Apart from adventitious causes due to difference in the experimental conditions in which the two types of experiment are carried out, the departures from a true grading in terms of objective stimulus ratios should depend on the same physiological (or other) systematic causes, and should be of a similar character.

The results of actual experiments of these two types are, in general, as predicted by the foregoing considerations. In the case of vision, both equal-appearing-interval series and j.n.d. series grade stimuli very nearly correctly in accordance with relative intensity over the very large range of intensity associated with normally comfortable seeing, departures from the true grading only becoming serious at low and excessive intensities. The results for sound are less definite, as a perusal of the section of this Report prepared by Dr. Semeonoff will show. This is also to be expected. Owing to the very indefinite clue to direction given by our auditory apparatus, we do not hear a ' picture ' of our environment in which the constituent sound waves reaching us are definitely associated with the sources or reflecting objects from which they come. Hearing is rather like seeing in a thick mist, in which we may perceive the general direction from which light is coming but see no objects. With sound therefore we are usually unaware of the exact origin of the stimulus and feel that we are simply immersed in a nebulous ' cloud ' of sound surrounding our heads. But this nebulous cloud is not the sensation ; it is the objective environment which, in virtue of the sensation it evokes, we ' hear,' just as when immersed in a translucent mist the nebulous cloud of luminescence we see is not our sensation, but constitutes the objective environment of whose presence we are made aware by our sensations. What we hear is outside us in exactly the same sense that what we see is outside us, a point that seems to be entirely overlooked by many writers on audition. However, owing to the nebulous nature of the objective world, as perceivable by hearing, it is relatively rare for us to make a definite association between any sound pattern and a unique source. It is only on the occasions when we know, for other reasons, that some object is the origin of the sound, as when we simultaneously see and hear a person speaking or an orchestra playing, and so on, that we make such an association at all. The associative bonds between any psychological relation and a corresponding phenomenal relation are likely, on this account, to be much less strongly developed for audition than for vision, where the bonds are reinforced by almost every experience. Nevertheless, such uniqueness for recognitive purposes as any auditory relation can have must be derived from that fraction of our experience in which associations are established ; and must, for the reasons we have discussed, correspond to sameness of relation-shape depending in audition, as in vision, on the apparent relative intensities of stimuli and not on their absolute magnitudes, and must tend, as in vision, to approximate to a recognition of the true relative intensities within a reasonably wide range of intensity.

The data on sound, despite wide differences in the results obtained by different experimenters using different experimental devices, definitely tend to show that stimulus gradings by the j.n.d. and mean-gradation methods are very similar and that in both cases the grading places stimuli approximately in a geometrical progression over a large range of intensity, the two results which we have predicted.

The results depend solely on the unique recognitive significance of sameness of relation-shape of apparent relation structures and provide no information whatever about quantitative relations of sensation intensities. All we are able to say about the correspondence which must exist between psychological relation structures and the phenomenal relation structures to which they are linked by association is that the psychological relation between psychological relation structures in virtue of which we are aware that the relation of sameness of relation-shape exists between phenomenal relation structures must, like the phenomenal relation itself, be symmetrical and transitive and so cannot involve absolute extensions of any psychological magnitude : also, that sensation intensities must increase with increase of stimulus intensity. But we cannot deduce the law of variation. The association of sensation intensity and stimulus intensity may be of an elastic kind, as in fact we know it to be from the phenomena of adaptation.

Why do we assume that there must be a quantitative relation between stimulus and sensation ? Quantitative relations only hold for relation structures composed of measureable magnitudes. Our familiarity with the multitudinous quantitative relations established by the methods of physics, and by the cruder but equivalent methods we employ in estimating measurable magnitudes in everyday life, induces the feeling that every relation between things for which the relations greater or less are significant must be a quantitative relation expressible by its numerical equivalent. This feeling has apparently led to the universal conviction that sensation intensity, to which the terms greater or less are obviously relevant, has an inherent association with number only awaiting discovery. It is assumed that between two sensation intensities S_1 and S_2 there is a 'true' relation, $S_1/S_2 = n$, where n is some number, and that the problem we are up against is to find some way of determining n in any given case, or of deducing it indirectly from the result of some experiment which depends on it. As I see it this is not the position. There is *no* relation $S_1/S_2 = n$ until we have defined S as a measurable magnitude by a practical criterion of equality and a practical operation of addition. Unless this is done—and no one argues that it can be done—there is no basis of association between members of the class of sensation intensities and members of the class of numbers, and no meaning in a numerical relation between sensation intensities. Equality of these intensities presents no difficulty, but no operation analogous to addition is possible. Every psychologist agrees that this is so ; but it is not realised that without it we are not merely unable to discover quantitative laws involving sensation intensity, but that there are not in fact any quantitative laws to discover.

The theory here advanced to explain the significance of the unique relation which determines the grading of stimuli by the mean-gradation method as derived from associative experience, is put forward solely on the grounds that when one is endeavouring to destroy the foundations of any firmly rooted belief it is desirable, where possible, to lay the foundations of a new one to take its place, and not to confine oneself to purely destructive criticism. The mistake must not, however, be made of regarding the alternative explanation as an integral part of the case against the old one. Whether the explanation here given proves to be acceptable or not, the case against the prevalent interpretation of these experiments is complete. It depends simply on the fact, demonstrated in the beginning, that they do not provide any practical criteria for associating any sensory magnitude with number in the particular manner which is essential for measurement, so no relation between sensory magnitudes and stimulus magnitudes can possibly be derived from them. If the explanation I have suggested should prove wholly, or in any important aspect, unacceptable, we are simply left for the time being with no explanation of the experiments. The old one will not do ; and the sooner writers on psycho-physical problems stop describing and interpreting their experiments in terms of sense distances and sensation scales the sooner will it be possible to seek a true interpretation of their results freed from the tendentious influence of a falsely suggestive terminology.

The foregoing general considerations cover all the other types of experiment in which the observer associates numerals with perceived stimuli composed in different ways, such for example as the well-known experiments of Dr. L. F. Richardson and his colleagues.

This Report is already too long to permit discussion of such methods in detail, but it can be said of all of them that whatever may be the criterion by which the observer assigns a number to an observed stimulus relation, or makes some equivalent decision in connection with it, such as marking a point on a line to correspond to the ' position ' of a percept in a series ranging between two extremes, it cannot consist of the intuitive perception of some quantitative relation between psychological magnitudes, for there are none to perceive. In all these experiments, as in those we have considered more fully, the guess, estimate, or judgment, whichever it is, relates to stimulus magnitudes and not to sensations. It may be a guess or estimate based on direct association with known cases of the same type of stimulus relations. This is a process we perform almost every day when we estimate lengths, weights, temperatures, etc., without measuring them. It depends merely on direct association, and in the case of those things with which we are very familiar may often be effected with considerable accuracy. Those experiments which are not explainable on the basis of direct association must have for their criterion the perception of sameness of relation shape between some elements of two or more relation structures.

The foregoing discussion centres round the possibility of measuring sensation intensity as an *A* magnitude—that is, as something expressible in terms of units of its own kind. A few words must now be devoted to the possibility of treating sensation intensity as a *B* magnitude, defined by an arbitrary relation to stimulus intensities. In the first place, assuming it can be done, it would serve no purpose whatever. It would merely result in our being able to say that the intensity of sensation corresponding to the stimulus *I* is the intensity of sensation corresponding to the stimulus *I*, which would not help us much in any psycho-physical problem. The utility of a temperature scale, which, as we saw earlier, is defined by an arbitrary relation to the properties of a standard thermometer, is that the thermometer can be used to measure the temperatures of other bodies. It would clearly be useless to define the temperature of a resistance thermometer as a function of its resistance, as we do, if it could only be used to measure its own temperature. All the definition would mean is that the temperature of the thermometer when its resistance is *R* ohms is the temperature corresponding to a resistance of *R* ohms. This would be the position as regards sensation intensity. We could not use the sensation scale established by the definition in one standard sensorium to measure the sensations in other people's sensoriums, because our criterion of equality for sensations is private. We cannot establish sensory equilibrium between people as we can establish thermal equilibrium between bodies. Each person would be a sensationmeter only capable of measuring its own sensation, like a thermometer only capable of measuring its own temperature. It could provide no information about anything.

However, we cannot even have whatever satisfaction there might be in establishing this perfectly useless sensation scale. As we saw when considering temperature, in order that a magnitude may be defined in this way it must be possible to postulate a one-one relation between the magnitude to be defined and the measurable magnitude to which it is to be related by the defining relation ; and that we are not free to make any such postulate unless, from the nature of the case, it is certain on *a priori* grounds that the defined magnitude can never enter experience except as ' the thing defined by the adopted relation.'

There is no one-one correspondence between sensation intensity and stimulus intensity. Sensation intensity enters experience directly, in its own right so to speak, and we know that, owing to adaptation, fatigue, or various other causes, the same stimulus may evoke sensations of markedly different intensity on different occasions. So we cannot define a scale of sensation intensity as a postulated function of stimulus intensity even were it of the slightest use to do so. The analogy with temperature advanced by some psychologists is entirely fallacious.

But what about the average sensation intensity corresponding to a stimulus I ? May not this be treated as a B magnitude ? Over how long a period are we to take this average ? Obviously if the whole of an observer's life is to be included there will be a one-one correspondence between mean S and I, and we are free to postulate any law we like to define a relation between them. But what have we achieved ? We cannot use this scale to measure the individual S corresponding to I on any particular occasion. It gives us no information other than that with which we started, that the mean sensation intensity corresponding to any intensity of stimulus is just whatever we have chosen to say it is.

We must conclude therefore that sensation intensity is not measurable either as an A magnitude or as a B magnitude. It is not measurable in any sense of the term.

IV. *Notes on Mr. Guild's statement by members of the Committee.*

A. *By Dr. R. H. Thouless.*

(1) This account of what is meant by ' measurement ' is excellently clear. I think ' measurement ' is primarily the physicist's term and I am willing to accept what they say as to what the word means, and I do not think it in any way restricts the possibility of quantitative experiment in psychology if it is agreed that it is not ' measurement of sensation.'

(2) The account of Fechner seems to make his account of the matter much more clear and rational than it really was. Thus with reference to ' Fechner's second principle ' (p. 309), I cannot find that Fechner formulated any such principle in the *Elemente*. He assumed it, according to my reading, without realising what he assumed, by stating Weber's Law in the form: $d\gamma = K \, . \, d\beta/\beta$.

(3) It is obvious that Fechner thought he could establish measurement of sensation in a sense which is indefensible. I am not sure that Mr. Guild disposes of a possible defence that what is really possible is a B measurement of sensation (for a single individual, under specified conditions of stimulation,

with a single kind of stimulus) using equality of just noticeable differences as a convention of measurement parallel to the use of equality of temperature differences causing equal volume changes of thermometry. Anyway it is obvious that the restrictions are such that measurement in this sense would be of little practical scientific value.

(4) I am not convinced by the argument of pp. 317 ff. Surely apparent shapes change with different angles of vision as do apparent sizes at different distances.

(5) I think Mr. Guild would agree that most of the quantitative experiments psychologists do when they attempt to ‘ measure sensation ’ could be done equally well and equally meaningfully if they gave up that assumption. He does say this, but I should like to see it emphasised. It considerably narrows the apparent difference between his view and the opposing. For example : ‘ Thus the prevalent idea . . .’ (p. 322) seems to me to exaggerate this difference. I should like to see added that if Mr. Guild's view is accepted and the mean-gradation experiment is merely a report of sameness of relations, it is nevertheless a real question whether if one arranged, let us say, a series of electric lamps so that each successive pair had the same brightness relation between them, whether this series would be a geometrical or arithmetical series of physical intensities. The phrase ‘ is devoid of any basis ’ seems to suggest that there is no real problem, whereas I think Mr. Guild would agree that there is a real problem but that it should be stated in other terms. 23.6.38

B. *By Dr. L. F. Richardson.*

Mr. Guild has made a logical analysis of the relation between sensation and stimulus. Those whose chief reliance is on logic, take risks of passing over assumptions without noticing them. Mr. Guild's analysis may be summarised in three steps :—

Step I. Is sensation an A-magnitude ? No !
Step II. Is sensation a B-magnitude ? No !
Step III. Therefore sensation is not a magnitude of any sort.

The tacit assumption is that the A and B magnitudes are the only kinds of magnitudes that can exist. But there is abundant experimental evidence (*vide* Dr. Semeonoff's report) from several independent investigators in England and America that intuited magnitudes exist. Of course A-magnitudes are usually the most reliable ; and A and B magnitudes, taken together, are the only sorts of magnitude which are respectable in practical physics, except for the estimation of tenths of small divisions. It is also evident that intuited magnitudes are subject to variations with the occasion and with the observer ; variations so large that they would not be tolerated in practical physics. But if, as Mr. Guild avers, A and B magnitudes are not available for sensation, then intuited magnitudes are not to be despised. The progress of psychology towards the status of a quantitative science is more likely to be advanced by experimental exploration of the relations of intuited magnitudes than by refusal to allow them to be considered.

Some people wish to see the word ‘ measurement ’ restricted to mean the determination of A and B magnitudes only. If that were done it would be necessary to point out that a mere terminological convention must not be allowed to prejudice discussions about the existence of magnitudes other than those called A and B.

There is a remark, independent of the foregoing considerations, to be made about Mr. Guild's Step I. He avers (on p. 310) that the method of equal appearing intervals does not yield an *A* magnitude because the relation of ' appearing equal ' is not symmetrical ; because the sensations are tied to stimuli which cannot be interchanged. But let us suppose that the experiment is conducted, in the customary manner, by two persons one of whom, called the experimenter, alone knows the stimulus values, while the other person, called the observer, alone judges equality of appearance. In these circumstances the relation of ' appearing equal ' is purely introspective. Is it not symmetrical ? 25.6.38

C. *By Mr. T. Smith.*

If by measurement we mean the association of numbers and properties by a rational systematic procedure such as the physicist employs (and—to say the least—this restriction appears necessary to avoid misunderstanding), I agree with Mr. Guild that sensations are not measurable. Apart from measurement there may be personal associations of numbers as well as of other concepts with sensations, but this in itself is not of great importance since the associations are peculiar to a single individual, though the fact of association may be of psychological interest. By training, the numbers in these associations can often be modified and controlled so that they correspond more or less to the numbers of some measurable property. In the absence of special training the numbers assigned to the members of any collection will vary notably from one observer to another. Ability to guess fairly accurately what measurement will give is an accomplishment of considerable utility, and this perhaps represents the nearest approach we can get to measurement on a sensory basis.

Simple tests I have made on a number of subjects suggest that the ' scales,' if the word is permitted, of untrained observers show marked differences from one another. Consistency only began to show when four objects were presented, and substantial agreement was reached on the magnitude which corresponds to the cross-ratio of four points on a straight line. In these experiments the individual scales were therefore projections of a common scale. While this is consistent with Mr. Guild's suggestion that likeness in a relation is a recognisable quality, it also suggests that Mr. Guild's interpretation of this relation in stimulus terms may be too narrow. A constant ratio of the stimuli from similar-appearing pairs is only one of a number of possible cases, and this or any other choice must be justified experimentally, and not by an *a priori* argument. 27.6.38

D. *By Dr. Wm. Brown.*

While appreciating the excellence of Mr. Guild's discussion of the question ' Are Sensation Intensities Measurable ? ' and agreeing with most of his arguments directed against Fechner's position, I am not satisfied that he has demolished the case for the direct measurement of *contrastes sensibles* (Delboeuf), commonly translated as ' sense distances.' ' Sense distances ' can be bisected with some degree of accuracy provided that the subject carries out the experiment a large number of times under appreciably constant conditions. In other words, the result is a statistical central tendency of statistical constancy as checked by its probable error. I find nothing in Mr. Guild's argument that would move me to withdraw anything that I have written on this matter in Chapter I of *The Essentials of Mental Measurement*. On the other hand, I do realise that a very much fuller discussion of the problem is needed, in the light of recent experimental work, and I am glad that the Committee is asking for a further year, during which it can deal with the question more fully. 1.7.38

E. *By Dr. J. H. Shaxby.*

It seems clear that the members of the Committee as a whole come out by that same door as in they went, and that no general complete agreement can be reached. The question we have been asked to decide, if not *mal posé*, at any rate turns to some extent on the definition of the term 'quantitative.'

Mr. Guild's article confirms this view. With its conclusions I concur on the whole, but I do not feel that his demonstration that sensory magnitudes are neither of class A nor of class B, disposes of the possibility that they may none the less be magnitudes (of class X say). That they are not magnitudes of practical importance to physicists or physiologists I agree, but this need not in itself consign them to limbo.

I suppose we should all agree that the conception of magnitude (like other conceptions) has a sensory basis ; one thing is greater than another because it looks larger or sounds louder or smells stronger. The measurement of the stimuli producing these different sensory effects has been the task of physics, and in the process it has been found that the introspective sensory estimates, while in the main giving a correct grading, are rough and often not even self-consistent ; they have therefore been superseded by the 'objective' physical modes of measurement by scales, balances and what not. But this very fact indicates the nature of these intuitive estimates ; no less than our balance, etc., they are methods of measuring stimuli.

We look at the sensory experience directly, so to speak, instead of looking at the vernier or spot of light or stopwatch. In so doing we perform a measurement of the stimulus and our result is such a measurement, more or less accurate, and nothing more psychologically fundamental than that ; certainly not a measure of the sensation itself. It is true that we use changes of sensation as our indicators of changes of stimulus, and if we have independent measures of stimuli we are entitled to use these to deduce the laws which our sensory indicators follow, e.g. (if Weber's Law holds) that our series of just noticeable differences functions as a logarithmic scale of stimulus-intensities. But this in no way justifies us in supposing that we have measured the intensity of our sensations. To do so is merely to lay down a postulate. Postulates may be useful or necessary ; Euclid's are necessary if we are to cross the *pons asinorum*. But this sensory postulate is mere lumber, for we make no further use of it. It does not help us to compare stimuli because the intuitive method of doing this does not require it, and it leads to no information about the physiological mechanisms of sense. 2.7.38

(V) *Statement by Prof. J. Drever.*

The Quantitative Relation between Physical Stimulus and Sensory Event.

In my turn I have been invited to present the case for an affirmative answer to the question whether sensation intensity is in any sense measurable, at the same time dealing with the main objections urged from the opposite point of view. As the arguments on the other side are based on general principles, so must also the answer to these arguments be similarly based. The most fundamental principle of all is the principle that both physicist and psychologist have a common starting-point in the world of sense experience, and a common aim in the fuller and clearer understanding of this world of sense experience. To that we shall return later ; in the meantime let us consider Mr. Guild's arguments.

While Mr. Guild professes to eschew metaphysics his whole description of the phenomenal world as composed of 'relation structures,' and a large part of his discussion of measurement is essentially metaphysical. It is perhaps true that 'there is nothing inherently numerical in the structure of the phenomenal world,' but it is true only because numeration is a conceptual process involved in our cognition of the phenomenal world. It is our knowledge of the phenomenal world that is really in question, not the phenomenal world as such. At this point let me correct a piece of bad psychology which crops up again and again in Mr. Guild's statement, especially in its later parts. He says 'we are so familiar with the description of phenomena in numerical terms that the association has become instinctive.' It is not a matter of association at all, except in so far as particular number *names* are associated with the number concepts—'four' for the Englishman, 'quatre' for the Frenchman, 'char' for the Hindu. This associationism runs riot all through the later parts of the statement, and in so far as it affects the argument the psychologist rejects it *in toto*. The thinking of relations is never explicable in terms of association.

The exposition of the principles of measurement is based on the distinction drawn by Dr. Norman Campbell between *A* magnitudes and *B* magnitudes, and only *A* magnitudes—that is magnitudes which can be measured by processes which do not imply the measurability of other magnitudes—are, strictly speaking, measurable. This would appear to mean that, strictly speaking, measurement reduces itself to enumeration, and that spatial magnitudes, or even lines only, are measurable. This is virtually a reduction of measurement to pre-history conditions. The important consideration from our present point of view is that it leads to the view that in order to establish a quantitative relation between two entities both entities must be measured each in terms of some unit appropriate to itself. Hence in order that we may be able to establish a quantitative relation between the intensity of the physical stimulus and the intensity of the sensation, we must be able to measure not only the physical stimulus in physical units but the sensation in sensation units. This is, I believe, an error, but it is an error which has been made by many psychologists as well as Mr. Guild, and the physicists for whom he speaks.

The theoretical possibility of measuring sensation intensity as such measurement was interpreted by Delboeuf, that is as distance on an imaginary scale of sensation intensity, as, for example, a loudness scale, must be admitted, even if we accept Mr. Guild's contention. Greys differ in degree of brightness, sounds differ in degree of loudness. Theoretically at least an individual can construct for himself a scale of brightness of greys, in which each grey appears a definite and equal distance away from the next grey on either side, and any new grey can be assigned its place on the scale. As regards an analogous loudness scale the position is somewhat complicated by the fact that loudnesses are not co-presentable in time, as the greys are, for reasons depending on the nature of the phenomena themselves, and here neither the scale itself nor measurement by means of the scale will be so accurate. The scale is an imaged scale as it were. But the theoretical position is not thereby affected. In the case of the loudness scale the specification of the various points on the scale including the zero will necessarily be in physical terms : to this point and to what it involves return will be made presently. What must be emphasised here is the theoretical possibility of the construction of such scales. The practical consideration upon which the most serious criticism of such scales can be based is that in strictness they are scales for the one individual only who constructs them, and for the time at which and the conditions, subjective and objective, under which they are constructed. It must, I think, be admitted that the subjectivity of a scale of this sort damns it from a practical point of view. But the subjectivity is a necessary consequence of the illegitimate demand that there must be a sensation scale in sensation units as well as a physical scale in physical units before sensation can be related to physical stimuli by way of measurement. The fact is that the demand, and indeed the greater part of Mr. Guild's argument relevant to the demand, implies a metaphysical theory even when it is not explicitly metaphysical.

We can only free ourselves from the incubus of this metaphysic by an entirely new start. First with regard to the evolution of measurement. The concepts of number and of magnitude are the two fundamental, and in part independent, notions from which measurement springs. Measurement necessarily involves comparison. Nothing is measured in terms of itself, except in the case of mere enumeration, and even that is not measurement, where there is not some comparison explicit or implicit. It is true that we can make the statement that there are twenty individuals in a group, when the number 20 may be taken as a measurement of the group in terms of the individual, but the notion of measurement in any such case only arises when the comparison of one group with another is in question, which means that ' greater,' ' less,' and ' equal ' are the basal ideas in all measurement. The questions ' how much greater ? ' or ' how much less ? ' are raised later. The first answers to these questions are given in terms of the other, ' twice as large,' ' half as large,' and so on—and the principle at once emerges that everything is measured not in terms of itself, but in terms of something else. The next step in the evolution of measurement is the measurement by means of standards which may be applied to the various magnitudes to be compared, and at this stage there is nothing incongruous in measuring space in terms of time and time in terms of space. Actually, for practical purposes all measurement is ultimately in terms of space. Our only means of measuring time, in fact, would seem to be in terms of space. When time units have been determined in this way, it becomes possible to measure motion—both constant and variable—in terms of time and space.

It would appear, therefore, that there is no difficulty whatever in finding analogies to the measurement of sensation intensity in terms of stimulus intensity without the necessity of measuring each in the first instance in terms of its own units. There would rather seem to be difficulty in finding analogies to the kind of measurement Mr. Guild contends for in the initial measurement of any aspect of the phenomenal world. Moreover it would also appear that if B is measurable in terms of A we only seek to devise a scale in terms of B units provided we wish to use B to measure something else. Measurement is not an end in itself. It is merely a means to the more exact representation, and therefore clearer understanding of the various connections and relations in the phenomenal world, which is of course the world of our sense experience. The ' relation-structure ' which Mr. Guild mistakes for the phenomenal world is a conceptual construct, arrived at as a result of, and by way of, measurement of the objects and events in the experienced world of sense.

We may take it then that in order to relate quantitatively stimulus intensity and sensation intensity, it is not necessary that we should be able to measure each in units of the same kind, but merely to measure the one—the stimulus intensity—and determine the manner in which the other—sensation intensity—varies in dependence upon the former. That loudness is a function of sound intensity does not admit of any doubt whatever, and a similar statement can be made of brightness, sweetness, and so on, in relation to their respective physical stimuli. The essential problem is the determination of the functional relationship between the intensity of the stimulus and the intensity of the sensation as an aspect of our experience of the stimulating object. At this point another objection must be urged against Mr. Guild's argument. He appears to assume that it is our perceptual experience of the object that we are attempting to correlate with the intensity of the stimulus, whereas it is merely the sensation aspect of that experience, abstracted from the experience as a whole.

Conclusion.

The Committee feels that the matter presented above is of great interest and value to those whose task it is to make measurements, mental and physical, and of importance sufficient to justify the Committee asking for reappointment to consider whether the views put forward are, or are not, irreconcilable.

The Committee therefore asks to be reappointed for one year without grant.

PLYMOUTH LABORATORY.

Report of the Committee appointed to nominate competent naturalists to perform definite pieces of work at the Marine Laboratory, Plymouth (Dr. W. T. Calman, C.B., F.R.S., *Chairman and Secretary*; Prof. H. Graham Cannon, F.R.S., Prof. H. Munro Fox, F.R.S., Dr. J. S. Huxley, F.R.S., Prof. H. G. Jackson, Prof. C. M. Yonge).

The grant of £50 was paid over to the Marine Biological Association on February 11, 1938.

Miss M. J. Dibb, King's College, London, has been nominated to occupy the Association's table at the Laboratory from July 12 to 29; she proposes to work on Protozoa parasitic on Polychætes.

Dr. Margaret W. Jepps, Glasgow, will occupy the table from October 1 to December 31, 1938, and will carry out research on the structure and life-histories of Foraminifera.

The Committee asks for reappointment, with renewal of the grant of £50.

ZOOLOGICAL RECORD.

Report of the Committee appointed to co-operate with other Sections interested, and with the Zoological Society, for the purpose of obtaining support for the 'Zoological Record' (Sir Sidney F. Harmer, K.B.E., F.R.S., *Chairman*; Dr. W. T. Calman, C.B., F.R.S., *Secretary*; Prof. E. S. Goodrich, F.R.S., Prof. D. M. S. Watson, F.R.S.).

The grant of £50 was paid over to the Zoological Society on May 13, 1938, as a contribution towards the cost of preparing and publishing Volume LXXIII of the *Zoological Record* for 1936.

The report of the Council of the Zoological Society for 1937 shows a further depletion of the 'Record Reserve Fund' due to excess of expenditure over receipts. The need for help from the contributing societies therefore continues, if the publication is to be carried on. The Committee accordingly asks for reappointment, with the renewal of the grant of £50.

ARTEMIA SALINA.

Report of the Committee appointed to investigate the progressive adaptation to new conditions in Artemia salina (Prof. R. A. Fisher, F.R.S., *Chairman* ; Dr. A. C. Fabergé, *Secretary* ; Dr. F. Gross, Mr. A. G. Lowndes, Dr. K. Mather, Dr. E. S. Russell, O.B.E., Prof. D. M. S. Watson, F.R.S.).

An outline of the programme of these experiments for the year 1937–38 was given in the last report of this Committee, presented at the Nottingham session of the British Association. This programme has been followed in all essentials.

A total of 27,440 nauplii have been tested, representing an amount of work considerably in excess of any one previous year. The testing and breeding of the material has been carried out almost entirely by Miss S. B. North.

The distribution of the tested nauplii among the six generations and the seven lines used this year is shown in Table I. The data for lines C_2, C_3, C_4, C_6, and C_7 are supplementary to those of the previous year. In addition, two new lines have been started, the reciprocal crosses of C_6 and C_7, two lines which had been selected through five generations last year : C_6 is a line which had shown a particularly strong and steady improvement in resistance.

It has been pointed out in the two previous reports that the chief factor vitiating the precision of the results is an excessive discrepancy between different broods of the same mating and generation. The first step taken to overcome this difficulty was the system of testing nauplii in six different grades of poison solution rather than in only one. This device proved inadequate to overcome the trouble, and in the present year untreated controls were used. Each brood is divided into eight approximately equal parts. Six of these batches are tested in six strengths of sodium arsenite, as previously, and the other two batches are placed in medium without poison, but are otherwise treated alike. By this means variations from brood to brood in the natural death-rate during the testing period can be taken into account ; though at the expense of much more complicated calculations.

It is clearly apparent from the results that the wide discrepancies between parallel broods mentioned above is not due to differences in natural death rate, but to variation in susceptibility to poison. An example of this, taken from the third selected generation of line C_3 is given in Table II (for the meaning of the letters designating sodium arsenite solutions strengths see Table III).

Thus these controls are not sufficient to eliminate the excessive heterogeneity, and other means must be sought to improve the precision of the experiment ; variation in temperature has been found to have no appreciable effect.

In order to utilise the data made available on natural death an entirely new method of statistical reduction has had to be developed, which involves fitting three parameters to the data. The first is the slope of the probit regression line, i.e. the variability of susceptibility *within* a brood. This parameter is approximately constant for all broods of one line-generation. The second is the 50 per cent. point, i.e. the strength of poison at which half the nauplii which have survived natural death, die by poison. The value of this parameter varies from brood to brood, its variation being the measure of heterogeneity *among* broods. The third parameter is the natural death-rate, which varies only slightly from brood to brood. As was said above, its evaluation does little to eliminate the heterogeneity between broods of the same line-generation.

The introduction of this laborious method of statistical treatment has resulted in a considerable time lag between the actual experiments and the interpretation of results. This makes it impossible, at the present time, to give a fuller discussion of the extensive data accumulated during the year.

Efforts are being continued to discover the causes of heterogeneity, and to devise other improvements in experimental technique which will increase precision.

The data obtained during 1937–38 are given in Tables IV–X. In Table III are given the strengths of sodium arsenite corresponding to the letters used in the other Tables.

The Committee asks to be reappointed with a grant of £20.

TABLE I.

Generation. \ Line	C_2	C_3	C_4	C_6	C_7	$C_6 \times C_7$	$C_7 \times C_6$
S_0 . . .	969	—	—	—	—	269	773
S_1 . . .	870	147	—	—	—	2,562	3488
S_2 . . .	158	1,314	165	—	—	2,822	761
S_3 . . .	—	2,963	609	280	116	1,761	—
S_4 . . .	—	2,402	1,741	281	—	1,371	—
S_5 . . .	—	307	530	407	374	—	—
Totals . .	1,997	7,133	3,045	968	490	8,785	5,022

TABLE II.

Illustrating variation of susceptibility between broods of the same parents.

Strength of Solution.	H	I	J	K	L	M	N	Control 1	Control 2
Brood 1 : Tested .	—	23	23	23	23	23	23	23	25
Surviving	—	22	23	21	14	10	10	21	23
Brood 2 : Tested .	14	14	14	14	14	14	—	16	14
Surviving	4	2	0	0	0	0	—	16	13

Table III.

Letter designating Poison Solution.	Strength of Solution of Sodium Arsenite, expressed as Normality.
E	0·003715
F	0·004102
G	0·004529
H	0·005000
I	0·005520
J	0·006095
K	0·006729
L	0·007430
M	0·008203
N	0·009057
O	0·001000

Table IV.—Line C_2.

	S_0		S_1		S_2	
	Tested.	Survived.	Tested.	Survived.	Tested.	Survived.
I	124	96	37	34	18	14
J	124	87	119	87	18	8
K	126	55	106	72	20	9
L	126	55	134	74	26	5
M	125	43	104	47	11	0
N	116	4	124	16	25	0
O	—	—	65	3	—	—
Cont. 1	116	105	109	97	24	20
Cont. 2	112	101	72	63	16	13

Table V.—Line C_3.

	S_1		S_2		S_3		S_4		S_5	
	T.	S.	T.	S.	T.	S.	T.	S.	T.	S.
G	21	11	—	—	—	—	—	—	—	—
H	21	12	49	39	63	33	32	8	—	—
I	21	12	165	119	252	150	367	126	11	4
J	21	9	165	82	399	174	287	122	39	31
K	18	0	165	82	243	89	287	89	39	23
L	37	4	169	78	430	97	267	57	39	19
M	—	—	169	61	393	49	299	43	39	13
N	—	—	150	9	458	39	299	32	39	8
O	—	—	—	—	—	—	46	2	39	8
Cont. 1	8	7	144	137	415	370	285	241	34	30
Cont. 2	—	—	138	109	310	274	233	184	28	22

TABLE VI.—Line C_4.

	S_2		S_3		S_4		S_5	
	T.	S.	T.	S.	T.	S.	T.	S.
E . .	—	—	23	22	—	—	—	—
F . .	—	—	23	22	—	—	—	—
G . .	13	12	79	58	157	36	—	—
H . .	27	18	100	68	167	67	44	31
I . .	27	23	108	87	223	74	59	34
J . .	27	17	98	80	223	48	67	41
K . .	28	10	86	43	223	25	74	35
L . .	28	3	81	39	261	33	74	33
M . .	15	2	11	0	45	0	66	19
N . .	—	—	—	—	45	0	15	3
Cont. 1 .	—	—	—	—	225	216	74	66
Cont. 2 .	—	—	—	—	152	141	68	57

TABLE VII.—Line C_6.

	S_3		S_4		S_5	
	T.	S.	T.	S.	T.	S.
I . . .	46	41	6	6	22	22
J . . .	46	31	36	24	37	26
K . . .	46	28	36	19	54	31
L . . .	47	27	49	30	61	34
M . . .	47	18	50	23	60	25
N . . .	48	8	51	2	54	2
O . . .	—	—	18	0	22	2
Cont. 1 . .	—	—	21	21	64	62
Cont. 2 . .	—	—	14	12	33	33

TABLE VIII.—Line C_7.

	S_3		S_5	
	T.	S.	T.	S.
H . . .	13	10	37	23
I . . .	13	1	48	41
J . . .	13	3	68	29
K . . .	13	0	68	22
L . . .	17	2	68	14
M . . .	17	1	66	2
N . . .	—	—	19	0
Cont. 1 . .	17	16	—	—
Cont. 2 . .	13	8	—	—

Table IX.—Line $C_6 \times C_7$.

	S_0		S_1		S_2					
	T.	S.	T.	S.	T.	S.	T.	S.	T.	S.
F	—	—	—	—	20	17	—	—	—	—
G	—	—	114	31	123	66	—	—	—	—
H	4	4	193	48	294	137	126	94	119	97
I	25	24	265	40	402	99	222	134	199	159
J	25	20	346	71	340	63	222	117	199	132
K	35	16	311	34	347	28	221	80	199	115
L	35	17	341	32	339	16	221	57	199	47
M	35	19	182	8	217	5	235	26	77	5
N	31	5	145	9	45	0	95	7	77	2
O	10	0	41	0	11	0	—	—	—	—
Cont. 1	35	34	340	317	341	313	227	223	199	178
Cont. 2	34	32	284	268	343	306	192	174	178	149

Table X.—Line $C_7 \times C_6$.

	S_0		S_1		S_2	
	T.	S.	T.	S.	T.	S.
G	26	2	127	37	11	5
H	55	10	302	135	94	61
I	82	20	406	145	94	61
J	102	31	440	93	94	47
K	102	29	440	83	101	30
L	102	29	440	25	101	24
M	76	15	318	8	83	19
N	47	9	143	0	—	—
O	20	0	—	—	—	—
Cont. 1	76	69	434	389	95	83
Cont. 2	85	82	438	375	88	77

FRESHWATER BIOLOGICAL STATION, WINDERMERE.

Report of the Committee appointed to aid competent investigators to carry out definite pieces of work at the Freshwater Biological Station, Wray Castle, Westmorland (Prof. F. E. Fritsch, F.R.S., *Chairman*; Dr. E. B. Worthington, *Secretary*; Prof. P. A. Buxton, Miss P. M. Jenkin, Dr. C. H. O'Donoghue, Dr. W. H. Pearsall).

During part of the current year the British Association's table at the laboratory has been occupied by Mr. G. H. Wailes while working on the planktonic Protozoa of Windermere. Mr. Wailes, formerly of Vancouver, British Columbia, resided at Wray Castle in the autumn of 1937, and has since received and examined samples of plankton collected each fortnight by the Association's staff. During the last part of the year Miss Pennington, of the Botanical Department, Reading University, has been appointed to occupy the table, in order to work on the succession of diatoms and pollen in cores raised from the bottom deposits of Windermere and other lakes. It is yet too early to report on Miss Pennington's research, but Mr. Wailes has drawn up the following short account of his study.

Plankton Protozoa of Windermere.

During a visit to Wray Castle in September, 1937, collecting was carried out with the object of observing in a living state the planktonic species of Protozoa occurring in Windermere, as a preliminary to tracing throughout a complete year the seasonal changes that take place in the plankton in both the north and south basins of the lake. Commencing on September 22, fortnightly samples of preserved Windermere plankton have been received and have been examined. They were obtained by hauling vertically a fine meshed net from a depth of 40 metres to the surface.

As a result of the collecting done in September, chiefly in the north basin, the following species were observed which up to the present (June) have not been recorded in the serial plankton gatherings, namely the Heliozoans *Acanthocystis spinifera* and *Raphidiophrys elegans*, a small naked dinoflagellate *Gymnodinium pulvisculus*, which, like others of that group, disintegrates under the action of preservatives, and a small ciliate belonging to the Tintinnioidae which proved to be an undescribed species and has since been named *Tintinnopsis wrayi* (*Ann. & Mag. Nat. Hist.*, May, 1938).

The Protozoa present in the plankton during the period observed (September to June) comprise two groups; the first, consisting of the Dinoflagellata and *Dinobryon divergens*, was absent from mid-November to mid-February, whereas the other was persistent during the winter and consisted of species of Rotifera, *Vorticella*, and an infusorian, *Mallomonas acaroides*.

Dinoflagellata comprised the species *Ceratium hirundinella* var. *gracile*, *Peridinium willei* and *Peridinium kincaidi* (the last-named has previously been recorded only from Alaska and British Columbia). Numerous cysts of these species and individuals of *P. kincaidi* occurred until mid-November; thereafter no dinoflagellates were observed again until mid-February. By March *P. willei* and *P. kincaidi* had become equally plentiful. Rotifera, represented by some seven or eight species, were numerous throughout the winter; they have been submitted to a specialist for specific determination.

The most notable feature of Windermere plankton was its different character at either end of the lake. In the north basin the quantity was comparatively small, and consisted for the most part of crustacea (Cladocera and Copepoda), whereas in the south basin the quantity was large, and consisted for the most part of phytoplankton throughout the period observed. This was due to the abundance of only a few species of Myxophyceae and Diatomaceae.

This abnormally abundant production of phytoplankton in the south basin continued without diminution all through the winter, and is apparently due to drainage effluents from towns and villages discharging into this portion of the lake. A similarly large production of marine winter phytoplankton occurs in the inlets on the west coast of Vancouver Island where fish reduction factories discharge their waste. In Windermere the zooplankton does not seem to be directly affected by this condition except in so far as the phytoplankton may afford a more abundant food supply; but further study of the conditions obtaining on the two basins may have a pertinent bearing on the problems of water supply and the practicability of artificially increasing human food supplies.

CYTOLOGY AND GENETICS.

Report of Co-ordinating Committee for Cytology and Genetics (Prof. Dame Helen Gwynne-Vaughan, G.B.E., *Chairman*; Dr. D. G. Catcheside, *Secretary*; Prof. F. T. Brooks, F.R.S., Prof. F. A. E. Crew, Dr. C. D. Darlington, Prof. R. A. Fisher, F.R.S., Mr. E. B. Ford, Prof. R. R. Gates, F.R.S., Dr. C. Gordon, Dr. J. Hammond, Dr. J. S. Huxley, F.R.S., Dr. T. J. Jenkin, Mr. W. J. C. Lawrence, Dr. F. W. Sansome, Dr. W. B. Turrill, Dr. C. H. Waddington, Dr. D. Wrinch).

The Committee have continued to assist and advise Organising Committees and Recorders in arranging for joint sessions and other means for promoting closer co-operation between cytology and genetics and other fields of biology.

It was thought that the bearing of recent cytological and genetical discoveries on other aspects of the old problem of the Mechanism of Evolution was imperfectly understood by many biologists. The Organising Committees of Sections D and K were approached with the suggestion that a joint symposium occupying a morning and afternoon session should be devoted to this subject. The proposal was approved, and a number of papers dealing with various phases of the problem have been arranged.

The Organising Committee of Section M were unable to find space in their programme for a discussion of Genetics in Relation to Agriculture. However, they sought the Committee's advice in arranging for genetical contributions to their symposia on Crop and Stock problems respectively.

The Committee have also felt that a useful service would be rendered to workers in other biological fields by the presentation of demonstrations. They have organised an exhibition illustrating the principles of the Genetics of Colour in Animals and Plants, demonstrating wherever possible the chemistry of the pigments and colour differences concerned. A descriptive brochure is in course of preparation.

MINING SITES IN WALES.

Report of the Committee appointed to investigate early mining sites in Wales (Mr. H. J. E. PEAKE, *Chairman*; Mr. OLIVER DAVIES, *Secretary*; Prof. V. GORDON CHILDE, Dr. C. H. DESCH, F.R.S., Mr. E. ESTYN EVANS, Prof. H. J. FLEURE, F.R.S., Prof. C. DARYLL FORDE, Sir CYRIL FOX, Dr. WILLOUGHBY GARDNER, Dr. F. J. NORTH, Mr. V. E. NASH WILLIAMS).

THIS spring a survey and excavation of the ancient mining-dumps at Great Orme's Head, Llandudno, was carried out on behalf of the Committee. Stone hammers, of the type believed to be approximately Roman in date, and cup-marked querns had previously been found there. The examination, however, showed that they occurred in no orderly sequence, and some were found on parts of the dumps which can hardly be more than a century and a half old; it is therefore probable that they have been thrown out of ancient workings by recent miners. Patient search may yet reveal the ancient dumps not too deeply buried by modern detritus. It will be particularly unfortunate if more copious evidence of the ancient workings is not discovered, as this must have been one of the most important ancient mines in Wales.

A few hours were also spent testing a habitation site below Great Orme's Head, near the Gogarth Hotel, which it was believed might be a mining settlement. The site yielded a considerable depth of stratification and some pottery, which has not yet been examined. No evidence was found for its association with the mines, so it will not further interest this Committee.

Thanks are particularly due to Mr. G. A. Humphreys, on behalf of the Mostyn Estates, for permission to excavate and for help in many small ways, and to the Ecclesiastical Commissioners for permission to dig on their lands.

The following specimens have been examined:

By Dr. C. Desch, at the National Physical Laboratory: copper bun-ingot, Penmaenmawr, sent by Mr. W. J. Hemp; contained 1·0 % lead, traces of iron and nickel, but no other metals.

At Queen's University, Belfast:

(*a*) Heavy compact slag with few gas-holes; colour, black with reddish stains; from Forden Camp, Montgomeryshire (Roman), supplied by Welshpool Museum. Contains 52·21 % iron, no copper, lead or zinc. An iron slag, probably derived from smelting and not from a smithy.

(*b*) Specimen of quartz, containing chalcopyrite and some galena, found in the Roman fort at Caersws and supplied by Welshpool Museum. Contains 25·45 % copper, 28·8 % iron, some sulphur, no lead, silver, antimony, arsenic, bismuth, zinc, nickel or cobalt. Apparently there happened to be no galena in the fragment analysed.

(*c*) Bornite, slightly magnetic and containing specks of native copper, from the Roman fort at Caersws, and supplied by Welshpool Museum. Contains 15·47 % copper, 11·69 % iron, 6·49 % sulphur, 0·02 % lead, no silver, antimony, arsenic, zinc, cobalt or nickel.

These last two specimens had presumably been collected in ancient times from one of the Montgomeryshire mines. They indicate the working of some of these mines under the Romans, and show that the road through central Wales, though primarily military, had also some economic significance. They afford confirmation of a fact pointed out in a previous report, that though lead is the predominant metal in the mineralised area of Plynlimon, the most ancient mines, marked by stone hammers, exist at occurrences of copper-ore.

(*d*) Fairly light and crumbly ferruginous slag, found with a small piece of metallic lead, Newtown mine (Montgomeryshire). Contains 53·22 % iron, 4·2 % zinc, no lead, arsenic, antimony or copper.

(*e*) Galena and pyrites disseminated in quartz, Newtown. Contains 12·08 % lead, 7·89 % iron, 2·3 % copper, no silver, bismuth or zinc. This mine was examined in 1937, and it is hoped shortly to publish a report in the *Montgomeryshire Collections*.

(*f*)–(*k*) Specimens from Dinorben hill-fort, supplied by Dr. Willoughby Gardner.

(*f*) 1134, copper bun-ingot, yellow and very malleable ; metallic, with a little malachite on the surface. Contains 96·72 % copper, very slight traces of lead and tin, no antimony, arsenic, silver, iron, zinc, bismuth, nickel or cobalt.

(*g*) 1331, iron slag, black, friable and full of gas-holes, resembling a smithy-slag. Contains 57·61 % iron and no copper.

(*h*) 1316, black iron slag with some gas-holes. Contains 52·66 % iron, traces of copper.

(*i*) 584, whitish-grey material of amorphous structure, containing 1·02 % lead, 0·93 % tin, no silver, much insoluble material. Probably earth which has been in contact with one of the numerous tin-lead alloys of Roman date.

(*j*) 1157, ferruginous material, with almost no insoluble residue. Contains 79·21 % iron and no copper. Probably rusted iron.

(*k*) Hard and well-fused slag, from the surface. Contains 52·9 % iron, no lead or copper.

(*l*) Heavy black slag, Talargoch, a mine believed to be Roman. The slag is crystalline in structure and without gas-holes, owing to slow cooling. It contains 40·4 % iron, 0·24 % lead, 2·14 % zinc, no copper.

DERBYSHIRE CAVES.

Sixteenth Interim Report of the Committee appointed to co-operate with a Committee of the Royal Anthropological Institute in the exploration of Caves in the Derbyshire District (Mr. M. C. Burkitt, *Chairman* ; Mr. A. Leslie Armstrong, *Secretary* ; Prof. H. J. Fleure, F.R.S., Miss D. A. E. Garrod, Dr. J. Wilfred Jackson, Prof. L. S. Palmer, Mr. H. J. E. Peake).

Creswell Crags.—Mr. Leslie Armstrong, F.S.A., reports as follows :

‘ *The Yew Tree Shelter.*—Excavations were continued on this site during the early autumn of 1937 and yielded further evidence of occupation contemporary with that of the Lower Middle and Middle zones of Mother Grundy's Parlour and proved this to be the principal period of its occupation during late Palæolithic times.

‘ A scatter of microlithic flakes and implements on the top of the deposit indicates casual occupation, comparable with that of the final occupation of Mother Grundy's Parlour, of Azilio-Tardenoisian facies.

'Woolly Rhinoceros and Wolf were added to the list of fauna previously recorded for this site.

'Half a perforated axe-hammer, of hard sandstone, was found in the surface soil, but was unaccompanied by any evidence to establish its date.

'*Boat House Cave.*—Since submitting the 1937 report, little progress has been made here, owing to a transference of activities to Whaley, due to the location of a further rock shelter there and the opportunity which arose for its immediate excavation.

'Work is now proceeding on the removal of the concrete which covers the cave earth in the Boat House Cave, and it is anticipated that this will be completed by the end of July over an area sufficient to permit excavation of the underlying cave deposits during the autumn and winter.

'*Whaley Rock Shelter, No.* 2.—This was located by Dr. Arthur Court, in August last, by trial sections which he dug on what appeared to be a most unpromising site. This consists of a talus of limestone rubble and rocks which, on removal, proved to completely mask a cliff at the rear, and appears to have resulted from the collapse of a former over-hang of the cliff which had provided a rock shelter during Pleistocene times.

'Above the cliff is a small plateau of limestone, sheltered by a rocky slope at the rear, which has apparently been favoured as a camping ground from Neolithic to Roman times. The successive occupiers of the plateau have thrown their camp debris over the adjoining cliff and this material is now found stratified in the talus.

'A systematic excavation was commenced here in September and is still progressing. The talus has been removed over a length of 20 ft., in successive layers, down to the Pleistocene horizon and the hidden face of the cliff exposed to a height of 12 ft. Except for a trial section, the Pleistocene deposit has not yet been excavated. The trial section has established the presence of Upper Palæolithic artifacts, in association with remains of Reindeer and Hyæna in this level, and its excavation will now be proceeded with.

'The talus has yielded Neolithic pottery of Peterborough type; also pottery of Bronze Age; Bronze-Iron Age overlap period; Iron Age; Roman and Romano-British wares; also artifacts of flint and bone, pot-boilers and animal bones in great abundance and a few human bones, including portions of three mandibles.

'An exhibition of the whole of the artifacts and a selection of animal remains, obtained in the Pin Hole Cave, was displayed in the Manchester Museum from November 1937 to April 1938, and thanks are extended to Mr. R. U. Sayce, M.A., Keeper of the Museum, for providing the facilities and arranging this exhibition. These exhibits were also included in the exhibition of recent archæological work at the Institute of Archæology, Regent's Park, London.

'A further grant of £25 is earnestly requested by the Committee for the continuation of the work at Creswell Crags and Whaley.'

BLOOD GROUPS.

Report of the Committee appointed to investigate blood groups among primitive peoples (Prof. H. J. Fleure, F.R.S., *Chairman* ; Prof. R. R. Gates, F.R.S., *Secretary* ; Dr. F. W. Lamb, Dr. G. M. Morant).

During the past year blood group testing has been going forward in certain areas, of which a preliminary report was made last year. Opportunity was taken by Prof. R. R. Gates, F.R.S., after the meeting of the Indian Science Congress Association in Calcutta, to visit various centres in Southern India, and arrangements have now been made through the official channels for blood grouping various native tribes, particularly in the States of Mysore and Travancore, where numerous different types exist. Further work in Assam has been in abeyance, but is now being taken up again, and results are also expected from other parts of India.

A further development begun this year is the testing of local groups of population more or less isolated in different parts of the British Isles, in conjunction with anthropometric studies of the same people. Dr. M. A. MacConaill visited Rachrai Island, on the north coast of Ireland, and blood tests of those available were made. The population appears to show peculiarities in blood groups as well as in other anthropological characters. It is hoped to extend this work to various other population groups which have remained more or less isolated. A survey of such groups will show the effects of local isolation, and will also indicate whether, in such populations, any statistical correlation exists between blood groups and other anthropological or racial characters.

SUMERIAN COPPER.

Eighth Report of the Committee appointed to report on the probable sources of the supply of Copper used by the Sumerians (Mr. H. J. E. Peake, *Chairman* ; Dr. C. H. Desch, F.R.S., *Secretary* ; Mr. H. Balfour, F.R.S., Mr. L. H. Dudley Buxton, Prof. V. Gordon Childe, Mr. O. Davies, Prof. H. J. Fleure, F.R.S., Dr. A. Raistrick, Dr. R. H. Rastall).

The last report was presented in 1936. Since then the work has been continued without a grant, and this has occasioned some delay. Some analyses were reported to the Committee at the Nottingham meeting in 1937, but were not published. The analytical work has been by no means confined to objects of Sumerian age, but many archæologists have taken advantage of the facilities provided to submit specimens of ancient metal. There is undoubtedly a demand for a permanent centre, with a staff accustomed to the analysis of such metals and familiar with the characteristics of ores from various regions. By the use of microchemical methods it is possible to make a complete analysis on 10 miligrammes of metal, so that the natural objection of museum curators to allow drillings to be taken from valuable objects is obviated, as a cavity left by drilling such a quantity is almost imperceptible. Such analyses are comparable in accuracy with those on larger quantities, owing to the special technique employed. The work is at present carried out in the Metallurgical Department of the National Physical Laboratory, under the direction of the Secretary. The total amount paid to the Department of Scientific and Industrial Research since the 1936 Report is £94 17*s.* 10*d.* Towards this contributions were received of £10 from the Copper Development Association and of £10 from Miss Winifred Lamb. A generous gift of £100 from Sir Robert Mond has now enabled this cost to be covered.

The analyses made have included a series of fourteen copper specimens from the Anatolian site of Kusura, submitted by Miss Lamb. The results are published in *Archæologia*, 1937, (ii) **36**, 1–64. Two contained tin, in quantities of 2·8 and 1·2% respectively, a few having much smaller quantities. The specimens from period A, however, contained arsenic in appreciable quantities, up to 0·59%, whilst the specimens from the later periods contained no more than traces of that element. Lead was present in only one specimen : that containing the larger quantity of tin. Three iron objects were found in the highest levels.

Some copper objects found by Mr. Mackay at Chanha-Daro proved to be free from tin, and to contain only minute quantities of arsenic and nickel, but some of them contained sulphur.

Ancient slags, collected in Persia towards Baluchistan by Dr. J. V. Harrison, were examined, but none of them could be identified as being derived from copper-smelting operations.

An interesting series of Central Asian bronzes, including several of the Minussinsk type, was submitted by Miss V. C. C. Collum. A preliminary description of them has been published in the *Journal of the Royal Central Asian Society*, 1938, **25**, 22–23. These were of varying composition, several being notable for their high content of lead. Miss Collum also supplied a series of bronze axes from Brittany, the analyses of which will be published shortly. The excavations which she had carried out for Sir Robert Mond in Guernsey have also led to an investigation of early iron objects, and several iron hammers of known Roman age have been obtained from museums and examined microscopically. A good deal of information about the structure of bloomery iron has now been collected.

A very extensive series of copper and bronze fragments from Troy I–IX, submitted by Prof. Blegen, is in course of examination. The series will be most conveniently reported on when complete. It is, however, interesting to note the marked differences between the chemical composition of objects from the earlier and the later levels.

Several specimens of electrum found by Sir Flinders Petrie at Tell Ajjul in Palestine were also analysed.

The Secretary has been in correspondence with a number of archæologists in other countries, who are now carrying out analyses of copper and bronze from a great variety of sites, so that an extensive mass of material is being accumulated. There are, however, many sites from which objects have been described as copper or bronze from their general appearance only, and detailed analyses are very desirable. There is an advantage in keeping the Committee in being, although with a more general title, as a centre for such information.

KENT'S CAVERN, TORQUAY.

Report of Committee appointed to co-operate with the Torquay Natural History Society in investigating Kent's Cavern (Sir A. Keith, F.R.S., *Chairman*; Prof. J. L. Myres, O.B.E., F.B.A., *Secretary*; Mr. M. C. Burkitt, Miss D. A. E. Garrod, Mr. A. D. Lacaille).

The following report has been received from the excavators :—

Work was recommenced on October 25, 1937, and continued until March 28, 1938, when, having excavated to a depth of 34 ft. below the datum line (the lowest point ever yet reached in the exploration of the Cavern), it appeared as though, through a distinct falling off in the number of finds, it would not be sufficiently profitable to go deeper considering the difficulty of working between rocks, and the time involved in getting the material brought to the surface. It was decided to close for the season, and next year to make a drive from the 'Vestibule' to the 'Sloping Chamber.'

Flints have been very scarce, nothing worth recording having been found, but fortunately several good bones and teeth were secured, including an ante-penultimate milk molar of a mammoth in fine condition, a large specimen of the base of a rhinoceros horn, two vertebræ of a salmon, which is quite new to Kent's Cavern, the humerus and furculum of a bird similar to a mallard; but the most striking finds were a metatarsal bone of a bison with two each of the first and second phalanges, all of which articulate perfectly, which, so far as is known, is the first instance of more than three bones being found together capable of articulation; teeth of horse, hyena, deer, rhinoceros, Irish deer, bear, and bison were also fairly numerous; coprolites were scarce.

Arthur H. Ogilvie. B. N. Tebbs.

The Committee applies for re-appointment, with a further grant of £5 toward the cost of unskilled labour to assist the voluntary excavators.

TRANSPLANT EXPERIMENTS.

Report of Committee on Transplant Experiments (Sir Arthur Hill, K.C.M.G., F.R.S., *Chairman*; Dr. W. B. Turrill, *Secretary*; Prof. F. W. Oliver, F.R.S., Prof. E. J. Salisbury, F.R.S., Prof. A. G. Tansley, F.R.S.).

The experiments are being continued at Potterne, Wiltshire, along the lines suggested by the Committee. Meetings have been held at Kew and at Potterne. A fifth biennial report has been accepted for publication in the *Journal of Ecology*, and a summary of results for the first ten years of the experiments has also been prepared and is to be published in the same periodical.

A grant of £5 was made at the Nottingham Meeting. Most of this has been spent on the repairing of instruments and on labour. It is requested that the remainder, if any by the time of the Cambridge Meeting, be allocated for use in the latter part of this year (1938) and that the Committee be kept in being. A full financial statement will be available by the time of the Cambridge Meeting.

INFORMATIVE CONTENT OF EDUCATION.

Report of the Committee appointed to consider and report on the gaps in the informative content of education, with special reference to the curricula of schools (Sir Richard Gregory, F.R.S., *Chairman*; Mr. G. D. Dunkerley, *Vice-Chairman*; Mr. A. E. Henshall, *Secretary*; Prof. C. W. Attlee, Miss L. Higson, Mr. H. G. Wells, Mr. A. Gray Jones).

Introduction.

At the Nottingham Meeting of the British Association for the Advancement of Science, Mr. H. G. Wells, in his Presidential Address to Section L, took as his subject 'The Informative Content of Education,' and outlined what he considered should be a minimum curriculum for all pupils. Consequent upon what was then said, this Committee was appointed 'To consider and report on the gaps in the informative content of education, with special reference to the curricula of schools.' The Committee had therefore to investigate in what way the actual work of the schools conformed with the minimum curriculum proposed by Mr. Wells.

An Analysis of Mr. Wells's Proposals.

The first step taken was to prepare an analysis of the curriculum suggested by Mr. Wells. For this purpose both his address and its illustrative chart were examined and an attempt was made to relate the proposals to the actual conditions of school organisation. It will be recalled that Mr. Wells divided his curriculum into Grades, of which A, B and C gave what he regarded as the absolute minimum for all pupils, although he added Grades D, E and F. It was with the first three, therefore, that this Committee was primarily concerned.

Fortunately Mr. Wells indicated the time which in his view should be given each week to this aspect of school work and also the total number of hours to be allocated to each grade. From these facts it became obvious that the information included in Grade A was intended for pupils between the ages of 5 and 7, that in Grade B for pupils of the ages 7 to 11, and Grade C for those aged 11 to 14 plus. The grades thus corresponded roughly to the divisions of the English elementary school system, viz. Infants, Juniors and Seniors.

The examination of the speech and chart, therefore, enabled the following analysis to be prepared and the sections to be taken in infants', junior and senior schools to be indicated.

GRADE A. *Ages 5–7. 300–400 hours.*

Subject.	Time allowed.	Scope of work in Chart.	Scope of work in Speech.
History	4/10 of time: 2 hrs. 25 min. each week.	Elementary ideas about human cultures and their development in time. Savage life—tools, weapons. Primitive homes—caves, shelters, huts, clothing. Occupations: 1. Agriculture, including Domestication of Animals. 2. Trade. Transport. Ships. 3. Predatory people. Warfare.	We begin by telling true stories of the past and other lands. Contrasting conditions under which children live with those of former centuries and those in other lands.
States of Matter	2 hrs. 25 min. per week.	States of matter. Composition of matter. Elementary physiography.	Water—liquid, gas, solid: boiling, evaporation, freezing. Mud pies, Weather } basis of physiography.
Geography	1 hour.		Local typography basis of geography. World as globe.
Biology			I think we might turn the bear, wolf, tiger, ape, from nightmare material into sympathetic creatures.
Descriptive Zoology and Botany			Plants as hiding places, homes and food for birds and beasts. Facts about animals and how they live.
Elementary Physiology of Plants and Animals			Plants in relation to animals. How plants and animals live.

Grade B. *Ages* 7–11. 1,000 *hours.*

Subject.	Time.	Chart.	Speech.
History	4/10 of 6 hrs.: 2 hrs. 25 min.	General History. Races of man. Early civilisations. General significance in history of: Persia. Greece. Carthage. Rome. China. America. Islam. Christianity. General idea of break-up of Christendom and the appearance of Modern Sovereign States. Elementary History of Great Britain and France.	Story of early civilisation. Growth of primary civilisation. The developing rôles of priest, king, farmer, warrior. Succession of stone, copper, iron. Introduction of the horse, construction of roads, development of shipping. Rise of Semitic-speaking peoples. Establishment of Persian, Macedonian, Roman Empires Coming of Aryan-speaking peoples. Rise and Growth of Islam. Rise and Growth of Christianity.
Geography	1 hour	Geography and Geology. Types of country and floras and faunas. A general survey of the world as a human habitat and as a source of power and wealth.	A little map-reading. Precise ideas of type of country. Distinctive floras and faunas of the main regions of the world. Sort of human life lived in each of these regions. Knowledge of topography to enable pupils to know the position, the reason for the position and the kind of places well-known cities are. E.g. London, Rio, New York, Rome and Suez.

Science.			
(*a*) Biology . .		Zoology and Botany, including extinct forms and their succession in time. Geological ages. General ideas about ecology and evolution.	Succession of living things in time. The processes in the prosperity, decline, extinction and replacement of species. Story of life from the beginning. Emergence of sub-man in the world and gradual emergence of mankind.
(*b*) Science of Inanimate Matter.		Physics and Chemistry. Leading up to modern concepts of matter. Mechanism and power. Elementary history of invention and discovery.	Foundation of pure physics and chemistry upon most modern lines.
(*c*) Physiology .		Physiology and Anatomy. Including clear, general ideas of animal and plant reproduction. Elementary pathology.	Working of our bodies. Reproduction. Chief diseases. Enfeeblements and Accidents.

GRADE C. *Ages* 11–14+. 1,000 *hours*.

Subject.	Time.	Chart.	Speech.
History . .	4/10 of available time, i.e. 400 in 4 yrs., 100 per year, 2½ hrs. per week.	Elements in Political Theory. Development of existing national and imperial boundaries. The increasing importance of economic changes in History and the search for competent economic direction.	Study of such phenomena as: Rise and fall of Ottoman Empire. Rise of Russia. History of Baltic. Rise and fall of Spanish Power. Rise and fall of Dutch Power. Unification of Germany and Italy. Leading theories of individualism, corporate state, communism. History of war.

GRADE C.—*continued.*

Subject.	Time.	Chart.	Speech.
Geography . .		Economic Geography and Geology of the World.	Detailed and explicit acquaintance with world geography, with different types of population in the world and the developed and undeveloped resources of the globe. Devastation of world's forests, replacement of pasture by sand deserts through haphazard cultivation, waste and exhaustion of natural resources, coal, petrol, water, etc.
Social Mechanism .		Short history of Communications and Trade. A History of Innovations in production and manufacture. The rôle of property and money in economic life.	Knowledge of conventions of property and money. Way in which money has changed slavery and serfdom into wages and employment. The way in which fluctuations of money affect 'industrial windmills.' Significance of inflation and deflation.
Personal Sociology		A short history of general ideas. Comparative religion. Study of social types leading to choice of rôle.	General ideas of the relation of oneself to the Universe. Primary propositions of chief religious and philosophical interpretations of the world. General study of social structure associated with social types to direct attention to choice of a *métier*.

The Schools and Mr. Wells's Suggested Curriculum.

The next step was to discover how far the work already taken in the schools coincides with, or falls short of, that included in Mr. Wells's suggested minimum curriculum, and if possible, the reasons for any differences that might be revealed. Accordingly, the following questions were prepared:

(1) What part (or parts) of the suggested Informative Content of Education for children aged   are now taken in your school?
(2) Of the remainder, what part (or parts) do you think
  (*a*) might be advantageously included in the curriculum?
  (*b*) should not be included?
(3) What obstacles prevent the inclusion in your curriculum of the sections given under (*a*) above?
(4) Why would you exclude the sections given under (*b*) above?
(5) What textbooks (if any) have you found of assistance in reference to the sections you include?
(Please give the textbook which has been found of assistance for each section.)

Elementary and Preparatory Schools.

Copies of both the analysis and the questionnaire were then submitted to a number of head teachers in elementary schools in various parts of the country and they were asked to furnish replies to the questions based upon the actual work taken in their own schools.

The following table gives the particulars as to the distribution of the questionnaire and the schools from which replies were received.

Kind of School.	Number to whom sent.	Number of replies received.	Towns where schools are situated.
Infants' .	6	3	Birmingham, Rhondda, Sunderland.
Junior .	9	7 { 3 head mistresses	London, Nottingham, Ealing.
		4 head masters	Kesteven, Cambridge, London, Cannock.
Senior .	11	8 { 3 head mistresses	Hull, Stafford, Bristol.
		5 head masters	Oxfordshire, Darlington, Torquay, Lowestoft.

In addition the questionnaire was submitted to the head masters of six preparatory schools or preparatory departments of secondary schools, but only three replies were received and of these one did not supply any information.

Secondary Schools.

Later the inquiry was extended to secondary schools, and for this purpose the following amended form of questionnaire was used:

(1) (*a*) What part (or parts) of the suggested informative content of education for children aged 11 to 14 years are taken in your school *for pupils between* 11 *and* 14?

(b) What part (if any) not so included is taken *with pupils between the ages of* 14 *and* 16 ?

(2) Of the parts not included, what part (or parts) do you think

(a) might be advantageously included in the curriculum *for pupils between* 11 *and* 14 ?

(b) might be advantageously included in the curriculum *for pupils between* 14 *and* 16 ?

(c) should not be included in the curriculum ?

(3) What obstacles prevent the inclusion in your curriculum of the section or sections given under (2*a*) above ?

(4) What obstacles prevent the inclusion in your curriculum of the section or sections given under (2*b*) above ?

(5) Why would you exclude the section, or sections, given under (2*c*) above ?

(6) What textbooks, if any, have you found of assistance in reference to the sections you include in your curriculum ?
(Please give the textbook which has been found of assistance for each section.)

This revised form, together with the detailed analysis of the curriculum proposed by Mr. Wells, was sent to twenty-one secondary schools, including Rugby, Shrewsbury, and Liverpool Collegiate School ; but replies were received from only four boys' schools and five girls' schools. Of these one supplied practically no information.

The Curriculum in the Elementary Schools.

(1) *Replies from Infants' Schools to Questions on Grade A.*

The replies revealed a fundamental difference between the method of approach used in the schools and that upon which the inquiry was based. The inquiry had reference to the factual information pupils were expected to acquire ; but the approach commonly adopted in infants' schools is not by way of definite instruction ; the imparting of knowledge is not regarded as of primary importance, and the division of what is taken into subjects is no longer practised. ' Subjects,' wrote the head mistress of a Sunderland infants' school, ' as set out under separate headings, have largely disappeared from the modern infants' school and given place to activities and experiences in a prepared environment.' ' There is a fundamental difference of opinion in regard to the treatment of children aged five to seven ' was the reply of a Birmingham head mistress. ' I feel that children should be provided with opportunities for actual experiences ; any knowledge in the form of facts will be gained through their project work and will be incidental.' This fundamental difference in the way infants' schools are regarded is of primary importance in this inquiry and must be borne in mind when the replies are under consideration.

Little attention is given to History as such in these schools. It is not taken as a subject, though ' much incidental knowledge of people of other lands and primitive people is gained from Bible stories, fairy stories, fables and myths.'

Likewise the information included under the heading of Geography is given only incidentally. In the Rhondda, the basis for information was said to be such simple aspects as ' name of home, street, school and town.' In the Sunderland school the instruction in this subject is based upon the teaching of local topography, following on school visits to such places as the seaside and the farm, and conversations about the district.

The suggested Biology is not taken in the Birmingham school, though the head mistress thinks that a little of it might be, 'to remove the "nightmare" reaction.' Stories of wild animals are told in the Rhondda school, and in Sunderland the head mistress reported that the information is given in the stories of animals connected with such projects as the farm, the circus and the Zoo.

In the same way the Descriptive Zoology and Botany are taught in the last-named school, through the care and observation of animals and plants kept at the school. Daily nature and seasonal news of animals, birds, etc., and the daily observations on plants in the various seasons, are a feature of the Rhondda school, and reference was made in the reply to a Nature Table.

No reference was made in any of these replies to a school garden, which is known to be a feature of some infants' schools and which would enable much of the informative content included in this section of Mr. Wells's proposed curriculum to be adequately taught.

There was an indirect reference to it, however, in answer to the question relative to the obstacles in the way of teaching what is considered desirable. 'School buildings,' it was said, 'need to be replanned to give more space for free movement and experimental work for the children. Classrooms should look out to gardens for nature study and care of animals.'

The replies to the question as to the part or parts of the scheme that should be excluded were:

(1) From Birmingham: 'Any subject matter which is beyond the power of the normal child to assimilate. Children under seven cannot appreciate differences in time.'

(2) From Rhondda: 'Detailed study of Geography, Biology, Botany, human cultures and development should be left to later stages of school life. Plants, animals, weather conditions, are dealt with incidentally in daily talks.'

(3) From Sunderland: 'History.'

The reasons given for these exclusions were:

(1) 'Fundamental difference of opinion in regard to treatment of children aged five to seven. I feel that children should be provided with opportunities for actual experiences. Any knowledge in the form of facts will be gained through their project work and will be very incidental. Definite lessons on the animals would not be included. After seven the child is ready for much of this formal teaching. Before seven some children gain much factual knowledge through their reading and their experiences, but there can be no uniformity if the children have progressed freely and individually. I feel that here our great task is social training, and I am putting formal teaching later and later.'

(2) 'The children's interest in the people and things in the world immediately around them, with its attendant vocabulary, should first be aroused and satisfied. Analysis of these conditions, and their origin and development, are dealt with in later school life. The two-year course in the infant school does not allow time for detailed study of Botany and Biology, nor does the children's ability permit it.'

(3) 'The children in this school, owing to the poverty of the conditions in which they live and their squalid surroundings, are lacking in many of the ordinary experiences of life when they enter school at five. They do not, therefore, in this department, reach the stage when they are interested in other lands and other times; at any rate, not to the extent of giving a definite course of lessons in such subjects. I consider that the history suggested by the chart belongs, for most children, to the age seven to eight, as does the transition from nursery tales to the true stories of other ages.

The relative value of time particularly has no significance for children up to seven. If their interest in children of other lands is aroused by any incident, then that is followed up and information given, but for the most part their stage of development is such that the " here " and the " now " supply sufficient scope for their curiosity. With children from homes of a wider culture perhaps this ground might be covered by the age of seven, but I do not think so. I would certainly include it in the first year of the junior stage.'

General Observations.

This section of the inquiry revealed the method of approach of teachers in infants' schools rather than the content of the syllabuses of the schools. In modern infants' schools the emphasis is placed upon the development of the child rather than upon the information taught. The distribution of the questionnaire was necessarily limited to a few representative schools : a wider distribution might have shown more clearly what information is acquired by the pupils. This is considerable, notwithstanding the fact that neither the time-tables nor the school syllabuses are based upon subjects of instruction. Visits to a number of infants' schools—if arrangements could be made for a sympathetic and skilled teacher to ask questions of the pupils—would enable a more accurate idea to be obtained as to the amount of factual information gained. It might prove to be more extensive than is generally thought, especially in relation to Science. The work in nature study, in the school garden, and with animal pets, quickens observation and prompts innumerable questions ; the information pupils thus acquire is probably greater than has hitherto been appreciated.

(2) *Replies from Junior Elementary Schools to Questions on Grade B.*

The replies to questions on Grade B were received from three head mistresses and four head masters of junior schools. Unfortunately the detailed answers desired were seldom given ; but in every instance the evidence afforded an illustration of the difference between the present methods of approach and those in common use twenty-five years ago.

History.

This difference is shown plainly in the information relative to the curriculum in History. Thus replies from a London head master and a Nottingham head master stated that all the suggested curriculum is taken ; and that from an Ealing head mistress, ' Practically this scheme is taken simply and pictorially, *minus* Races of Man, references to the Great Empires, and to Islam and Semitic Races.' Of the others, a London head mistress takes the ' Story of Early Civilisation ' and the ' Growth of Christianity ' ; a Cambridge head master includes the ' Coming of Aryan-speaking Peoples,' the ' Rise and Growth of Christianity,' and the ' Elementary History of Great Britain ' ; and a Cannock head master includes the ' Story of Early Civilisation,' the ' Growth of Primary Civilisation,' the ' Succession of Stone, Copper, Iron,' the ' Introduction of the Horse,' the ' Construction of Roads,' the ' Development of Shipping,' the ' Establishment of Persian, Macedonian and Roman Empires,' the ' Coming of Aryan-speaking Peoples,' and the ' Rise and Growth of Christianity.'

The time suggested for History by Mr. Wells was approximately 2 hours 25 minutes per week. This was longer than was generally given to the subject. One correspondent stated that only one-sixth of that amount was given, while another allocated one hour. It appears obvious that if the whole of the work indicated in Mr. Wells's proposals is to be taken—certainly if it is to be taken thoroughly—a longer time is necessary. Thus the reply from Kesteven stated that it is desirable that the whole suggested scheme should be taken, but that only 2 hours 10 minutes are available for both History and Geography. On the other hand, the Cannock head master thought the following parts are superfluous for junior children: 'The Developing Rôles of Priest, King, Farmer, Warrior'; the 'Rise of the Semitic-speaking People'; and the 'Rise and Growth of Islam.'

Geography.

As will also be observed from the replies referring to the other grades, the work actually taken in this subject most nearly approximates to that suggested in Mr. Wells's curriculum. In every case except one the replies stated that all contained in Mr. Wells's outline is taken; and the exception simply stated that no Geology is taken. Here, too, the time devoted to the subject, when reference is made thereto, approximates to that allocated by Mr. Wells.

Science.

(*a*) *Botany.*—In Science, however, the scope of the work attempted in the schools falls far short of what Mr. Wells regards as necessary. Biology is seldom taken, and even when taken it is not on the lines indicated. The only two replies giving any information in relation to this subject stated, one, that 'Succession of Living Things in Time' is taken, and the other, that Zoology and Botany are taught in reference only to creatures and plants familiar to children.

(*b*) *Science of Inanimate Matter* likewise receives somewhat scant attention. 'Very simple principles' are taught in one of the schools from which information was received; Physics and Chemistry are taught in another, but not on the lines indicated; and 'Elementary History of Invention and Discovery' is taken in a third. But in the other replies either no mention was made or else it was stated that no part of the syllabus is attempted.

(*c*) *Physiology.*—Slightly more attention is given to Physiology. 'The Working of our Bodies' is taught at one school; 'Some Reproduction of Plant and Animal Life from Direct Observation' in another. Elementary Hygiene was mentioned in yet another reply, and this is possibly included more commonly in the curriculum of junior schools in connection with 'Health Talks,' a universal feature of school work.

The whole of the Science teaching was said in two replies to be given in the form of 'Nature Study.' How much of what is included in Mr. Wells's proposals is taken in this work was not indicated.

Observations on the Proposed Curriculum.

Few suggestions were made as to the inclusion of those parts of the curriculum at present omitted Only two correspondents indicated that they would like to include other sections of the work; one wished to include all the parts of the History and Geography, the 'Elementary History of Invention and Discovery,' and 'The Working of our Bodies,' but said that inadequate time and the necessary grouping of classes made that impossible ; the other would add ‘ General Ideas of Evolution,’ but the size of classes, 46–48, was said to prevent it.

The suggestions as to parts to be excluded were more numerous. They were :

1. ‘ Biology section, Reproduction, Diseases, Enfeeblements and Accidents, Developing Rôles of Priests, etc,. and Rise of Semitic People,’ because ‘ subject matter is unsuitable and entirely outside the experience of the child.’

2. All the Science except ‘ Elementary History of Invention and Discovery ’ and ‘ Working of our Bodies,’ for the reason that they are ‘ too abstract and too deductive.’

3. Geology, Pure Physics and Chemistry, because, in the opinion of the writer, ‘ The object in teaching *Science* in the Junior School should be rather to lay the foundation for a method of approach and orderly thinking. This can best be achieved with the teaching of Nature Study.’

4. Physiology and Anatomy as well as Physics and Chemistry. The reason given by this correspondent was, ‘ The matter under the head *Physiology* I consider contrary to a junior child’s natural development and requirements, which should be of a constructive character.’ She also considered that the work would be hampered by size of classes, lack of specialist teachers and lack of accommodation and equipment.

5. Sections of History, Biology and Science of Inanimate Matter ; the reasons given for exclusion being :

‘ (*a*) *History*.—Too much time spent on early World History leaves no time for modern inventions, great social reformers, and gradual changes in social life. One hour is all that I could allow in the crowded curriculum of a Junior School, with its need for the three R’s and numerous activities.

‘ (*b*) *Physics and Chemistry*.—Too advanced ; besides, no facilities.

‘ (*c*) *Physiology*.—Personal hygiene only necessary at this stage. Reproduction should be taught when quite young by parent, or if at school, at the adolescent stage.

‘ (*d*) *Biology*.—(Theory of Evolution in Human and Plant Life.) I think these children are more interested in things as they are at present.’

Having regard to the position revealed in relation to the Science syllabus, it would appear that this is a subject calling for detailed consideration by the Research Committee.

General Observations.

The History and Geography taken in schools, especially the latter, correspond somewhat closely to the recommendations in Mr. Wells’s chart. So far as History is concerned, there are marked differences in the scope of the work attempted in different junior schools. More or less drastic changes are taking place in the method of approach to this subject, and World History is receiving increasingly greater attention. The replies to the questionnaire show that the schools are at present passing through a period of transition in respect to the teaching of History. Similar changes relative to Geography have been taking place for the last thirty years at least, and the syllabuses of the majority of schools include most of what Mr. Wells advocated.

In regard to Science there is much variation in the work taken in junior schools themselves. But even if this is borne in mind, there is, speaking generally, a great difference between what is actually taught and what Mr. Wells advocated should be taken. Possibly in reference to junior schools the same observation might be made as was offered when infants' schools were under consideration, viz., that much of what is suggested should be learned by pupils is already taught, although not as part of a separate subject. Some of the information included in Botany is taught in Geography, talks of other lands, or Nature Study. The part played by the aquarium (a feature of many schools) in this work was not indicated in the information received. Health talks are included in the syllabus of all junior schools and experimental work taken to illustrate them. Some Physiology and Anatomy are doubtless taught in connection therewith. Possibly the modern methods of education adopted in schools, and especially the new approach to the curriculum, have handicapped the inquiry in its efforts to discover what is the actual information gained by pupils during the years they spend in the junior schools. It is obvious, however, that more money will have to be spent on equipment and books to enable the schools to do justice to the expansion in the curriculum dictated by the needs of to-day.

(3) *Replies received from Senior and Central Schools working under Elementary School Regulations to Questions on Grade C.*

The questionnaire on Grade C was submitted to teachers in various types of school working under the Elementary School Regulations. These included selective and non-selective central schools, and rural and urban senior schools. Some were departmental schools either for boys or girls, and others were mixed. The schools to which the inquiry was sent may thus be taken as representative of elementary schools providing for children of the ages 11 to 14 plus. In the selective central schools the majority of the pupils continue in attendance until the age of 15.

An examination of the replies indicated that the analysis of Mr. Wells's proposed curriculum did not convey to all those who received it an adequate idea of what the address with its chart conveyed to those who heard it delivered at Nottingham. The aim of Mr. Wells did not always appear to be fully understood; certainly his reasons for advocating this minimum of informative content of education were not fully appreciated even if they were comprehended. This was unfortunate, since it may have led correspondents to regard the inquiry from the wrong angle.

The replies revealed a wide divergence between present practice and Mr. Wells's proposals, and to summarise them briefly is a rather difficult task. In these circumstances possibly the best course will be to indicate the sections of the subjects included in the proposed curriculum which are taken in each school.

History.

In the Lowestoft school, which is a selective central school, the sections taken are:

'National and Imperial Boundaries.'
'Economic Changes in History.'
'Rise and Fall of Empires and Powers.'
'History of War.'

In regard to the remainder the head master wrote: 'It is perhaps fair to say that most of the remaining parts might be "advantageously included." Some of the phrases are rather staggering for a school curriculum, but they probably mean much less than they appear to mean.'

A Bristol girls' school includes in its History syllabus :

'Development of Imperial and National Boundaries';
'The Rise and Fall of Countries and Empires' taken generally;

and no suggestion was made of desirable additions, though the opinion was expressed that 'Leading Theories of Individualism, Communism, Corporate State' should be excluded. The reasons given for this opinion were that girls in a non-selective central school 'are intellectually incapable and of insufficient experience at their age, to benefit by study of these subjects,' and that 'the girls come from different homes, where political opinions may be freely expressed *at home*, and they may be (and probably will be) strongly biassed towards a political party supported by the home.'

A school in Oxfordshire takes :

'The Development of Existing National and Imperial Boundaries';
'The Increasing Importance of Economic Changes in History'; and
'The Search for Competent Economic Direction.'

The head master would also like to include 'Elements of Political Theory' for pupils during the 14–15 year, but he would exclude the 'History of War,' the 'Rise of Russia,' the 'History of the Baltic.' His reasons are lack of time, and the unsuitability of these subjects for rural children who are more practical than bookish in type. 'Theories of Individualism, Corporate State and Communism,' he wrote, 'could not be studied with profit by rural elementary children until 14–15, as they have no background to which to attach such knowledge and are too young to have opinions thereon of their own.'

A Staffordshire head mistress reported that 'practically all is taken in one syllabus or another with the exception of "Detailed Study of European History" as mentioned in Mr. Wells's speech. . . . All of it is taken in so far as the sections make contact with English or Imperial History.' In this school the 'special bias is given to (1) Social History and (2) Imperial History.'

This head mistress thought that when 14–15 is established as the last year of school life a fairly comprehensive study of Modern European History might well form the basis of this year's work in History and Economics. At present the available time does not permit of this being included. It also prevents the inclusion of the 'Rise of Russia,' the 'Rise of Dutch Power,' the 'Unification of Germany and Italy,' and the 'Leading Theories of Individualism, Corporate State and Communism.' Even if time did permit, however, she would not include the 'History of War.'

Another head mistress who has charge of a school in Hull includes these sections :

'Development of Existing National and Imperial Boundaries.'
'The study of such phenomena as : Rise and Fall of Ottoman Empire, taken baldly as it impinges on the Crusading Era and the Renaissance.'
'History of the Baltic.'
'Rise and Fall of Spanish Power.'

She reported that an approach to the 'Leading Theories of Individualism, Corporate State and Communism' has emerged from the wireless talks on 'History traced Backward' and 'Topical Talks.'

Of the remaining correspondents one reported that the History taught is not on the lines of the suggested curriculum but more in accord with the section under the heading of 'Social Mechanism'; and the other gave the bald answer 'None' to the questions, adding that the whole of the Informative Content of Education ‘ is wholly unsuitable for the immature minds of children 14–15 years of age.’

Geography.

As was noted when reference was made to the replies to questions on Grade B, the Geography taken in the school corresponds closely to that contained in Mr. Wells's syllabus in that subject. Thus of the Lowestoft school it was reported : ‘ All parts are taken (using discretion about “ detailed and explicit ”) ’ ; of the school in Torquay, ‘ *Broadly* speaking, we cover the suggested content, but the statement in the speech has an academic flavour shunned by us, especially when dealing with B and C classes. We probably teach as much as Mr. Wells wants us to teach, but in an easier atmosphere than he creates ’ ; of a girls' school in Stafford, ‘ Taken almost exactly as indicated, except that the Geology side is only simply touched upon ’ ; and of a Hull girls' school, ‘ All except the Geology of the World.’ Of the others, it was said of one that Economic Geography, local Geology and World Geography are taken ; and of the other, ‘ Economic Geography and Geology of World are taught generally.’ A note was made in two instances that World Geography is not taken in a detailed and explicit but in a general manner.

Social Mechanism.

The information under this heading was not given so fully as that included in Geography. Indeed, in one reply it was ignored and in the others only a small part was included. Thus in the selective central school in Lowestoft, ‘ Communications and Trade, Production and Manufacture, and Money ’ are taught ; in a Bristol girls' school, ‘ A Short History of Communications and Trade, a History of Innovations in Production and Manufacture, and the Rôle of Property and Money in Economic Life ’ ; a rural school in Oxfordshire includes in its syllabus ‘ A Short History of Communications and Trade ’ taught as the opportunity offers in the History and Geography lessons ; a girls' school in Stafford takes ‘ Short History of Communications and Trade, the History of Innovations in Production and Manufacture,’ but admits that they are very inadequately covered, and only deals with the ‘ Rôle of Property and Money in Economic Life ’ as it affects the other sections of the subject ; and a girls' non-selective school in Hull includes ‘ Short History of Communications and Trade ’ and ‘ A History of Innovations in Production and Manufacture,’ not as a definite study but as ideas thereon emerge more or less definitely.

The head master of the rural school, while wishing to include more detailed study of what he already takes, would definitely exclude ‘ The Rôle of Property and Money in Economic Life.’ A head mistress said she would never include in a girls' curriculum any studied course on ‘ The way in which fluctuations of money affect “ industrial windmills ” and Significance of Inflation and Deflation.’

Personal Sociology.

Less is taken of the work suggested in this section than of that suggested in any of the other sections. Only very small fragments of it appear to be attempted and the replies were sometimes rather vague. Thus in reply to questions on this part of the suggested curriculum, one stated ‘ General Social Development is taken.’ In other cases the part taught was indicated, e.g. ‘ Man's relationship and duty to his neighbour, and nationally to other countries' in one; 'General ideas of relation of self to universe,' taught incidentally in History and Geography, in another, of which the head master wrote, 'but details too abstract for rural children of 11 to 14 years of age'; 'Taken, but on rather different lines. Comparative Religion is dealt with only in Imperial History and Geography. No great emphasis is given to direct attention to the choice of a *métier*' in a third. In one girls' school Comparative Religion is taught, the oldest group taking a short course of study in the main teaching of Buddha and Mohammed at the end of the fourth year.

One head master thought that 'Incidental mention of other religions and general study of types of civilised life' might be taken, but regarded the remainder as too abstract for elementary school children. A head mistress, too, commented on this point, stating that she 'does not consider children of this age-group are capable of comprehending the ideas contained in the suggestions.'

General Observations.

An analysis of the replies reveals a number of interesting facts, the most outstanding being the degree of freedom enjoyed by teachers in elementary schools in arranging the curriculum of the schools for which they have responsibility. As a consequence of this there is a great variety of curricula in English schools catering for children of the same age, a variety which characterises both the subjects taken and the sections of the subjects included. Another noteworthy fact which emerges is that there is a marked tendency to give greater attention to practical subjects, and the time available for giving definite information is therefore more limited than it was in the early days of this century.

More germane to the present inquiry is an opinion which in one form or another finds frequent expression. It is, that the curriculum outlined by Mr. Wells, though it may be desirable, is beyond the capacity of the pupils of this age. The opinion is expressed somewhat hesitatingly by these correspondents in elementary schools; indeed, they more often indicate it by the omission of any reference to sections; but it is none the less obvious.

It may be that the suggested curriculum is too ambitious and comprehensive; that the subjects are beyond the capacity of the pupils. But since many of the sections are taken in the schools—though not all of them in any one school—only actual experiment can afford conclusive evidence of how much could be taught, if adequate conditions were obtained and schools were appropriately staffed. Meanwhile the inquiry itself will without doubt have a stimulating effect.

Replies from Preparatory Schools and Departments to Questions on Grade C.

Since preparatory schools and preparatory departments of public schools cater for children of the same age-group as those found in the senior schools in the elementary education system, the same questionnaire was submitted to six schools of this type. Only three replies were received. Of these one was in the following terms:

> 'I do not think it is possible to give a formal detailed reply to the questionnaire on Mr. Wells's views of History. It is useless to consider Section C, ages 11–14, except on the assumption that the ground

suggested for the stages A and B has previously been covered; the knowledge and capacity assumed for these ages as possibilities seem to me absurd.'

History.

The other two indicated what sections are taken in History and Geography. At Brentwood School, in History 'The Rise and Fall of Spanish and Dutch Powers' come in for 11–14. The rest of the syllabus more in the Sixth Forms; parts of the 7–11 are given to 11–12 boys.'

At Gadebridge Park the syllabus includes 'The Rise and Fall of Spanish and Dutch Powers, Unification of Germany and Italy and the History of War.'

Geography.

At Brentwood 'Most of this is covered,' while at Gadebridge Park the parts of the suggested syllabus taken are:

'World geography. Different types of population in the world. Developed and undeveloped resources of the globe. Devastation of the world's forests, dongas and the like; natural resources.'

Social Mechanism.

At the first-named school this 'comes in Lower Sixth'; no mention was made of it in the reply from the second school except that the head master thought that the following might be advantageously included in the curriculum: 'Short history of communications and trade' and 'A History of Innovations—perhaps.'

His colleague at Brentwood School observes in reply to the inquiry as to what might be added:

'Most of syllabus is taught in different parts of the school; I should like to include Physiology but at present "Reproduction" is the chief part dealt with; accidents we deal with only in the Ambulance section of the cadet corps.'

Sections Excluded.

The replies to the question on parts that should be excluded were of interest. In one case they included all given under Personal Sociology, which, said the writer, 'should only be discussed with well-balanced senior boys having other main interests in life; a boy may easily lose his mental balance.' This he gave as the reason for the exclusion of this section of the curriculum, 'because,' he added, 'I have had personal experience of undergraduates breaking down mentally.'

In the other case the correspondent would not include:

'Geology of the World. Increasing importance of economic changes in History, etc. The Rôle of Property in economic life. A short history of general ideas. Comparative Religion. Study of social types leading to choice of rôle.'

In explanation of this he stated:

'The preparatory schools, owing to the immense importance and time given to Classics and Mathematics and the ignoring by Public Schools of Geography, are only able to devote some 2 hours a week to "Geography" and Geology can well be left to the Public Schools. Naturally some Geology is necessary to the understanding of even elementary Geography. No Geographer is qualified to teach Geology;

it is outside their province. Lack of space makes me dogmatic. The other items are outside, or ought to be, the understanding of a normal, healthy boy. Could such subjects be made interesting to the immature? I doubt it.'

When referring to the obstacles in the way of introducing a ' Short History of Communications and Trade ' and possibly ' A History of Innovations,' he wrote :

> ' The fact that our present curriculum takes a normal boy all his time to assimilate, whether he works for the Naval Entrance Examination, the Common Entrance Examination or a Scholarship Examination. We must leave the Public Schools something to teach their boys. Facts, not ideas, come more easily to boys between the ages of 11 to 14, therefore make hay . . . We hate teaching theories and politics to boys, unable to refute what is told them successfully ; it's unsporting and un-British.'

Replies from Secondary Schools to Questions on Grade C.

An analysis of the replies presents some difficulty. This arises partly from the variation of schemes and the distribution of the sections taken over a wider age-range and partly from the method of approach to the subject, a method which differs considerably from that indicated by Mr. Wells. The questionnaire referred to the ' Informative Content ' of an education which might be—Mr. Wells thinks *should* be—given to all pupils by the age of 14. ' Our educational system is so different from that envisaged by H. G. Wells that it is difficult to comment,' said Mr. Lyon of Rugby. ' Mr. Wells is an interesting theorist ; but if the children whose capacities he estimates so glibly were before him in a class he would discover in a very salutary way that their reactions to knowledge he prescribes were rather different from what he imagines them to be,' wrote Miss Clarke, of Manchester High School for Girls.

These observations raise an important question which appears to have been in the minds of many correspondents. This is : ' How far are the pupils in schools able, at the ages stipulated, to receive the information included in the outline syllabus of Mr. Wells ? ' Possibly the teachers underestimate the capacities of their pupils, as Mr. Wells suggests ; but it is noteworthy that in these returns from secondary schools there is unanimity regarding this point. What was indicated by Mr. Wells as necessary is thought to be beyond the capacity of the pupils at the age at which it is suggested that it should be taken ; but in some cases the information is included for an older age-group.

Thus Miss Gwatkin (Streatham Hill Girls' School) wrote :

> ' We keep most of our girls till 18 and nearly all of them until 17, and prefer to deal with many of the matters in your schedule when they are over the age of 16 and more mature.'

Mr. Barton, of the Grammar School, Bristol, expressed the opinion that ' Here and there, from the standpoint of a practical teacher, the scheme suggests precociousness, and I dare say the air of an ambitious set-out is partly accounted for by the difficulty of concise indications in other than academic language. The word " informative " seems to me a little misleading ; e.g. our " ideas of the relation of oneself to the Universe " come largely into the realm of speculative opinion and feeling rather than of information in the usual sense. Such doubts however are inevitable, and broadly speaking I think the consideration of the scheme has value, if only to encourage the taking of a wider and more fundamental view of the schoolmaster's objects.'

It is because the Committee of Section L desire, if possible, to encourage the taking of a wider and more fundamental view that this inquiry was instituted. But it is necessary to know how much is already attempted, in which direction expansion is necessary and how far such alterations are possible and desirable before any conclusions can be formulated.

Mr. Evans (Woodhouse Grammar School, Sheffield) indicated another reason why the whole of the Informative Content of Education as contained in Mr. Wells's suggestions cannot be given :

> 'Our opinion is that the syllabus of work suggested by Mr. Wells is far too ambitious to be tackled in any school with any degree of success with pupils up to the age of 14 plus. . . . So far as History is concerned, most of the work under Grade B is taken here in Grade C. I am sure that any attempt by the average teacher to try to cover the ground suggested by Mr. Wells would end in chaos and confusion in the poor child's mind, and he would derive very little benefit from the attempt to teach him all that is suggested. It is possible, of course, that a teacher who is a real genius at his work might make a success of this colossal task.'

The last sentence suggests that, given the right teacher, the pupils could be given the information included in the scheme outlined by Mr. Wells. If this were so, then the work of the Committee would include recommendations as to the ways and means of providing the right teachers.

History.

The opinion that even a teacher who is a genius could teach all that was suggested is not shared, however, by all those who sent replies to the questionnaire, some of whom would not include all sections of the work. For example, the Head Master of Bristol Grammar School said of 'Elements of Political Theory,' 'Development of Existing National and Imperial Boundaries,' 'Increasing Importance of Economic Changes in History and the Search for Competent Economic Direction,' that they should not be included, as the problems require a riper mentality than is possessed by the average boy of 16. In this school, consideration of these sections is deferred to an advanced course, and even then the treatment is less formal than incidental to the work in literature, art and general essays.

The Head Master of Rugby suggested that pupils should be given a thorough knowledge of the History outlined in Grades A and B (i.e. the parts suggested to be taken by pupils below 11 years of age), and 50 per cent. of Grade C, substituting the further study of British and Imperial affairs for the other half. This is suggested, however, for pupils aged 14 to 16.

The Head Mistress of Manchester High School for Girls stated :

> 'In our history syllabus, the early civilisations, and much of the attendant matter suggested by Mr. Wells, are taught at the age of fourteen. This year's course ends with the fall of the Roman Empire ; and the history of Great Britain, of her European neighbours, and their overseas expansion, which Mr. Wells suggests should be completed before the age of fourteen, occupy our girls for another four years of their school course.'

Only Sheffield Woodhouse School gave an affirmative reply to the questions on the following parts of the scheme of History : 'Development of Existing National and Imperial Boundaries,' 'Increasing Importance of Economic Changes,' though the Head Mistress of Clapham Secondary School stated that practically all were included in the instruction for the period ending 14 years of age, except such as are postponed for the ages 14 to 16. The Head Mistress of Manchester High School reported that such parts are taken as fall within the History syllabus for School Certificate (English History 1783–1914, and European History 1789–1914), while Liverpool Collegiate School said they are taken not as a course but as they arise from the study of Europe.

It is noticeable that while the Head Master of Bristol Grammar School does not include the parts to which reference is made in the foregoing paragraph and the Head of Sheffield Woodhouse School does, in relation to 'Study of such phenomena as: Rise and Fall of Ottoman Empire; Rise of Russia; History of Baltic; Rise and Fall of Spanish Power and of Dutch Power; Unification of Germany and Italy,' the former replied 'Yes' except in reference to 'History of Baltic'; and the latter replied 'No' but thought the 'Rise and Fall of Spanish and Dutch Powers' might be included. Both take the 'Unification of Germany and Italy' with pupils 14 to 16. These sections are also taken at Streatham High School and were said to be included by Liverpool Collegiate School in the General and World History Course which is given as a preliminary background for English and European History.

There is greater unanimity regarding the remaining sections: 'Leading Theories of Individualism, Corporate State, Communism,' and the 'History of War.' Streatham High School reported that the first is taught incidentally, but Bristol Grammar School and Sheffield Woodhouse School both stated definitely that it should not be taught as the pupils have not sufficient knowledge on which to base theories. Liverpool Collegiate School reply expressed the opinion that it should only be taken in the Sixth Form; since if attempted before, the teaching can only be superficial and may be misleading.

In no instance was the 'History of War' taught. After stating that it is unsuitable except with Sixth Form, the Head Mistress of Manchester High School proceeded to suggest that it should not be included at all, its place being taken preferably by the 'Achievements of Peace.'

Subsequent to writing the above report a further reply was received from Dudley Girls' High School. This stated that 'All the suggested subjects are included in the curriculum, although it is not built round these points but differently grouped, i.e. we do not work solely from this angle and include much that is not mentioned. We lay more stress upon British History.' The periods studied at this school are: To the French Revolution by pupils 11 to 14 years of age; and from the French Revolution to the present day by pupils between the ages of 14 to 16.

Geography.

The instruction in Geography given in the schools appears to approximate more nearly to Mr. Wells's scheme than does that in any other subject. In Bristol Grammar School it was said that Economic Geography and Geology of the World is covered in an elementary and unpretentious way by the pupils aged 11–14, and most of the other sections might be covered if time allowed. Rugby reported that all of it is included in the syllabus; Liverpool that 'all may be included but owing to lack of time some parts of it may not be reached until the 14–16 stage,' and added, 'This is not on the ground of its unsuitability but simply because time does not allow.'

Some of the correspondents indicated their method of approach. Thus the Streatham Head Mistress recorded that ‘ World Geography is taken at the age of 10 plus in the form of journeys and history of explorations. So there should be knowledge of the (i) Continents ; (ii) kinds of religion ; (iii) occupations.’ This is for pupils 11–14. For those 14–16 she said : ‘ Then World Regions are taken as synthesis of previous building up of knowledge of various parts.’

In Manchester High School for Girls this Geography is taught to pupils 11–14 incidentally as the study of the various world regions proceeds, but is not isolated and taken in regard to the world as a whole. Thus most of the ground is covered by the age of 18. In her general remarks the Head Mistress said : ‘ To propose covering the ground by the age of 14 is absurd. The limitations in general knowledge in children of 11 to 13 make the teaching of scattered items of general information purely dogmatic.’

A similar *caveat* was entered by the Head Master of Sheffield Woodhouse School. After stating that ‘ General knowledge of natural resources and their exploitation ’ is taken, he proceeded : ‘ To suggest that children of this age should have detailed and explicit acquaintance with undeveloped resources of the globe seems to me fantastic ; to begin with I find it difficult to understand what is meant by “ detailed and explicit acquaintance with world geography, with different types of population, and the developed and undeveloped resources of the globe.” ’

Geology is not, apparently, given so much attention as the other sections. In Sheffield Woodhouse School it was said to be least stressed. At Clapham it is not ‘ taken as a separate study but references to Geology and geological theory are not infrequent in the course of lessons in Geography.’ One correspondent, the Head Mistress of Streatham, expressed the opinion that ‘ Geology as such should not be included. This is a science in itself, and too difficult. Geomorphology would be a better word, but even that could only be taken very generally. Geology should be left to University study.’

Social Mechanism.

The replies to questions on Social Mechanism were not helpful. The first two sections :

‘ Short History of Communications and Trade,’
‘ History of Innovations in Production and Manufacture,’

were said to be taken in Bristol Grammar School and Liverpool Collegiate School, but in each case by pupils 14 to 16 years of age. In relation to the remaining sections :

‘ The Rôle of Property and Money in Economic Life,’
‘ Knowledge of Conventions of Property and Money,’
‘ Way in which Money has changed Slavery and Serfdom into Wages and Employment,’
‘ Way in which Fluctuations of Money affect “ Industrial Windmills,” ’
‘ Significance of Inflation and Deflation,’

four made no reference and a fifth replied that they should not be taken since they require a riper mentality than is to be expected before the age of 16.

From Streatham High School the reply to this section was : ‘A regular course of economics lasting two years is taken in the Sixth Form (16–18). Usually economic history or a course of modern problems is also taken.’

The Head Mistress of Manchester High School stated : ‘A good deal of what is described as Social Mechanism finds its way into history teaching.

Simple economics and economic history are absolutely essential to a just appreciation of historical development as a whole.'

Again there is the suggestion that the information contained can only be taken with a Sixth Form and that below that stage only very superficial matters can be dealt with.

Personal Sociology.

The least satisfactory replies had reference to the section on Personal Sociology. Three made no mention of it and with slight exceptions the others were of the opinion that the various subjects should not be taken. The exceptions were in the case of Streatham High School. There 'Comparative Religion' was said to be taken in Divinity; 'General Ideas of the Relation of Oneself to the Universe' was said by Liverpool Collegiate School to emerge from religious teaching and addresses at Prayers with the whole school; and 'General Study of Social Structure, etc.' was given by a Bradford head master as a part that might be taken.

The Head Mistress of Manchester High School is of the opinion that 'The material outlined under Personal Sociology is not suitable before the last year in the Sixth Form (17–18) and can be approached only in the most elementary way even then.' The Head Master of Bristol Grammar School remarked that 'It is questionable whether the detailed consideration of much of the matter under Personal Sociology is proper to the school stage at all.'

General Observations.

The replies received from secondary schools demonstrate the influence of examinations upon the syllabuses of work. While the statement that the First School Examination controls the work in secondary schools may be an exaggeration, the fact that it does greatly influence both the scope of the syllabus and the method of approach is obvious. The thorough and sometimes detailed knowledge required to answer examination papers has a limiting effect upon the scope of the work attempted.

The practice of taking thoroughly what is attempted may also account for the opinion that some sections should not be taught. On the other hand, the opinion that some parts of what Mr. Wells would include should not be taught to school pupils was definitely expressed by head teachers of experience and of known progressive views. This was especially true of two sections, Social Mechanism and Personal Sociology, and it is only fair that the strong opposition to their inclusion should be noted.

Summary of Replies.

In summarising the results of the questionnaire, the two features which call for comment are:

1. The consensus of opinion that the informative content of education outlined by Mr. Wells is both too wide in scope to be covered during the present school life of the great majority of children of this country, and too advanced in its demands upon the capacity of the pupils for whom the various sections of the subjects were suggested.

2. The differences of opinion in relation to what can be included in the curriculum; what some correspondents believe cannot be taken with pupils of a given age-group and cannot be included within the scope of a reasonable curriculum, is actually taken in other schools with pupils of the same age.

In reference to the first it should perhaps be observed that the phraseology used by Mr. Wells was that most suitable for the audience on the occasion of the delivery; but it was not always appropriate for use in schemes of work for elementary or secondary schools. As one correspondent wrote: 'Some of the phrases are rather staggering for a school curriculum, but they probably mean less than they appear to mean.'

In illustration of this statement reference may be made to some sections named in the outline given by Mr. Wells. 'Elementary ideas about human cultures and their development in time,' when suggested for the infants' schools, sounds formidable. But when the actual stories told to children of this age are recalled, including the Biblical stories, stories of Hiawatha, etc., and when the outlines of History are examined from this angle, the suggestions may not appear quite so alarming.

Similarly, 'States of Matter,' for infants, has a terrifying sound. But talks on ice, water and air in some form or other, and in association with some experience or activity, are taken in every school. Biology, Zoology, Botany and Physiology mentioned in any curriculum for very young children would immediately arouse suspicion, if not antagonism; yet it is doubtful whether the whole of Mr. Wells's suggestions are not included in Nature talks, Observation Records, Gardening and such-like normal activities of nearly every infants' school in the country.

The differences between what is actually taken and what Mr. Wells suggested in regard to other age-groups may be more marked; but the contradictory nature of some of the replies appears to emphasise the need for further and fuller inquiry. With regard to some of the work suggested for these groups it may be found that the terminology has been too readily accepted as ambitious and pretentious; and insufficient attention has been paid to the *actual content* of the proposed curriculum and to the *real scope* of the work already included in schemes of work. Such an approach may account for the reply 'None' given to the whole questionnaire, the correspondent adding that the whole of the Informative Content of Education 'is wholly unsuitable for the immature minds of children of 14–15 years of age.' An *ex cathedra* pronouncement of this kind may denote a reluctance to experiment, and a tendency to assume that all is well in our present curriculum.

In further illustration reference may be made to the sections of History for Grade C, 'The increasing importance of economic changes in History,' and especially to 'The search for competent economic direction.' So expressed, they are, in relation to an elementary school curriculum, almost awe-inspiring; yet judging by the information received there are some senior schools in which both sections are taught and taught effectively.

The Committee are of the opinion, therefore, that further investigation is desirable to discover how far the actual teaching in the schools, regardless of the terminology employed, does cover the various sections of the subjects to which reference is made in the outline of an Informative Content of Education.

Possibly there are parts of the curriculum suggested by Mr. Wells which cannot be taught either because they are beyond the capacity of the pupils or because the time factor will not allow them to be included. In relation to the first of these, the following observations of Sir Richard Livingstone, speaking as President of Section L at Blackpool, may be recalled:

> 'I should like to suggest certain principles which we must observe if our efforts are to be successful, and to which little attention has hitherto been paid. . . . The first of these principles is that education must be

adjusted not only to the natural capacities of the pupil but also to the stage of development which his brain has reached ; that certain forms of study are appropriate to certain ages. That is a platitude. What need then to stress a principle which everyone accepts ? Yet, if accepted, is it remembered by an age which has acquiesced in the idea that most of the population should leave school at 14, and is now comforted by the thought that in future they may not leave it till a year later ? At the ages of 14 or 15 the mind cannot cope with, if it can conceive, the subjects which compose a liberal education and are vital to the citizen. A boy reads literature—" Hamlet " or " King Lear "—and should read them. But what can the profound scepticisms of Hamlet, the passion and agony of Lear mean to him ? He reads history. Can he form a true conception of Charles and Cromwell, Bismarck and Napoleon III ? At 18 we may scan the surface of history and literature, but we cannot see below it. Still more does this apply to the political questions on which an elector has to express an opinion. Unless you believe that these subjects are not meant for the masses and that the voter needs no further education for his duty than experience of life, the newspapers, and the speeches of political candidates, you are admitting the absurdity of an education which stops at 14 or 15.'

In relation to the second reason—the time factor—further inquiry into the actual curricula of the schools is necessary before a final conclusion can be reached ; such inquiry may even involve consideration of the relative importance of various sections of the curriculum as instruments of learning and as a means for the preparation of pupils for life. In the meanwhile an inspection of the returns received shows, as has already been indicated, divergence of practice and many contradictions in the opinions expressed. These were so marked that the following list has been prepared.

GRADE B.—REPLIES FROM JUNIOR SCHOOLS.

History.

Chart.	Speech.	No. of replies which gave information allowing analysis to be made :—			
		Included.	Might be included	Should be excluded.	No reply.
Races of man .	. . . . .	1	2	2	2
Early civilisations.	Story of early civilisation, growth of primary civilisation . .	6	–	–	1
	Developing rôles of priest, king, farmer, warrior .	3	1	2	1
	Succession of stone, copper, iron . .	3	1	1	1
	Introduction of horse .	3	1	1	2
	Construction of roads .	4	1	–	2
	Development of shipping	4	1	–	2
	Rise of Semitic-speaking peoples . . .	1	1	4	1

Chart.	Speech.	No. of replies which gave information allowing analysis to be made :—			
		Included.	Might be included.	Should be excluded.	No reply.
	Coming of Aryan-speaking peoples . .	4	1	–	2
General significance of : Persia . .	Establishment of : Persian Empire .	2	1	2	2
	Macedonian Empire .	2	1	2	2
Greece .	. . . . . .	1	1	2	3
Carthage .	. . . . . .	1	1	2	2
Rome .	Roman Empire . .	2	1	2	2
China . .	. . . . . .	1	1	2	3
America .	. . . . . .	1	1	2	3
Islam . .	Rise and growth of Islam	1	1	3	2
Christianity .	Rise and growth of Christianity . .	5	1	–	1
General idea of break-up of Christendom and	. . . . .	1	1	1	4
Appearance of modern sovereign states .	. . . . .	2	1	1	3
Elem. history of Great Britain .	. . . . .	3	1	–	4
Elem. history of France .	. . . . .	1	1	–	5

Geography.

Types of country .	. . . . .	6	–	–	1
	Precise ideas of type of country . . .	4	–	–	3
Floras and faunas .	. . . . .	6	–	–	1
	Distinctive floras and faunas of main regions	4	–	–	3
General survey of world as human habitat and	. . . . .	6	–	–	.1
	Sort of human life lived in each region . .	4	–	–	3

Chart.	Speech.	No. of replies which gave information allowing analysis to be made :—			
		Included.	Might be included.	Should be excluded.	No reply.
	Knowledge of topography to enable pupils to know the position, reason for position, and kind of places well-known cities are—London, Rio, New York, Rome, Suez .	4	–	–	3
	A little map-reading .	4	–	–	3
Source of power and wealth .	. . . . .	6	–	–	1
Biology, Zoology and Botany.					
Zoology and Botany, including extinct forms and their succession in time . .	. . . . .	1	–	4	2
	Succession of living things in time . .	1	–	4	2
Geological ages	. . . . .	–	–	4	3
General ideas about ecology and evolution :					
	Processes in prosperity, decline, extinction and replacement of species	–	1	4	2
	Story of life from the beginning . .	–	1	4	2
	Emergence of sub-man and gradual emergence of mankind . .	–	1	4	2
Science of Inanimate Matter (Physics and Chemistry).					
Leading up to modern concepts of matter .	. . . . .	1	–	5	1
Mechanism and power .	. . . . .	1	–	4	2

Chart.	Speech.	No. of replies which gave information allowing analysis to be made:— In-cluded.	Might be in-cluded.	Should be ex-cluded	No reply.
Elementary history of invention and discovery .	. . . . .	2	I	3	I
	Foundation of pure physics and chemistry on modern lines . .	I	–	5	I

Physiology.

Chart.	Speech.	In-cluded.	Might be in-cluded.	Should be ex-cluded	No reply.
Physiology and Anatomy. Clear, general ideas of: Animal reproduction	Reproduction . .	–	I	6	–
Plant reproduction. .	. . . . .	–	I	6	–
	Working of our bodies .	I	I	5	–
Elementary pathology:					
	Chief diseases . .	–	–	7	–
	Enfeeblements . .	–	–	7	–
	Accidents – .	–	–	7	–

Grade C.—Replies from Senior Schools.

History.

Chart.	Speech.	No. of replies which gave information allowing analysis to be made:— In-cluded.	Might be in-cluded.	Should be ex-cluded.	No reply.
Elements in political theory		–	I	I	–
Development of existing national and imperial boundaries .	. . . . .	5	–	I	3

Chart.	Speech.	No. of replies which gave information allowing analysis to be made :— In-cluded.	Might be in-cluded.	Should be ex-cluded.	No reply.
	Study of such phenomena as :				
	Rise and fall of Ottoman Empire . .	5	1	–	2
	Rise of Russia . .	4	–	1	2
	History of Baltic .	4	–	1	4
	Rise and fall of Spanish power . . .	2	–	–	5
	Rise and fall of Dutch power . . .	2	–	–	–
	Unification of Germany and Italy . . .	–	2	–	5
	Leading theories of :				
	Individualism . .	–	1	2	4
	Corporate state . .	–	1	2	4
	Communism . .	–	1	3	3
	History of War . .	1	–	2	4
Increasing importance of economic changes in history .	. . . . .	2	–	–	5
Search for competent economic direction	. . . . .	1	1	–	5

Geography.

	Detailed and explicit acquaintance with world geography . .	5	–	–	3
	Different types of population	4	–	–	3
	Developed resources of globe . . .	4	–	–	3
	Undeveloped resources of globe . . .	4	–	1	2
	Devastation of forests .	4	–	–	3
	Replacement of pasture by sand deserts through haphazard cultivation .	4	–	–	3
	Waste and exhaustion of natural resources, coal, petrol, water, etc. .	4	–	–	3

Chart.	Speech.	No. of replies which gave information allowing analysis to be made:— Included.	Might be included.	Should be excluded.	No reply.
Economic Geography .	. . . . .	4	–	–	3
Geology .	. . . . .	3	–	1	3

Social Mechanism.

Chart.	Speech.	Included.	Might be included.	Should be excluded.	No reply.
Short history of communications and trade . .	. . . . .	6	1	–	1
History of innovations in production and manufacture .	. . . . .	6	1	–	1
Rôle of property and money in economic life:					
	Knowledge of conventions of property and money . . .	2	1	3	1
	Way in which money has changed slavery and serfdom into wages and employment . .	2	1	3	1
	Way in which fluctuations of money affect 'industrial windmills' .	–	–	3	5
	Significance of inflation and deflation . .	–	–	3	5

Personal Sociology.

Chart.	Speech.	Included.	Might be included.	Should be excluded.	No reply.
Short history of general ideas :	General ideas of relation of self to universe .	2	–	1	5
Comparative Religion .	Primary propositions of chief religious and philosophical interpretations of the world .	1	1	3	3

Chart.	Speech.	No. of replies which gave information allowing analysis to be made:— Included.	Might be included.	Should be excluded.	No reply.
Study of social types, .	General study of social structure associated with social types .	1	1	3	3
leading to choice of rôle. .	to direct attention to choice of a *métier* .	—	—	2	6

Grade C.—Replies from Preparatory Schools.

As stated in preceding pages, only three replies were received from preparatory schools and departments. One stated that the knowledge and capacity assumed 'seem to me to be absurd,' and gave no replies to questions. The other two gave answers as follows :

1. *History.*

One included 'Rise and Fall of Spanish and Dutch Powers.' Rest of syllabus taken mostly with over-14's.

The other included the same two sections, but said that 'Increasing Importance of Economic Changes in History' and 'Search for Competent Economic Direction' should not be taken.

2. *Geography.*

The first said 'Most is covered'; the second included all except Geology, which, it was stated, should be excluded.

3. *Social Mechanism.*

The first stated that this was taken with the Lower Sixth Form. The second considered that 'Short History of Communications and Trade,' and possibly 'History of Innovations,' might be included; but he considered that the following should not be taken :

'Rôle of Property and Money';

'Knowledge of Conventions of Property and Money';

'Way in which Money has changed Slavery and Serfdom into Wages and Employment';

'Way in which Fluctuations of Money affect "industrial windmills"'; and

'Significance of Inflation and Deflation.'

4. *Personal Sociology.*

Both correspondents stated that the whole of this section should be excluded, one adding that it should only be discussed with well-balanced senior boys having other main interests in life, as 'a boy may easily lose his mental balance.'

GRADE C.—REPLIES FROM SECONDARY SCHOOLS.

History.

Chart.	Speech.	Including section Up to 14.	Including section Over 14.	Might be included Up to 14.	Might be included Over 14.	Should be excluded Up to 14.	Should be excluded Over 14.
Elements in Political Theory	. . . .	I	–	–	–	2	I
Development of existing national and imperial boundaries . .	. . . .	I	I	–	–	2	I
	Study of such phenomena as :						
	Rise and fall of Ottoman Empire . .	2	I	–	–	2	–
	Rise of Russia .	2	I	–	–	2	–
	History of Baltic	–	–	–	–	2	–
	Rise and fall of Spanish power . .	2	–	I	–	I	–
	Rise and fall of Dutch power	2	–	I	–	I	–
	Unification of Germany and Italy .	–	4	–	–	3	–
	Leading theories of :						
	Individualism .	–	–	–	–	3	2
	Corporate state.	–	–	–	–	3	2
	Communism .	–	–	–	–	3	2
	History of War .	–	–	–	–	3	2
Increasing importance of economic changes in history .	. . . .	I	I	–	–	2	I
Search for competent economic direction . .	. . . .	–	I	–	–	2	I
		The other replies did not give definite information on the points, but referred to general schemes, e.g. 'Demands of School Certificate Exam.'					

Geography.

Chart.	Speech.	Including section Up to 14.	Including section Over 14.	Might be included Up to 14.	Might be included Over 14.	Should be excluded Up to 14	Should be excluded Over 14.	Remarks.
	Detailed and explicit acquaintance with world geography .	I	I	–	–	–	–	4 said: 'Practically all taken with pupils below 14.'
	Different types of population	I	I	–	–	–	–	2 said: 'Not in this form.'
	Developed resources of the globe .	–	–	–	–	–	–	I said that these were taken incidentally.
	Undeveloped resources of the globe	–	2	–	–	–	–	
	Devastation of forests, replacement of pasture by sand deserts through haphazard cultivation .	–	2	–	–	I	–	I stated: 'Taken with pupils aged 16 plus.'
	Waste and exhaustion of natural resources, coal, petrol, water, etc. . .	–	2	–	–	I	–	
Economic Geography .	. . . . .	I	I	–	–	–	–	I wrote: 'Some idea of Econ. Geography.'
Geology .	. . . . .	I	I	–	I	I	–	
				(I made no reference to this section.)				

Social Mechanism.

Chart.	Speech.	Including section Up to 14.	Including section Over 14.	Might be included Up to 14.	Might be included Over 14.	Should be excluded Up to 14.	Should be excluded Over 14.
Short history of communications and trade	. . . .	–	2	–	–	1	–
History of innovations in production and manufacture .	. . . .	–	2	–	–	1	–
Rôle of property and money in economic life .	. . .	–	2	–	–	2	1
	Knowledge of conventions of property and money.	–	–	–	–	1	1
	Way in which money has changed slavery and serfdom into wages and employment .	2	–	–	–	1	–
	Way in which fluctuations of money affect 'industrial windmills'. .	1	–	–	–	1	–
	Significance of inflation and deflation . . .	1	–	–	–	1	–
		4 replies made no reference to this part of the curriculum. 1 stated that a good deal of this section was included in the History course.					

Personal Sociology.

Chart.	Speech.	Including section Up to 14.	Including section Over 14.	Might be included Up to 14.	Might be included Over 14.	Should be excluded Up to 14.	Should be excluded Over 14.
Short history of general ideas .	. . . .	–	–	–	–	3	3
	General ideas of relation of self to the Universe . . .	–	1	–	–	2	2

Chart.	Speech.	Including section Up to 14.	Including section Over 14.	Might be included Up to 14.	Might be included Over 14.	Should be excluded Up to 14.	Should be excluded Over 14.
Comparative Religion . .	Primary propositions of chief religious and philosophical interpretations of the world . .	–	1*	–	–	3	3
Study of social types, . .	General study of social structure associated with social types, .	–	–	–	–	2	2
leading to choice of rôle . .	to direct attention to choice of *métier*	–	–	–	1	2	1

4 made no reference to this part of the questionnaire.

* The one stating that Comparative Religion was taken added that it emerged from religious instruction. Another stated that Comparative Religion could only be taken with 17–18 group.

The various questions discussed in the foregoing pages require further elucidation, for which more detailed investigation will be necessary. The Research Committee therefore suggest that permission should be given for the work to be continued during the coming year.

SECTIONAL TRANSACTIONS.

SECTION A.
MATHEMATICAL AND PHYSICAL SCIENCES.

Thursday, August 18.

SYMPOSIUM on *Nuclear physics* (10.0).

Prof. N. BOHR.—*Introduction.*

Due to the extreme facility of energy exchange between the closely packed particles in atomic nuclei, nuclear reactions show certain typical features which differ strikingly from those of ordinary atomic reactions. In particular nuclear transmutations initiated by collisions with heavy particles take place in two well-separated stages of which the first consists in the formation of a semi-stable compound nucleus, where the excitation energy is distributed among the nuclear particles in a similar way to that in a heated body, and the second in the subsequent disintegration of this system or its de-activation by emission of radiation, exhibiting instructive analogies to evaporation or thermal radiation respectively. Similarly the excitation of nuclei by radiation, resulting in the release of heavy particles, suggests a comparison with the well-known phenomena of selective absorption of infra-red radiation by solid or liquid substances. It is shown how these views combined with simple arguments of quantum theory account in a comprehensive way for the experimental evidence regarding such nuclear phenomena.

Prof. W. BOTHE.—*Some results concerning nuclear levels* (10.45).

The general outlines of the spectroscopic investigation of atomic nuclei are briefly given. Some new results are communicated and the following cases of special interest are dealt with in detail :

(1) Resonance levels occurring with (n, α)-reactions.
(2) Connection between the resonance levels in a case of branched reaction.
(3) The nuclear photoelectric effect.
(4) Nuclear levels occurring with the β-decay.

Dr. J. D. COCKCROFT, F.R.S.—*The High-Voltage Laboratory and Cyclotron of the Cavendish Laboratory and their application to nuclear research* (11.30).

The Cavendish Laboratory has recently extended its equipment for nuclear research by the building of a High Voltage Laboratory and a Cyclotron. The High Voltage Laboratory houses a 1·2 million volt D.C.

generator, and a 2 million volt generator is being installed. These generators speed up streams of charged particles for use in transmutation experiments.

The Cyclotron accelerates particles by giving them a succession of impulses as they move in the field of a powerful electromagnet. The magnet of the Cavendish equipment has pole pieces 90 cm. in diameter and should make possible the production of deuterons of energy up to 10 million volts.

The Cyclotron seems likely to find its most important application in providing very strong sources of the new radioactive substances produced by transmutation. It also makes possible a much wider range of transmutations than can be produced by ions of only 2 million volts energy. The D.C. generator, on the other hand, provides much more homogeneous beams of particles and is more suitable for precision work on the details of nuclear processes.

Dr. P. I. Dee.—*Excited states of light nuclei* (11.45).

Recent experimental work which has been carried out in the Cavendish High Voltage Laboratory has given evidence of the existence of many new excited states of certain light nuclei. The bombardment of fluorine with artificially accelerated deuterons, for example, has been shown to result in the production of five homogeneous groups of α-particles, four of which may be associated with the formation of excited states of ^{17}O nuclei.

Evidence in support of the existence of these excited states of ^{17}O has also been obtained by Dr. C. W. Gilbert from cloud track photographs of the disintegration of neon by fast neutrons.

The energies of excitation of a number of other light nuclei have been determined by the investigation of the excitation functions of the γ-rays which result from processes of proton capture. The resonance character of the excitation of the γ-radiation which results from the capture of protons by carbon, for example, has been shown to be more complex than had previously been supposed. An intense production of γ-rays at a proton energy of 560 K.V. has been proved to be due to the capture of protons by ^{13}C, which results in the formation of excited ^{14}N nuclei having an energy of excitation of about 8·0 M.V.

Dr. N. Feather. *Some neutron-produced radioactivities* (12.05).

The radioactivities of various substances irradiated by the neutrons produced by bombarding lithium by deuterons have been studied by a combination of the absorption and coincidence methods, using tube counters. Information has been obtained regarding certain cases of nuclear isomerism and also regarding the long-lived products formed by bombarding thorium. The β and γ radiations from a number of other radioelements have also been investigated.

Dr. E. Bretscher and Mr. J. V. Dunworth have collaborated in the experimental work, and members of the personnel of the Cavendish High Voltage Laboratory have been responsible for carrying out the irradiations.

General Discussion (12.25) (*continued on Friday afternoon*).

Afternoon.

Visits to (*a*) Cavendish and Mond Laboratories; (*b*) Mathematical Laboratory (for details see under Department A*). Film illustrating solar prominences.

Friday, August 19.

PRESIDENTIAL ADDRESS by Dr. C. G. DARWIN, F.R.S., on *Logic and probability in physics* (10.0). (See p. 21.)

Prof. H. SHAPLEY.—*Metagalactic gradients and the expanding universe hypothesis* (11.20).

The evidence for important metagalactic density gradients, which are of sufficient magnitude to invalidate the assumption of uniformity throughout the regions of space now attainable, is derived from surveys of the population and distribution of galaxies within a radius of over one hundred million light-years. The surveys cover extensive areas of the sky and display large-scale structural features of the metagalaxy.

The character of the inner metagalaxy is demonstrated by the distribution of galaxies brighter than the thirteenth magnitude over the whole sky and by preliminary results on the distribution down to the fifteenth magnitude in the south galactic cap. The extension into southern declinations of the great cluster of bright galaxies in Virgo adds information on large-scale irregularity. Methods are shown of analysing distribution in both high and low galactic latitudes. Regions near the Milky Way plane, in which external galaxies are numerous, are studied for a determination of the extent of absorption within our own galactic system. Surveys involving 200,000 galaxies in the equatorial and galactic polar caps are presented.

Density differences between the north and the south sides of the Milky Way are found to be considerable. The radial gradient discussed by Hubble is compared with similar gradients across the sky, especially with that which is found to extend over 125° across the south galactic cap. This gradient presents such conspicuous density changes that it demonstrates the impossibility of using the observed distribution of galaxies to derive a coefficient of expansion of the universe. It is clear that such large-scale irregularities are an important feature of the metagalaxy and must be considered in cosmological theories.

Prof. R. W. WOOD.—*Diffraction gratings for astrophysical purposes* (12.0).

Recent improvements in the technique of ruling gratings have made possible the concentration of 85 per cent. of the incident light in the first order spectrum, with a ruling of 15,000 lines to the inch. Two plane gratings ruled on 8-inch aluminised pyrex discs, one concentrating in the first, the other in the second order, are now in constant use in the spectrograph of the 100-inch telescope at Mount Wilson, and have proved superior to prisms, especially in the ultra-violet.

Measurements have been made, with a photronic cell and monochromatic light of various wave-lengths, showing the distribution of intensity for the central image and various orders, at different angles of incidence. The central image may contain as low as 1 per cent. of the total light.

Large replicas have been made giving equally high concentration and these are being used at the Harvard Observatory for the determination of star colours, the grating, covered by a purple filter, being mounted a few inches in front of the photographic plate. A new attachment to the dividing engine makes possible the ruling of large concave gratings of very short focus with a groove of constant shape over the entire area, thus abolishing what I have called the ' target pattern ' (circular zones of low efficiency).

Dr. H. E. Ives.—*The rate of a moving atomic clock* (12.30).

According to the theory of Larmor and Lorentz, a moving clock should assume a slower rate than a stationary one. It was pointed out by Einstein in 1907 that the newly discovered Doppler effect in canal-rays offered a means of testing this prediction, but this test has been commonly considered as beyond experimental practicability. This objection has recently been removed, owing to the development by Dempster of a new design of canal-ray tube.

The present investigation is an experimental test of the Larmor-Lorentz prediction, using these tubes. The hydrogen line 4861 A.U. has been used, observations being made by means of a plane grating of 15,000 lines to the inch, made by Professor R. W. Wood. The method of observation gave on one plate, the lines due to motion in opposite senses. The experiment gave a positive result, showing shifts which are independent of the orientation of the apparatus, and which agree, within the limits of experimental error, with the theoretical values.

On the assumption of a stagnant ether, this experiment, with that of Kennedy and Thorndyke, establishes the reality of the Larmor-Lorentz variation of clock rate, and the Fitzgerald contraction.

(CONCURRENTLY WITH ABOVE PAPERS.)

Symposium on *Magnetic alloys and X-ray structure* (11.20).

Prof. W. L. Bragg, O.B.E., F.R.S.—*Introduction.*

The magnetic properties of materials used for technical purposes have been improved in the most remarkable way in recent years. Better alloys have been discovered with a high permeability for small magnetising forces, or with low hysteresis loss when subjected to magnetic cycles, or with a high coercive force when used for permanent magnets. At the same time, the theory of magnetism has made rapid advances in the hands of the theoretical physicists, and X-ray methods of determining the atomic arrangement in these materials have been improved. The present position is very interesting because it is to be hoped that theory may now begin to play a part in technical achievement.

In this introduction to the discussion a brief survey is made of the magnetic properties of materials, and suggestions put forward as to the lines along which improvement may be expected.

Dr. E. C. Stoner, F.R.S.—*The general theory of ferromagnetism* (11.40).

In the Weiss treatment, which provides a qualitatively satisfactory formal correlation of many properties of ferromagnetics, it is postulated that the elementary magnets are acted on by a molecular field, equivalent in effect to a magnetic field proportional to the intensity of magnetisation. Such a field will give rise to spontaneous magnetisation below a critical temperature, the Curie temperature, and to paramagnetic behaviour above it. The spontaneous magnetisation normally extends unidirectionally over only small regions, domains, and the effect of an external field is to align the directions of magnetisation of the domains.

The elementary magnets in ferromagnetic metals are electron spins, and the molecular field has been satisfactorily interpreted as arising from quantum mechanical interchange interaction. The fundamental problems are those of accounting for the number of effective spins per atom, and the magnitude of the interchange interaction in different materials, and of developing a quantitatively satisfactory treatment of the temperature variation of magnetisation and related effects.

Of most importance technologically is the behaviour of ferromagnetics in relatively low fields. The determinative factors for the sequence of reversible and irreversible processes occurring during magnetisation include the natural crystal anisotropy, the magnetostrictive properties of the material, and the distribution and magnitude of internal strains. Although a qualitative interpretation can be given of the main effects, the development of a quantitative treatment for particular materials is as yet at an early stage.

Dr. A. J. Bradley.—*X-ray structure and ferromagnetism* (12.10).

Ferromagnetic alloys contain iron, cobalt or nickel (in the Heusler alloys, manganese). They have crystal structures of a simple type :—face-centred cubic, body-centred cubic and (rarely) hexagonal close-packed. Of these types the body-centred cubic is the most favourable for the development of magnetic properties. Some non-magnetic alloys become magnetic when the structure is changed to body-centred cubic from another form. The materials used for permanent magnets (known as 'hard' magnetic materials) must be distinguished from 'soft' magnetic materials such as are used for transformer cores. The latter have well-formed crystals, the structures of which are in no way abnormal. The former, though essentially of the same types (body-centred cubic and face-centred cubic), never have perfectly formed crystals. Some kind of strain is essential for the development of high coercive force. For example the alloy may be on the point of breaking up into two phases of different compositions. The mechanism of this process may be such as to produce an intermediate metastable state. The alloy remains a permanent magnet so long as this state persists. Careful heat treatment is required to ensure that decomposition proceeds only to the point where the alloy has the best magnetic properties.

(Continued below).

Afternoon.

Symposium on *Nuclear physics* (continued from Thursday) (2.15).

Prof. R. Peierls.—*Resonance in high energy reactions.*

Mr. S. Devons.—*Resonance scattering of α particles.*

Prof. C. D. Ellis, F.R.S.—*Resonance levels in slow neutron processes.*

Dr. P. B. Moon.—*A slow neutron velocity spectrometer.*

Prof. E. J. Williams.—*Loss of energy by fast particles in nuclear collisions.*

Dr. W. E. Burcham.—*Disintegration of fluorine by protons and deuterons.*

Dr. M. Goldhaber.—*Radioactivity produced by nuclear excitation.*

(CONCURRENTLY WITH ABOVE.)

SYMPOSIUM on *Magnetic alloys and X-ray structure* (continued) (2.15).

Dr. W. SUCKSMITH.—*The variation of magnetic saturation intensities with temperature in the iron-nickel-aluminium system.*

In view of the necessity for making measurements on a large number of alloys of widely varying physical properties, a new method of measuring the saturation intensity (in fields up to 18,000 gauss) from liquid air temperatures up to the Curie temperature, has been developed. The method requires only about $\frac{1}{20}$th gram of the alloy, and is not dependent upon the shape of the specimen. The measurements are equally valid for materials ranging from coarse powders to roughly cut cylinders.

Measurements have been made on alloys of which the X-ray structure has been investigated by Bradley and Taylor, the same specimens having been utilised through the collaboration of Dr. Bradley.

Some of the multiphase regions have been investigated, and the results show that the phase boundaries as determined by magnetic methods conform closely to those given by the X-ray data. The different regions usually have characteristic properties, and the effect of heat treatment upon structure changes can be followed quite closely by observation of the magnetic saturation intensity at different temperatures.

Mr. D. A. OLIVER.—*Martensitic permanent magnet steels and dispersion-hardening alloys* (2.45).

The structures of martensitic permanent magnet steels and dispersion-hardening alloys are illustrated by a selection of photomicrographs. The necessity for X-ray examination is stressed. A summary of the important magnetic properties is given with special reference to those alloys which are of commercial importance. The effect of impurities on magnetic properties is discussed and recent data on the effect of carbon presented. Mention is also made of the improved magnetic properties which can be obtained when these magnetically hard alloys are cooled in a magnetic field. A few experiments are carried out illustrating either the properties or the applications of the newer permanent magnet alloys.

GENERAL DISCUSSION (3.15).

Visit to works of Cambridge Instrument Company.

Saturday, August 20.

SYMPOSIUM on *High-altitude cosmic radiation* (10.0).

Prof. P. M. S. BLACKETT, F.R.S.—*Introduction.*

Prof. W. H. FURRY.—*A discussion of some recent experiments on the properties of cosmic ray particles* (10.45).

It is now realised that most cosmic ray showers can be explained by the assumption that electrons multiply by radiative collisions and pair production as required by the present radiation theory. The most conclusive evidence comes from cloud chamber photographs obtained by Fussell.

Three layers of lead in the chamber are spaced so that the successive stages of the multiplication process can be seen. Of two thousand showers photographed, three were of a markedly different type, diverging from a point at more or less random angles and containing heavy particles ; these cannot be explained by the multiplication hypothesis.

It has been known since the experiments of Bothe and Kolhörster and of Rossi that cosmic rays contain single corpuscles of much greater penetrating power than the radiation theory allows for electrons. More detailed information about the penetrating power has been obtained by Street and Stevenson. Two cloud chambers are used ; in one the momentum is measured by the curvature in a magnetic field, and in the second the penetration through layers of lead is observed. A number of particles are found which must be supposed to be neither electrons nor protons. Various observers have obtained tracks from which the mass could be estimated by the density of ionisation ; values obtained are about two hundred times the electron's mass.

Prof. W. Bothe.—*New results in cosmic rays* (11.30).

Dr. E. J. Williams.—*The heavy electron* (11.45).

General Discussion (12.0).

Dr. R. W. Wood.—*Crystal growth* (film) (12.30).

Dr. K. T. Fischer.—*The temperature coefficient of balances* (12.45).

Afternoon.

Visit to the Observatory, Solar Physics Observatory, Pendulum House, and Cavendish Field Laboratory.

Monday, August 22.

Discussion on *Low-temperature physics, with special reference to Helium II* (10.0).

Dr. H. B. G. Casimir.—*Introduction : Low temperature properties of matter.*

The problems of low temperature physics can be divided into two groups : those depending exclusively on the motion of atoms and molecules as a whole and those connected with the inner degrees of freedom of the atom. The study of electrons in metals and of paramagnetism are the two most important examples of this second group. In the limit of very low temperatures ($T < 4°$ K) the properties of a non-conducting non-paramagnetic solid are comparatively simple. The heat motion can be described as a superposition of sound waves ; the specific heat is proportional to T^3 and can be calculated from the elastic constants. Also the theory of heat conduction becomes very simple.

It is to be expected that at this limit the interaction between the lattice and the inner degrees of freedom will also be simplified. The theory of electrons in metals leads to the result that the resistance due to interaction with lattice vibrations decreases very rapidly ($\sim T^5$). In the case of paramagnetism, the lattice vibrations come into play only in so far as they must establish the temperature equilibrium between spin and the surroundings; the mechanism of this process is not completely understood.

The case of HeII shows that in problems of the first group there are also interesting difficulties.

Dr. J. F. Allen.—*The properties of liquid Helium II* (10.30).

Liquid helium is a substance which differs most remarkably from any other liquid. The phase diagram of helium possesses no triple point for equilibrium between gas, liquid, and solid. Instead, as far as one can ascertain, the liquid phase persists down to the Absolute Zero, and the liquidus and solidus curves become parallel at that temperature. The liquid phase consists of two modifications, and the transformation between them occurs at 2·19° K (the λ-point). The modifications, called HeI and HeII, are totally different phenomenologically. HeI is a normal liquid, while HeII possesses properties completely different from those of any other known substance. The most striking phenomena exhibited by HeII are as follows:—A negative temperature coefficient of expansion; a very high specific heat which suffers a discontinuity at the λ-point; a thermal conductivity which is approximately five hundred times as great as that of copper at room temperature; and a mode of heat transport which appears to involve a transfer of momentum. When one measures the viscosity of HeII by means of a rotating disc, one obtains a value of 10^{-5} CGS units, i.e. comparable to a gas. On the other hand, when measured by the flow method the viscosity becomes immeasurably small and is certainly less than 10^{-10} CGS units. So far no comprehensive theory has been developed to explain all of the properties of HeII.

Prof. J. H. Van Vleck.—*The molecular field and the determination of very low temperatures* (11.15).

In experiments on magnetic cooling, it is customary to determine the temperature by assuming that the susceptibility obeys Curie's law $\chi = C/T$. Actually, this law cannot hold because of (a) the Stark splitting of energy levels caused by the crystalline fields from the non-magnetic atoms surrounding the paramagnetic ion and (b) the dipole-dipole and perhaps exchange forces coupling together paramagnetic ions. The effect (a) is wanting in $CsTi(SO_4)_2 12H_2O$, while (b) disappears at infinite magnetic dilution. Before reliable determinations of the temperature can be made from susceptibility (in distinction from thermodynamic) measurements, it is necessary to devise an adequate theory of (a) and (b). Difficulties in the way of doing this, as well as the progress so far made, are summarised. In particular, the usual Lorentz field $H + 4\pi M/3$ is only an approximate representation of (b) valid only if the temperature is not too low. It is not even clear whether dipole-dipole forces can ever make a body become ferromagnetic. A discussion is included of the analogous electrical case, where possibly the hypothesis of hindered rotation may not be necessary to prevent spontaneous polarisation in isotropic dielectrics. Ultimately, magnetic cooling experiments should throw considerable light on inter-molecular forces in the solid state.

Dr. F. Simon.—*Experiments below* 1° *abs.* (11.45).

The experiments carried out by Dr. Kurti, Dr. Lainé, Dr. Squire and the author with the magnet at Bellevue (Paris) are described.

The experiments were chiefly concerned with the study of the 'ferromagnetic' properties of iron-ammonium alum at very low temperatures, which were also extended to diluted salts and to an investigation in additional fields. The reasons which may be responsible for this 'ferromagnetism' are discussed. A new method for establishing temperatures on the absolute scale is described.

Discussion continued in afternoon (see below).

Sir J. J. Thomson, O.M., F.R.S.—*Some recent experiments on electronic waves* (12.15).

(concurrently with above session.)

Joint Discussion with Section G (Engineering) on *Fundamental magnetic measurements with special reference to incremental conditions* (10.0). (See under Section G.)

Afternoon.

Discussion on *Low temperature physics* (continued) (2.15).

Dr. K. Mendelssohn.—*Recent developments in superconductivity.*

Dr. E. T. S. Appleyard.—*The superconductivity of thin mercury films.*

Dr. H. Jones.—*The superconductivity of alloys.*

Dr. N. Kurti.—*Experiments below* 1° *absolute.*

Mr. E. S. Shire.—*Paramagnetic relaxation below* 1° *absolute.*

Dr. H. B. G. Casimir.—*Spin-lattice interaction.*

Mr. J. Ashmead.—*The production of intense magnetic fields for magnetic cooling experiments.*

Mr. J. G. Daunt.—*New experiments on the transfer effect.*

Dr. H. London.—*Investigation of liquid helium II by a Knudsen manometer.*

Dr. E. Ganz.—*The thermal conductivity of liquid helium II under pressure.*

Mr. A. H. Cooke.—*The attainment of low temperatures by pumping off liquid helium.*

Mr. Pickard.—*The construction of a standard expansion liquefier.*

Visit to works of British Tabulating Machine Co., Letchworth.

Tuesday, August 23.

SYMPOSIUM on *Seismology* (10.0).

Dr. F. J. W. WHIPPLE.—*Report of the Seismological Committee.*

Mr. J. S. HUGHES and Miss E. F. BELLAMY.—*The International Seismological Summary* (10.20).

Prof. O. T. JONES, F.R.S.—*Introduction to discussion* (10.40).

The purpose of the remarks is to emphasise the importance of seismological studies in their application to purely geological problems. Two such problems are of particular interest to Cambridge geologists, the depth of the Palæozoic floor and the course and depth of certain deep-buried channels, in particular that which runs through Chesterford and Newport, and has not hitherto been traced further. The Palæozoic floor is known directly only in borings, the nearest of which was about thirty miles from Cambridge. Dr. Bullard took up the determination of its depth by seismic methods, and an account of the results will be given by Mr. Gaskell. It is suggested that explosions made in quarrying may be utilised for similar purposes in many parts of the country.

The investigation of the elastic properties of rocks is also of interest both to seismologists and geologists. The author suggested to Dr. Phillips the study of those of Coal Measure strata, and in the course of some brilliant work he obtained much more definite information about elastic after-working than had previously been obtained. The results may have an important bearing on the operation of stresses responsible for earthquakes and their aftershocks. If this is so it gives another illustration of the advantage of mutual co-operation between geologists and seismologists.

Miss I. LEHMANN.—*Characteristic seismograms at different distances* (11.20).

Dr. D. W. PHILLIPS.—*Imperfections of elasticity in rocks* (11.40).

A study of the properties of Coal Measure rocks when subjected to different kinds of forces revealed considerable departure from truly elastic behaviour. Very many examples of sandstones, siltstones, mudstones, shales and coals have been subjected to examination in compression, bending and torsion.

Both the longitudinal and lateral deformations were measured simultaneously on cylinders of these rocks when subjected to compression. For the first two or three cycles of loading and unloading there was a progressively decreasing set, then for each succeeding cycle there was complete recovery of strain though the stress-strain curve always exhibited ' hysteresis loop.' There was a pronounced increase in both the longitudinal and lateral deformations when the load was maintained constant. At low loads the lateral time strain was small in comparison with the longitudinal time strain, but as the load increased the lateral time strain became equal to, and sometimes exceeded, the longitudinal time strain. Similar time effects were observed during unloading ; the strains removed immediately on the reduction of load were followed by a further gradual recovery. The elastic modulus and Poisson's ratio usually increased with increase in the load applied.

When subjected to bending these rocks required, as in compression, two or three cycles of loading and unloading to remove a set on no load. Further cycles resulted in complete recovery, with a hysteresis loop. Very pronounced increase in deformation took place when the load on the rock beams was maintained constant, and in some rocks these time strains increased with an increase of load up to a certain load, the time strain for higher loads progressively decreasing.

In a few cases the further deformation with time had been allowed to progress until finally the beams fractured, some under loads which were less than the loads, applied without allowing time effect, necessary to fracture similar beams cut from the same rock samples.

When subjected to torsion these rocks exhibited a time effect as in the case of compression and bending.

Mr. T. F. Gaskell.—*Seismic exploration of eastern England* (12.0).

The surface rocks of East England consist of Jurassic and Cretaceous clays and chalk. It has long been known that these are underlain by a planed-off surface of Palæozoic rocks, but the depth of this surface was only known at a few isolated points. The refraction method of seismic prospecting has been applied to map this surface. Charges of gelignite up to 15 lb. are used to make impulses in the ground, and six electrical seismographs record the arrival of the waves produced. The recording apparatus is transported in a van, and depths of the Palæozoic have been determined at stations distributed over a large area of Eastern England.

Afternoon.

Symposium on *Seismology* (continued) (2.20).

Dr. H. Jeffreys, F.R.S.—*Deep foci and aftershocks.*

The work done by Dr. Phillips throws light on some difficult seismological questions. Two hypotheses concerning the mechanical properties of rocks are that of a finite viscosity at small stresses, or zero strength, and that of an infinite viscosity at small stresses, or finite strength; the former is associated most prominently with the name of Wegener, the latter with that of Barrell. The latter agrees better with the distribution of gravity anomalies, and with the fact that stresses capable of producing major earthquakes can develop at depths down to about 700 km. It has not, however, been easy to see how the idea of perfect elasticity up to a definite limiting stress can be reconciled with the existence of aftershocks continuing for months after the main shock. Dr. Phillips's work shows how this can be done, by the recognition of the distinction between the stress that leads to immediate fracture and that which leads to fracture only if it is left on long enough.

The intensive study of deep focus earthquakes is likely to lead to solutions of some difficult seismological problems that are almost insoluble from the data of normal shocks, namely the depth of the core, the nature of the 20° discontinuity, and the times of the transverse wave up to distances of about 25°.

Dr. R. Stoneley, F.R.S.—*Times of travel of the L phase* (2.50).

It was found by Prof. H. H. Turner that L readings given by stations have travel times equivalent to 0·48 min./degree, but that sometimes onsets corresponding to 0·41 min./degree predominate. The former correspond to the arrival of long Rayleigh waves, the latter to long Love waves. An analysis of a number of earthquakes listed in the *I.S.S.* shows a marked separation of the L readings into two groups, with travel-times clustering round the above values. The relative proportions of the two types suggest intrinsic differences in the dislocations that initiate the waves. Differences are shown for earthquakes in the same region, so that the effect cannot be attributed entirely to the distribution of stations or of land and sea. To ascertain the dependence on intensity of shock a reliable measure of intensity would be needed.

Prof. J. D. Bernal, F.R.S.—*Crystallographic relations of seismology* (3.10).

A possible explanation of the 20° discontinuity is to be found in the hypothetical existence at lower levels of the earth's crust of a denser and more elastic crystal form of olivine $(MgFe)_2SiO_4$. Such a form has never been observed owing to the impossibility of producing sufficiently high pressure, but it might be expected that the condition of silicates at high pressure should be shown by germanates at ordinary pressures, owing to the greater size of the germanium ion. Magnesium germanate has been observed to exist both in olivine structure and in the cubic spinel type of structure. A calculation based on this structure would seem to be able to account in a rough quantitative way for the properties required for the substances at low levels.

DEPARTMENT OF MATHEMATICS (A*).

Thursday, August 18.

Symposium on *Newtonian root evaluations* (10.0).
Chairman: Dr. J. Wishart.

Prof. A. Ostrowski.—*On Newton's method of approximation* (10.0).

To compare the amount of work in Newton's method of approximation for a root ζ of the equation $f(z) = 0$ with the effect of the method, a unit of the calculatory work is introduced—a *Horner*, that is the work of calculating the value of $f^{(\nu)}(z)$, this unit being assumed as sensibly independent of ν and the number of digits in z.

On the other hand, the rate of the approximation of ζ by γ is measured by the order of $|\gamma - \zeta|$, if the initial value x_0 tends to ζ, the order of $|x_0 - \zeta|$ being assumed as 1.

Newton's method gives with 6 Horners an approximation of the order 8. A modification of Newton's method is proposed allowing to obtain with 6 Horners an approximation of the order 16. It consists in using alternately the two formulæ:

$$x_1 = x_0 - \frac{f(x_0)}{f'(x_0)}, \qquad x_2 = x_1 - \frac{f(x_1)(x_1 - x_0)}{2f(x_1) - f(x_0)}.$$

Prof. E. H. Neville.—*Computational labour in modifications of Newton's method of approximation* (10.30).

The processes of interpolation have been developed on the assumption that the arguments for which they are to be used may be anywhere in the interval between consecutive tabular entries. Root-extraction, treated otherwise than as inverse interpolation, is a step-by-step determination of a sequence $x_1, x_2, x_3, \ldots$ To obtain x_{n+1} from $x_1, x_2, \ldots, x_n$ and functional values at these points by inverse interpolation of the ordinary kind is to ignore the possibility of profiting from the circumstance that x_{r+1} is much nearer to x_r than to x_{r-1}; on the other hand, in Newton's formula and in any slight modification of it, the labour of each step is apt to be considerably greater than the labour of a linear interpolation, and since we determine x_{n+1} from x_n only, we are continually abandoning information which we have been at pains to acquire. We need not balance disadvantages: Prof. Ostrowski's method is one compromise which retains some of the advantages of each extreme; another, simpler and in the long run more efficient, is the recurrent use of the one formula

$$x_{n+1} = x_n - m_{n,n-1}y_n + \frac{(m_{n,n-2} - m_{n-1,n-2})y_n y_{n-1}}{y_n - y_{n-1}}$$

where $m_{r,s} = (x_r - x_s)/(y_r - y_s)$.

Mr. D. H. Sadler.—*The estimation of computational labour* (10.40).

The difficulties of absolute estimation are summarised, and the care that must be taken in forming relative estimates is stressed. Illustrations are given in the simple case of computing a polynomial expression.

Symposium on *Combinatorial mathematics in the design of experiments* (11.10).

Chairman: Prof. R. A. Fisher, F.R.S.

Dr. C. C. Craig.—*Some remarks on randomisation* (11.10).

The usefulness and validity from the point of view of fiducial probability of significance tests based on the principle of randomisation is well recognised. However, it seems of some interest to the author to illustrate how the effectiveness of such a test may depend on the populations from which, in fact, the samples were drawn. In particular, suppose two samples of N are drawn, one from each of two normal populations with equal variances but unequal means. By sampling methods, the probability that the test based on randomisation will indicate that the population means differ is studied.

Mr. H. W. Norton.—*The* 7×7 *Latin squares* (11.30).

A discussion of 7×7 Latin squares leading to Græco-Latin squares, and of the enumeration of the 7×7 Latin squares.

Dr. W. J. Youden.—*Complex square designs in plant physiology and their connection with incomplete randomised blocks* (11.50).

In recent years the use of experimental designs based on the combinatorial properties of numbers has been developed in plant physiology and pathology as in other fields. The natural structure of experimental plants makes it desirable to eliminate causes of variation due both to the individuality of plants, and to leaf order, using a double elimination as in the Latin Square. In addition the principle of balanced incomplete blocks is needed owing to the limited number of leaves. A group of designs combining the two qualities, and which have proved useful in practice, is exhibited. The remaining unsolved combinatorial problems are indicated.

Mr. F. Yates.—*The use of lattice squares in plant improvement* (12.10).

Efficient methods of comparing, under field conditions, large numbers of new varieties produced by genetical segregation are a vital need in practical plant improvement. The new quasi-factorial and allied designs give methods of considerably increasing the accuracy of the field comparisons, using the same amount of experimental material, and supersede the older methods of arrangement, such as the use of ' control ' varieties. Many of these designs depend on the existence of Græco-Latin and higher order orthogonal squares, and the question of the existence of such squares, first investigated by Euler, has therefore become of practical importance. In particular the existence of complete sets of orthogonal squares is necessary for the construction of designs in lattice squares (which enable p^2 varieties to be arranged in squares of side p, eliminating fertility differences between rows and between columns) and for the construction of certain types of incomplete randomised blocks (i.e. randomised blocks each of which contains only a proportion of all the varieties to be compared).

Mr. W. L. Stevens.—*Completely orthogonalised squares* (12.30).

It is known that for certain values of p, $(p - 1)$ Latin squares may be formed such that any two of the squares are mutually orthogonal. Solutions are now known for $p = 2, 3, 4, 5, 7, 8, 9$, and any prime number. It is believed that a solution exists when p is any power of a prime. The case proved is for the square of any prime, and the theory has been applied to develop a completely orthogonalised square of side 25.

Afternoon.

Visit to the Mathematical Laboratory. Symposium and Demonstration on *Mechanical methods of computation* (3.0).

Prof. J. E. Lennard-Jones, F.R.S.—*Bush differential analyser.*

Mr. M. V. Wilks.—*Mallock machine* (3.20).

Demonstration of Bush and Mallock machines (3.30).

Dr. J. Wishart and Mr. D. H. Sadler.—*Description and demonstration of Hollerith and National machines* (4.45).

Friday, August 19.

Symposium on *From function to printed table : some aspects of the work of preparing a table of a mathematical function* (11.30).
Chairman : Prof. E. H. Neville.

Dr. W. G. Bickley.—*Computation from series and by recurrence formulæ.*

Some elementary considerations concerning computation by power series, especially with regard to labour saving and checking, are discussed.

For greater values of the argument, convergence of the power series is delayed, and asymptotic series must be used. In straightforward application, the accuracy is definitely limited by the size of the smallest term. Two means of increasing the accuracy, the Euler transformation and the convergence factor, are described.

Recurrence formulæ are very useful, but they usually lose accuracy when used in one direction. This fact can upon occasion be turned to advantage.

Two outstanding difficulties, with some slowly convergent series, and with asymptotic series whose terms are all of the same sign, remain.

Dr. J. C. P. Miller.—*Step-by-step integration of a differential equation, with some remarks on interpolation* (12.0).

Expansions in series, whether convergent or divergent, are often inconvenient for the systematic tabulation of a solution of a differential equation for some ranges of the argument, although they are indispensable as checks. Hence, in order to compute pivotal values, i.e. values which form a basis for subsequent subtabulation, we frequently use step-by-step processes over some part of the range. Two such processes are briefly described, namely the Double Summation and Taylor Series methods, as applied to certain second order equations.

Methods of checking and ways of estimating and minimising cumulative errors, as well as the application of the Taylor Series method to interpolation, are also considered.

Dr. A. J. Thompson.—*The printing of mathematical tables* (12.30).

The paper describes the several processes that come between the completion of the calculation of a mathematical table and its appearance as a printed volume. *Inter alia*, it deals with the preparation of the printer's copy, with typographic details (such as choice of type, spacing and rules) and with the methods of ensuring accuracy. The standpoint is that of the computer of the table, and technical matter is reduced to a minimum. The paper is illustrated by photographs of a number of tables.

Monday, August 22.

Prof. G. D. Birkhoff.—*Analytic deformations* (10.0).

Prof. S. Lefschetz.—*Fixed points of transformations* (11.15).

The author first points out by examples the rôle of the problem in various mathematical disciplines. If the elements transformed are points of an abstract space R we are dealing with a problem in topology. Suppose that we have a transformation T of R into itself. Under certain very general conditions (C) if T is a transformation of R into itself there may be given a topological character θ (T) having the property that when $\theta \neq 0$ there is at least one fixed point. Conditions (C) embrace continuous single-valued transformations of a polyhedron into itself, and more generally of a very broad class of locally connected spaces (absolute neighbourhood retracts when R is compact metric). Special noteworthy case: R is the Hilbert parallelotope. For all these cases an explicit expression of θ may be given in terms of the transformations which T induces on the cycles of the space.

Prof. W. V. D. Hodge, F.R.S.—*Some applications of harmonic integrals* (12.0).

On an analytic variety which is an absolute orientable manifold a p-fold integral which is exact has the property that its value taken over a bounding p-cycle is zero, but it may have a non-zero value when taken over a p-cycle which is not homologous to zero; this we call the period of the integral on the cycle. It is known that if R_p is the pth Betti number of the manifold there exist exact integrals which have arbitrarily assigned periods on R_p independent p-cycles. If the manifold carries a Riemannian metric

$$g_{ij}dx^i dx^j$$

we can associate with a p-fold integral

$$\frac{1}{p!}\int P_{i_1 \ldots i_p} dx^{i_1} \ldots dx^{i_p} \tag{1}$$

an $(n-p)$-fold integral

$$\frac{1}{p!(n-p)!}\int \sqrt{g}.\,\varepsilon_{i_1 \ldots i_p j_1 \ldots j_{n-p}} g^{i_1 k_1} \ldots g^{i_p k_p} P_{k_1 \ldots k_p} dx^{j_1} \ldots dx^{j_{n-p}} \tag{2}$$

If the integrals (1) and (2) are both exact we say that (1) is a harmonic integral. It follows that (2) is also harmonic. It is known that there exists exactly one p-fold harmonic integral having assigned periods on R_p independent p-cycles of the manifold.

In a mathematical theory in which an analytic manifold appears it is often possible to assign a Riemannian metric to the manifold in such a way that the harmonic integrals prove useful weapons in the development of the theory.

Dr. B. Kaufmann.—*Topological methods in the theory of conformal representation* (12.45).

In the theory of conformal representation there are two main groups of problems: the ' inner ' problems concerning mappings of plane regions, and the problems on boundary relations. The most important result in the first group is Riemann's fundamental theorem, and in the second Fatou's theorem. The development of the theory in these two directions has led to two well-known topological conceptions: the Riemann surface, and ideal elements (prime ends). Through Hilbert's work at the beginning of this century it became possible to extend the inner mapping theorems to regions of arbitrary connectivity (Hilbert, Köbe). But the boundary problems in the general case remained unsolved. However, the general theory of ideal elements makes it possible to approach these problems. The existence as well as the nature and the structure of the ideal elements is revealed by a close study of a certain 0-dimensional group of limit cycles in an n-dimensional region by methods of an appropriate homology theory. This group can be turned into a certain abstract space in which the ideal elements can be seen and described. With the help of a method of canonical dissections of regions of infinite connectivity (which might be called Souslin dissections) some first results on boundary relations are obtained. These ultimate results can be understood without any knowledge of the theory of ideal elements.

Afternoon.

Visit to works of British Tabulating Machine Co., Letchworth.

Tuesday, August 23.

Prof. A. Speiser.—*Elliptic functions from an elementary standpoint* (10.0).

It is a well-known theorem, that a simply connected Riemann surface may be transformed conformally on the Euclidean plane or on the interior of a circle. With the aid of this fact it is proved that the general theory of elliptic integrals consists ultimately in the possibility of paving the plane with congruent quadrilaterals of any shape.

Dr. B. H. Neumann.—*General decompositions of groups* (10.30).

A group G is said to be the *general product* of its sub-groups A and B whenever (i) $A.B = G$; (ii) $A \cap B = \{1\}$. A and B are called *general factors* or *complementary* sub-groups. In the special case, when both factors are self-conjugate in G, we have the well-known *direct product*.

The following problems have been attacked, the first with a view to applications in geometry:

(i) Given G and a sub-group A; to decide whether A is a general factor of G and to find complementary factors.

(ii) To characterise the groups in which certain types of sub-groups (e.g. the Sylow sub-groups, or all sub-groups) are general factors (P. Hall).

(iii) Given A and B, to find their general products.

However, much remains to be done, specially as regards the third problem.

Mr. P. Hall.—*The verbal classification of groups* (11.15).

Let $f(x_1, \ldots, x_n)$ be any word, G any group, $V = V_f(G)$ that sub-group of G which is generated by all elements of the form $f(a_1, \ldots, a_n)$, where the a's are arbitrary elements of G. Let $W = W_f(G)$ be the (unique) greatest self-conjugate sub-group of G with the property that

$$f(a_1b_1, \ldots, a_nb_n) = f(a_1, \ldots, a_n)$$

for every choice of a's in G and b's in W. Then, if V′ and W′ are the corresponding sub-groups of another group G′, the latter is said to be *isological* with G (in respect of f) if there exists between G/W and G′/W′ an isomorphism, $aW \rightarrow a'W'$, such that the correspondence $f(a_1, \ldots, a_n) \rightarrow f(a'_1, \ldots a'_n)$ determines an isomorphism between V and V′. This relation of isologism between groups has the properties of equivalence, and separates all groups into a number of mutually exclusive families in respect of their behaviour as regards the given word f. By choosing a different word in place of f, we obtain (in general) a different classification. The most interesting choice is to take $f \equiv x_1^{-1}x_2^{-1}x_1x_2$. This gives the commutatorial classification, which is especially appropriate for the discussion of prime-power groups. In this case, V is the derived group, W the central, and isological groups have many numerical invariants in common.

Dr. Olga Taussky.—*Differential equations and hypercomplex systems* (11.45).

It is known that each of a pair of functions satisfying the Cauchy-Riemann equations satisfies the Laplace equation. Similarly for each of a set of four functions satisfying the Dirac equations. It can be shown that the same holds for each of a set of eight functions satisfying a certain set of eight linear differential equations.

Let $l_i = c_{kj} \frac{\delta u_i}{\delta x_j}$, $(i = 1, \ldots, n)$, be n linear differential forms with constant coefficients such that

$$\frac{\delta^2 u_i}{\delta x_1^2} + \ldots + \frac{\delta^2 u_i}{\delta x_n^2} = a_{i1} \frac{\delta}{\delta x i_1} l_1 + \ldots + a_{in} \frac{\delta}{\delta x i_n} l_n, \quad i = 1, \ldots, n,$$

where the a_{ik} are constants and the i_k any of the numbers $1, \ldots, n$. The numbers n for which such relations exist can be completely determined by properties of (not necessarily associative) hypercomplex systems over the real numbers. The best known non-associative hypercomplex system—the Cayley numbers—is closely connected with the set of eight linear differential equations mentioned above.

Laplace's operators in two and four variables are special cases of a class of differential operators which are connected in the following way with hypercomplex systems over the real numbers. Let S be a hypercomplex system with n base elements $e_1, \ldots, e_n$ and let $x_1e_1 + \ldots + x_ne_n$ be a general element of S, where the x_i are any real numbers. The norm of $x_1e_1 + \ldots + x_ne_n$, if defined by means of the regular representation of S, is a homogeneous function $f(x_1, \ldots, x_n)$ of the nth degree in $x_1, \ldots, x_n$. If the co-ordinates are replaced by the differential operators $\frac{\delta}{\delta x_1}, \ldots, \frac{\delta}{\delta x_n}$ a differential operator $f\left(\frac{\delta}{\delta x_1}, \ldots, \frac{\delta}{\delta x_n}\right)$ is obtained. Let S be the system of complex numbers or of quaternions. The operator which is so obtained is Laplace's operator.

Mr. Garrett Birkhoff.—*Lattice forms* (12.15).

Wednesday, August 24.

Mr. J. H. C. Whitehead.—*A generalisation of groups* (10.0).

The starting point is the equivalence of simplicial complexes under three kinds of elementary transformations and their inverses. The first are elementary sub-divisions, giving combinatorial equivalence. The second are of the form K → K + aA, where $a\dot{\text{A}}$, but not aA or A, belongs to K, A and aA being k- and $(k + 1)$-simplexes for an arbitrary value of k, and $\dot{\text{A}}$ being the boundary of A. The third consists of these, together with transformations of the form K → K + A, where $\dot{\text{A}}$, but not A, belongs to K, and the dimensionality of the simplex A exceeds some fixed m, which may be arbitrarily chosen in the first place. Complexes which are equivalent under the second and third kind are said to have the same nucleus and the same m-group respectively. The justification for the term m-group lies in the theorem that two complexes have the same fundamental group if, and only if, they have the same 2-group. The m-group of a complex is seen to be a topological invariant, for each value of m, and the nucleus is a topological invariant provided the fundamental group satisfies a certain condition, which is stated in terms of the 'integral group-ring.' An immediate application is that certain invariants discovered by K. Reidemeister, and shown by him to be combinatorial invariants, are actually topological invariants of a complex. This completes not only the combinatorial, but also the topological classification of lens spaces.

Dr. S. Eilenberg.—*On continuous mapping into spheres* (11.15).

Let M^n be a finite or infinite simplicial, orientable, n-dimensional manifold; $X \subset M^n$ a closed and compact sub-set of M^n and $P^k \subset M^n - X$ a closed (finite or infinite) k-dimensional sub-polyhedron of M^n. For each k-dimensional simplex a_i^k of P^k, let s_i^{n-k-1} be an $(n-k-1)$-dimensional spherical manifold, contained in $M^n - X - P^k$ and 'simply linked' with a_i^k.

Given a continuous mapping f, of $M^n - P^k$ into an m-dimensional spherical manifold S^m, the mapping $f(s_i^{n-k-1}) \subset S^m$ determines a unique element $\alpha_i(f)$ of the $(n-k-1)$th homotopy group $\pi_{n-k-1}(S^m)$, of S^m. We write

$$\gamma^k(f) = \Sigma\alpha_i(f)a_i^k,$$

summed for all the k-dimensional simplexes of P^k.

(I) $\gamma^k(f)$ is a k-dimensional (finite or infinite) *cycle* in P^k, with coefficients from the group $\pi_{n-k-1}(S^m)$.

(II) If $\gamma^k(f)$ is homologous to zero in $M^n - X$, there exists a $(k-1)$-dimensional closed sub-polyhedron $P^{k-1} \subset M^n - X$, of M^n, and a continuous mapping $f^*(M^n - P^{k-1}) \subset S^m$ such that $f(x) = f^*(x)$ for each $x \varepsilon X$.

An application: consider in S^n two disjunct sets S_1^m and S_2^{n-m-1}, homeomorphic with S^m and S^{n-m-1} respectively. S_1^m is called a retract of $S^n - S_2^{n-m-1}$ if there exists a continuous mapping $f(S^n - S_2^{n-m-1}) \subset S_1^m$ such that $f(x) = x$ for each $x \varepsilon S_1^m$.

(III) S_1^m is a retract of $S^n - S_2^{n-m-1}$ if, and only if, the linking coefficient of S_1^m and S_2^{n-m-1} is ± 1 (according to the orientations).

Prof. M. Fréchet.—*Hilbert space* (11.45).

SECTION B.—CHEMISTRY.

Thursday, August 18.

Introduction by Prof. Sir William J. Pope, K.B.E., F.R.S. (10.0).

Presidential Address by Prof. C. S. Gibson, O.B.E., F.R.S., on *Recent advances in the chemistry of gold.* (See p. 35.)

Discussion on *Recent advances in the organic chemistry of the metals, with special reference to the noble metals.* (*Exhibition*) (11.15).

Dr. F. G. Mann.—*Introduction.*

Prof. L. O. Brockway (12.0).

Prof. N. V. Sidgwick, C.B.E., F.R.S. (12.20).

General Discussion.

Afternoon.

Visit to the works of the Cambridge Instrument Company.

Prof. C. S. Gibson, O.B.E., F.R.S.; assisted by Dr. F. G. Mann, Mr. H. V. Thompson and Dr. F. H. Brain.—*Demonstration in Section B lecture room on the production of gold films by chemical methods* (5.15).

During 1856, Faraday was occupied in determining the experimental conditions for the production of thin metallic, chiefly gold, films with a view to the investigation of their optical properties. This work had important consequences in other directions and Faraday refers to ' this long and as yet nearly fruitless set of experiments on gold ' probably because he was not successful in producing gold films to his own satisfaction. It is interesting, however, that Faraday appears to foreshadow the modern method of producing films of gold and other metals by the ' sputtering ' process.

The demonstration is concerned with some methods of the production of gold films and their application in the arts. The application of gold films to surfaces of glass and porcelain has long been known and the production of gold mirrors—having magical properties and being the criteria of excellence—whether of glass to which beaten gold was applied mechanically or of polished alloy was known to the early Chinese, Egyptians, Greeks and Romans. A recipe for the production of a golden mirror is given by Geber. The application of gold films to ceramics is described and demonstrated as far as possible by Mr. H. V. Thompson, M.A., with the collaboration of Mr. Bernard Moore and Messrs. Colclough China, Ltd., of Stoke-on-Trent. Dr. F. G. Mann shows the production of gold films on glass by the action of heat on the trialkylphosphineaurous halides which he has recently described. Prof. C. S. Gibson, F.R.S., and Dr. F. H. Brain demonstrate the production of gold films by the decomposition of organic gold compounds at the ordinary temperature and indicate their application in the arts especially as mirrors and for decorative purposes by a number of specimens.

Friday, August 19.

Symposium on *Modern methods of chemical analysis* (*Exhibition*) (10.0).

Dr. J. J. Fox, C.B., O.B.E.—*Introduction.*

The requirements of analytical chemistry are now so extensive, that the older methods have had to be revised and extended in many directions. Particularly the development of methods of micro- and semi-micro-methods has attracted workers all over the world. It is not too much to state that these methods, and their accuracy, have rendered possible investigations which could not have been carried out without them. An important advance in analytical methods arises from the extending utilisation of physical processes. These have resulted in advance in two directions, namely, accuracy and speed of analysis. For example, the use of spectrographic methods renders it possible to examine alloys of various kinds with speed and sufficient accuracy for many industrial processes. Further, the spectrographic method is available especially for the determination of minute quantities of various elements in biochemical, agricultural, and general chemical analysis. More recently we have utilised infra-red spectroscopy in such determinations as the proportions of *o*- and *p*-hydroxy-diphenyl sometimes found present in traces in synthetic phenol.

For the purposes of demonstration two methods, now much used in our laboratory, have been chosen. The first is the determination of moisture by means of the variation in the dielectric constant. This method is particularly suitable when large numbers of similar products, e.g., cereal powders, have to be examined for moisture. The apparatus must be calibrated for each kind of material and with suitably devised cells determinations can be made readily in a few minutes, thus enabling products to be sorted out rapidly.

Extension of the method to liquids is obvious and apparatus is on the market for the purpose. It should be noted that the usual form of apparatus fails when the moisture is high and electrolytes are present.

A second method is the polarographic method associated with the name of Heyrovsky. An apparatus has been devised whereby the curves are plotted by means of a recording pen, thus dispensing with the necessity for visual or photographic recording. It must be stressed that in work of this kind calibration of the apparatus is essential for the problem in hand. This applies equally to many other physical methods, e.g. colorimetric and nephelometric determinations.

Prof. Walther Gerlach.—*Spectrochemical analysis with special reference to biological preparations* (*Demonstration*) (10.30).

Prof. Fritz Feigl.—*Inorganic and organic spot-analysis* (*Demonstration*) (11.15).

The so-called spot-analysis is a microchemical technique of qualitative analysis ; it consists in the application of highly sensitive reactions to the detection of inorganic and organic compounds in one drop of solution or with traces of the solid substance. Such spot tests are carried out by mixing one drop of the solution and one drop of the reagent on filter paper or in small crucibles or on the so-called spot plates. Filter papers which are impregnated with the particular reagent are very useful. The special apparatus required is very simple. By means of spot analysis it is possible to carry out specific detections on minute amounts of material and to recognise quantities down to fractions of a millionth of a gram. The saving in material, time and work is the predominating characteristic of spot analysis.

The right choice of suitable reactions is of importance. Only such reactions as are sensitive and specific are used. Therefore the theory of spot analysis is bound up with the chemistry of so-called specific reactions and with all measures whereby sensitivity can be increased. Of great importance are the application of organic reagents, the employment of catalysis and the use of colloidal and capillary phenomena. The formation of fluorescent compounds is also used.

Dr. Janet Matthews.—*Microanalysis* (11.35).

Inorganic micro-methods of quantitative analysis are now sufficiently developed to warrant their adoption in both research and technical problems. The filter stick technique has been successfully used already in problems of plant nutrition in growth experiments with barley. Without the use of micro-methods the research would have entailed the growth and drying of so much plant material as to render it quite unfeasible; nitrate, potash, phosphate, calcium, magnesium and approximate silica were determined in about 200 samples, which on the macro scale would have involved enormously increased labour.

Another application of micro-methods actively in progress is in the analysis of dust, especially in connection with work on silicosis and allied diseases caused by the inhalation of dangerous dusts. Micro-methods enable a quantitative gravimetric analysis to be carried out on a total of about 50 mgm. of material, the analyses including silica, iron, alumina, calcium, magnesium, sodium, potassium and loss on ignition.

The methods are also capable of application in the cement and glass industries, the paper industry and many other industries, and preliminary work on some of these applications is at present in progress. This shows that a trained analyst can learn micro-methods extremely rapidly and in the first week of work attain, for example, figures for silica in glass with errors of less than $\pm$ 0·1 per cent. difference from the calculated value.

A brief description is given of methods for the determination of silica, iron, sulphate, phosphate and nitrate which are found to give good results on the micro-scale.

Dr. H. Jackson.—*Technique of hydrogenation* (*Demonstration*) (11.55).

A short survey is given of various types of apparatus which have been devised during the past few years for the accurate micro-estimation of the degree of unsaturation of organic compounds, together with a more detailed account and demonstration of the technique used in a particular form of apparatus. This is followed by a description and demonstration of the application of the technique to the construction of an all glass, quantitative system functioning at atmospheric pressure, which is designed to cover all ordinary laboratory requirements.

Dr. K. K. Nygaard and Dr. Th. Guthe.—*Application of the photo-electric principle to the determination of ascorbic acid* (12.5).

By the use of an original, previously described apparatus termed the Photelgraph, the authors have succeeded in automatically recording, by the photo-electric principle, various processes in which a relative change in trans-illumination of the specimen occurs during the process. (Coagulation of blood ; The Wassermann reaction.)

This principle has been applied to a study of the well-known specific reduction of a solution of methylene-blue under the influence of artificial light in the presence of ascorbic acid. Under standardised conditions this process is quantitative as concerns each of the three main factors participating in this photo-chemical process.

The present method records automatically and graphically on photo-sensitive paper the degree of reduction taking place.

With constant, known values of the artificial light, exposing methylene-blue solution of known, constant concentration, the geometric appearance of the tracing obtained indicates indirectly the quantity of the third and variable factor, the ascorbic acid. This quantity expressed in micrograms per cm^3 of solution is obtained by comparison of the tracing with that of a solution containing a known concentration of ascorbic acid.

By this method it has been possible to determine quantities less than 0·05 microgram of ascorbic acid per 100 cm^3 of solution.

General Discussion (12.20).

AFTERNOON.

Visit to the University Departments of Chemistry and Metallurgy.

Monday, August 22.

DISCUSSION on *Clays* (10.0).

Chairman : Prof. E. K. RIDEAL, M.B.E., F.R.S.

Prof. W. L. BRAGG, F.R.S.—*General features of the atomic structure of silicates : inferences to be drawn from them as to the structure of clay minerals.*

The minerals found in clay are often of very variable chemical constitution, and are imperfectly crystallised. The evidence as to atomic pattern given by X-ray diffraction is meagre and difficult to interpret. It is therefore necessary to supplement it by making use of all the knowledge we have about the grouping of atoms in silicates in general, which has been obtained by studying well-crystallised types.

The silicon-oxygen framework of a silicate, composed of tetrahedral groups linked by their corners, is so rigid and strong that it determines the form of the whole structure. In most, if not all, clay structures, the framework takes the form of sheets of tetrahedra linked by their bases, with free vertices. Such sheets are found in mica, which has been analysed completely. Here they occur in pairs, with vertices opposite each other and linked by aluminium or magnesium atoms, so as to form a strong double sheet. In certain other minerals the sheets are single.

Such sheets may either be directly superimposed in the mineral, or be separated by intermediate layers containing ions, or water molecules. The physical chemistry of the clays is bound up with the attachment to the sheets, or detachment from the sheets, of ions and molecules. In making hypotheses about the behaviour of clay, one must bear in mind the general principles concerning atomic situations and replacement which have emerged from the study of silicates of all kinds. These will be briefly summarised.

Dr. G. NAGELSCHMIDT.—*Structure and properties of imperfectly crystallised clay minerals* (10.20).

According to their power to diffract X-rays two groups of clay minerals can be distinguished. The minerals of the first group show more perfect crystallisation and give better developed powder diagrams than the minerals of the second group. The first group includes kaolinite and pyrophyllite, and the second group halloysite and montmorillonite. The atomic arrangements in the second group are mainly derived by analogies and require further confirmation.

Montmorillonite is taken as example of the second, imperfectly crystallised group, and its chemical variations are described as isomorphous substitutions within the lattice. These substitutions lead to negative charges, which are compensated by excess cations. The bulk of the excess cations is exchangeable. Montmorillonite shows reversible one-dimensional lattice shrinkage and expansion upon variations in water content. The amount of water held in equilibrium depends on the vapour pressure, and, at a given vapour pressure, on the kind of excess cations present.

The study of the structure and properties of these minerals is important for the understanding and control of many processes in the ceramic and bleaching industries, and in soil management.

Discussion (10.40).

Dr. R. K. Schofield.—*Origin of the electric charges on clay particles* (11.10).

Clay particles are generally electrically charged and therefore retain an equivalent quantity of ions which can only be removed by exchange with other ions carrying the same charge. Exchange of ions is of great importance in the industrial handling of clay and in land reclamation.

Some of the charge on clay particles is due to isomorphous substitutions within the crystal lattice and is *permanent* in the sense that it is not influenced by the hydrogen ion concentration of the medium in which the clay is suspended. There are also ' spots ' on the particles which are charged or uncharged according to the reaction of the medium. They are of two kinds : acidic spots where negative charges can develop through the dissociation of hydrogen ions, and basic spots where positive charges can develop through the combination of hydrogen ions. The process in the case of the acidity spots is probably

$$\geqslant \mathrm{Si} - \mathrm{OH} \rightleftarrows \geqslant \mathrm{Si} - \overline{\mathrm{O}} \;\; + \mathrm{H}^{+}$$

the silicon atoms being those situated at the edges of the silicon oxygen sheets. The chemical nature of the basic spots is uncertain. They are not found in the clay minerals so far identified but are frequent in the common clays. The equilibrium is possibly

$$- \overset{+}{\mathrm{Al}} - \mathrm{OH} \rightleftarrows - \mathrm{Al} = \mathrm{O} + \mathrm{H}^{+}$$

and may be due to an over-crowding in the ' gibbsite ' layer.

A study of the variation of the electric charge with pH enables the amounts of permanent charge and of the acidic and basic groups to be determined. Approximate values have also been obtained for the dissociation constants of the groups concerned. In certain clays the number of basic groups exceeds that of the permanent (negative) charges. These exhibit well-defined isoelectric points.

Prof. J. D. Bernal, F.R.S.—*The hydroxyl bond in clay minerals* (11.30).

The essential process that takes place in the formation of clays from rock minerals such as felspar or mica is hydration. In the first stages, however, water does not form part of the clay as such, but as hydroxyl groups bound to magnesium, aluminium, or more rarely silicon ions. A hydroxyl group bound to one of these ions is capable of attaching itself to other hydroxyl or oxygen atoms in neighbouring layers owing to the polarising power of the hydrogen it contains. The strength of the hydroxyl bond thus formed depends on the charge of the ion to which the oxygen atoms are attached. It is strongest for a silicon, weakest for a magnesium ion, and this is also the order of the capacity to lose the hydrogen ion altogether or the order of decreasing acidity of the clay particle.

In greater degrees of hydration water molecules are bound to the hydroxyl group, but in a way which resembles the structure of ice more than it does that of free water, owing to the directing effect of the hydroxyls.

General Discussion (11.50).

AFTERNOON.

Visit to the Steel Works of Messrs. Stewarts and Lloyds Ltd., Corby, Northamptonshire.

Tuesday, August 23.

DISCUSSION on *Repercussions of synthetic organic chemistry on biology and medicine* (9.45).

Prof. Sir F. GOWLAND HOPKINS, O.M., F.R.S.—*Introduction.*

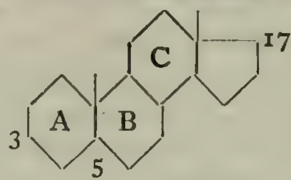
Prof. E. C. DODDS, M.V.O.—*Synthetic œstrogenic compounds* (10.0).

The demonstration of the œstrogenic activity of simple molecules, bearing little or no relationship to the structure of the natural œstrogenic hormones, indicates that a complete change of view must be made on the question of the specificity of biological action. The work described shows that œstrogenic activity can be obtained by a whole series of different molecules without any apparent common physical or chemical property. The high degree of activity of 4 : 4′-dihydroxy-α : β-diethyl stilbene adds considerably more interest to this subject since this substance is several times more potent than the naturally occurring hormone. It would appear, therefore, that biological activity may be imitated by a whole series of substances, possibly quite foreign to the body. The bearing of this on the whole question of hormones and vitamins is of the greatest importance.

Prof. L. RUZICKA.—*Relationship between chemical constitution and physiological activity of androstane derivatives* (10.20).

The following androgens have been demonstrated in the human or animal organism : (1) *Androsterone* and *trans-Dehydro-androsterone* in human male urine ; (2) *Testosterone* in bull's testicles ; (3) *Adrenosterone* in the suprarenal glands of bullocks and cows ; (4) *Androstadienone* in the urine of a man suffering from a tumor of the suprarenal gland. Of considerable importance is the quantitative difference in the physiological properties of androsterone and testosterone. Doses of androsterone and testosterone which exert an equal action on the growth of the capon comb show a quantitatively different influence on the growth of the seminal vesicles and prostate in rats, the testosterone being about five times more active in this last test. The difference in activity was the main reason for the preparation of numerous androgenic substances whereby it was hoped to ascertain the characteristic chemical constitution that was associated with typical testosterone activity. Only derivatives of androstane have been found to possess androgene activity ; it has not been possible to prepare androgenic compounds that differ in their constitution from androstane derivatives in the same degree as Dodds has succeeded in obtaining artificial œstrogens having a structure completely different from œstrane derivatives.

More than fifty androstane derivatives have been prepared and their growth-promoting action on the capon comb and on the auxiliary sex glands of the castrated rat have been investigated. The majority of these androstane derivatives can be classed together in one group, the members of which differ from one another only in the details in positions 3, 5 or 17 of the androstane nucleus. The physiological activity in both tests depends upon the nature of the substituents in these three positions and on their steric configuration. One of the possible two steric configurations is in each case more active physiologically and the physiological difference between compounds possessing these two configurations is greatest in the case of compounds differing in the 5-position, whereas position 17 has less influence and position 3 the least influence. With reference to position 5, that configuration is favoured physiologically which consists in a *trans* configuration of the rings A and B. The corresponding *cis* isomers are physiologically quite inactive. On carbon atom 17 the *trans* position of the hydroxyl with respect to the neighbouring methyl group leads to increased activity compared with the corresponding *cis* compounds, and on carbon atom 3 the isomers showing *cis* configuration of the hydroxyl with respect to the hydrogen in 5 are physiologically more active.



The introduction of a double bond in position 5 leads only to a slight alteration in physiological activity. Moreover the double bond does not appear to be intimately concerned with testosterone-like activity when compared with the corresponding saturated derivatives. Of importance, however, is the presence of a keto group in position 3 which, in respect to the action on the seminal vesicles and prostate, is greatly superior to the corresponding hydroxyl derivative. A reversed relationship appears to exist for carbon atom 17 where a hydroxy group is found to be more active than a keto group.

An increased activity of testosterone when it is injected in oil solution can be obtained by esterification, especially the propionate shows remarkably enhanced activity.

It is established that a whole series of androstane derivatives show a weak œstrogenic activity. It is not possible to define the details of the chemical structure characteristic for this activity in such an exact way as it is possible to determine the structural details associated with androgenic activity. It seems, however, that the presence of a double bond in position 5 is of essential importance for the œstrogenic activity of androgens. For progesterone-like activity in androstane derivatives the double bond also appears to be necessary.

Dr. A. S. Parkes.—*Multiple biological activities of hormones and allied substances* (10.40).

The gonadal hormones and allied substances fall into three classes : the œstrone group, the progesterone group, and the androsterone-testosterone group. The substances of the first group, of which the better known are œstrone, œstradiol and œstriol, have primarily the power to evoke in the female reproductive tract the changes characteristic of the time of ovulation. In the male, these substances have an effect on the accessory reproductive organs, causing metaplasia of the epithelium or hypertrophy of the fibrous tissue, or both, according to the species of animal and the duration of treatment. No activity which can be called specifically androgenic is shown by these compounds. In both sexes the œstrogens depress pituitary activity, as may be seen by the effects on growth and on the gonads.

Progesterone has primarily the power to cause progestational changes in the female reproductive tract. It has little direct effect on the reproductive organs of the male or on the atrophic or undeveloped organs of the female—it has no androgenic or œstrogenic power.

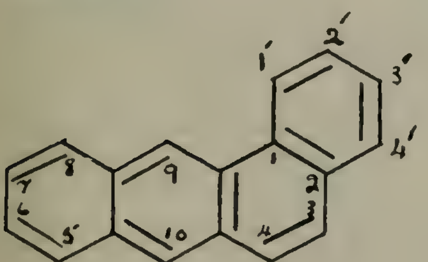
The androsterone-testosterone group comprise three compounds which have been isolated from natural sources and a large number which have been prepared artificially. Most of these are able to stimulate the atrophic accessory organs and secondary sexual characters of castrated animals, i.e. they are androgenic. Many of them are gynæcogenic in that they will stimulate development of the reproductive tract in immature or ovariectomised females, while at least one, *trans*-androstenediol, is œstrogenic in the sense that it will cause cornification of the vagina in mice. Testosterone and several other androgens when methylated in position 17, have the progesterone-like power to cause progestational changes in the uterus.

Several of the androgens are able to depress the activity of the pituitary and cause atrophy of the gonads with consequent inhibition of the sexual cycle of the female. A similar indirect effect is also responsible for the depression of adrenal development seen in the male mouse, or in the female or castrated male receiving androgen.

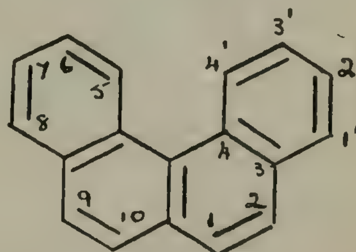
Both progesterone and the androgens are able to protect the organism from certain effects of the œstrogens, an action which may be partly responsible for the effect of androgens on the intact female.

Prof. J. W. Cook, F.R.S.—*Polycyclic hydrocarbons with cancer-producing action* (11.15).

There is now a considerable group of polycyclic aromatic hydrocarbons which have the power of causing cancer. The disease so produced is indistinguishable in its characteristics from that which occurs in man. The chief classes of cancer-producing hydrocarbons are (*a*) those derived from 1 : 2-benzanthracene (I) and (*b*) 3 : 4-benzphenanthrene (II) and its derivatives.



(I)



(II)

The former class has been extensively investigated and it is now possible to state some of the factors of molecular structure which are associated with cancer-producing activity. The twelve monomethyl derivatives of 1 : 2-benzanthracene have been synthesised and tested, and the only ones which have shown definite carcinogenic action are those with the substituent at positions 10, 5, 9, 6 (this represents a decreasing order of efficiency). Further, if simple alkyl substituents are present in two favourable positions of substitution they reinforce one another so that more potent compounds result. Such compounds are the 5 : 6-dimethyl and other 5 : 6-substituted benzanthracenes (Cook), 5 : 9-dimethyl-1 : 2-benzanthracene (Newman) and 5 : 10-dimethyl-1 : 2-benzanthracene (Fieser).

The most powerful carcinogenic agent so far known is 9 : 10-dimethyl-

1 : 2-benzanthracene (Bachmann), which gives multiple tumours much more rapidly than any other compound tested.

Apart from 9 : 10-dimethyl-1 : 2-benzanthracene two of the most active cancer-producing compounds are 3 : 4-benzpyrene, a constituent of coal tar which is undoubtedly responsible for skin cancer among tar workers, and methylcholanthrene, which may be prepared in the laboratory from cholesterol and the bile acids. Both these hydrocarbons are benzanthracene derivatives with substituents at positions 9 (in the case of 3 : 4-benzpyrene) and 5, 6 and 10 (in the case of methylcholanthrene).

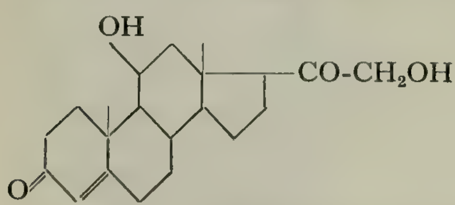
3 : 4-Benzphenanthrene has weak cancer-producing activity; its 2-methyl derivative is fairly potent, as is also 1 : 2 : 3 : 4-dibenzphenanthrene. The influence of substituents in other positions is being investigated (Hewett).

Carcinogenic chemical compounds other than polycyclic hydrocarbons are also known.

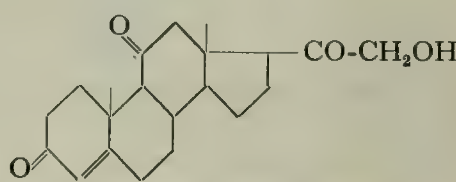
Dr. T. Reichstein.—*Partial synthesis of compounds related to the adrenal cortical hormones* (11.35).

The suprarenal glands are essential to life and the total removal of these organs leads to death. Each gland consists of two separate parts, the medulla and the cortex, the latter part of the organ being associated with the life-maintaining function. During the years 1929–1930 it was shown that extracts of adrenal tissue could be prepared which on injection into adrenalectomised animals maintained life. It seemed therefore that the most important function of the adrenal cortex was the production of one or more hormones. Such extracts are now prepared in large quantities for clinical use.

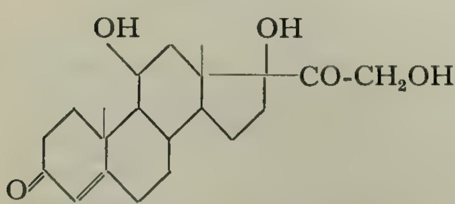
In several laboratories attempts have been made to isolate the 'cortical hormone' in pure form, and as a result of this work more than fifteen different chemically pure substances have been separated all of which appear to be sterol derivatives. Of these compounds only the four following substances possess biological activity in adrenalectomised animals.



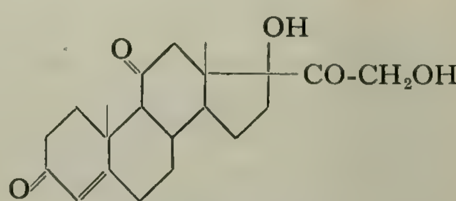
I Corticosterone



II Dehydro-corticosterone



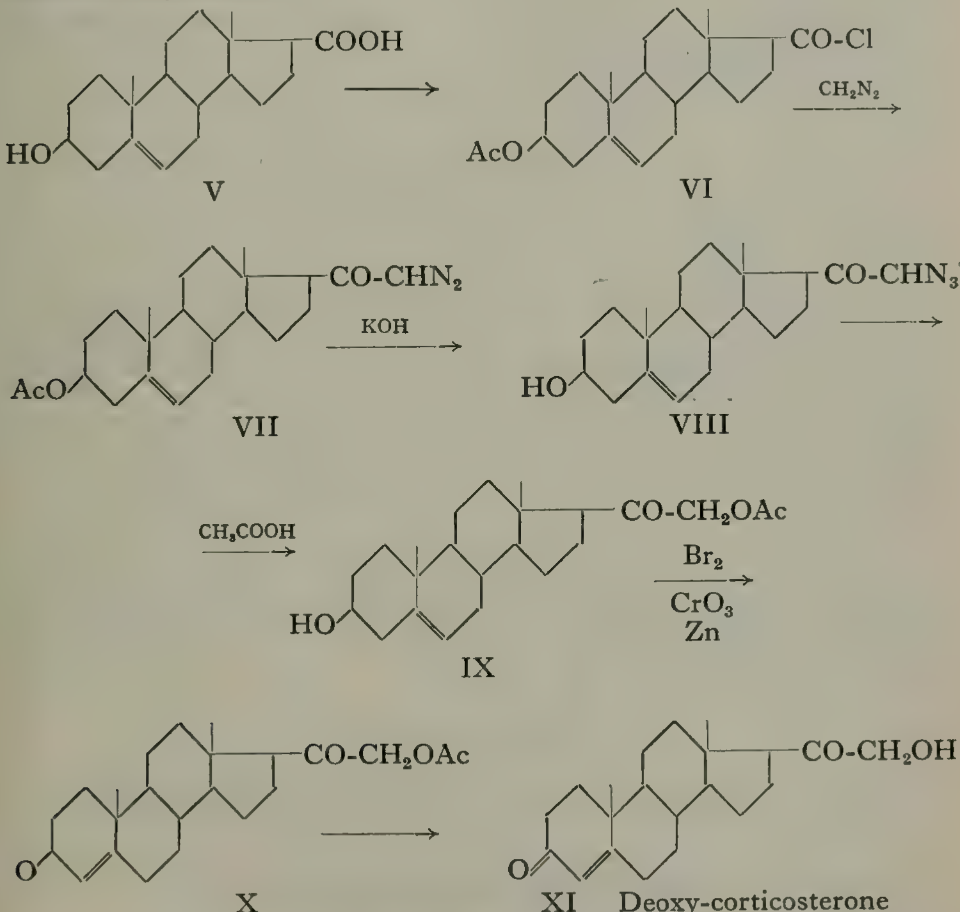
III 17-Hydroxy-corticosterone



IV 17-Hydroxy-dehydro-corticosterone

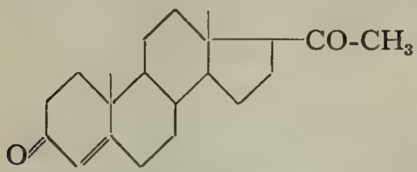
Compounds I and II are more active than III and IV. These investigations are made difficult owing to the approximate nature of the biological tests and the very large quantities of material they require. Furthermore there is as yet no International Standard Unit of biological activity. It is generally agreed that certain amorphous adrenal cortex concentrates show greater biological activity than the pure corticosterone. It is possible therefore that there is in the whole extract an unidentified substance of greater activity. It is certain, however, that not a single life-maintaining hormone is produced by the adrenal cortex but a group of related substances some of which possess biological activity.

Since it is not yet possible to make a complete synthesis of the sterol nucleus, only partial syntheses of compounds related to the adrenal cortical hormones have been achieved. The following scheme shows (steps V to XI) in outline a synthesis of deoxy-corticosterone (XI) from 3-hydroxy-aetio-cholenic acid (V) which can be obtained by degradation of cholesterol or stigmasterol.

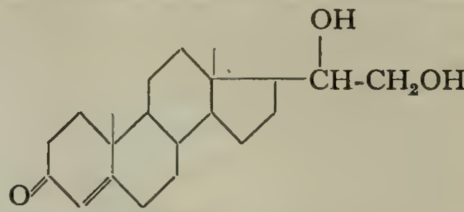


Deoxy-corticosterone (XI) differs from natural corticosterone (I) in not possessing a hydroxy-group in the 11-position. Biological tests have mostly been carried out with the acetate (X) of deoxy-corticosterone and the results indicate that the activity of this compound is greater than that of natural corticosterone. The acetate eliminates the pathological symptoms resulting from adrenalectomy in the same manner as the natural hormone.

It seems probable that deoxy-corticosterone is the simplest compound showing full 'cortin-activity.' Hydrogenation of the double bond practically eliminates the biological activity. Elimination of the hydroxy-group in the 21-position leads to a compound (XII) which occurs naturally, is known as progesterone, and is the active principle of the corpus luteum.



(XII) Progesterone



(XIII)

This compound, however, shows no cortin-activity, which proves that the hydroxy-group in the side chain is essential. Compound (XIII) has also been synthesised, and this compound and also compound (IX) show no noticeable activity. More exact knowledge as to the relation between chemical constitution and biological activity must await the syntheses of further closely related compounds and of more detailed biological studies.

Prof. A. R. Todd.—*Vitamin B_1 and its synthetic analogues* (11.55).

The existence of an antineuritic factor (vitamin B_1, aneurin) has been known for almost forty years, but its isolation in a pure condition from rice polishings was only achieved in 1926. The vitamin, usually isolated as its crystalline chloride hydrochloride $C_{12}H_{18}ON_4SCl_2$ has been the subject of much chemical investigation leading to the elucidation of its structure and its complete synthesis, the latter being realised independently by German, American, and British workers. As a result of these syntheses the vitamin is now prepared commercially and has become available for clinical and nutritional purposes. With a view to determining the structural features necessary for vitamin activity a number of synthetic analogues have been prepared. The results of this work indicate a remarkable structural specificity, for, apart from alteration in the nature and position of the alkyl substituent on the pyrimidine nucleus, any change in the vitamin molecule destroys the physiological action almost completely. Although it is not yet possible to state with certainty the exact function of vitamin B_1 in plants and animals, it is clear that it plays an important rôle as part of an enzyme system in carbohydrate metabolism.

General Discussion (12.15).

Afternoon.

Visit to the University Departments of Biochemistry and Parasitology.

SECTION C.—GEOLOGY.

Thursday, August 18.

Prof. O. T. Jones, F.R.S.—*The geology of the Cambridge district* (10.0).

Joint Discussion with Section K (Botany) on *The post-glacial history of the Fenlands* (11.15).

Dr. H. Godwin.—*Introduction.*

The post-glacial deposits of the Fenland consist chiefly of peat beds on the landward side and brackish water or marine silts and clays on the seaward side. These interdigitate as a result of former conditions of marine transgression and regression. The work of the Fenland Research Committee since its foundation in 1932 under the Presidency of Prof. Sir A. C. Seward, F.R.S., has been the correlation of the history of formation of these deposits with the evidence of geologists, archæologists, botanists and other specialists with as many as possible other events of local post-glacial history. For the outlines of the conclusions of this Committee see articles in the *Scientific Survey*, by Godwin, Clark, Phillips and others.

Dr. W. A. Macfadyen.—*Foraminifera from the post-glacial Fenland deposits.*

The Foraminifera include an indigenous fauna, and derived specimens from the Cretaceous and Jurassic. Derived Jurassic specimens are only occasional, and seem to originate close to where they are now found. The Chalk forms are a constant feature of the silts, and are considered to have been brought up the water channels from the sea coast, from outcrops of Chalk or Chalky Boulder Clay. In the buttery clay they are correlated with the subsidiary silt content.

The indigenous Foraminifera include no extinct forms, and indicate no difference in climate from that of the present day. They may be used as a scale of the salinity, which varied from estuarine to practically fresh water. The silts were characteristic of estuarine conditions, while the clays were apparently deposited in lagoons, into which estuarine water occasionally overflowed.

Different species of a genus of Foraminifera exhibit varying tolerance of admixed fresh water, and this is here most clearly exemplified in the genera *Quinqueloculina*, *Trochammina*, *Bolivina*, *Lagena*, *Discorbis*, *Nonion* and *Elphidium*. *Nonion depressulus* and *Rotalia beccarii* can flourish in water that is practically fresh, and the species of *Trochammina* appear definitely to prefer a somewhat brackish habitat.

Mr. H. L. P. Jolly.—*Levels and bench marks.*

The bench marks of the Ordnance Survey are habitually placed upon structures which bid fair to be the most stable. Hence the great subsidences in the fen levels due to drainage are not recorded by any of the re-levellings which have been carried out. For much of the area there exist records of three levellings dated approximately 60, 30 and 10 years ago. In general, these show no appreciable changes wherever the bench marks are on buildings situated more than 20 ft. above mean sea level; situated, that is, on outcrops of Cretaceous or older rock and not on the alluvium. Some bench marks have of necessity been placed in the drained districts and these, being generally on houses or other brick structures placed on or near an artificial bank, have shown subsidences of from nothing to 1 ft. or even nearly 2 ft. Such measurements serve, however, only to give precision to what is otherwise very patent to the eye, for the houses bear much evidence of subsidence in the shape of cracks, tilting or even collapse. This is especially the case where, as often, the house is built as close to the bank as possible, so that one end of it rests on the bank and the other on or near the fen. The Middle Fen Bank at Prickwillow has a colony of houses, all of which show distortion of some kind. The maximum subsidence shown there by Ordnance Survey bench marks is 0·74 ft. between the years 1870 and 1901 and a further 1·0 ft. between 1901 and 1925. Evidence of soil subsidence beyond that recorded by the bench marks may occasionally be seen where an unused front door to a cottage is now seen to be 2½ ft. above ground level and yet unprovided with any step.

A line of special levels has been completed quite recently in the vicinity of the famous iron column at Holme Fen with a view to finding out what subsidence has taken place since the year 1885. At that date there were already spot (i.e. ground) heights on a drift in the vicinity a foot or two below mean sea level, a not uncommon thing in the Fens. At the foot of the column the ground level is now found to be 7 ft. below mean sea level.

Dr. J. G. D. Clark.—*Archæological correlation.*

Archæological-geological correlations have been effected in the Cambridgeshire Fens by sectioning post-glacial deposits formed in close proximity to and contemporaneously with sites inhabited by prehistoric man. By recording accurately in the section the ' scatters ' (bones, flints and sherds) from successive settlements their stratigraphical relationships have been established and their contexts in the natural sequence of events accurately fixed. Correlations effected by this method are considered more reliable than those obtained by means of chance finds of stray objects or hoards, many of which have been inserted from higher levels. By utilising a ' scatter ' of objects of varying weights the factor of sinkage is also brought under control.

Sections cut on either side of the channel of the extinct course of the Little Ouse on Peacock's [1] and Plantation Farms, Shippea Hill, near Ely, gave three archæological levels in the post-glacial sequence, viz :

Early Bronze Age in the base of the Upper Peat.	− 6 ft. O.D.
N.B.—Fen clay sterile.	
Neolithic ' A ' near the top of the Lower Peat.	− 15 ft. O.D.
Late Mesolithic in a black band at a lower depth in the Lower Peat.	− 17 ft. O.D.

The subsidence revealed by the O.D. levels reached its climax during the Early Iron Age, when the Fens were virtually evacuated. This is well illustrated by comparing maps [2] showing the distribution of finds dating from the Bronze and Early Iron Ages respectively : areas densely settled during the former period appear to have been abandoned completely during the latter.

Mr. C. W. Phillips.—*Conditions in Roman times.*

At the opening of the Roman period the Fens were deserted, but by A.D. 100 an extensive agricultural occupation of native type had set in, chiefly on the silt lands.

It is probable that the area was an Imperial domain. Work at Welney has shown that by the end of the second century sea-floods began, but the wealth and activity of the region continued with little abatement, so far as we know, till late in the fourth century. In Anglo-Saxon times the region was again a wilderness.

The particular interest of the occupation is its size, intensive character, and the various types of native agriculture displayed. The suggestion is that the population was drawn from more than one part of Britain and that

[1] It is hoped to re-open this key section on the occasion of the Cambridge Meeting of the Association.

[2] See the Scientific Survey prepared for the present meeting of the Association.

it was entirely peasant in character. No administrative centre is known, but this may have been at Durobrivæ (Castor-Water Newton) on the western fringe of the region. There is no positive evidence of Roman drainage works on any scale, and the occupation and abandonment of the region appears to have depended in the main on the operation of natural causes.

Prof. H. H. Swinnerton.—*The marshland of east Lincolnshire.*

The Lincolnshire marshland is the northerly continuation of the Fenland. As its coastal margin is undergoing marine erosion many natural exposures of the underlying deposits are available. These show that the general history of the area is one of recurrent alteration in the relative level of land and sea, accompanied by the laying down of alternating deposits of estuarine clays and fen peats. The youngest deposits consist of silty clay containing cockles, oysters and *Scrobicularia*, which show that it was laid down in the low tide zone. This clay rests upon a surface well defined by a thin peat, the presence of a Roman site and the debris of many early Iron Age salt-workings. These facts suggest a stationary condition for the area from the close of the Bronze Age to the last century of the Roman occupation, followed by a rapid subsidence of nearly 20 feet. Underlying this surface the following deposits occur from above downwards : thin freshwater clay ; 8 feet of marine silts, crowded with the remains of salt marsh plants ; and 2 feet of peats, enclosing the ruins of a forest. The last named have yielded one implement of neolithic type, and the composition of the peat points to the climatic conditions of the latter part of the Atlantic Period. The clays with salt marsh plants thus represent a subsidence, during the Bronze Age, which took place so slowly that the area was always situated within the limits of the high tide zone.

Afternoon.

Excursion to Barnwell, Cherryhinton, and Barrington.

Friday, August 19.

Discussion on *The distribution and migration of certain animal groups in the British Lower Palæozoic Fauna* (10.0).

Dr. C. J. Stubblefield.—1. *The Trilobites.*

'Larval' trilobites were presumably planktonic, adults neritic or nektonic. Some genera are more usually found in mudstones, others in calcareous or sandy deposits, many are independent of facies ; trilobites are notably absent from truly planktonic deposits—radiolarian cherts, graptolite shales (s.s.). The geographical affinities, changes and distribution of successive faunas are discussed. The contrast between the faunas of the Scots-Irish area and the Anglo-Welsh area persists from Lower Cambrian to Balclatchie times with possible intermigration in the south about the time of *Nemagraptus gracilis*. These Scots-Irish faunas have affinities with the Appalachian and Baltic regions. In Arenig times, in the Anglo-Welsh area the earlier faunal affinity with Scandinavia is lost ; new genera appear in South Wales ; the arenaceous Synhomalonotid fauna spreads to North Wales and Shropshire but the Cyclopygid-Trinucleid fauna also reaches the Lake District. Llanvirn faunas of South Wales and Shropshire have Bohemian relationships ; Llandeilo, and early Caradoc of East Shropshire also show southern elements. The Caradoc Homalonotid-*Acaste* fauna (in part of southern origin) spreads from East Shropshire across North Wales and to north and east fringes of Lake District ; it later receives Chasmopids (of Baltic origin) probably from the west. A foreign fauna invades mudstones in Scotland and Central Wales about the time of the Caradoc/Ashgillian junction and the Welsh and Lake District limestones take on a Scots-Irish faunal aspect. Later Ashgillian faunas discussed. Llandovery faunas less differentiated geographically than Ordovician, some genera appear or re-appear earlier in north than in south, ancestral Wenlock elements evident. Wenlock faunas most luxuriantly developed in south-east. Ludlow trilobites (in Anglo-Welsh area only) waning in importance.

Dr. G. L. Elles.—2. *The Graptolites.*

Graptolites (Graptoloidea) essentially planktonic, distribution intimately connected with mode of life ; effected by currents, whether (*a*) attached to host, (*b*) as free swimming germs or (*c*) as free swimming maturer individuals ; time taken in migration negligible compared with rate of deposit of rocks in which they occur. Factors affecting completeness of succession : (*a*) quietness of waters ; (*b*) facies ; (*c*) structural considerations. Complete succession at any one time may be : (1) Condensed (perhaps planktonic) ; (2) Spread out (drifted) ; (1) is richer fauna, (2) gives relative ages more accurately. Remarkable continuity in faunas where succession complete and fundamental world-wide assemblages at corresponding horizons, some difference in detail in different regions possibly due to migration.

Analysis of successive Graptolite Faunas of the British Lower Palæozoic Geosyncline in relation to those of extra British areas. Possible interpretation of certain features.

Dr. A. Lamont.—3. *The Brachiopods.*

Shallow water, sessile ; brief drifting life ; wide or deep seas, muddy belts, climate, land, as barriers.

Coarse-ribbed *Orthis carausii* (Arenig) in sands. Large body-spaced *Porambonites intercedens* (Llanvirn) in limestone indicating clear water. *P. filosa* (? horizon) in limestone indicating clear water. Small *Leptelloideæ* in muds of Tramore limestone ; higher beds reaching a *Mesograptus foliaceus-Nemagraptus gracilis* horizon. Common source of Scots, Irish, Welsh faunas about this time. American comparisons.

Dinorthis flabellulum, *Nicolella actoniæ*, *Orthis calligramma*, coarse-ribbed, large volume, in sandy limestone or sand ; poor horizon-markers. Compressed or diminutive individuals in shales. (?) H_2S poisoning. Adaptation to poverty of oxygen in water above mud. *Sowerbyella*, narrow body uses minimum oxygen, extended margin collects from wide area. Individuality of Anglo-Welsh Caradocian fauna, *Wattsella*, *Kjærina*, not accounted for by sea-floor. (?) Land barrier through Saltees.

Scots-Irish conquest (Ashgillian) of Anglo-Welsh area ; *Schizophorella fallax* in Dolhir beds Glyn Ceinog ; *Fardenia* cf. *scotica* in Cyrn-y-brain beds, Llangollen. Percé (Quebec) comparisons.

Llandovery—cosmopolitan. Distribution of *Pentamerus oblongus*. Maroon shales, Walsall boring, current-bedded, large *Brachyprion*, *Schuchertella*, *Stricklandia lirata* forma typica ; the last in length of apertural margin/body volume ratio exceeds *S. lirata* var. α from coastal sands. Considerable oxygen available due to (1) small organic content of muds, (2) little volcanic material, (3) large surface area of sea in relation to depth, (4) disturbance by wind.

Other problems.

Dr. W. K. SPENCER, F.R.S.—4. *The Starfishes and Cystids.*

These are not homogeneous faunas developing in their own areas, but migratory faunas brought in at various times by various floodings. History then can only be followed in relationship to larger considerations.

1. *Cambrian.*—Only fragmentary 'Cystid' remains are recorded from Britain. *Stromatocystis* is found in Newfoundland and Bohemia suggesting a trough connecting these areas. The Carpoids (a stock near akin to ancestral starfish) are confined to Bohemia and Languedoc.

2. *Tremadoc.*—New cystids and first starfish in Languedoc fauna, *Macrocystella* (cystid) in Shropshire.

3. *Arenig.*—Ramsay Island fauna.

4. *Llanvirn.*—A rich and varied development from fauna (2) in Bohemia ($D_1\gamma$); greater part of starfish fauna still archaic, also brittle stars with true starfish; carpoids and cystids. Only British echinoderm belonging to the Bohemian-Languedoc fauna is a species of *Palæura* found in the zone of *Didymograptus bifidus* (Upper Hope Shales of Shropshire).

5. *Caradoc-Bala Fauna.*—Middle Ordovician of Chinese Turkestan has the three species of Spy Wood Grit (early Caradoc) of West Shropshire fauna and *Stenaster* also found in Wales. The Bala Cystids are also related to those found in Asia. The genus *Siluraster* found in the Bala beds in Wales and Shropshire has distinct relationships with the Bohemian $D_1\gamma$ and D_3 faunas. The Bala fauna also has relationships with the Canadian and Kentucky Trenton faunas.

6. *Starfish Bed, Girvan.*—Rich Echinoderm fauna; (*a*) Starfish are related to those of Richmondian of Ohio Basin (and some Trenton forms) together with a new fauna. This combination later dominates the Silurian and Devonian. The faunas are quite different from Welsh-Bohemian fauna (5), the only Welsh-Bohemian-Central Asiatic elements are those also found in the Trenton. (*b*) Cystid fauna has same two constituents, i.e. American and a new fauna which is really an old fauna (2) as developed in Languedoc. The number of peculiar genera common to both is very remarkable. An explanation might be an Eastern reservoir sending migrants by an Arctic route to Girvan. Starfish are not known from the contemporary Welsh, Irish or Baltic beds. Cystids in these three areas are as a whole different from those of Girvan and have a common element, apparently Baltic in origin.

7. *Wenlock* of Pentland Hills shows fauna of affinities with Girvan (6) together with new species from the north-east. Wenlock of Central England brings in new forms related to contemporaneous American faunas, differing from Scottish fauna.

8. *Ludlow* of Leintwardine and Lake District has derivatives of Girvan (6), suggesting that that fauna had moved southward, plus some elements related to Languedoc (2).

AFTERNOON.

Excursion to Upware and Warboys.

Saturday, August 20.

Excursion to Thrapston and Stamford.

Sunday, August 21.

Excursion to the Brecklands.

Monday, August 22.

PRESIDENTIAL ADDRESS by Prof. H. H. SWINNERTON on *Development and evolution* (10.0). (See p. 57.)

DISCUSSION on *The origin of carbonate rocks associated with alkali-rich intrusions* (11.15).

Dr. H. VON ECKERMANN.

Daly mentions the Alnö alkaline area as clear evidence of carbonate-syntexis. A new survey, however, has proved the existence of a confocal stereometrical distribution both of the carbonatites and the alkaline rocks in general. The emplacement of the rocks is similar to that of Julianehaab and Umptek, as emphasised by Backlund. The carbonatites of Alnö, consequently, are not metamorphic limestone-xenoliths. Nor is such a syntexis supported by mineralogical and chemical evidence or by our present knowledge of the Fennoscandian rock-ground.

Surrounding the Alnö-neck, carbonatitic (calcitic or dolomitic) cone-sheets, converging towards the apex of a diatreme, are cut by vertical, radiating alnöitic dykes corresponding to the damkjernites of Fen and representing iron-rich magma risen from below on the blowing out of the diatreme. A gravitational magmatic differentiation accompanied by rising CO_2-concentration is suggested. The CO_2 seems to have greatly affected the normal equilibria of the rock components.

The discovery of carbonate-bearing differentiates of alkaline character, associated with basic Jotnian magmas, suggests a differential relationship between the latter and the alkaline rocks of Fennoscandia. The incongruent melting of orthoclase, previously emphasised by Bowen, may be reconsidered from a new angle, involving the Jotnian tensional earthcrust-stresses and the presence of CO_2.

The Fennoscandian alkaline intrusions—except the post-Caledonian Seiland dykes—are all of the same pre-Cambrian and late- or post-Jotnian age. Whether they are associated with carbonatites or not, they owe their birth to the same fundamental principles. The accumulated evidence of the last few years justifies the draft of a tentative common petrogenetic scheme, which may serve as a basis of further discussion.

Lt.-Col. W. CAMPBELL SMITH.—*Alkali-rocks associated with limestones of apparently intrusive nature in southern Nyasaland.*

In the neighbourhood of Lake Chilwa and elsewhere in southern Nyasaland some alkali-rocks occur in and about a number of vents of supposedly volcanic origin filled with brecciated feldspar-rock intimately associated with crystalline limestones. The field-occurrence of these rocks has been fully described by Dr. F. Dixey of the Geological Survey of Nyasaland. The crystalline limestones of the Basement Complex in the district are of negligible bulk as compared with those of the volcanic vents and are different in composition. The feldspathic breccias of many of the vents consist of angular fragments of orthoclase-rock with unusually high potash content, up to 13 %. The gneisses and other rocks around the vents are intensely altered, the end-product being a feldspar-pyroxene rock in which the pyroxenes are ægirine and ægirine-augite. These rocks correspond to the fenites and tveitåsites of the Fen district in Norway. The fenitisation seems here to be due to emanations accompanying the emplacement of the limestones and feldspathic breccias rather than to the small intrusions of alkali-rocks (foyaite, ijolite, and nephelinite) which cut the vents and are clearly later.

It does not seem possible to explain the crystalline limestones in these vents as due to carbonate replacement. They seem to be of magmatic origin and to be comparable to the magmatic limestones or carbonatites of Alnö in Sweden and the Fen district. Their origin and their mode of emplacement are problems which still await solution, and these problems may be related to the problem of the origin of the associated alkali-rocks.

Mr. S. I. Tomkeieff.—*The rôle of carbon dioxide in igneous magma.*

All igneous rocks are known to contain CO_2 in varying quantities, but it is probable that the greater part of the CO_2 originally present in any magma escaped during the consolidation. Certain rocks and rock-series are especially rich in CO_2, and given suitable conditions during the last magmatic stages, not only did the carbonates crystallise out, but they formed an independent carbonatite magma-fraction. Many occurrences of such igneous carbonatite rocks are known, the one best studied being the Fen district of Norway.

The circular outcrop of alkaline and carbonatite rocks of Fen probably represents the stoped head of a pipe infilled with a basic alkaline differentiate of essexitic magma (Oslo district type). One may postulate that the upper zone in the pipe was originally composed of an alkali-pyroxenite magma rich in CO_2. Crystallisation-differentiation, combined with diffusion of alkalies and volatiles, gave rise to three main rock-series :

(1) Urtite—Jacupirangite series.
(2) Iron-ore series (Rödberg—Hematite ore).
(3) Ca-Carbonate series (Kasenite—Sövite).

The magma of the principal series (Urtite—Jacupirangite) assimilating the country rock (Granite) gave rise to a hybrid alkali-syenite magma (Juvite-Tveitåsite series). The bordering gneiss-granite transfused by alkali-alumina emanations derived from the main magma was transformed into a pulaskite rock (Fenite). The residual liquid of differentiating magma, rich in CO_2 and iron oxides, gave rise to the late consolidated fractions of iron ore and carbonatites. The evidence both of its field occurrence and of its petrographical characters of Damkjernite suggests that this rock had been derived from a lower zone of the pipe composed of alkali-peridotite rich in CO_2. The shattering of the consolidated portion of this magma by the residual volatiles and its subsequent eruption through the rocks belonging to the earlier stage, gave rise to the Damkjernite—Rauhaugite series rich in Mg and K.

The rôle of CO_2 in magma is not limited to the formation of magmatic calcite and carbonatites. It is quite obvious that the presence of CO_2 affects the equilibrium between other components in the magma and in this way determines the formation of minerals. One may suppose that the spilite-keratophyre series, and probably the lamprophyre series as well, owe their peculiar character to the presence in them of a relatively large amount of CO_2. The spilite-keratophyre series in its chemical composition does not differ much from a typical alkalic series, such as the rocks of Oslo district or the British Carboniferous-Permian igneous rocks, except for its considerable larger amount of CO_2.

Prof. C. E. Tilley, F.R.S.

Afternoon.

Excursion to Wood Ditton and Underwood Hall.

Tuesday, August 23.

Dr. S. Buchan.—*Pollution and exhaustion of London's underground water supply* (10.0).

A general progressive lowering of the level of water in the underground reservoir of London has been taking place over a long period, but during the past few years the fall has become more marked and it is now evident that, unless the fall is checked, parts of the reservoir will be exhausted in 35 years or so. The truly artesian conditions of a century ago are gone, and in one area the surface of the water stands 300 ft. lower than it did 60 years ago.

Water is being extracted from the centre of the London Basin more rapidly than it is replenished. Locally, the sands above the Chalk have been drained and dry areas are spreading as the water-level falls, while, in the Chalk, areas are developing from which only a poor supply is likely to be obtained.

The importance of the geological structures in controlling the distribution of water is becoming apparent now that it is possible to define the principal areas from which the supply is flowing to London.

Owing to the geological structure the lowering of the water-level has caused brackish water to flow from the tidal reaches of the river Thames into the Chalk and to pollute the supply in an area of high-yielding wells. As the fall in level continues, pollution will become more intense and will affect a greater area of London as well as an increased depth of Chalk.

The large number of abandoned wells create another potential danger to the water supply. Deterioration of their seal or lining tubes will allow the entry of contaminated water from the superficial deposits to the Chalk.

Mr. S. I. Tomkeieff.—*Zonal olivines and their petrogenetic significance* (10.35).

The measurement of the optic axial angle of olivines from various igneous rocks shows that nearly all olivines are zonal. The only exceptions are olivines from ultra-basic rocks and olivines from alkaline acid and intermediate rocks. As a rule the zoning is continuous and it shows a progressive enrichment of the mineral in iron towards the periphery. The difference in composition between the centre and the outer rim can reach 40 % fayalite, but such cases are rare. In the British Carboniferous dolerites, for example, olivine on the average shows 31 % fayalite in the centre and 39 % in the outer zone, while the olivine from the British Tertiary dolerites shows 18 % fayalite in the centre and 40 % in the outer zone.

A progressive variation in the average composition of olivine occurring in the different phases of a single intrusive mass is demonstrated by the study of the dolerite sill of Fair Head, Co. Antrim. In this sill olivine has the following average composition: (1) from the glomeroporphyritic aggregates (allivalite) representing an early phase of crystallisation—24 % fayalite; (2) from the dolerite—43 % fayalite; (3) from the dolerite-pegmatite schlieren, a late phase—71 % fayalite.

There seems also to be some relation between the variation in the composition of olivine and that in the composition of the rock in which it occurs, an increase in the iron content of the olivine being usually accompanied by a similar increase in the alkalies and silica content of the rock.

The parallelism between the zonality of olivine crystals, the variability of the composition of olivine in the successive magmatic phases and the relation between the composition of the olivine and the magma, has a definite bearing on the question of petrogenesis. The recorded observations—which are in perfect agreement with the recently published study on the forsterite-fayalite binary system by Bowen and Schairer—show the possible control exercised by olivine on magma and the relation between the composition of the olivine and that of the magma.

Dr. F. Walker.—*The differentiation of the Palisade diabase sill, New Jersey* (11.10).

A detailed quantitative examination of the best exposed sections through the famous Palisade Diabase Sill leads to the following conclusions:

(i) It is doubtful whether the 'olivine layer' in the lower part accumlated by gravitational settling of olivine.
(ii) There is a definite gradational concentration of pyroxene above the 'olivine layer,' indicating sinking of pyroxene.
(iii) Assimilation of arkose on a large scale is improbable.
(iv) The proportion of free silica in the sill as a whole is very small, but there is a marked concentration just below the upper chilled phase.
(v) The magmatic history of the sill ended with pronounced hydrothermal activity.

Dr. A. Wade and Dr. R. T. Prider.—*The geology and petrology of the leucite rocks of the Kimberley district, Western Australia* (11.45).

Nineteen occurrences of post-Permian volcanic rocks have been found in the West Kimberley area. Plugs, partly eroded craters and fissure intrusions have been recognised. The structure and distribution of these occurrences indicate that the intrusions have ascended along fault planes which are connected with the structure of the underlying pre-Cambrian rocks.

The rocks are made up of varying proportions of leucite, phlogopite, diopside, simpsonite (a new K-Mg. amphibole related to katophorite), wadeite (a new K-Zr silicate), rutile, chlorite and indeterminable groundmass. Four new rock types (fitzroyite, cedricite, mamilite and wolgidite) are described.

Although leucite is developed to the complete exclusion of sanidine, the rocks contain more than sufficient silica to have formed orthoclase instead of leucite. The magma, from which these rocks crystallised, was of peculiar character—its main features being high potash dominant over alumina, high magnesia and titania and very low soda content. The minor constituents are comparatively abundant.

This magma was probably derived from a potassic mica-peridotite magma by the early crystallisation and removal of olivine. The siliceous and potassic residuum crystallised at temperatures above the leucite-orthoclase reaction temperature and was extruded as a crystal mush which has effected little change in the intruded sediments. Chilling of the acid residuum has inhibited the leucite to orthoclase reaction.

The only comparable rocks are the wyomingites and orendites of the Leucite Hills, Wyoming.

Dr. F. Coles Phillips.—*The fabric of some 'Tarskavaig Moines'* (12.20).

The 'Tarskavaig Moines,' a series of phyllites and schistose grits, occur in the Sleat district, Skye, to the north-west of the Moine Thrust, above a subsidiary dislocation, the Tarskavaig Thrust. On the Geological Map of Scotland issued in 1892 they were coloured as Torridonian. They were later regarded by C. T. Clough (mainly because of supposed similarities in stratigraphical sequence) as less-altered representatives of the same great formation as the Moine rocks on the other side of the Moine Thrust. This view has recently been questioned by H. H. Read, who considers the Tarskavaig rocks to be Torridonian affected by the post-Cambrian dislocations, and regards apparent transitions from unaltered Torridonian to true Moine rocks as a result of metamorphic convergence. The fabric has therefore been examined in an attempt to determine the direction or directions of movements to which these rocks have been subjected, and comparisons are instituted between the grain-fabric of the Tarskavaig rocks and of Torridonian and Moine rocks from adjacent districts. These are believed to show that true Moine rocks have been affected by regional movements along a south-west and north-east direction, no trace of which can be found in the grain-fabric of the Tarskavaig rocks.

AFTERNOON.

Excursion to Barley and Barkway.

Dr. S. R. Nockolds and Dr. J. E. Richey, F.R.S.—*Replacement veins in the Mourne Mountains granites* (2.15).

All the Tertiary granite masses of the Mourne Mountains are traversed here and there by narrow replacement veins which belong, in general, to the greisen class ; the prevalent variety being dark-green in colour. They are either steeply inclined or vertical, are found in greatest number at places close to a granite margin, and can be shown in several instances to be parallel to the plane of contact. Their position in the igneous history of the granites is indicated by the fact that they cut the later aplite veins but are themselves traversed by well-developed joint planes.

The dominant type of vein is composed of aggregates of topaz and a peculiar blue-green mica together with quartz. Other constituents include some independent topaz, fluorite, stilbite, chlorite and sometimes a little biotite.

Subordinate types are represented by white mica-chlorite-quartz veins with minor fluorite and stilbite, grey quartz-magnetite veins and black veins rich in manganese ores.

There occur, in addition, certain fissure veins which follow, and are later than, the joints. The dominant variety is composed of iron rich chlorite, associated with colourless granular fluorite, albite and very subordinate orthite.

Mr. S. O. Agrell.—*Adinoles of Dinas Head, Cornwall* (2.45).

Adinoles associated with spilosites and spotted slates occur at the contact of an albite-dolerite intrusion with black limestone-bearing slates of Upper Devonian age.

They consist of albite and quartz with accessory leucoxene, and with or without chlorite, dravite, ankerite and calcite.

Four main types are recognised :

1. Normal adinoles—structureless albite-quartz rocks showing sedimentary banding and grading into rocks composed essentially of dravite.
2. Adinoles with pseudomorphs probably after andalusite.
3. Adinoles with globular masses of ankerite showing concentric structures.
4. Polygonal and spherulitic adinoles.

Chemically, the adinoles resemble quartz-keratophyres and their tuffs, but the evidence at Dinas Head shows that they are due to the effect of the intrusion on the sedimentary rocks. The first change was purely thermal and was followed by albitisation and then by carbonatisation, the metasomatising fluids coming from the dolerite.

The adinolisation is a volume for volume replacement and calculation on analyses shows that soda, silica and sometimes boric oxide have been fixed in the slates.

As a result of faulting and of the ramifications of the intrusion the adinoles appear up to eighty feet thick, but actually, they form a veneer over the headland and never extend more than thirty feet from an igneous contact.

Mr. G. Andrew.—*Some granitic intrusions in the Central Eastern Desert of Egypt* (3.15).

The intrusive granites may be divided into groups on the basis of the nature of the contact.

1. *Abu Ziran type.*—Injection-gneiss, strongly banded, generally foliated, protoclastic or cataclastic structures common. Xenoliths within mass granoblastic, near margin foliated. Injection zone in pelitic rocks accompanied by kyanite, staurolite and almandine as contact minerals. Recrystallisation falls off away from granite-margin, but is still of regional type. Intrusion of syntectonic type.

2. *Belih type.*—Porphyritic, often foliated locally. Contact irregular, either steeply plunging discordant, or approximately horizontal discordant in bathyliths. Country-rock thoroughly recrystallised, normal hornfels-structures and contact-minerals, veined. Assimilation considerable, with xenoliths common and remote from margin, traceable in granite in the form of ' basic clots.' Margin usually granite-porphyry.

3. *Um Disi type.*—Even grained, often fine-grained, non-porphyritic, unfoliated. Contact sharp, steeply plunging discordant. Country-rock affected to a notably less degree in comparison with Belih type. Normal hornfels-structure and minerals. Veining rare, assimilation negligible, and xenoliths are rare, and sharply bounded. Margin often coarse, pegmatitic.

4. *El Atrash type.*—Almost entirely a quartz-feldspar rock, in small masses, frequently with a dyke form (W.N.W. to N.W. strike). Coarse varieties granitic, finer grained type is spherulitic or micrographic. Contact metamorphism very slight, confined to a few metres width, even in masses of 4 sq. km. area. Rarely xenolithic.

Type 1 is only known in the regionally metamorphosed paraschists south of G. Me'atiq. The remainder intrude the Dokhan series and other unmetamorphosed sediments, and are arranged in order of age. The riebeckite granites are not included in this table and form a still younger group than 4.

Mr. G. Andrew.—*On the upper pre-Cambrian of the Eastern Desert of Egypt* (3.30).

Between latitude 26° N. and 28° N. the stratified rocks forming the hill region west of the Red Sea are largely unmetamorphosed. Three series may be distinguished :

3. The Hammamat series, consisting of purple (*lie-de-vin*) and green mudstones, greywackes and conglomerates.
2. The Dokhan series, consisting of similar sediments, with intercalated pyroclastic rocks and lavas.
1. The Atalla-Rubshi series : grey mudstones, often phyllitic, some greywackes, and rhyolites with tuffs.

The stratigraphical relations of the three series to one another are not known, since all contacts seen are faults. Dips are high, strikes variable : north-north-west in the Atalla-Rubshi series, north-west to north-north-west in the Hammamat, and north-east to north in the Dokhan series. The lithological types of the Hammamat series may be recognised over a wide area in an unmetamorphosed state, e.g. Bir Kareim, Dungash, Wadi Khashab, Wadi Hamata, and north of Um Garaiart. In the same way the Dokhan type occurs in Wadi Hamish (Wadi Shaït), Wadi Sheikh Shadli—G. Abu Hammamid, Wadi Huluz, and in the Wadi Allagi region. The Atalla-Rubshi series is less distinctive, and not recognisable elsewhere at present. These series may be classed as Algonkian, and include the Eparchæan and part of the Metarchæan of Hume (1934). The rocks are typically non-schistose, except in narrow zones of movement, and in the contact-zone of some granites.

Dr. A. G. MacGregor.—*Characteristics of West Indian tridymite and cristobalite* (3.45).

Exhibit of microphotographs (lantern slides) illustrating the mode of occurrence and diagnostic features of the tridymite and cristobalite of the porphyritic bandaites (labradorite-phyric dacites) of Montserrat, B.W.I. Tridymite is an abundant primary groundmass-constituent ; cristobalite is also very prevalent and can, in some cases, be shown to replace original tridymite. The cristobalite exhibits certain features recalling those often described as characteristic of tridymite.

Wednesday, August 24.

Dr. E. B. Bailey, F.R.S.—*Caledonian tectonics and metamorphism in Skye* (10.0).

The geology of the Kishorn Nappe is reviewed from Loch Kishorn on the mainland through the greater part of Sleat in Skye. The fundamental facts are taken from Peach, Horne and Clough. The latter's magnificent work in Skye has been amended in certain details, and this has tended to clarify the general situation.

The Kishorn Nappe consists mainly of Torridonian, with Lewisian north of Loch Alsh (Sheets 81, 71), and Cambro-Ordovician west of Ord on Loch Eishort (Sheet 71). The nappe is underlain through much of its extent by a complex of moved Cambro-Ordovician, which is exposed at the head of Loch Kishorn and again in the Suardal Anticlines of Skye and in a window east of Ord. The top of the Kishorn Nappe is furnished by the base of the Moine Nappe, except near the Point of Sleat, where the Tarskavaig Nappes intervene (Sheets 71, 61).

A great inversion, called the Loch Alsh Inversion, is the main structural feature within the Kishorn Nappe. It is part of a recumbent fold that runs obliquely to the course of the Kishorn and Moine thrusts, that truncate it from below and above. Thus at Loch Kishorn, only the upper inverted limb of the recumbent fold is preserved ; at Loch Alsh, both limbs occur ; while in most of Sleat, only the lower normal limb is found. The upper limb shows a cleavage, or foliation, that scarcely penetrates at all into the lower limb, and with this foliation there is in places mineral development leading to a production of minute micas, including brown biotite.

The generally normal lower limb of the Loch Alsh Fold develops an additional local recumbent fold exposed west of Ord, near the window already mentioned. The inversion connected with this Ord Fold is unaccompanied by cleavage or metamorphism. Moreover, it does not extend up to the base of the overlying Tarskavaig Nappes.

The Tarskavaig Nappes emerge from under the Moine Thrust, and agree in structural position with the Loch Alsh Inversion, except that they have travelled forward by thrusting rather than inversion (*cf.*, however, the Balmacara Thrust of the Loch Alsh district). Their metamorphic grade is closely comparable with that of parts of the Loch Alsh Inversion. It is true that in addition to a widespread development of minute biotite, in part brown, Clough found 1 mm. garnets at one locality; but these latter, after separation by A. F. Hallimond, have been analysed by C. O. Harvey, and proved to contain sufficient manganese to be natural associates of biotite in its early stages of formation. The writer feels that the correlation of the Tarskavaig 'Moines' with the Torridonian, a view favoured by Peach, Read and others, is distinctly strengthened. He welcomes the altogether new evidence furnished by F. C. Phillips during the current session of the B.A.

It is hoped to expand this account in a forthcoming Geological Survey *Bulletin*.

Dr. E. B. Bailey, F.R.S.—*Tectonics, erosion and deposition* (10.30).

(1) *Antecedent Drainage.*—In maturity antecedent drainage often looks wellnigh unbelievable, for it seems like special pleading to speak of a barrier of hard rock raised so slowly as not to divert a river of soft water. One is apt to forget that during early stages of mountain elevation a river may have to cope with nothing more resistant than sand and clay. By the time it reaches hard core rocks it may already be entrenched in a valley thousands of feet deep, and therefore able to defy intermittent attacks by earth movement, however strong the material that is employed. This sort of relation is illustrated in some of the anticlines associated with the Caucasus.

(2) *Cross-mountain Contrasts.*—Sometimes the two sides of a mountain chain, great as the Urals or small as the Malverns, show a wonderful contrast. On the one side the junction between the mountain-rocks and those of the adjacent plain may be tectonic, and on the other side, erosional. As a broad generalisation this is illustrated at the junction of the Urals, westwards with the Palæozoics of the Russian Platform and eastwards with the Tertiaries and Quaternaries of Asia ; or at the junction of the Malverns, westwards with the Old Red of Hereford and eastwards with the New Red

of Worcestershire. In either case we are dealing with a mountain chain, or ridge, that to begin with separated a high plateau from a low plain. Until deposition overtook erosion the main tendency was for the low plain to extend ever farther into the mountain territory. What remained of the hard core rocks of the mountain functioned meanwhile as a bulwark, protecting the high plateau from encroachment of the plain. Then came deposition, building up the low plain in relation to the high plateau. Thus the original contrast of level on the two sides of the mountain was eventually replaced by a contrast of material.

Mr. J. F. Kirkaldy.—*The constituents of the pebble beds of the Lower Cretaceous rocks of England and the light they throw on the palæogeography of the time* (10.45).

This communication is an interim report of an investigation of the constituents of the pebbly horizons in the Lower Cretaceous beds of South England. More precise evidence than hitherto available, as to the nature of the rocks then undergoing denudation and the directions of the supply of detritus, is accumulating.

In the Wealden beds of Dorset there is a highly distinctive suite of pebbles indicating the erosion of the metamorphic aureole of the Dartmoor Granite. This suite, except for one or two doubtful pebbles, has not been found in the Weald. The resemblance between the pebbles from the north-west Weald, which are being examined by Dr. Wells and Mr. Gossling, and those from the Lower Cretaceous sands of Bedfordshire, Oxfordshire, Berkshire and Wiltshire indicates in part, at least, a common derivation from the London Platform. In this connection the distribution of the pebbles of silicified oolitic and dolomitic limestone is particularly significant. In east Kent, however, the pebble suite is of a somewhat different character.

The many interesting types of pebbles found are described and inferences made as to their place of origin.

Dr. S. M. K. Henderson.—*The Dalradian Succession of the Southern Highlands* (11.0).

A brief account of the results of the study of current and graded bedding in the Leny Grits, Aberfoyle Slates and Ben Ledi Grits. These three groups have generally been adopted as the youngest members of the Perthshire Succession, and were included thus by Dr. Bailey in his *Iltay Nappe*.

From Loch Lubnaig in the north-east to Loch Lomond in the south-west (Geological Survey, Sheet 38) current and graded bedding has always given the same evidence. The generalised dip is 60° to the north-west.

The evidence shows that the Leny Grits to the south-east of the Aberfoyle Slates are upside down, and are younger than the latter, which they underlie. On the north-west side, the Ben Ledi Grits are also younger than the Aberfoyle Slates which they superimpose.

On lithological grounds it seems reasonable to correlate the Leny and Ben Ledi Grits as one formation younger than the Aberfoyle Slates, the former being the under limb, and the latter the upper limb of a steeply overturned anticline. This would then be a structure comparable to the Carrick Castle Fold, an anticline closing to the south-east, in the *Iltay Nappe* of Bailey. The series of dislocations between the Leny Grits and the Highland Border Rocks may, upon further investigation, prove to be the base of this large overfold of Dalradian rocks.

Dr. T. S. Westoll.—*The distribution of certain specialised Carboniferous bony fishes* (11.15).

The small fishes *Haplolepis* (*Eurylepis* Newberry) and *Pyritocephalus* Fritsch have been found to be closely related, and are probably descendants of *Canobius*; *Pyritocephalus* is much more specialised. Newberry's *E. lineatus*, from Linton, Ohio, is found to be almost identical with *P. sculptus* Fritsch from Nýřany, Czecho-Slovakia. This suggested a correlation of the two horizons, which was fully confirmed by palæobotanical evidence; but it was later found that *Teleopterina* Berg, from Mazon Creek, Ill., and a fragmentary skull from Newsham, Northumberland, must also be referred to *Pyritocephalus*. The two genera occur at several horizons in the Westphalian and (?) basal Stephanian, namely:

Newsham, Northumberland	Flora E.
Longton, Staffs	Flora F.
Mazon Creek, Illinois	Flora G or G-H.
Linton, Ohio Nýřany, Czecho-Slovakia	Flora H or H-I.

(Palæobotanical horizons after Dix's scheme.)

These fishes are of great morphological interest as they approach Holosteans in certain characters. *Pyritocephalus* has very specialised fenestrations in the skull-roof, perhaps due to the large eyeball, and both genera have peculiarities in their dermal bones.

The distribution of these highly specialised small freshwater fishes raises important palæogeographical issues, while their recurrence, with certain Amphibia, at different horizons indicates the existence of a well-marked vertebrate facies-fauna.

Dr. J. B. Simpson.—*Fossil pollen in Scottish Jurassic rocks* (11.30).

In the Jurassic strata that outcrop on the east coast of Sutherland, Scotland, coal seams and carbonaceous layers are present at horizons in the Lower Lias, Estuarine Series, and Kimeridge Clay.

Examination of the microspore content of these coals has disclosed the presence in them of pollen of gymnospermous and dicotyledonous types as well as the spores of cryptogamic plants.

The gymnosperms are represented by winged pollen grains such as we find in the Abietineæ at the present day, and the variety of the forms already indicates a considerable degree of differentiation.

The pollen of dicotyledonous types represent at least two living families—Magnoliaceæ and Nymphæaceæ. In both families, too, more than one genus is present. The Magnoliaceæ are represented by one form similar to *Magnolia*, and another closely akin to *Drimys*. The forms placed in the Nymphæaceæ show close affinities to the living genera *Nelumbium* and *Castalia*. The *Nelumbium* types show the characteristic bisymmetry of this class of pollen in polar view, and in other details also, resemble Tertiary and modern forms of the genus. The types identified as *Castalia* resemble the pollen of the tropical species of this genus.

The presence of pollen of dicotyledons in these rocks is of special interest as being amongst the earliest fossil records of Angiosperms.

SECTION D.—ZOOLOGY.

Thursday, August 18.

Presidential Address by Dr. S. W. Kemp, F.R.S., on *Oceanography and the fluctuations in the abundance of marine animals* (10.0). (See p. 85.)

Mr. C. F. Hickling.—*Applications of our knowledge of the biology of British food fishes* (11.0).

The paper first describes the principles of the two most important types of fishing gear—namely, the drift net and the trawl, and points out the limitations of these gears when their results are used as samples of the populations of fish in the sea. But bearing these limitations in mind, the results of the fishing operations of the commercial fishing fleets may be used to keep a watch upon the state of the fish stocks available for capture. Moreover, these results, when interpreted in the light of the biology of the fish, may be used to predict the future course of the fisheries. These points are illustrated by reference to the herring, haddock, cod, and hake.

Mr. M. Graham.—*The rational exploitation of the fisheries* (11.40).

Statistics of trawl fisheries, such as those of haddock and plaice at Iceland, tend to show eventually that the response to increased fishing effort is, if anything, a decreased yield.

This phenomenon can be easily explained in terms of age-composition and growth-rate of stocks, which in many cases are available from investigations.

The whole theory may be expressed by the ' logistic ' curve and its first derivative, which can be derived from the two assumptions (1) that the weight of stock that an area can support is limited, (2) that the rate of natural increase of a stock—reproduction plus growth minus natural mortality—is proportional to the difference of weight of the stock at a given moment and the limiting weight that the area will support. This theory has been applied to the marketable species of the North Sea, taken together, in order to estimate the present waste of fishing effort and the maximum yield.

Important implications are that unless the rate of fishing, including the mesh and form of nets and power of vessels, be controlled, the profit of undertakings is kept at a low value. Without control, gambler's optimism in this industry tends to keep the rate of fishing as high as possible and the profit consequently at an extremely low level. Conversely, however, there is a possibility of a large profit in concerted action to avoid the expenditure entailed in keeping the rate of fishing high.

A start has been made, in that most of the European countries concerned have signed a convention agreeing to use the minimum mesh allowed in Great Britain. The investigations which show the efficacy of this provision are briefly described.

Dr. J. B. Tait.—*Significance of the physico-chemical environment in fisheries research* (12.15).

Afternoon.

Prof. T. W. M. Cameron.—*Some fish-carried Trematodes in Canada* (2.15).

The great number of fresh-water lakes in Canada, its varied fish fauna and its variety of fish-eating mammals and birds, have made possible a large variety of fish-carried parasites. Of these, the most varied are the Trematoda, of which three species are discussed in this paper. *Apophallus venustus* (Heterophyidæ) is a minute intestinal trematode parasitising numerous birds and mammals (including man). It is found in the lower valley of the river Ottawa, where its first host is the pulmonate snail, *Goniobasis livescens*, and its second hosts are numerous species of fresh-water fish. *Cryptocotyle lingua* (Heterophyidæ), a second intestinal form, is confined to the eastern seaboard from the Labrador to the United States and has almost certainly been introduced from Europe. Its first host is *Littorina littorea*, and its second, various species of salt-water fish ; the adults occur in both birds and mammals and are serious parasites of foxes. *Parametorchis noveboracensis* (Opisthorchidæ) occurs from northern Quebec to Saskatchewan and is a serious liver-fluke of various mammals (including sledge dogs and man). Its first host appears to be a species of snail of the genus *Amnicola* ; its second is the fish called the Sucker, *Catestomus commersoni*.

Mr. Homer A. Jack.—*The zoological field stations of the United States* (2.45).

A zoological field station is an institution of approximate university ranking which offers facilities for primarily *field* instruction and/or research in one or more of the zoological sciences, and is a *separate* administrative unit located in the field. The first zoological field station in the United States was the Anderson School of Natural History, founded in 1873 by Louis Agassiz. To-day there are almost sixty stations in the United States. These vary considerably in ecological location, administration, equipment, living conditions, type of students, investigators, and professors, and in available opportunities for instruction and research.

This study was conducted to record and analyse material on field stations to aid : (1) prospective and actual students and investigators in the biological sciences to select intelligently the stations most adapted to their needs ; and (2) directors of stations in showing them how their fellow administrators are solving the problems attendant to the efficient organisation and conduct of a field station. A plan is given for the interchange of students, investigators, and professors between field stations in the United States and other countries. This could not only enrich the zoological sciences, but also strengthen international understanding.

Miss G. E. Pickford.—*The Vampyromorpha—a new order of Dibranchiate Cephalopods* (3.15).

Since their discovery by Chun the *Vampyroteuthidæ* have been regarded as aberrant if rather primitive Octopoda. Recognised as Octopodan characters were the eight conspicuous arms united by a deep web and the apparently normal union of head with mantle. Outstanding as primitive characters were the fins, the arrangement of suckers and cirri on the arms and the lack of condensation of the central nervous system. Specialised features, such as black pigmentation, phosphorescent organs and peculiar tentacles lodged in pockets of the web, could be regarded as adaptations to a bathypelagic life. Robson first recognised the Vampyromorpha as a sub-order distinct from other cirrate armed Octopoda.

Two well-preserved, but unfortunately immature, female specimens in the Bingham Oceanographic Collection of Yale University have provided material for a detailed anatomical study. It is possible now to state definitely that they exhibit no positive characters which would justify retention as a sub-order of the Octopoda. Although similar to that of Octopoda, the separate origin of the genital artery and the orientation of the heart must be regarded as characters ancestral to Octopods and Decapods alike. The spacious cœlom, gill structure, funnel valves, and other characters which apparently relate the Vampyromorphs to living Decapods are also ancestral rather than definitively Decapodan in nature. The retractile filaments undoubtedly represent modified arms, but since it is the second rather than the fourth arm pair which is thus modified one cannot postulate relations with living Decapods. The structure of the shell with its broad pro-ostracum resembles that of Liassic teuthoideans and the direct articulation of the fin bases upon the shell is a primitive feature postulated by Naef but not found in any adult living form.

It is evident that the Vampyromorpha represent a distinct and ancient type of dibranchiate Cephalopod. They should be treated as a group of equivalent rank with Octopods and Decapods.

Mrs. M. D. Brindley.—*The succession of Hemiptera-Heteroptera in the afforested areas of Breckland* (3.45).

Breckland is the name given to a well-defined area in the western parts of Norfolk and Suffolk. It is characterised by tracts of sandy heath with a distinctive vegetation which is conditioned primarily by the soil and the relatively dry climate. During the last sixteen years, about 50,000 acres of this land have been afforested with conifers, and the planting has greatly modified the flora and fauna. This communication, after a short general description of the region, deals with the change as it has affected the insect group Heteroptera in a selected area over a period of seven years, covering the transition from a heath to a conifer-dwelling type of population.

At present three elements can be distinguished in the Heteroptera fauna, viz. (i) widely distributed, usually polyphagous forms, (ii) species peculiar to heathlands, and (iii) forms whose host-plants are conifers. By gradual infiltration, as the trees grow up, the third group becomes dominant. The adjustment of the different species to the changing environment, and the influence of the age and size of the plantations on distribution are discussed.

Prof. J. Stanley Gardiner, F.R.S.—*Wicken Fen* (4.15).

Exhibition illustrating the *Genetics and chemistry of plant and animal pigments*.

One of the characters of plants and animals most frequently used in genetical investigations is colour. Until recently separation of colour types has depended solely on visual comparisons, which may sometimes be misleading and are always inadequate since they represent only a first analysis. Further understanding of the developmental processes involved requires an analysis of the chemical nature and physical state of the pigments responsible. This has become possible in some cases.

The exhibit was designed to illustrate various phases in the development of this aspect of physiological genetics from purely descriptive genetics. The analysis has gone farthest in the case of flower pigments, especially the anthocyanins and anthoxanthins. With these gene action can be examined, for the first time, in its fundamental sense, namely as governing simple chemical changes, such as oxidation, reduction, methylation or glycoside formation. In other cases, as in *Drosophila*, the budgerigar and the clover chlorophyll deficients, we know something about which pigments are affected but nothing of the nature of the changes.

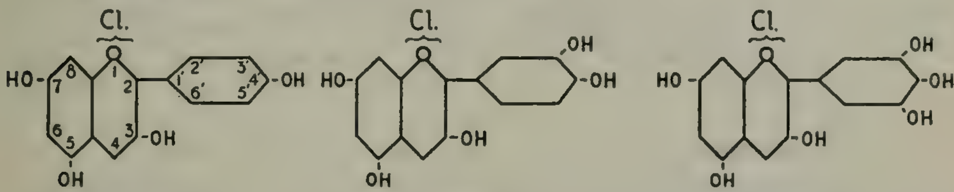
Mr. W. J. C. Lawrence and Mr. J. R. Price.—*Flower pigments.*

The Chemical Basis of Flower Colour.

The substances responsible for flower colour may be divided into two classes, sap-soluble and non-sap-soluble. The sap-soluble pigments comprise the anthocyanins, anthoxanthins and certain nitrogenous substances.

The Anthocyanins are the most important flower colouring matters, and are responsible for scarlet, red and blue colours. They occur as glycosides, that is, they are compound molecules formed by the union of the true colouring matter with one or more molecules of a sugar.

The colour-producing part of the anthocyanin molecule, known as the anthocyanidin, may be derived from one of three main structures—pelargonidin, cyanidin and delphinidin—which differ only in the number of hydroxyl groups (—OH) in the phenyl ring :



Pelargonidin   Cyanidin   Delphinidin

As the formulæ show, cyanidin has one and delphinidin two more oxygen atoms in the molecule than pelargonidin. These differences represent one of the principal factors upon which variation in flower colour depends, since an increase in the number of oxygen atoms (in the form of hydroxyl groups) results in a marked increase in blueness of tone. This is illustrated in the first part of the exhibit.

As mentioned above, the anthocyanins occur as compounds involving one or more molecules of a sugar. Of these sugar molecules one is always attached at the 3 position ; if there is a second sugar molecule it may be attached either directly to the first one or it may unite with the anthocyanidin in a different position, at 5. Hence there are two classes of glycosides : (*a*) those with one or two sugar molecules attached at position 3, and (*b*) those with sugar molecules at both 3 and 5. These two classes are visibly different in colour and constitute another important factor in flower colour variation, the 3 : 5-diglycosides being bluer than the corresponding 3-type.

A third variable involving structural difference in the anthocyanins is the existence or otherwise of methylated hydroxyl groups, where the hydrogen atom of a hydroxyl group has been replaced by a methyl (CH_3) radicle. This results in an increase in redness. As a rule the only hydroxyl groups methylated are those at positions 3′ and 5′ ; that at 4′ is never methylated. Thus there is one methyl ether of cyanidin and there are two of delphinidin (3′-mono- and 3′ : 5′-di-).

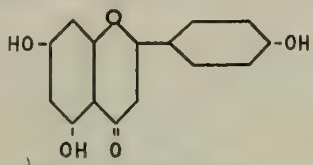
So far three factors influencing the colour of anthocyanins have been dealt with, namely :

(*a*) The number of hydroxyl groups in the molecule.
(*b*) The position of attachment of the sugar molecules.
(*c*) The methylation of hydroxyl groups.

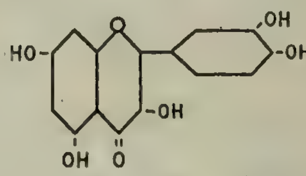
Combinations of these three give rise to twelve anthocyanins, differing slightly in colour and covering a wide range from scarlet to purple.

These factors are all dependent upon structural changes in the anthocyanin molecule, that is, the differences are internal. However, conditions external to the molecule may also affect the colour of the anthocyanins. The most important of these is a phenomenon known as copigmentation, which will be referred to in connection with the anthoxanthins. Secondly, modification of the flower colour can be brought about by variation in the *p*H of the cell sap, the colour becoming bluer as the *p*H is increased.

The Anthoxanthins.—The substances included under this heading are closely related chemically to the anthocyanins, but differ in colour, ranging from pale ivory to deep yellow. Like the anthocyanins, they are sap soluble and usually occur as glycosides. Structural variation is greater than in the case of the anthocyanidins, but the majority are analogous and fall into two classes, the flavones and flavonols, which differ in that the flavones have no hydroxyl group at position 3.



Apigenin - a flavone.



Quercetin - a flavonol.

Increase in the number of hydroxyl groups present in an anthocyanidin molecule results in increased blueness. A similar effect is manifest in the flavones and flavonols, which become more yellow.

There are four ways in which the anthoxanthins are concerned in flower colour:

(*a*) In flowers which have no anthocyanin they may be directly responsible for some or all of the colour.

(*b*) If a yellow anthoxanthin occurs together with an anthocyanin, the resultant colour is a blend of the two.

(*c*) In the presence of anthocyanins, ivory anthoxanthins, as would be expected, do not contribute directly to the colour; nevertheless they may be of great importance on account of their 'copigmenting' action. When present in the same solution as an anthocyanin they combine loosely, in some way as yet unknown, with the anthocyanin, to give a much bluer colour. The effect is very marked, and flowers with a copigmented cyanidin derivative may appear bluer than those containing an uncopigmented delphinidin glycoside. It is probably not an exaggeration to say that upwards of 70 % of garden flowers are copigmented, to some extent at least. The degree of copigmentation varies with the nature of the anthocyanin, delphinidin being most readily copigmented and pelargonidin derivatives least. It also varies with the nature of the flavone or flavonol. There is no exact information on this point, but observations show that yellow anthoxanthins do not generally behave as copigments.

(*d*) It has been pointed out that there is a close relationship between anthocyanins and anthoxanthins, as is shown by inspection of their respective formulæ. Therefore their syntheses in the plant might be expected to follow similar lines. Evidence in favour of this suggests that the anthocyanins and anthoxanthins are formed from the same starting material, which may be limited in quantity. This results in competition between the two, and if most of the source is utilised in the synthesis of one pigment, then of necessity less of the other is produced. For example, the presence of much anthoxanthin may lead to almost complete suppression of anthocyanin, resulting in delicately flushed flowers.

Plastid Pigments.—The plastid colouring matters comprise a number of yellow or orange substances such as xanthophyll or carotin, which are insoluble in the cell sap and are quite independent of the sap-soluble colouring matters. In the absence of anthocyanins they are either solely responsible for flower colour, or are supplementary to the yellow anthoxanthins. In the presence of anthocyanins their effect is purely that of a background; thus in the tulip the introduction of yellow plastid pigment changes the colour from pink, crimson or purple to orange, scarlet or brown respectively.

The first part of the exhibit includes examples of the various types of pigments and shows the effect on anthocyanin colour of variation in the number of hydroxyl groups, glycosidal type, methylation of hydroxyl groups and copigmentation. Mixture and background effects of anthocyanins with anthoxanthins and plastids are also shown. In addition there are three of the rarer flower colouring matters. Gesnerin from *Gesnera* species is unusual in that it has no hydroxyl group in the 3 position. *Celosia* and *Iresine* contain nitrogenous pigments similar to that found in beetroot. Flowers of *Papaver nudicaule* also contain a introgenous colouring matter, but of a different type.

The Genetics of Flower Colour.

Flower colour variation has been studied genetically for many years, but until recently, as pointed out earlier, the only possible criterion of colour types was the visual one. The result was a chaotic mass of information about the inheritance of flower colour, which was only disentangled when the means by which variation is brought about were recognised. The position now is that nearly all of the factors listed in the table are known to be controlled by single genes.

Table.

Variation in flower colour is brought about by one or more of the following factors. Changes are shown in one direction only; the reverse may be inferred.

Anthocyanins.	i. Increase in number of hydroxyl groups	Increased blueness.
	ii. Alteration from 3- to 3 : 5- sugar types	,, ,,
	iii. Methylation of one or more hydroxyl groups	,, redness.
	iv. Increase in pH	,, blueness.
	v. Copigmentation	,, ,,
Anthoxanthins.	vi. Increase in number of hydroxyl groups	Increased yellowness. Alteration of background. Change in copigment effect.
	vii. Interaction of anthocyanins and anthoxanthins	Partial suppression of one or both types.
Plastid Pigments.	viii. Alteration in nature of plastid pigment	Yellow becomes orange. Alteration of background.

In addition there are genes which govern the presence or absence of anthocyanins, anthoxanthins or plastid pigments, and genes which produce a general or local intensification or dilution of colour.

The biochemical value of this work lies in the fact that single genes have been shown to control simple chemical reactions, such as oxidation resulting in the introduction of a hydroxyl group, and glucoside formation. In sweet peas two whites crossed together may give coloured progeny, each parent introducing a different gene necessary for anthocyanin formation. Similarly in maize, thirteen dominant genes are necessary for the production of chlorophyll. It seems then that each stage in the synthesis of any plant product is governed by a single gene, and the geneticist is therefore able to separate the metabolic processes into their different stages. This should make it possible for the biochemist to find out what the reactions are and how genes bring them about.

The second part of the exhibit is designed to show that the differences in flower colour are controlled by single genes which conform to the Mendelian laws of inheritance. Examples are given of the inheritance of alternative pairs of gene-controlled characters :

3 : 1 ratio in F_2	(*a*) Anthocyanin—no anthocyanin. (*b*) Anthoxanthin copigment—no copigment. (*c*) Delphinidin (oxidation)—cyanidin (no oxidation). (*d*) Higher *p*H—lower *p*H.
1 : 1 ratio from back-cross	(*e*) Diglycoside—monoglycoside.

These five show complete dominance of the first character, that is, the heterozygote is indistinguishable from the homozygous dominant—one gene is doing the work of two. Incomplete dominance is shown in the cross red × white *Antirrhinum* ; the first generation is intermediate (pink), and in the second generation a ratio 1 red : 2 pinks : 1 white is obtained. The red and white breed true, but the pinks always throw red, pink and white.

In the cross purple × scarlet *Verbena* independent segregation of two pairs of characters is shown, with recombination in the second generation resulting in the production of two new colour types. For example, pelargonidin 3 : 5-diglycoside is produced by bringing together the gene for diglycoside carried by one plant with that for pelargonidin from the other parent.

In a similar manner it is possible to obtain an anthocyanin different from that of either of the parents. The salmon *Streptocarpus* carries a gene for methylation, but this gene has no effect when the anthocyanin is derived from pelargonidin. On crossing with a variety containing cyanidin from which the methylation gene is absent, the flowers of the progeny contain a methylated cyanidin derivative.

The segregation of three pairs of factors is shown in the second generation from the cross blue × salmon *Streptocarpus*. Of the three genes involved

M produces malvidin.
P produces peonidin.
I produces copigmented flavone.

The combinations are as follows :

36	27 MPI	copigmented malvidin glycoside (bluish mauve).
	9 MpI	
12	9 MPi	uncopigmented ,, ,, (reddish ,,).
	3 Mpi	
	9 mPI	copigmented peonidin glycoside (bluish rose).
	3 mPi	uncopigmented ,, ,, (reddish ,,).
	3 mpI	copigmented pelargonidin glycoside (salmon pink).
	1 mpi	uncopigmented ,, ,, (salmon).

Capt. R. D. Williams.—*Chlorophyll deficiencies and flower colour in Red Clover* (*Trifolium pratense*).

The plant is a diploid and is normally cross-fertilised ; inbreeding has disclosed numerous recessives determining chlorophyll production and flower colour. Nearly half the chlorophyll deficients are lethal in the early seedling stages. Some of the viable mutants are exhibited. The light green types (g_b, g_c, g_d, g_f, g_h) show a slightly lighter green colour than the normal. Others (g_i) are paler, while some (g_a, g_e) are yellow-green in colour. The colour difference may be shown throughout the life of the plant or, as in g_k, the plant may become progressively greener with age. In other cases, the gene determines a variegated (v_a, v_b), flecked (v_c) or speckled (v_d) light and dark green mosaic in the leaf.

The normal purplish red flower colour is determined by at least twelve dominant complementary factors. Seven of the recessive types are shown, viz. pure white (c), white (c_y), very faint pink (c_a), pale pink (c_{li}), pink (c_p), pale mauve (c_l), slaty blue (c_b).

Miss U. Philip.—*Colours in Drosophila.*

The normal or wild type eye-colour of *Drosophila melanogaster* is a bright cherry red. Chemical investigation has shown it to be a combination of three water-soluble pigments : a red one which is irreversibly turned into a brown one, and a yellow one which becomes red by oxidation, a process which is reversible. These pigments are deposited during two distinct phases of the pupal stage in pigment cells surrounding the ommatidia proper.

About fifty different recessive eye-colour characters are known. These can be classed according to the way in which they affect the normal composition of the eye-colour. (1) The pigment granules may be present in proportions similar to those in the wild type but fewer in number, as in the series of eye-colours at the sex-linked locus of ' white.' Of the continuous range of types five are selected, namely ' coral,' which is not markedly different from normal, only the transparency being affected ; ' apricot,' ' eosin ' and ' tinged,' which are intermediates ; and finally ' white,' in which no pigment is deposited. (2) In eye-colours of the ' vermilion ' type less brown pigment is present than in the wild type eye. By the transplantation technique it has been established that several different genes represent changes at different stages in eye-colour development. (3) In the mutant ' brown ' the red component is missing altogether, giving the aberrant eye a darker and more dilute appearance. In the combination ' scarlet-brown ' no pigment at all is formed, ' scarlet ' cutting out the brown component and ' brown ' the red one. (4) Without affecting the density of the pigmentation the red may be changed to a dark ' sepia ' colour, possibly by oxidation. (5) By dilution and different rate of pigment deposition the bluer ' purple ' eye-colours are obtained.

It is interesting to note that the eye-colours of other *Drosophila* species, though the wild type may be different and may possibly correspond to a mutant of *D. melanogaster*, show a similar range of variation. This point is illustrated by genotypes obtained from inbreeding wild populations of *D. subobscura*.

The wild type body colour of *D. melanogaster* is yellow with black stripes on the abdomen and a certain amount of black pigment on the thorax and bristles. In ' yellow ' the black pigment is reduced in amount, the stripes on the abdomen are light brown, there is no diffuse black pigment on the thorax, and the bristles are light. In ' black ' and ' ebony ' the yellow colour is covered by black. In the combination ' yellow-black ' the animal has a yellow ground colour with a dusting of black.

Several forms of dilution have been found in the nearly completely black *D. subobscura*. As a yellow mutant has been found, it is clear that this species is capable of forming a yellow pigment, which is hidden under the strong concentration of black colouring matter.

Dr. J. N. Pickard.—*Rabbit coat-colours.*

The demonstration of rabbit pelts has been arranged to show how a knowledge of genetics can be used for practical purposes in animal husbandry. In the centre of the top row of pelts is an ' agouti ' or wild rabbit fur, which is the original colour. To the left and right of this are pelts showing colour mutations. Combinations have been made between these mutations and as a result the pelts of the lower row have been produced. For example, it is expected that on crossing a ' black and tan ' rabbit (a^tB C) to a ' chinchilla ' (A B c^{ch}) and inbreeding the offspring, that one ' silver fox ' (a^tB c^{ch}) would be produced amongst every sixteen youngsters bred. It, mated to its like, would breed only ' silver foxes.'

There have been produced varieties of rabbits whose pelts when made up closely resemble almost all of the rarer fur-bearing animals. The demonstration shows, by means of coloured ribbons, the crosses which can be made in a number of such cases, although, for lack of space, the shortest method of producing a new variety is not shown in all instances. In addition to the normal coated pelts, ' Rexes ' with plush-like furs, and ' Angoras ' with coats measuring up to ten inches or more in length, can be ' made ' in any colour.

Mr. M. S. Pease.—*Yellow fat in Rabbits.*

The colour in rabbits with yellow fat is due to xanthophyll absorbed from the green food. Rabbits with white fat have in their livers an enzyme which breaks down the xanthophyll. The presence of this enzyme is determined by a dominant gene; in its absence the enzyme is not formed and xanthophyll is deposited in the fat.

Prof. R. C. Punnett, F.R.S.—*Colour in Budgerigars.*

The principle colour varieties depend upon genes affecting amount of melanin, production of a lipochrome and alteration of feather structure. Of the three allelomorphs affecting melanin production, R_r produces the normal intensity, R_g the reduced amount shown in ' grey-wing ' types, and R_p the ' pallid ' forms. The dominant L produces a yellow lipochrome; interaction with the R series gives various shades of green. When the lipochrome is absent, as in l birds, there is a series of blue forms. The various tones of green and blue depend upon S and s. In birds with S the feathers are modified in structure so that blue is less vividly reflected. Heterozygotes are intermediate, being olive in the green series (L), and cobalt in the blue series (l).

Prof. R. R. Gates, F.R.S.—*Colour inheritance in Man.*

(1) *Eye-colour.*—Simplex blue eyes are due to the absence of brown (melanin) pigment from the anterior surface of the iris, the blue appearance resulting from the posterior purple pigment of the choroid being reflected through the muscle fibres in the iris. In European populations and in crosses with American Indians, Eskimos and other races blue is a simple recessive to brown, with certain complications not fully understood (see Gates, *Heredity in Man*). Dominantly inherited blue eyes have recently been observed in natives of Ceylon believed to be of pure Singhalese descent. It may be noted that the intense ' black ' eyes of American Indians and of most Singhalese as well as various other races is probably due to an intensifying factor. Some Singhalese have pale brown eyes. The yellowish sclerotic coat of the eye in negroes is due to dilute melanin pigment.

(2) *Hair Colour.*—A recent statistical study of the post-natal development of hair and eye colour in 2,670 schoolboys of Sheffield (MacConaill and Ralphs, *Ann. Eugenics*, **7**, 218–225, 1936) classifies those with blond hair and blue eyes as leucochromes. This type falls steadily in numbers from five years of age to a steady ratio of 17 per 1,000 at puberty. The leucochrome is regarded as a Mendelian recessive, the other types of pigmentation, allochromes (i.e. dark hair and/or eyes) showing delayed dominance. This is true of European peoples in general ; but in more pigmented races the babies have intense ' black ' eyes and hair from a very early age. Blue eyes and blond hair probably arose through mutations from dark hair and eyes. Red hair is due to a lipochrome pigment independently inherited.

(3) *Skin colour.*—In a study of skin pigmentation in parents and offspring, the Davenports (*Amer. Nat.* **44**, 641 and 705, 1910) found that in Caucasian families blond × blond have only blond children and two albino parents only albino children. Brunet is epistatic to blond, and some intermediates at least are heterozygous. From negro × white crosses, Davenport (*Carnegie Inst. Wash. Publ.* No. 188, 1913) concluded that two factors for skin colour were present in the negro. In Ojibway Indian × white crosses Gates concluded that two or more factors for skin colour are present, one of which also affects eye-colour or is closely linked to a factor for eye-colour.

Friday, August 19.

JOINT DISCUSSION with Section K (Botany), on *The mechanism of evolution* (10.0).

Chairman : Prof. D. M. S. WATSON, F.R.S.

Dr. J. S. HUXLEY, F.R.S.—*Character gradients* (10.0).

It has been found that many characters of organisms vary in a graded way, apparently always in direct or indirect correlation with variations in the external environment. *Cline* has been suggested as a technical term for such regularities, to be employed as a subsidiary (not alternative) method in taxonomy to that of naming areal groups. This latter method, applied alone, attaches undue importance to named as against unnamed groups, and suggests an unreal uniformity within named groups. The best known cases of clines apply to warm-blooded animals and have been subsumed under the so-called rules of Gloger, Allen, etc. These rules apply only to the *means* of subspecies or allied species ; but regularities *within* named areal groups may also occur. Thus clines may be inter- or intra-group. Clines for different characters may run in different directions (shrikes, American sparrows) or in different ways in the same direction (bees). In many cases clines concern physiologically important properties such as temperature-resistance (*Drosophila*), ecological preference (*Plantago*), reproductive adaptations (*Lymantria*), etc. When intra-group clines occur, these may be gradual, but joined by steep ' genoclines ' where two subspecies meet.

It is probable that research will show many more examples of clines, even in apparently uniform populations, though small isolated populations tend to develop ' accidental ' (non-adaptive) characters unrelated to any character-gradient (see Sewall Wright for the theoretical basis of this well-known fact).

If so, we may envisage small-scale evolution as operating in some such way as this :

(1) Selection adapts the characters of large populations locally to the environmental conditions, thus producing clines. The slope of the cline will depend on the intensity of selection, the degree of variance available, and the freedom of gene-spread due to interbreeding. The clines may be geographical over wide areas, or ecological over small distances.

(2) Barriers due to isolation, geographical or ecological (or in some cases genetic), may interrupt the uniform clines. If the barriers are complete, the slope of the new intra-group clines will be much reduced due to interruption of gene-flow, though the clines will remain in inter-group form. If the barriers are incomplete (contiguous subspecies) the slope of the intra-group clines will be less reduced, and they will be connected by steep genoclines.

In addition, the clines will be partially obscured by the fixation of ' accidental ' characters.

(3) Subsequent migration may still further obscure the original regularities.

Extremely interesting comparative studies could be made of the slope of clines (inter- and intra-group) for the same character in related species.

Prof. R. A. Fisher, F.R.S.—*Selective intensities in nature* (10.25).

Polymorphic species provide an exceptional opportunity for estimating the intensity of selective agencies in nature. Some results are presented, based on the collections of grouse locusts organised by Dr. R. K. Nabours and his associates.

Prof. A. E. Trueman.—*Orthogenesis* (11.50).

The term orthogenesis, proposed by Eimer in 1895, has been variously used by later workers. To some it has implied no more than evolution by definite and successive variations in a given direction, as contrasted with indefinite variation ; but many workers have used the term with some implication of causation, the direction of evolution being supposed to be dependent upon some internal factor, or at least to be independent of the environment.

While some biologists have lately put forward evidence of orthogenetic evolution, orthogenesis has received most sympathetic attention from some palæontologists. It must be stressed, however, that while palæontologists occasionally secure reasonably good evidence of lines of evolution, their material is usually too incomplete to afford a basis for useful speculation concerning the causes of evolution. The term ‘ orthogenesis ’ is thus more suitably used to indicate the nature of the phenomena observed than as an explanation of the mechanism.

Owing partly to the ‘ mystical ’ nature of the supposed internal factors involved in some interpretations of orthogenesis, other terms have been suggested by palæontologists for similar phenomena. Kitchin wrote of ‘ programme evolution ’ while W. D. Lang formulated the ‘ Doctrine of Definite Trends ’ : it is noteworthy that more recently the term ‘ trend ’ has been used with a similar meaning (apparently independently) by M. M. Metcalfe in his work on Infusoria.

The following conclusions have been reached by palæontologists dealing with many different groups :

(*a*) In many groups there appears to be definite variation confined to a comparatively small number of trends.

(*b*) Similar forms (homœomorphs) are frequently produced in different lines by ‘ parallel evolution.’ These are not always contemporaneous and in many cases the changes have been thought to be unrelated to any environmental conditions (e.g. certain Brachiopods and Ammonites) ; in other cases the changes were probably adaptive (e.g. the frequently developed ‘ oxycone ’ Ammonites).

(c) Similar stages in ‘ unit characters ’ may be recognised in different stocks when the whole skeleton is not homœomorphic.

(*d*) In some cases, especially in their later stages, the changes are clearly out of harmony with the environment (e.g. *Gryphæa*, some Cretaceous Bryozoa) and may lead to extinction.

Mr. J. Z. Young.—*The evolution of the relationship of organisation and environment* (11.15).

Capt. C. Diver.—*Polymorphism* (11.40).

Polymorphism is a well-marked expression of variability, but the term polymorphic is often restricted to those cases in which several distinct phases occur together within the same population. This restriction lays some stress on discontinuity and suggests a dividing line which is neither sound in theory nor easily drawn in practice. The variability of an organism must be strictly confined within those limits of biochemical and structural possibility which will still allow the organism to function as a co-ordinated whole ; and species appear to vary in the amount of this potential variability they express at any time. It is necessary first to determine the nature of the factors which provoke and control any particular polymorphic display, before it is possible to estimate its evolutionary significance.

The problem can be illustrated by numerous examples, but attention is particularly directed to three cases :—*Limnæa peregra*, a very plastic species but not markedly polymorphic in the restricted sense ; *Helix* (*Cepæa*), a species showing pronounced polymorphism ; and *Plantago maritima*, a species which displays polymorphism together with considerable ecological plasticity.

Dr. P. D. F. Murray.—*Consequential evolution* (12.5).

The term ' Consequential Evolution ' is used when some change occurs as a necessary result of some environmental or genotypic change and is exerted via a developmental process which is itself unaltered. The conception is discussed in the light of such instances as the effect of size changes on proportions of parts (allometric growth in Titanotheres, horses, etc.), the effects of variation in genes controlling the rates of biological processes, and of alterations in gradient systems on developing patterns.

General Discussion (12.20).

Afternoon.

Joint Discussion (with Section K) on *Mechanism of evolution* (*continued*) (2.15).

Chairman : Prof. E. J. Salisbury, F.R.S.

General theme of the papers.—*Isolation and speciation.*

Dr. E. B. Worthington.—*Geographical isolation with special reference to fresh waters* (2.15).

Evolution is controlled in part by external factors, and these can be studied best where isolation, such as that afforded by islands or freshwaters, limits the environment. In freshwaters the mixture of fauna and flora is prevented by (*a*) physical barriers, such as watersheds and waterfalls, (*b*) ecological barriers. The evolution of a habitat (e.g. a lake silting up or undergoing chemical change) involves changes in fauna and flora. In a wet climate such as England this is usually effected by the loss of some species and the addition of others from outside, but in a dry climate where isolation is more complete (e.g. parts of Africa) organic evolution may keep pace with the evolution of habitats. This is demonstrated best in the tropics where generations are passed through more quickly, and evolution is therefore more rapid. In the presence of predators the intermediate stages in species formation (when the organisms do not fit their environment) rarely survive ; hence evolution is most rapid and most obvious where predators are relatively unimportant. These principles are illustrated from British and African waters.

Dr. W. B. Turrill.—*Ecological isolation* (2.35).

The difficulty of limiting the phrase ' ecological isolation ' is discussed and it is pointed out that ecological isolation is often exactly associated with geographical and other kinds of isolation. The evolutionary and taxonomic significance of this association is considered. Examples of isolation caused by the action of climatic, edaphic, and biotic factors respectively are given. The importance of the complicated interaction of ecological factors with one another is emphasised and it is shown that ' habit ' is a plant character (or group of characters) frequently sifted out by natural selection.

The taxonomic categories are primarily matters of scientific convenience and the criteria used to delimit them need only be kept constant for any one purpose. The species category is the most important for many purposes and speciation is a concern of the taxonomist. Examples of speciation correlated with ecological isolation (and sometimes probably initiated by it) are given, especially from the flora of the Balkan Peninsula. It is concluded that ecological isolation is (and especially has been) one of the factors involved in speciation in the wild, but that it is often broken down by man's interference. The importance of the study of wild floras and faunas, from all standpoints, thus becomes evident, if the processes not only of speciation but of evolution in general are to be understood.

Dr. W. H. THORPE.—*Physiological isolation* (2.55).

Dr. C. D. DARLINGTON.—*Genetic isolation* (3.15).

John Ray described a species as a group breeding true within its own limits. Modern genetics show that this is a definition valid in theory and practice. It means that the origin of two species from one must depend on a barrier which effectively prevents inter-breeding in nature. We now know that such barrriers are broadly of two kinds, external and internal. The internal barriers act physiologically or mechanically but are determined genetically. Genetic isolation may act in plants by the pollen growing more quickly on styles having certain similar genes than on styles having those genes in a mutant form. It may act in an analogous way in animals through the mating instincts or capabilities. The building up of such differential systems cannot be achieved in one step by a gene mutation. In nature it seems that its origin usually depends on the action of a group of genes. Such a group can be held together only when associated with a sudden change in the arrangement of genes. Changes in arrangement alone can produce genetic isolation, without the differential action of genes, by causing sterility of the hybrid. Since genetic isolation will frequently give rise to geographical or ecological isolation in nature and they will regularly give rise to genetic isolation, the order of events cannot always be inferred. But where genetic isolation alone is concerned, the several steps establishing it have been disentangled in particular instances by gene and chromosome analysis, and placed in the chronological order indispensable to their evolutionary development.

Dr. D. G. CATCHESIDE.—*Chromosomal isolation* (3.35).

Chromosomal isolation may operate by preventing the formation of a hybrid or, more usually, by preventing in a hybrid the normal exchange of genetic material between parts of chromosomes, whole chromosomes or sets of chromosomes. The former frequently occurs in flowering plants where-ever there is an upset in the normal one-to-two relation between the chromosome numbers of pollen and of style or an upset in the two-to-three relation between embryo and endosperm. The latter method operates at meiosis wherever there is a numerical or a structural difference, such as an inversion or an interchange, between the chromosomes of the gametes producing the individual. There is a reduction in fertility, which is particularly at the expense of cross-over gametes in structural hybrids. Simple examples illustrating these principles are described.

DISCUSSION (3.55). Opened by Dr. S. C. HARLAND.

Saturday, August 20.

Excursion to Wicken Fen and the Breck country.

Monday, August 22.

SYMPOSIUM on *Sense perception and the evolution of colour and pattern* (10.0).

Dr. J. S. HUXLEY, F.R.S.—*The bearing of allæsthetic characters on our knowledge of sense perception in animals.*

Allæsthetic characters are those adapted for exerting an effect via the (distance) receptors of another organism of the same or different species. Criteria of various degrees of validity can be established for the allæsthetic nature of characters. The nature of allæsthetic characters permits deductions as to the type of sense perception to be found in the organisms affected, e.g. the commonness of bright colours in birds and entomophilous flowers and their rarity in sub-primate mammals points to the existence of colour vision in birds and insects, its absence in the mammals—a conclusion later confirmed by experimental evidence. Similarly bird-pollinated flowers tend to be of a different colour from insect-pollinated forms, owing to the difference in the reception of red and ultra-violet in the two groups.

Visual allæsthetic characters are of various functional types. The first division is into cryptic and sematic. Sematic characters fall into various groups—warning, threat, recognitional, deflective, and display. Each has its own features of pattern, etc. Consideration of the various types allows us to draw interesting conclusions as to the receptor and perceptor faculties of the organisms at which they are directed.

A marked general similarity of visual perception in different groups of animals is indicated, with some limited but striking exceptions.

Dr. H. B. COTT.—*Adaptive appearance and interspecific relationships* (10.30).

In the interrelationships between animals of the same, or of different species, external appearance plays a considerable part. Broadly speaking, the various phenomena of adaptive coloration fall into three main classes, according to the visible results achieved—namely, concealment, advertisement, and disguise; and their respective functions, in the interspecific relationships of animals, are to elude, to attract, or to deceive the eyes of potential enemies or prospective prey. These visual effects are related to the two primary needs of the individual—food and safety: in other words, they are correlated with offence and defence—they facilitate the capture of prey, or escape from the predator. The appearance produced may be extremely elaborate in artistry, and highly effective in action—depending not merely upon profound modifications of structure, colour, and pattern, but of attitude and instinctive behaviour.

The view that adaptive appearances have evolved in relation to the visual perceptions of animals—that they appeal to the eye—is supported by a great body of evidence, which is considered from various standpoints: (1) The arrangements of colour and pattern which for optical reasons are best adapted to produce special visual effects, are those actually employed in the coloration of different cryptic, aposematic, and mimetic species. (2) Disruptive patterns, especially those of the coincident type (Cott, 1935, *Rept. Brit. Assn.*, p. 384), are independent of, and frequently cut right across, underlying structural elements—anatomical features becoming subordinate to the illusionary appearance superimposed upon them. (3) Particular colour schemes are largely independent of affinity, and furnish a special application of the principle of adaptive radiation and convergence. (4) The general habits, the resting attitudes, and the special protective and aggressive reactions of animals are closely correlated with their scheme of coloration, and their surroundings, and are to be explained in terms of the psychology of vision.

Mr. D. Lack.—*Bird courtship and aggressive behaviour* (11.0).

The term ' courtship display ' has been applied extremely loosely in bird behaviour. Many displays really serve a threat function, and displays which are directed at the female may occur in three different phases of the breeding cycle : (1) by the unmated male before pairing up ; (2) by one or both sexes after the pair have associated and leading up to copulation ; (3) by both sexes during the post-nuptial period.

Sexual selection in Darwin's sense typically has reference only to the first phase. The evidence for preferential matings in birds is summarised.

Recent experiments with stuffed specimens illustrate the influence of threat and display characters in producing aggressive behaviour and courtship, and the part they play in sex recognition. Experiments with robins, which will sometimes attack parts of stuffed specimens, show that the problem is not simply one of ' recognition,' but is very complex.

Mr. I. H. Burkill.—*Insect vision and the perception of flowers* (11.30).

That bees and butterflies see the flowers they visit is axiomatic. A feeding hive-bee by differential stimuli in the upper and the lower parts of the eyes is guided at a suitable distance from the vegetation : then a dot of colour, say a buttercup flower, is seen at short range ; at 6 in. it is seen to be lobed, and by a reflex the feet are brought to a position for landing. Satiety, like hibernation, asks for rest ; and the same front facets find a hole which will admit the body—a hole with a contrasting rim is best realised. Colour helps ; for bees see colours, though not quite as we see them. The homing bee apparently sees the landscape in chiaroscuro : and the numbers which fail to home indicate that recognition is difficult.

Insects' eyes differ enormously in the number of visual units, and probably in receptivity : they are so unlike ours, that it is well to set up an insect type. The best type is the hive bee's. The prodigious diligence of that insect in its simple errand makes it peculiarly useful for experiment. When we pass to insects themselves more brightly coloured, the question of seeing mates and flowers with the same colour-sense comes in.

Lastly, bilateral symmetry in flowers is in the same plane as bilateral symmetry in a flying insect, and the two are connected.

Discussion (12.0).

Dr. C. G. Butler.—*Phases in locusts* (12.30).

Uvarov (1921) and Faure (1932) have shown that whereas the species of migratory locusts were formerly regarded as being more or less monomorphic, they are really polymorphic, and can occur in two extreme forms or *phases*, i.e. the phase *gregaria* and the phase *solitaria*, with a large number of intermediates (phase *transiens*) between them.

The phase *gregaria* differs from the phase *solitaria* especially in the coloration of the nymphs or hoppers, and in such adult characters as the shape of the pronotum and relative length of other parts of the body.

Strelnikov (1936) and Butler and Innes (1936) have shown that there are fundamental physiological differences between the phases.

Experimental data show that the formation of the gregarious phase from the solitary occurs as a direct result of crowding hoppers into a limited space. This transformation is brought about by the increased activity due to mutual stimulation.

Attempts are made to analyse in the light of recent research the factors involved in this crowding and thence to determine why the phases take on their particular characters.

Afternoon.

Semi-popular Lecture by Mr. H. C. Gilson on *Lake Titicaca* (2.15).

An expedition financed by the Percy Sladen Trust left England in March 1937 to spend six months from April to September studying the conditions of life and the fauna and flora of Lake Titicaca and other bodies of water in its neighbourhood. Lake Titicaca is a tectonic lake some 100 miles long by 30 wide and about 1,000 ft. deep at the deepest. It lies in latitude 16° S. at an altitude of 12,500 ft. above the sea, on the high central plateau of the Andes, some two-thirds of its area being in Peru and the rest in Bolivia. It is an area of inland drainage which has probably been isolated from other water masses since the Eocene period, and it was already known from the work of other expeditions to have a somewhat peculiar fauna. This proved to be rich in individuals but poor in species, making it a very interesting study for the ecologist.

An account illustrated by lantern slides and cinematograph film is given of the life and work of the expedition and of the native 'Indians,' who are the descendants of the people of the Inca régime.

Exhibition of biological films (3.15).

Films produced by Strand Film Co., Ltd., under the direction of Dr. J. S. Huxley, F.R.S. :—

Fingers and thumbs.
Monkey into man.

Films produced by Gaumont-British Instructional Films Co., under the direction of Mr. H. R. Hewer :—

The Liverfluke.
The Crayfish.
The development of the Trout.

Tuesday, August 23.

Symposium on *The rôle of the environment in animal locomotion* (10.0).

Prof. J. Gray, F.R.S.—*Introduction.*

Mr. J. E. Harris.—*Aquatic forms* (10.15).

Prof. J. Gray, F.R.S., and Dr. Lissmann.—*Terrestrial forms* (10.45).

Dr. F. S. J. Hollick.—*Aerial forms—insect flight* (11.15).

Dr. C. Horton-Smith.—*Aerial forms—bird flight* (11.45).

Discussion (12.15).

AFTERNOON.

Mr. F. S. RUSSELL, F.R.S.—*Diversification of form in medusæ* (2.15).

Medusæ are animals with a very simple basic plan, living in a comparatively constant environment, yet they have evolved great diversity of form. This paper is concerned only with the Anthomedusæ and Leptomedusæ. An account is given of the different types of form to be found in each of the chief organs throughout the group as a whole. Hydromedusæ swim by pulsations of the bell, whose aperture is partially closed by a velum. On contraction of the bell the velar aperture decreases in size and the velum thus exercises control over the jet of water expelled from the subumbrella cavity. It is suggested that this method of swimming has limited the size to which Hydromedusæ have evolved. This is contrasted with the large size of the Scyphomedusæ, whose umbrella margin is lobed, and which swim by an essentially different method. Some implications of the varieties of form found in other organs are discussed and it is shown how in every character the Anthomedusæ show great variety while the Leptomedusæ appear to be very limited in the forms evolved.

Mr. E. R. GUNTHER.—*A fishery survey of the Patagonian continental shelf* (2.45).

The extensive area eastwards of South America known as the Patagonian Continental Shelf has been the subject of recent survey. The shelf, comprising some 185,000 square miles, includes a varied benthic fauna whose distribution suggests the presence of distinct habitats. Salient ecological characteristics are noted, and the distributions of various species are shown to be correlated with such factors as temperature, depth, and texture of the sea floor.

The fish inhabiting these grounds were sampled with a commercial otter trawl at some 200 stations. The survey, while essentially a reconnaissance, aimed also at collecting information which would lead to knowledge of the habits and movements of the fish. Their distributions are considered and are shown to be divisible into regional groups of which the species occupying the plain of the shelf were the most abundant. Fish giving evidence of migration are compared with more stationary types, and their habits are inferred from such features as their length, colour, body-form and food.

The survey, which is under the direction of the Discovery Committee, was spread over three seasons and was carried out by the Royal Research Ship *William Scoresby* in her programme of research and development in the Dependencies of the Falkland Islands.

Dr. T. S. WESTOLL.—*The origin of the Tetrapods and their relation to the bony fishes* (3.15).

The discovery of Upper Devonian Stegocephalia (Ichthyostegalia) from East Greenland enabled Säve-Söderbergh to discuss the relationship of primitive Tetrapods to Crossopterygian fishes, and to revise the homology of the dermal bones. The newly discovered *Elpistostege*, from the basal Upper Devonian, is probably a Tetrapod ancestor, and shows that both the classical homologies and Säve-Söderbergh's revision are misleading. The correlation is in most cases a simple bone-for-bone equivalence, but the Crossopterygian 'frontal' is homologous with the Tetrapod parietal.

The Tetrapods were certainly derived from Crossopterygii, and the process involved extensive changes in proportions; the preorbital region was drawn out while the otico-occipital region was much shortened.

The Actinopterygian 'parietal,' 'frontal,' etc., are not homologous with the similarly named Tetrapod elements; the 'frontal' is approximately equivalent to the Tetrapod parietal. The Actinopterygii evolved quite independently.

Various considerations strongly suggest that the primitive Choanate and Actinopterygian skulls had a large number of dermal bones, some of which were closely related to latero-sensory organs. In primitive forms of all groups these bones become more important at the expense of intervening bones. The latero-sensory canals are therefore of great value in determining bone homologies, but in many later forms new factors modify or invalidate this primitive key.

Dr. W. H. Thorpe.—*Respiration in parasitic insects* (3.45).

SECTION E.—GEOGRAPHY.

Thursday, August 18.

Presidential Address by Prof. Griffith Taylor on *Correlations and culture; a study in technique* (10.0). (See p. 103.)

Symposium on *The Scientific Delegation to India, 1937–8: geographical impressions* (11.30).

Prof. A. G. Ogilvie, O.B.E.—*The Chota Nagpur plateau.*

The Chota Nagpur Highlands, almost bisected by the furrow used by the Bengal Nagpur Railway, exhibit several distinct erosion surfaces, now tentatively described. The river net, with its three dominant trends (N.W.–S.E., E.N.E.–W.S.W., and W.–E.), suggests great captures by the rivers Brahmani, Baitarani and Subarnarekha following the formation of the Bengal Bay and Lowland. Westward recession of the plateau edges results in abundant *inselbergs* on the eastern fringes. The relation of forests (chiefly second growth *sal*) and agricultural land to surface relief is discussed, the position of mines indicated, and the distribution of population surveyed in outline.

Prof. H. J. Fleure, F.R.S.—*The aborigines of the Chota Nagpur plateau* (12.0).

Mr. J. McFarlane.—*Jamshedpur; an Indian industrial centre* (12.30).

Prof. C. B. Fawcett.—*South India* (2.15).

Some typical landscapes—tank and village, paddy field, hill country, coastal forest—and the people and occupations in them.

Friday, August 19.

Mr. G. C. L. Bertram.—*King George VI Sound in the Antarctic* (10.0).

King George VI Sound was discovered in the course of the British Graham Land Expedition, 1934–37 (led by Mr. John Rymill). Previous information about southern Graham Land was based mainly on the interpretation of what was seen by Sir Hubert Wilkins in a flight down the east coast in December 1928. After this flight, southern Graham Land was considered to be an archipelago with channels at sea level connecting the Weddell and Bellinghausen seas. The work of the British Graham Land Expedition, 1934–37, showed that in fact the whole of Graham Land forms a single narrow peninsula and that the ' Antarctic Archipelago ' as such is non-existent. On the west side of Graham Land, separating it from Alexander I Island, is a remarkable channel nearly 300 miles in length, to which has been given the name of King George VI Sound. This sound averages 15 miles in breadth, runs almost due north and south, and is bounded on either side by mountainous country running up to over 6,000 ft. The mountains of the Graham Land side are a continuation of the main Andean chain and are made up of rocks of similar petrological facies. In contrast the Alexander Island side for the most part is composed of a great thickness of late Mesozoic sediments. Apart from its geological interest, the Sound is remarkable in containing the only known area of confined shelf-ice ; this shelf-ice being some 3,000 ft. thick.

Prof. F. Debenham.—*The Geographical Laboratory* (10.45).

Miss J. B. Mitchell.—*Suffolk agriculture in the Middle Ages* (11.30).

Suffolk agriculture in the Middle Ages presents a varied picture : there are strong contrasts between the predominantly pastoral region of the Breckland in the north-west and the agricultural economy of the clays and loams of the east. The west was at this period farmed in open fields while the east, by the sixteenth century, was largely enclosed.

The 1327 and 1524 subsidy returns for Suffolk, when plotted, give an indication of the distribution of regional prosperity throughout the area at the beginning and towards the end of the period. The *Inquisitiones Nonarum* of 1341 are relatively detailed for most parts of Suffolk : sufficiently so to give a strong indication of the land utilisation of the county. Much of the data can be mapped and a close correlation is revealed between the soil type and the economy practised : the poor sandy soils of the Breckland are used for sheep farming ; the central boulder clays are essentially corn-growing lands ; and the fertile loams and silts of the eastern river valleys appear to carry an important dairying industry.

Reyce's *Breviary of Suffolk*, published in 1618, gives an excellent picture of the county at the beginning of the seventeenth century. His account shows the same agricultural pattern as was suggested by the fourteenth-century figures.

Mr. F. Walker.—*East Anglia and the Civil War* (12.15).

One of the most striking features of the historical geography of eastern England is the continuous regional distinctiveness which has characterised East Anglia since the very earliest times, and it is an extremely interesting fact that practical expression was given to this ' individuality ' during the Civil War of the middle of the seventeenth century.

In the later stages of the prehistoric period, and at intervals during the early mediæval period, East Anglia formed a definite political unit, but with the disappearance of such territorial divisions its unity became gradually less apparent, and in the period between the eleventh century and the outbreak of the Civil War it was distinguished from the rest of England largely because of the peculiar character of its social, economic, and religious history.

During this period, however, the special features of East Anglian life such as rapid economic progress, the development of trade with the continent, the growth of religious reform and the strength of the commercial and industrial middle classes of society all served to produce a remarkable unity of outlook which culminated in the unparalleled unanimity of this region in its support of the Parliamentary party during the Civil War.

Thus we may see in the Eastern Counties Association the last example of a definite political unit corresponding to the natural region of East Anglia and we may regard its formation as an attempt by the Parliamentary party to utilise to the full the strategic importance of a region whose attitude towards the religious and economic problems of the time had become a foregone conclusion.

AFTERNOON.

Mr. R. H. KINVIG.—*Film : Bombay to Jamshedpur* (2.0).

Mr. J. A. STEERS.—*The physiography of North-west Norfolk* (2.30).

Dr. H. C. DARBY.—*The draining of the Fens* (3.15).

Mr. F. A. SINGLETON.—*The MacDonnell Ranges of Central Australia* (with film) (5.30).

Attention is drawn to the remarkable notches or gaps cut by the streams which cross the parallel ridges of the MacDonnell Ranges, extending east and west for about 250 miles in Central Australia. The usual explanation of antecedent courses for the rivers, though accepted in the case of the James and Waterhouse Ranges, is questioned for the MacDonnell Ranges, for which the possibility of superimposition from a cretaceous cover is tentatively suggested.

Saturday, August 20.

Excursion to Hunstanton and district.

Sunday, August 21.

Excursion to Fenland.

Monday, August 22.

Mr. W. V. LEWIS.—*Cirque formation in Iceland* (10.0).

The *bergschrund* hypothesis of Willard Johnson was almost universally welcomed as an explanation of cirque erosion. It has recently met with much criticism and Johnson, himself, modified his views before his death. According to this theory only the upper portions of the head-walls of cirques should show plucking and shattering, the lower slopes, which were buried deeply under the ice, showing glacial smoothing as a result of abrasive action only. When some of the deeper Welsh and Scottish cirques are examined they are found to reveal plucking and shattering on their head-walls right down to the junction with the cirque floor, even when this face exceeds 1,000 ft. in height. The plucking action that Johnson so vividly described at the foot of the bergschrund appears to have functioned over the whole of the head-wall.

The evidence of Icelandic cirques suggests that an elaboration of the *bergschrund* hypothesis might better account for the facts than the hypothesis as first put forward by Johnson. During the height of summer melting in Iceland the head-walls of the cirques frequently had a large amount of melt-water pouring down them. This melt-water resulted from direct precipitation, from the melting of winter snows clinging to the upper portions of the head-wall, and in certain cases the supply was considerably augmented by melt-water from a small ice-cap resting on the summit above. The volume of water varied greatly from day to day. On reaching the *névé* the water almost invariably melted its way under the ice, thus following the rock face below the glacier. This melt-water was frequently noted pouring down the head-wall where this was visible along the bergschrund, when it was usually augmented by further melting in the *névé* above.

In a cave which followed the rock face under an ice-fall of considerable thickness it was found that even on a warm day the moisture on the rock face was frozen hard. This and further evidence of a similar nature led to the suggestion that much of the melt-water seen pouring down the head-wall, when it gets a certain distance under the glacier, freezes, and that this is particularly liable to happen at night and during cold spells.

The foregoing evidence suggests the following sequence of events in cirque erosion. Large areas of the head-wall are covered with moisture when melting is active and much of this moisture subsequently freezes. The resulting frost action rots the head-wall and loosens large and small rock fragments. These are then removed by becoming incorporated in the glacier as it moves slowly away from the head-wall. This action is naturally most effective when the walls are steep, so that steep slopes, once formed, tend to be self-propagating. The cirque therefore becomes enlarged by the head-walls slowly eating back into the mountain mass.

SYMPOSIUM on *English ports and estuaries in their geographical setting* (10.45).

Mr. F. H. W. GREEN.—*Southampton—hydrographic factors.*

In discussing the economic geography of a seaport such as Southampton, it is very necessary that the facts of its physical hydrography be accurately and systematically stated. This is comparable to the accurate description of the geomorphology of a region on land, which is recognised as a necessary prelude to any discussion concerning its economic development.

The tide is one of the more important aspects of port hydrography, and in the case of Southampton it is of peculiar interest and significance. The unusual complications, which make themselves manifest here, necessitate more careful study than is demanded in assessing the significance of the tide at most British ports. The existence of the complications is, on the other hand, of great practical value in that it enables methods to be devised of forecasting deviations from ' predicted ' tidal times and heights ; some of these have already proved themselves to be surprisingly accurate. An analysis of the tides at Southampton also suggests more accurate methods, applicable to other ports, of forecasting deviations from predicted tide due to meteorological causes.

Mr. A. E. Stephens.—*Plymouth in the sailing ship era* (11.15).

The threefold harbour of Plymouth consists of the Hamoaze, Catwater and the Sound. The value of the last named was minimised by its exposure to the winds of greatest strength and frequency and longest ' fetch,' with the direction of which its axis (N.E.–S.W.) coincided. In strong south-west gales there was shelter in Cawsand Bay under Rame Head, or in emergency in the Sound north of Drake's Island. Catwater, commodious and land locked, was easily entered (except in south-west gales) by a channel leading near the lee north side of the entry. Hamoaze, also deep, commodious and landlocked, had a narrow, sinuous entry, beset by shoals and subject to strong tidal currents and eddies.

The relative frequency of winds from the western quarter made Catwater a more important commercial harbour than Hamoaze. The Hamoaze settlements were relatively unimportant, except for Saltash at the head, commanding cross river traffic and traffic in minerals down the Tavy. To its normal trade function, Plymouth added that of ' clearing house ' for naval prizes. Its commercial development was hindered by its poor and restricted hinterland, but it was a haven of refuge in time of storm, to vessels wind bound at the Channel entrance, or short of water and provisions after an ocean voyage. Sufficiently remote from the continent to avoid surprise attacks, and with facilities for collecting a fleet and victualling an army, its safe commodious harbour was an important rendezvous for foreign military expeditions. The establishment in 1689 of a Royal Dockyard in Hamoaze remedied the long-standing weakness of an English fleet centred only on the south-east coast. The absence of settlements was an advantage and the difficult entry to Hamoaze was a natural protection against foreign attack, although coupled with the exposure of the Sound it rendered Plymouth inferior to Portsmouth as a main fleet rendezvous. The original yard was designed to repair cruising vessels only, but, gradually enlarged, it could build and repair all classes of vessels. Not until more than fifty years after its foundation were land works for its protection begun, and until steamships introduced the possibility of surprise attack, the protection offered by the fleet was always considered more important.

Mr. G. Hayes and Miss M. Chriss.—*The Mersey entrance* (11.45).

Early history of the Mersey entrance and effects of subsidence of the litoral. Origins and formation of the sand banks and coastal sand hills. State of the harbour at end of the seventeenth century : the ' Formby ' channel. Eighteenth-century changes and the disappearance of some neighbouring harbours. The ' New ' channel. Successive surveys from early nineteenth century to 1914. Revetments and training walls. Further surveys to 1938. Navigational aids. The Bar and its immediate future.

Mr. W. G. East.—*The Humber and Humberside in historical times* (12.15).

This paper renews the inquiry into the former physical conditions of the Humber, about which much has already been written, though several problems remain obscure. Prehistoric forests were submerged, new land was formed in recent centuries through ' warping,' and mediæval settlements disappeared through inundation during a period of excessive storms which is attested by history and indicated by Pettersson on the basis of astronomical evidence. A slide of the reclaimed 'warplands' of the Holderness shore of the Humber is presented, together with others showing the condition of the Humber as depicted in old charts from *c.* 1580 onwards. These old charts, widely varying in their value as evidence, indicate stages in the warping process and suggest that, below Hull, the deep-water channel has not substantially changed since the time of Elizabeth.

Mr. O. Borer.—*The Wash* (2.15).

The river system of the North Sea after the Glacial period; the source of the silt in the Wash; the direction of the channels in 1829, 1873, 1924 and 1936.

Reclamation in the Wash since Roman times and now in hand.

Changes in the Outfalls of the Ouse, Nene and Witham and the effect on reclamation.

AFTERNOON.

Visit to the River Great Ouse Catchment Board and the model of the Great Ouse.

Tuesday, August 23.

Discussion on *Some aspects of the regional concept* (10.0).

(Dr. S. W. Wooldridge, Dr. R. E. Dickinson, Dr. R. O. Buchanan, Miss H. G. Wanklyn, Prof. C. Daryll Forde.)

AFTERNOON.

Dr. E. H. Selwood.—*The classification of communities by occupations* (2.15).

Using the Census Tables an estimate may be made of the occupational mode of thought of a community. A number of communities are dominated by outstanding occupations but the remaining categories are not negligible in arriving at an appreciation of the character. The following classes may well be recognised:—agricultural, mining, craft and service (health) types of communities.

As in all biological subjects there are intermediate cases and sub-classes must be recognised.

There has been little change in the occupational character of communities in the decade 1921–31; a small increase in the percentage of transport workers, a decided increase in the trading and personal-attendant groups and a decrease in the manufacturing (craft) group (not so striking if the class containing the undefined and unemployed be distributed).

After the definite types have been culled from the whole, there remain a number of 'balanced' communities from which it is possible to define such terms as 'fishing community,' market town, port and 'residential town,' but no definite concept can be given to the term 'county-town.'

Mr. E. W. Gilbert.—*The growth of inland and seaside health resorts* (3.0).

The inland and seaside health resorts now form an important feature of English life, but they receive only a very brief mention in the standard geographical text-books. These towns have a well-marked individuality and their functional peculiarities can be analysed from occupational statistics. The growth of these urban settlements has been remarkable; of the 105 towns in England whose population exceeded 50,000 persons in 1931, ten can be described as inland or seaside health resorts. The movement of population into these places is of two classes :—(1) The permanent settlement of retired individuals and of persons who cater for visitors. (2) The seasonal migration of holiday-makers which now assumes vast proportions.

This paper includes a summary account of the causes of the growth of seaside resorts in the eighteenth and nineteenth centuries. The original development of these resorts may be ascribed to several causes :—(1) The practice of drinking mineral waters at spas in the seventeenth century and earlier. (2) The practice of bathing in the sea from about 1720, and of drinking sea-water for health, which began in about 1750. The new sea bathing resorts copied the manners of the older inland spas. It will be noticed that the freedom from war after the Napoleonic struggles made the coast safe, and that the building of the railways rapidly accentuated the movement of population to the sea. The history of the communications, in the case of each town, is of great significance, as is its relative proximity to the areas of industrial population.

These towns can be divided into the following types :—(1) inland spas such as Bath, Cheltenham, Leamington, Tunbridge Wells and Harrogate; (2) ancient ports or fishing harbours converted into seaside resorts such as Brighton, Scarborough, Weymouth and Hastings. Brighton is the only town of this type which developed at all rapidly before the railway age. Southampton was for a short period a resort of this character; (3) new towns, such as Bournemouth, Blackpool, Southport and Southend, whose mushroom growth on virgin sites can be compared with the development of American towns. Bournemouth and Southport are two of the few English towns whose founders are known. The first house was built at Bournemouth in 1812 and there was only one postman as late as 1860, when the population had not yet reached 2,000. The population of Bournemouth in 1931 was over 116,000, and Blackpool, which exceeded 101,000 in that year, contained less than 4,000 in 1861.

DISCUSSION on *Some aspects of the regional concept* (continued) (3.45).

Wednesday, August 24.

Dr. VAUGHAN CORNISH.—*The Sidmouth coast and the preservation of its scenery* (9.45).

In order to preserve the scenic beauty for which Sidmouth is celebrated it is necessary that the cliff lands of Peak hill on the west, and Salcombe hill on the east, should be completely protected from sophistication.

The following measures have been taken for the preservation of the cliff lands of Salcombe hill from the summit (540 ft. in height) to the foot of the hill in the secluded valley of Salcombe Regis. These come within the author's farm of South Combe. The perfection of their charm is due to the union of an immense ocean prospect with the peace of a pastoral scene, and the author has therefore decided not to make the cliff summit a public playground but to preserve it as farm land, while giving the public a right of way by all those footpaths to which access has hitherto been by courtesy.

On the lower fields of South Combe the draft Town Planning scheme sanctioned the erection of sixteen houses, but as they would have marred the view of the sea from the village of Salcombe Regis the author decided to dedicate these fields also as a private open space. An agreement having been entered into giving the Sidmouth Urban District Council power to enforce these restrictions in perpetuity, the Council have responded by reserving their own adjacent lands as an open space.

The purpose of the present paper is to draw attention to the importance of such co-operation between landowners and local authorities in the task of preserving the amenities of England's five hundred miles of cliff land.

Miss M. F. Davies.—*Irrigation in the Canterbury Plains, New Zealand* (10.30).

The central portion of the Canterbury Plains suffers severely in most years from drought in the summer months. The low rainfall and the porosity of the soil result in general wilting of the vegetation, especially of the new strains of imported grasses. Owing to these circumstances the carrying capacity of the land is low, and farming is on an extensive rather than an intensive basis. Recent experiments, however, have shown that when the land is irrigated, production and carrying capacity can be increased twelve-fold.

This same area lies between two great rivers, the Rangitata and the Rakaia, which make their way across the plains in broad shingle beds at a distance of some 35 miles from each other. The present Government scheme includes the diversion of the waters of the Rangitata along the base of the foot-hills to the Rakaia, the irrigation of the major part of Ashburton County, and the establishment of a hydro-electric station at the junction of the Main Diversion Race with the Rakaia river.

On account of the porosity of the soil and the gradient of the land, a special method of irrigation has been adopted. The workings of this new system are described by means of diagrams, slides and a cinematograph film in natural colour.

SECTION F.—ECONOMIC SCIENCE AND STATISTICS.

Thursday, August 18.

Mr. C. W. Guillebaud.—*The economic recovery of Germany* (10.0).

In 1932 Germany was in the depths of a most acute depression. She had 6,000,000 unemployed and industry, agriculture and banking were almost paralysed. She attacked the problem partly by tax remissions, marriage bonuses and many other measures designed to stimulate consumption and promote private investment, and partly by public works on a vast scale financed by short term bills drawn by private contractors and discounted by the banks and the Reichsbank. The growth of investment increased incomes and the larger incomes provided the necessary savings.

Unemployment was halved between 1932 and 1934 largely owing to the deliberate creation of employment. From 1935 onwards re-armament has played a predominant rôle. With the adoption of the second Four-Year Plan at the end of 1936 Germany is endeavouring to make herself independent of foreign sources of supply of a number of important raw materials.

The resulting increase in investment has, on the one hand, not merely abolished unemployment but caused an acute shortage of labour, and on the other hand, increased the difficulty of maintaining stability of wages and prices. However there has been no inflation and prices are little higher than in 1932. The national income increased from 45 milliard marks in 1932 to 68 milliards in 1937, and the standard of living of the workers as a whole has risen largely, though not proportionately, owing to the scarcity of some commodities and to deterioration in the quality of others.

The greatest economic problems with which Germany is to-day confronted relate to the shortage of labour and raw materials ; to the necessity for neutralising much of the income created by full employment and the high level of investment ; and to foreign trade.

Prof. J. Tinbergen.—*Statistical testing of the trade cycle mechanism* (11.0).

For the fluctuations in a given phenomenon (or variable) there are ' proximate ' and ' deeper ' causes. A proximate cause to investment activity may be profits ; if a proximate cause of profits is consumers' outlay, the latter is one of the ' deeper ' causes of investment activity. There are ' deeper ' causes of the second, third, etc., degree. The whole network of all these causal connections is the economic mechanism, or, if we confine ourselves to short-run fluctuations, the trade cycle mechanism. Economic theories tell us what are the proximate causes of a given variable's fluctuations. They do not, in general, tell us about their relative importance. Nevertheless this knowledge is vital for problems of trade cycle policy. Statistical methods enable us, in a number of cases, to determine this relative importance. Extending the methods used for the determining of demand and supply curves to large complexes of markets instead of single markets yields some of the information desired.

Some results of work in this field. The large elasticity of the supply of labour, commodities and credit in the U.S. after the War. The remarkable rôle of the stock exchange. Some consequences of the policy of President Roosevelt.

Mr. R. F. Bretherton.—*Public investment and trade cycle policy* (12.0).

Capital expenditure by the central government, local authorities, and semi-public bodies in Great Britain has since the War formed a large, though variable, proportion of the total gross investment of the community. The idea of timing this expenditure in such a way as to counteract, rather than to exaggerate, the fluctuations of private investment in the course of the Trade Cycle is not new : it was suggested by the Poor Law Commission of 1905, has been elaborated by Bowley and some other economists, and has been recently accepted as a regular instrument of trade cycle control in the United States and Sweden. But in Great Britain it has not yet been given any real trial.

This paper is specially concerned with investment by the local authorities. Since the War, this had varied between £155 millions and £70 millions a year ; but it has varied *with* the indices of private business, though with a considerable time-lag. The reasons for this are investigated, and the effects upon employment are estimated.

The case for attempting at least to stabilise public investment is stated and discussed, and some of the difficulties are then examined. They may be classified as (1) financial—the present arrangements of British local government finance are not well suited to encourage spending in periods of depression ; (2) economic—the close interlocking of public and private investment in many fields, especially the trading services ; (3) technical and administrative—the difficulty of commencing and discontinuing large schemes of capital expenditure at short notice : it is suggested that control over ordinary repairs and renewals expenditure might be useful here ; (4) the relative narrowness of the field—the long-term trend of investment by local authorities in the near future may well be sharply downward, unless new fields of activity are rapidly developed.

Friday, August 19.

Presidential Address by Mr. R. F. Harrod on *Scope and method of economics* (10.0). (See p. 139.)

Mr. J. M. Keynes, C.B.—*The policy of Government storage of foodstuffs and raw materials* (11.30). (Read by Mr. G. F. Shove.)

It is an outstanding fault of the competitive system that there is not sufficient incentive for the storing of raw materials so as to average periods of high and low demand, except by means of excessive price fluctuations. There is, therefore, a *prima facie* case for government action to supplement this deficiency, which is not easily supplied by the competitive system from within. In present circumstances three considerations combine to reinforce this *prima facie* case : (1) storage for war purposes ; (2) with the object of mitigating the fluctuations of the trade cycle ; and (3) the stabilisation of prices by holding some part of the Central banking reserves, not in gold, but in a composite commodity.

Afternoon.

Prof. M. Ginsberg.—*The present position of sociology* (2.45).

Mr. T. H. Marshall.—*Professionalism* (3.45).

All civilised societies—and possibly primitive societies as well—have recognised a certain group of occupations as superior to all others. The principle of selection varies, but the superior occupations have usually been referred to as ‘ free ’ or ‘ liberal ’ in contrast to commercial and industrial pursuits. Our first task is to see whether this distinction has any foundation in the real character of the professions, or whether it is a rational cloak for the self-interest of those who give a scarcity value to their services by creating monopoly rights for themselves. Any conclusion reached for the professions in England a century or more ago must be revised for the present day. The lists of professions has lengthened and their character has changed.

The development can be related to three causes, the growth of science, the evolution of capitalism, and the advance of democracy through equalitarianism towards the social service State. Contemporary changes in the organisation of commerce and industry have further weakened the contrast by which the professions used to be recognisable. Should we describe the process by saying that the professions have been commercialised or that commerce has been professionalised ? The answer should probably be that both groups have shifted their position, but that the meeting-point at which they tend to coalesce is somewhat nearer to professionalism than to commercialism, in the old meaning of those words.

The result is the emergence of a small, but influential, class which cannot be identified either with the capitalists or with the proletariat. The vital question, which it is important to ask but difficult to answer, is ' What attitude is this new class inclined to take up towards the conflict between capital and labour, and is it likely to evolve a system of its own which differs from that advocated by either of these two contending parties ? '

Monday, August 22.

Mr. G. Walker.—*The economics of road haulage since the Road and Rail Traffic Act of 1933* (10.0).

The provisions of the Road and Rail Traffic Act, 1933, regulating the grant of licences to carriers by road are recited. ' Suitable transport facilities ' in ' excess of requirements ' are held by the Tribunal to create ' wasteful ' competition which is against public interest and therefore contrary to the intention of the Act. The ' suitability ' or otherwise of existing facilities is proved by the evidence of traders. But this evidence is not always accepted as final. The terms upon which newcomers are allowed to enter the industry, and upon which established haulers are permitted to expand, are described. Renewal of an existing licence is not automatic. ' Need ' must be proved for the continuation of an existing service just as much as for the provision of a new one, or the extension of an old. That the rate to be charged by a proposed service is lower than the charges asked by existing services is not evidence which will justify the grant of a licence. This principle affects the provision of road service in competition with the railway, for road charges are commonly lower than railway rates. Summary of the results of the restriction practised by the Act as interpreted by the Tribunal on the provision of transport in general, and on the road haulage business in particular.

Dr. J. Marschak.—*Measurement of the mobility of labour* (11.0).

(This paper, presented on behalf of the Migration Group of the Institute of Statistics, Cambridge, is a production of a very close co-operation between Miss H. Makower, Miss V. Lamb, Mr. H. W. Robinson and Dr. J. Marschak.)

Before any attempt can be made to judge the relative importance of the different factors which prevent a quick adjustment of the labour supply to a changing demand, we must devise some measure of labour mobility. Crude figures of migrants, relating to various counties, years or industries, must be reduced to comparable indices. The mobility index can be defined as the number of people moving annually from one district to another per 1,000 people in each district and per unit of ' incentive to move.' The incentive to move may be measured by the absolute or relative difference in prosperity (as indicated by employment rates, average earnings or average purchasing power) as between two districts.

Figures of net migration gains and losses for various counties and industries can be compiled from data of the Ministry of Labour and the Registrar-General. These figures have been used to compile the changes of mobility for a given county (or industry) over time.

Net migration figures do not, however, give movements between one county (or industry) and another ; yet in order to measure the influence of distance (in the case of counties) or of affinities of skill (in the case of industries) upon mobility, we need to know both the origin and the destination of the migrants. Using three different types of material available at the Oxford Employment Exchange, the immigration into Oxford from various counties of Great Britain was analysed. It was then possible to calculate mobility indices, with respect to Oxford, for various counties (for men and women separately).

These indices were found to be closely correlated with distance and to vary more than proportionately with distance : the ' elasticity of migration on distance ' was found, with various data, always to lie between 1·6 and 2·0. The residual variations in mobility, i.e. those which could not be accounted for by distance, were then examined in the light of the industrial structure of the counties of origin. It was found, for instance, that the Oxford motor industry seems to present little attraction to textile workers but relatively strong attraction to insured persons working in agricultural districts (this does not include agricultural workers, for whom no figures were obtainable). Further, mobility varies according to the type of unemployment in the district of origin : the chronically unemployed appear to be less mobile than the short-period unemployed.

The working hypotheses underlying the mobility measures used should be tried out with more general material. It might then be possible (1) to explain the actual movements of labour between any two counties (or industries), and (2) to measure changes in mobility due to causes other than distance and industrial structure (e.g. administrative measures of training and transfer).

Mr. R. C. Tress.—*Local diversity of industry and the rate of unemployment* (12.0).

An area in which a large percentage of workers are occupied in a single industry is liable to be faced at some time or other with a heavy unemployment problem. The occasion will depend upon the industry, but certain factors involved in its concentration are likely to intensify the problem whenever it should occur. An accurate policy of diversification would need to consider the industrial structure of a locality as an organic whole, as a series of relationships between different types of industry, viewed in terms of employment prospects. Determination of the optimum size of such an area would also consider the economies of localisation and of the scales of plants.

For certain purposes a cruder method is permissible. That suggested defines optimum diversity in terms of an even distribution of the occupied population amongst a number of arbitrarily defined industries, the relationships between which, as distinct from their size, are ignored. It is here used to examine recent changes in the industrial structure of a number of towns where there was heavy unemployment in 1931, and where a high percentage of the population were occupied in one or two industries.

Afternoon.

Sir William Beveridge, K.C.B.—*Unemployment in relation to the trade cycle* (2.15).

The paper gives the results of an analysis of the Unemployment percentages recorded for about 100 insured industries from 1927–37, with a view to comparing the position of the different industries in respect of the proportionate increase of unemployment in the depression and the times at which downward and upward movements took place.

Broad comparisons are made between industries of different types (extractive, instrumental, other capital goods, consumers' necessaries and services, other consumers' goods).

So far as data are available, a similar study is made for the years 1906–13 covering the depression of 1908–9.

Prof. Dr. Ernst Grünfeld.—*The rôle of Co-operative Societies in the social movement.* (*Taken as read.*)

By social movement we mean the organised endeavours of the working classes to better their situation under industrial capitalism. It started from actual evils that it was desired to remedy, if necessary, by means of revolution, so that the workmen might possess equal rights with the other classes and have the chance of gaining controlling power. Its causes are everywhere the same, its means and ends vary, but include similarities.

The main impulse of the movement went out from the trade unions. Together with Co-operative, Friendly and similar Societies, they were, and still are, the means to progress through self-help. Besides these, attempts were made to obtain political power and so force the State to carry out the wishes of the working classes.

What was the rôle of Co-operative Societies in this movement ? Co-operative production, the ideal of pre-Marxian Socialists, was tried in England by Christian Socialists, in Germany by Lassalle, but acquired importance in France and Italy only. The evolutionary type of Co-operative Societies as founded in Germany by Schulze-Delitzsch consisted principally of Co-operative Societies which were mutual loan societies for the working classes and still more for the lower middle class. The Rochdale consumers' organisation ultimately became the standard and ideal for the whole world, either in its original form or with variations.

Two points should be stressed : (1) The whole Co-operative Societies' movement was strongly under the influence of intellectuals, who were not of the working class. (2) Co-operative Societies were regarded by the workmen either as of primary importance, as the only means of achieving the aim of the social movement, or as of secondary importance, as one means among several.

Tuesday, August 23.

Mr. G. F. Shove.—*The interpretation and allocation of cost* (10.0).

The subject investigated is the influence of cost upon the price of a product which is one of several produced by the same concern. Particular attention is given to the influence of ' overhead ' or ' common ' costs. (1) Various interpretations of ' cost ' are considered and one is selected as being suitable for this inquiry without departing too far from ordinary usage. (2) It is shown that, on this interpretation, conclusions about the relation between cost and price, familiar in connection with artificially simplified cases, can be generalised so as to cover actual conditions encountered in the conduct of business. (3) It is argued that the influence of ' overhead ' or ' common ' costs on prices depends more on the planning of businesses, their financial policy and mutual competition, than on their practice in the matter of ' costing.'

Mr. R. L. Hall.—*The business view of the relation between price and cost* (11.0).

A group of economists in Oxford, who have been making a study of Trade Cycle problems, have asked business men questions about the influences which affect their decisions on such matters as output and price policy. The answers suggest that in many cases there is an attempt to fix prices on the basis of average costs, using an estimate of turnover in order to allocate overheads. The power to fix prices is due to market imperfections: but though the demand is often thought to be inelastic, little attempt is made to exploit this.

In works on economic theory it is usual to state that profit is maximised when marginal cost is equal to marginal revenue. In the long period the price must cover average cost, including normal profit, and when there is free entry to the industry it is not likely to be much more than this. The changes of price in the direction of full cost are explained by changes in the number and size of sources of supply in the industry.

The paper discusses the relation between the economist's view of profitable behaviour and the actions which appear to be characteristic of business men who try to sell at full cost. The conclusion is reached that there is often an element of oligopoly which makes neither of the hypotheses, of competition and of monopoly, applicable. There is a tendency for price to remain at the point where it happens to be at any time, unless the costs of all firms in the industry change together. A discussion is given of the factors determining the price point, and the view is taken that it is most likely to be at full cost. If this is so, the long period competitive price is reached more directly than is sometimes supposed.

Mr. G. D. A. MacDougall.—*Overhead costs and economic welfare* (12.0).

Schemes of output restriction and of compulsory scrapping of equipment are often advocated as a means of maintaining the earning power of invested capital. Such schemes are in general regarded by the economist as conflicting with the public interest; but it is suggested in this paper that they may in some cases be justified.

Current decisions to remain in business and to produce depend on long term expectations. Even in perfectly competitive conditions (given full employment) there may therefore be wasteful maintenance (in the widest sense) and wasteful production, from the community's point of view, if long term expectations are over-optimistic. Over-optimism is probable in declining industries, and there may therefore be a case for compulsory restriction of output and scrapping of equipment.

In imperfectly competitive conditions there may be a case for compulsory scrapping for a different reason, but not normally for output restriction.

When there is not full employment, policies that would otherwise be ' ideal ' will still in general be desirable if they do not diminish national employment. It is possible to devise a method of analysis that may be of assistance in deciding under what conditions a so-called ' ideal ' policy of reorganisation is likely to increase, and under what conditions it is likely to decrease, national employment.

Wednesday, August 24.

Mr. P. Barnett Whale.—*International short term capital movements* (10.0).

The paper aims at making a classification of international short term capital movements with a view to showing their effects on monetary circulation, trade, prices and incomes in the countries concerned. The classification offered is an empirical one rather than a formal logical division of all the possibilities.

Four main classes are suggested :

(1) Those which most resemble normal long period capital movements, i.e. that seek to take advantage of superior investment opportunities abroad and result in additional real investment abroad.
(2) Those which provide gold to meet increasing monetary requirements in the borrowing country.
(3) Equalising capital movements to meet balance of payments differences, motivated either by prospects of exchange profits or by interest differences.
(4) Capital flights.

It is argued that (1) and (2) have no sort of deflationary effect in the receiving country, but may have some such effect in the lending country ; that (3) are deflationary or contribute to deflation in the receiving country in a manner which may be considered appropriate, given adherence to an international standard : that (4) tend to produce altogether undesirable deflation in the receiving country. Some consideration will be given to the possibilities of offsetting the effects of (4).

Finally two other types of short term capital movement are referred to, namely the acquisition of foreign balances as reserves and speculative investment abroad on security exchanges. It is suggested that the effects of these are similar sometimes to those of one of the above classes and sometimes to those of another.

Prof. E. Roll.—*The trend of Britain's balance of payments* (11.0).

This paper surveys the development of Britain's balance of payments between 1931 and 1937. It examines the relative weight of the different items and endeavours to assess their influence, during the period in question, on the trend of the whole of Britain's international accounts. Special attention is given to the striking fluctuations in the terms of trade ; and in this connection some well-known criticisms of protectionist policy are further emphasised.

Mr. E. A. Radice.—*The measurement of the causes of variations in savings in Great Britain* (12.0).

Once the main determinants of different types of saving have been analysed, the effects of each of them may be estimated by the method of multiple correlation of the appropriate time series. The most important cause of variation in all types of saving is variation in income. The rate of interest is also an important variable mainly because it has important effects on shifts between different types of saving, and changes in the contributions of various income groups to particular types of saving. If the effects of these variations are eliminated, good estimates can be made of 'income elasticities' for the different types of saving. Figures are given showing the proportions of any increase in total incomes which would flow into post office savings banks, building societies, insurance companies, friendly societies, etc. A linear relationship can similarly be estimated between the incomes of all individuals and their savings, and also between business savings and profits. Figures for investment in post-war years also lead to a linear relationship between net investment and profits. Clark's quarterly figures for 1929–36 lead to a linear relationship between investment and the rate of increase in total income two years previously. The four equations, when solved, involve cyclical fluctuations in income and investment with an eight-year period.

SECTION G.—ENGINEERING.

Thursday, August 18.

Presidential Address by Prof. R. V. Southwell, F.R.S., on *The changing outlook of engineering science* (10.0). (See p. 163.)

Short Papers by junior engineers (11.30).

Mr. D. G. Christopherson.—*Relaxation method for the solution of Poisson's Equation.*

Mr. J. R. Green.—*Relaxation applied to calculate the flow of a compressible fluid.*

Mr. F. B. Greatrex.—*Transients in transformers* (*Oscillograph demonstration*).

Mr. J. E. M. Coombes.—*Suppression of radio interference caused by trolley buses.*

Afternoon.

Visit to works of Messrs. Kryn and Lahy, Letchworth (Steel castings).

Friday, August 19.

Mr. R. W. Allen, C.B.E.—*Some experiences of the use of scale models in general engineering* (10.0).

The paper provides instances of the application of the well-known technique of investigation by means of scale models to general engineering. The purposes and scope of model testing are surveyed with particular reference to the design of centrifugal pumps, fans and the fluid conveying passages generally associated therewith. Three examples taken from centrifugal pump design and one from the design of fans are mentioned in some detail, as are three instances of fluid passage problems. Of these latter, two instances are of channels for conveying water, while the third is a reference to an extensive investigation into the supply of forced draught air to ships' boiler rooms. Attention is drawn to the close hydraulic connection between the design of centrifugal and propeller water pumps and the corresponding fans for displacing air. It is also pointed out that the term scale model comprises the scaling of physical properties as well as of linear dimensions. These considerations are illustrated by an account of a technique recently developed for the testing of water pumps by the use of air as a working fluid. The paper concludes with references to applications of scale models in other branches of general engineering.

Discussion by Mr. E. F. Relf, Prof. C. M. White, and Dr. J. P. Gott.

SHORT PAPERS by junior engineers (12.0).

Mr. G. C. ECCLES.—*A moving coil vibrometer.*

AFTERNOON.

Visits to—

(*a*) Works of Messrs. Chivers & Sons, Histon.
(*b*) Model of Ouse Catchment Board.

Monday, August 22.

Major F. C. COOK.—*Road development in Great Britain* (10.0).

Prof. F. C. LEA.—*A torque converter for motor cars* (11.0).

SHORT PAPERS by junior engineers (12.0).

Mr. C. H. EDGECOMBE.—*The braking of railway trains and some tests made to determine the coefficient of friction under various conditions.*

Mr. D. M. WILCOX.—*The calculation of train running times.*

(CONCURRENTLY WITH THE ABOVE SESSION.)

JOINT DISCUSSION with Section A (Mathematics and Physics) on *Magnetic measurements with special reference to incremental conditions* (10.0).

Chairman: Prof. W. CRAMP.

Prof. W. CRAMP.—*The position regarding incremental measurements.*

Mr. C. E. WEBB.—*Recent improvements in soft magnetic materials.*

Mr. D. C. GALL.—*Instruments suitable for incremental magnetic measurements.*

The paper deals with the instruments and circuits suitable for the measurement of incremental permeability upon rings and cores at low degrees of alternating modulation.

Dr. L. G. A. SIMS and Mr. J. SPINKS.—*The place of ballistic measurements in incremental magnetism.*

The paper draws attention to the possible adaptation of the classic ballistic measurement to routine incremental magnetic testing. It is proved experimentally that very close agreement occurs between ballistic and alternating current measurements so long as a low frequency sine wave of current is used in the alternating current case. The paper shows further that either for ring samples or for transformer core assemblies there is a considerable range of superimposed inductions over which general agreement between all methods of measurement, including the ballistic method, can be relied upon, so long as the alternating current tests relate to silicon steels and to low frequencies. The ballistic measurement is convenient for adjusting circuit conditions to give stated degrees of modulation of the polarisations, and this has been used in the paper to establish values of induction corresponding to full modulation of the polarising force for a typical range of silicon steel samples. It is suggested that this condition might form an acceptable means of limiting the range of incremental measurements for industrial purposes and that with such limitation the problems of measurement would be considerably simplified. Graphs and tables of results are provided to support this view.

Mr. J. Greig and Mr. J. E. Parton.—*Harmonic power in iron testing.*

Currents and voltages which are more or less non-sinusoidal are, in general, obtained in circuits employed to produce alternating magnetisation in iron cores. Certain methods of power measurement which are of great convenience and utility in iron loss testing depend for their accuracy on the attainment of closely sinusoidal variation of *either* the voltage or the current involved in the measurement, and as this sinusoidal variation is an ideal which can only be approached, it may be necessary, where such methods are employed, to determine the range within which power measurements may be made with any required degree of accuracy.

The paper records the results of a number of tests in which an A.C. potentiometer was employed to measure the iron loss in a ring specimen under polarised conditions at a frequency of 50 cycles. The accuracy of the potentiometer measurement was estimated from a comparison with the indications of a quadrant electrometer used as a wattmeter. Theoretical considerations suggested that the required information might be obtained by a simple practical method involving a measurement of the harmonic content of the exciting current. It was confirmed that a useful estimate of the accuracy of a loss measurement by the potentiometer could be made in this way.

Mr. E. V. D. Glazier and Mr. J. E. Parton.—*Relative measurements on rings and cores in incremental testing.*

The paper is concerned with certain important practical aspects of magnetic testing which are bound to arise in connection with the drafting of a British Standard Specification. Thus the possibility of using a transformer stamping assembly in place of the usual ring assembly for the measurement of the incremental magnetic properties of iron is discussed with the aid of characteristic experimental data. The differences found in incremental permeability and iron loss values as measured on rings and on transformer cores of the same material vary with the degree of polarisation. They cannot be entirely attributed to the unavoidable air gap present in the transformer assembly. If fundamental data upon the iron itself are required, ring specimens are still considered essential, but, on the other hand, it is found that good repeat accuracy is obtainable on the assembled transformer core specimen, so that results taken upon a given specimen can be regarded as a reliable guide to the behaviour of the size of core concerned. The effects upon incremental permeability of tightening the core clamps, of re-assembling the specimen, of employing different sized cores of a given material and of assembling with butt or interleaved joints are shown by experimental curves. Finally the paper deals with the practicability of dispensing with a search winding during routine loss and permeability measurements.

Afternoon.

Visit to works of Messrs. Pye Radio Ltd.

Tuesday, August 23.

SYMPOSIUM on *Vibration* (10.0).

Prof. C. E. INGLIS, O.B.E., F.R.S.—*Resonance in relation to mechanical vibrations* (*Demonstration*).

Dr. F. H. TODD.—*Vibration in ships.*

A ship, being a steel structure, is particularly liable to vibration, and a little may be felt in practically every vessel. The exciting forces arise in the propellers or from lack of balance in the main or auxiliary machinery. The vibration which results may be either of a local nature, affecting only a small part of the structure, or may be a general vibration of the whole hull girder. When synchronism arises between the frequency of the disturbing force and a natural frequency of the whole hull, serious resonant vibration occurs, even though the magnitude of the disturbing force may be quite small.

Vibration which is found to exist on a completed ship may be reduced in amount by improving the balance of the machinery, ensuring accuracy of manufacture in the propellers, perhaps by a change in design of the latter with a consequent change in propeller revolutions per minute, by the use of a vibration damper and, if it is of a local nature, by local stiffening of the structure.

Resonant vibration of the whole hull could be avoided in the finished ship if the natural hull frequencies could be calculated in the design stage, since the frequencies of the disturbing forces would then still be capable of adjustment. Methods for calculating these natural frequencies have been developed, considering the hull girder as a simply supported beam immersed in water, and the calculated values have been compared with those obtained by observation on board ship. The first natural frequency for vertical vibration, with two nodes in the length of the vessel, can now be predicted with reasonable accuracy, but further data are required for different types of ships, and, particularly, reliable figures for the first horizontal frequency.

When it is remembered that a ship is the home of a crew for months at a time, and of passengers for perhaps several weeks, the importance of the effects of vibration upon comfort cannot be overestimated.

Major B. C. CARTER.—*Vibration in aircraft.*

The first part of the paper comprises descriptions of types of vibration that occur in aircraft, and the second part deals with means of investigating such vibration.

After brief reference to aircraft vibration due to aerodynamic forces, the vibratory disturbances that may be produced by engines and airscrews are considered. The manner in which torsional vibration of crankshaft-airscrew systems comes about is then explained and tables of natural frequencies are given in respect of representative direct-drive and geared engines: methods of reducing torsional vibration are commented upon and brief reference is made to lateral vibration of crankshafts. There follows an examination of airscrew-blade vibration.

Mechanical and optical apparatus and instruments for investigating vibration are next described and some typical records are given. Included in this are the Cambridge and the R.A.E. vibrographs, the D.V.L. and the

R.A.E. torsiographs, apparatus for exciting vibration in airscrew blades, and a moving-film camera for attachment to airscrew hubs whereby records of airscrew-blade vibration have been obtained in flight by photographing the filament of a small electric light bulb secured to a blade tip by adhesive compound.

Finally, electronic aids to the measurement of vibration are reviewed and some particular examples are described.

AFTERNOON.

Excursion to Ely and Newmarket.

Wednesday, August 24.

Mr. C. C. MASON, O.B.E.—*Engineering instruments* (*with demonstration*) (10.0).

SHORT PAPERS by junior engineers (11.0).

Mr. A. W. SKEMPTON.—*Settlement analysis of engineering structures.*

Mr. S. J. PALMER.—*Stresses in welded pipes with internal pressure and end thrust.*

SECTION H.—ANTHROPOLOGY.

A special exhibition of South Arabian antiquities, excavated by Miss G. Caton Thompson in the Hadhramaut during the winter of 1937–8, was on view throughout the meeting in the Fitzwilliam Museum, Department of Classical Antiquities.

Thursday, August 18.

Rev. E. W. SMITH.—*A Congo pygmy language* (10.0).

DISCUSSION on *Australia* (10.30).

Dr. DONALD F. THOMSON.—*The Australian aborigine and the problems of administration.*

The problems associated with the administration of native affairs in Australia to-day are discussed briefly. This problem falls into three sections :

1. The undetribalised native, who is still in possession of his culture and social organisation.
2. The detribalised aborigine whose culture is wholly or partly broken down.
3. The half-caste.

The discussion is concerned chiefly with the first question. The position of native affairs in Australia, as well as depopulation and the principal factors at work in this, are discussed. A summary is given of the recommendations presented by the writer in a memorandum to the Commonwealth Government following an investigation of native affairs in Arnhem Land, Northern Territory.

The following proposals, which represent essential measures only, are suggested, and aim chiefly at the removal of the major wrongs and injustices confronting the aboriginal population in Australia and of endeavouring to arrest immediately the increasing decline in population, pending the settled policy and the establishment of a Department of Native Affairs :

> That the remnant of native tribes in Federal Territory not yet disorganised or detribalised be absolutely segregated, to preserve intact their social and political institutions, their organisation and culture in its entirety ; that the Native Reserve Arnhem Land be created an inviolable reserve ; and that similar steps be taken to render inviolable any other reserves in which the native population remains undetribalised. That all the established watering depôts (within the Reserve) be abolished, since the pearling vessels are manned chiefly by Japanese whose presence is inimical to the welfare of the native population. That the whole policy of administration of native justice be revised ; the anomaly whereby Police Constables act as Protectors of aboriginals be abolished, and that special courts, suitably constituted, be established to deal with natives and native offences.

An important objection to the present system of justice is that the usual deterrent effects of imprisonment—the sense of shame, the loss of prestige in the eyes of his fellows, and the stigma that attaches to even a nominal term of imprisonment, which make it a powerful deterrent to the white man—are entirely lost on the aborigine, who suffers not at all in social prestige by punishment under white man's law.

While it is not suggested that the Government be committed to a permanent and unprogressive policy of segregation, it is strongly urged that since the previous contacts of the aborigines with western civilisation have been wholly destructive, it should be the policy to maintain these reserves inviolable for the natives who are still in possession of their culture until and unless a sound working policy and one in the best interests of the aboriginals is established, tested, and proved by experience over a long period among the natives who are already detribalised.

The establishment of a separate Department of Native Affairs under a trained director, staffed by men selected for their sympathies and qualifications for what must be regarded as a highly specialised work, is proposed, with the objective of finally bringing the whole of the administration of native affairs in Australia under one uniform control.

Prof. A. R. Radcliffe-Brown.—*Anthropological research in Australia* (10.55).

On the basis of scanty available data it has been estimated that the aborigines originally numbered 250,000 or 300,000. At the present time there are less than 60,000 and probably less than 50,000. They have thus been reduced in 150 years to one-fifth. There were originally several hundred tribes, each with its own language and its own social system. Many of these are now extinct. Others are reduced to a mere handful in which only the old people of 50 or 60 years of age retain any knowledge of their language and their former customs. There are hardly any tribes in which the social system is not either broken down or beginning to break down under the influence of the white man. The Australian aborigines present a highly specialised form of society based on simple hunting and collecting subsistence. Their great value for science is due to the fact that they present for our study a large number of societies or social systems of a single general type with many variations.

In Australia history has carried out an immense experiment for us upon the results of which we ought to be able to draw important conclusions. For during the many hundreds, or probably thousands, of years that the Australian aborigines have lived in relative isolation from the rest of the world they have been developing their languages, their forms of social organisation, their ritual, their cosmological beliefs. We can never hope to know how the process of development actually took place, but the results of the process are there for our study if we can only study them before they disappear for ever.

The study of Australian tribes began with the work of enthusiastic amateurs, many of them missionaries. Financed by the Rockefeller Foundation, a systematic survey of the still surviving tribes was begun in 1926, and the scientific results fully justify the further expenditure of a good many thousands of pounds. The work was planned at Sydney University and carried out by Prof. Lloyd Warner, Prof. Elkin, Dr. D. F. Thomson and others. We have now a good knowledge of the general features of social organisation and totemism in a large proportion of the surviving tribes. What is now needed is something more, namely a number of intensive studies of carefully selected tribes in which a trained research-worker is willing to stay at least two years, and preferably four, in the field, master the native language in all its intricacies, record native texts and study the whole system of ideas and beliefs of the people. Only by a sufficient number of such investigations will it be possible to make full scientific use of the opportunities that Australia offers to us and reach an adequate understanding of a fascinating form of society doomed very soon to vanish.

General Discussion (11.20).

Dr. Donald F. Thomson.—*Film of Australian life* (12.0).

Afternoon.

Dr. J. G. D. Clark.—*Recent excavations in the Fens* (2.0).

Dr. G. E. Daniel.—*The portholed megaliths of the British Isles* (2.35).

This paper consists of a description of the isolated portholed megaliths and of those associated with prehistoric burial chambers which occur in the British Isles, an analysis of their distribution and morphology, and an attempt to explain their relationships to the portholes which occur in other parts of Europe and to suggest various theories to account for their origin and diffusion. Illustration by lantern slides.

Mr. C. W. Phillips.—*The Roman occupation of the Fenland* (3.10).

At the opening of the Roman period the Fens were deserted, but by A.D. 100 an extensive agricultural occupation of native type had set in, chiefly on the silt lands.

It is probable that the area was an Imperial domain.

Work at Welney has shown that by the end of the second century sea floods began, but the wealth and activity of the region continued with little abatement, so far as we know, till late in the fourth century. In Anglo-Saxon times the region was again a wilderness.

The particular interest of the occupation is its size, intensive character, and the various types of native agriculture displayed. The suggestion is that the population was drawn from more than one part of Britain and that it was entirely peasant in character. No administrative centre is known, but this may have been at Durobrivae (Castor, Water Newton) on the western fringe of the region. There is no positive evidence of Roman drainage works on any scale and the occupation and abandonment of the region appears to have depended in the main on the operation of natural causes.

Mr. T. C. Lethbridge.—*Weapons from Fenland waterways and their relationship to ancient warfare* (3.45).

Weapons of many periods are frequently dredged from Fenland rivers or excavated from the beds of extinct watercourses (roddons) in the Fens. These have usually been regarded as casual losses, but it will be shown that, although they are seldom actually found in association, they frequently fall into groups which can be correlated with recorded military actions in historical times, or with invasions and other movements deduced from archæological evidence in earlier periods. Thus a number of Late Bronze Age spears and an early La Tène sword found at a certain point on the river Wissey may be the result of fighting during one of the invasions that broke up the Bronze Age civilisation of East Anglia ; and later groups may be correlated with Hereward's defence of the Isle of Ely against William the Conqueror, and with local incidents in the Barons' Wars, the Peasants' Revolt, etc.

Dr. Henry Field.—*The physical characters of the modern peoples of Iran and Irak* (4.20).

Anthropometric data obtained in Iran (Persia) by the Field Museum Anthropological Expedition to the Near East, 1934, reveal that the modern Iranis belong for the most part to the Mediterranean race although Alpinoid, Armenoid, Proto-Nordic, Mongoloid and Negroid elements are present in the population, which numbers about twelve million individuals. The Iranian Plateau dolichocephals can be divided into three groups, the most important being the convex-nosed individuals. This type appears to have developed on the Iranian Plateau. Among the brachycephals, who migrated into Iran at an early date, there is a concave or straight-nosed, square-jawed type, possibly Proto-Alpine, as well as a convex-nosed, high-vaulted head of Armenoid type.

A brief comparison is made with the peoples of Iraq.

Friday, August 19.

Symposium on *The Swanscombe find* (10.0).

Mr. A. T. Marston.—*The Swanscombe find.*

The human remains consist of the complete occipital and left parietal bones of a young adult which were found in June 1935 and March 1936 respectively, 24 ft. below the surface in the stratified Middle Gravels of the Barnfield Pit, Swanscombe, Kent. Both bones lay in the same seam of gravel although they were separated by a distance of 8 yds. The associated animal bones, *Elephas antiquus*, *Rhinoceros*, etc., indicate interglacial conditions, and the sequence of Acheulean implements recovered brings to light the fact that the Barnfield deposits are earlier than those gravels at a higher base level in Pearson's Pit, Dartford Heath.

The Barnfield deposits are the infilling of a stranded river 1,400 ft. in width, with a base level of about 73 ft. O.D., whose channel was cut probably in the pre-Boyn Hill Erosion Stage. In it is preserved an ascending sequence of terraces.

Lower Gravel Aggradation.	Clactonian.
Older Middle Gravel Aggradation.	Abbevillian (?) to
Middle Gravel Erosion Stage.	Acheulean.
Later Middle Gravel Aggradation.	Acheulean.
Upper Loam, and later still.	Acheulean.
New Craylands Pit Stage.	Twisted ovate.

The precise position of the Swanscombe skull was at the commencement of the Middle Gravel Erosion Stage, and it was found in the first oblique bank left stranded as the waters became rejuvenated to cut a fresh channel from the surface of the Older Middle Gravels (the habitation level of Swanscombe Main) down through the Lower Loam and Lower Gravels to touch the Thanet Sand at about 73 ft. O.D.

The depth of the subsequent aggradation from this level 73 ft. O.D. to that of the surface of the deposits at Dartford Heath represents the pre-Boyn Hill status of the Swanscombe skull horizon.

The still later excavation of the valleys of the Darent and Cray, and of the formation of the descending terraces of the Lower Thames completes the picture of the age of the Swanscombe skull.

A sub-plenal evolutionary stage of human development is evidenced by the skull, perhaps best shown by the features of the endocranial cast. The cranial capacity is estimated at less than 1,100 cc. (1,065 cc. from the restored endocranial cast A.T.M.). It is considered by the writer that the bone contacts at the antero-inferior and the postero-inferior angles of the parietal bone indicate the sub-plenal stage of the flexion of the cranial base; that the plane of the foramen magnum looked downwards and backwards; and that there was a certain amount of proclination of the head. The shortness of the basilar process and the form of the pharyngeal tubercle are distinctly human and non-anthropoid features.

Prof. W. B. R. King.—*The geological evidence* (10.30).

The fact that the Swanscombe skull was found in a deposit which is part of the Boyn Hill or 100-ft. Terrace of the Thames adds greatly to its importance.

The fluviatile deposits of this terrace are divided into an upper and a lower part, separated by a period of erosion, but both parts yield the same general type of mammalian fauna, which is also found in the Clacton channel at lower levels.

This is interpreted as showing that the river after depositing the lower gravels cut down and formed the Clacton channel, but later aggraded its bed again to lay down the higher (skull-bearing) gravel.

The fauna indicates a temperate climate. On the north of the Thames in Essex the gravels of this terrace rest on a spread of boulder clay referred to the Great Chalky or Chalky Jurassic Boulder Clay of the Great Eastern glaciation of East Anglia. At a later date valleys were cut through the fluviatile terrace gravels and solifluxion deposits of the definitely cold period represented by Coombe Rock descended into the valleys lying across the truncated gravels.

The skull-bearing gravels belong, therefore, to the temperate interglacial between the Great Eastern glaciation of East Anglia and the cold period represented by the Main Coombe Rock of the Thames Valley.

Mr. M. A. C. Hinton, F.R.S.—*The faunal evidence* (10.50).

Mr. C. F. C. Hawkes.—*The archæological associations of the Swanscombe skull* (11.10).

Whereas the Lower Gravel and Lower Loam of the 100-ft. Thames Terrace at Swanscombe yielded in Barnfield Pit industries of the Clactonian series, the Middle Gravel was characterised mainly by an Early-Middle Acheulean comparable to the Acheulean III of Breuil. This industry extended through the Lower and Upper Middle Gravels including the channel in which the two bones of the Swanscombe skull were found: the comparatively rare occurrences of a Clactonian III (High Lodge) industry being unrepresented in their vicinity, and typical Acheulean implements being abundant both in close proximity to them and elsewhere, it may safely be inferred that this Early-Middle Acheulean industry is that to be associated with the individual represented by the skull; furthermore, the stratigraphy precludes any admixture of later material. Such an association of a human fossil with a hand-axe industry does not appear to have been recorded previously in Europe.

Sir Arthur Keith, F.R.S.—*The Swanscombe fossil* (11.20).

The bones unearthed at Swanscombe are very important because they give authentic information of the only mid-Pleistocene European known to us. It has been calculated that the original length of the skull was 185 mm. (quite as long as the skull of many living Englishmen) and the width 144 mm. The roof of the skull was low, but no lower than in some modern skulls. The capacity of the brain chamber has been estimated at 1,350 cc., which places Swanscombe man in the upper limit of the smaller-brained races of humanity.

The convolutions of the brain have left a sharp impression on the interior of the skull bones, and from casts made from the bones it is clearly seen that the convolutions form a complex pattern in which there are striking resemblances to that of the modern human brain. But there are points of difference which indicate an evolving organ which has not yet reached the modern level, particularly in those regions which are believed to sustain the higher functions of the brain. In the occipital region, as in Piltdown man, there is an extreme degree of asymmetry which is usually regarded as an indication of specialisation.

Even the oldest of the Neanderthal skulls are marked and moulded in quite a different way from the bones of the Swanscombe skull. The only fossil skull which possesses comparable markings is that of Piltdown man—a much older English skull. Prolonged investigation has convinced the author that there is a relationship between the Piltdown and Swanscombe men, the Piltdown bones being by far the more primitive in their characterisation.

Prof. E. W. Le Gros Clark, F.R.S., and Dr. G. M. Morant.—*The Swanscombe fossil* (11.40).

The Swanscombe fossil consists of complete occipital and left parietal bones of an individual whose age is estimated to be 20 to 25. A comparison of the measurements and contours of the bones shows that the great majority of their characters, whether considered singly or in combination, fall well within the range of variation exhibited by *Homo sapiens*. It can also be inferred that the cranial capacity of the complete skull (estimated at about 1,325 cc.) was quite unexceptional. There are certain peculiarities of the archaic specimen such as the breadth of the occipital bone, the thickness of both bones, the extension backwards of the sphenoidal air sinus into the basi-occipital bone, and the high degree of asymmetry of the markings on the endocranial aspect of the occipital bone. These features, however, can be matched in modern skulls. The endocranial cast shows that the cerebral hemispheres were well convoluted, and there is no certain evidence that the convolutional pattern was in any way of a primitive type. Although the cast gives the impression of a somewhat 'ill-filled' skull (i.e. the contours are rather angular instead of being well rounded), it indicates that the brain of this Acheulian fossil had acquired a status comparable with that of modern man.

It is, of course, necessary to admit that the missing parts of the Swanscombe skull may have been distinctly different from those of modern man, but *on the evidence of the fragments which are available* it is suggested that this fossil man was indistinguishable from *Homo sapiens*.

Dr. D. A. E. Garrod.

The observations of Sir A. Keith and Prof. Le Gros Clark on the Swanscombe fossil prepare us for the possibility that we may have to recognise the existence of man of modern type associated with the hand-axe cultures. If this should prove to be the case, it will have a bearing on the Kanam and Kanjera problem. However unsatisfactory the circumstances surrounding the discovery of these fragments (and this must be recognised), the difficulties raised, inherent in the specimens themselves, will be resolved if man of modern type should prove to be associated with Acheulean tools at Swanscombe. It is only just to recognise that Dr. Leakey has always maintained the probability of this association, and it now seems likely that further discoveries at Swanscombe will show that he is right.

(CONCURRENTLY WITH ABOVE SESSION.)

Dr. E. J. Lindgren.—*Winter life in Swedish Lapland* (10.0).

In the Sirkas district of Norbottens län there are both Karesuando Lapps, recently emigrated from the north, and Jokkmokk Lapps, old inhabitants of the area. At first there was much friction between the newcomers, driven southwards by Norwegian boundary restrictions, and the Lapps whose pastures they invaded, but mutual adjustments have now been made.

Every year the Lapps leave their summer encampments in the mountains to spend the winter in the forested country further south, some of them travelling over a hundred miles. The children then go away to boarding-school, while most of the women, infants and a few old men stay in small towns where they live in log huts usually rented by the month from Swedes or Finns.

The rest of the men live in tents or huts out in the woods, inspecting the reindeer at least every other day and often rounding them up so that other owners can separate, identify, and drive off any of theirs which have strayed. The herders may ski over thirty-five miles, returning long after dark, in carrying out their tasks. The question of pasture is the most urgent preoccupation ; in bad years, when the deep snow is overlaid by a thick frozen crust, whole herds may die of starvation. Railway trains also kill reindeer which stray on to the track, but compensation is paid by the company.

In early February as many members of each family as can afford to go to Jokkmokk by bus or train, which have superseded reindeer-drawn sleds, gather for the annual market. The Lapps sell their reindeer skins and buy cloth and various supplies for the coming year. Special services are held in the Lapp language in the Lutheran Church. A meeting with the officials also takes place, at which administrative details and proposed legislation affecting the Lapps' welfare are freely discussed. The Lapps show an increasing tendency to formulate and express their own opinions. These do not always coincide with the official view, although several measures have been adopted by a benevolent government to lessen the difficulties now involved in living chiefly by reindeer-breeding, with much restricted pasture lands.

Illustrated by slides and a colour film taken by Mr. N. A. C. Croft.

Mr. B. R. S. Megaw.—*Manx house types* (10.35).

Mr. J. Hornell.—*The polygenetic origins of plank-built boats* (11.10).

In essays on the evolution of plank-built boats attention has never been directed sufficiently to the technique of the constructional methods characteristic of different regions and of the different types. The universal habit has been to take the vessels in their completed state and then to apply morphological methods to their study from this limited aspect.

The conclusions so reached may be correct but they remain controversial hypotheses that cannot be proved conclusively ; neither can they indicate definitely whether the divergent forms were reached by a combination of diffusion with local variation or whether they arose through independent invention.

Working on the principle now put forward that the only sound procedure in this inquiry is to take into primary consideration the technique employed by the builders of the various types, the conclusion is reached that there are at least four types of planked boats which have had independent evolution. These are :

(*a*) The clinker or clench-built type, characterised by inserted frames.
(*b*) The carvel-built type ; planked on a pre-formed framework.
(*c*) The frameless river craft of ancient Egypt and the present-day *naggr* of Nubia.
(*d*) The junks of China, strengthened by transverse bulkheads in place of frames.

The evidence offered substantiates the view that the two in present-day use in Europe have evolved from dugout canoes. It is equally clear that they developed by different paths into forms superficially remarkably similar and with the capacity to perform like duties.

The other two types are just as clearly derived through two distinct lines of evolution from raft forms of different material and construction ; the Egyptian from the papyrus raft-canoe pointed at each end ; the Chinese from one constructed of bamboos or of logs of timber.

Dr. Margaret Murray.—*Some sociological aspects of Cambridge* (11.50).

Anthropology should not be applied merely to recording 'ye beastly customs of ye heathen,' but should be used for studying our own people. An English anthropologist studying England has one great advantage over those who go among the heathen in being able to speak the language fluently; accurate information is therefore more easily obtained. The peculiar condition of Cambridge, with its sharp division into Town and University, offers an interesting field for anthropological investigation.

Mr. Percy Leason.—*A new view of Quaternary cave Art* (12.30).

A review of the current and accepted ideas of the depiction of 'action' in the art of the Quaternary period is undertaken, the subject being approached from the viewpoint of the artist rather than the anthropologist. The view of the extraordinarily life-like attitudes and the splendid 'action' exhibited by many of these famous examples is reviewed and discussed. A criticism is given of the view that they reveal an almost 'photographic' accuracy of vision that was exercised by Quaternary man in order that his models should be 'made at all hazards to look as if they were moving.'

It is suggested that the life-like postures exhibited by many of these figures are apparent rather than real, and that on closer examination they show faults that are evident even to the 'slowest' eye. In particular there is a striking contrast between the treatment of the bodies of the animals and the feet.

This, however, is not to be attributed to any lack of capacity on the part of the artist, for the general 'tip-toe' appearance, and the often relaxed appearance of the legs, when they should be tense if the beast were involved in the action in which it appears to be depicted, is actually due to the fact that the cave artists made painstaking studies of dead animals. This is consistent, not only with the postures in which many fine examples are depicted, but with the skill with which the body and other characteristics are often delineated.

Furthermore it is demonstrated that if this theory is accepted another difficulty of the artist in viewing the many examples of cave art is also explained—i.e. that the sighting point is often lower than the centre of the animal's body and very often lower than the level of the feet.

A series of illustrations in support of the thesis was presented.

Afternoon.

Symposium on *The Middle Palæolithic* (2.0).

Mr. M. C. Burkitt.—*Introduction.*

In Lower Palæolithic times at least two distinct civilisations, each comprising a number of cultures, can be distinguished even in such a restricted area as Europe. The use of well-known French culture names for these allied cultures—even if they belong to the same culture cycle or civilisation—creates difficulties. Prehistorians should determine the cultural sequence for each geographical area, name them using local names, and only later correlate them with cultures belonging to the same civilisation elsewhere.

One of the civilisations during Lower Palæolithic times in Europe includes a large number of cultures whose industries were more particularly made on flakes. Only in eastern France and Eastern England do we find these industries associated with others made from cores (the *coup de poing* civilisation). The flake-tool industries are all allied, although slightly differing between themselves. It is unsatisfactory to class them all together and to use western European cultural names (Clactonian, Levalloisian, etc.) to describe them. Each regional flake-tool culture should be considered separately.

Sir Arthur Keith, F.R.S.—*Men of the Middle Palæolithic.*

Europe.—Men of the Middle Stone Age in Europe are usually described under the specific name *Homo neanderthalensis*, but as Neanderthal has become widely used in a generic sense it makes for clarity if we speak of those who lived in Spain, France, Belgium, Italy and south Germany as *Chapellians*, as they are best represented by the man found at La Chapelle aux Saints and described in 1910 by Marcelin Boule. Chapellians show a considerable range in structural characterisation and there appear to have been local races or varieties. In south and central Germany fossil remains have been found near Weimar (Ehringsdorf) and in Würtemberg (Steinheim) which belong to an earlier phase than Chapellians and, though Neanderthal in type, are peculiar in several respects. They may be called *Ehringsdorfians*. The earliest and the most remarkable of those remains is the skull of a woman, found at Steinheim in 1933, which bears resemblances to mankind of modern type. At Krapina, in Croatia, yet another local Neanderthal type has been found. *Krapinians* were of light build ; their limb bones were moulded on slender lines ; they were not so big-brained as some of the Chapellians and Ehringsdorfians. Not a single fossil bone of the Neanderthal type has been found in England, though the type came as near to us as Jersey.

Palestine.—Though Palestinians of the Middle Stone Age are Neanderthalian in many of their characters they have numerous features of modern man. In many details of structure they resemble the earliest Caucasians known to us—men of the Cromagnon type. They were tall, robust and big-headed.

Java.—Eleven fragmentary skulls were found on a terrace of the Solo river in 1931. *Solo* man was a low-browed type with superficial resemblances to Neanderthal man, yet different in characterisation. The type seems descended from the earlier *Pithecanthropus* and there is evidence which leads us to believe that Solo man represents a type ancestral to the aborigine of Australia.

Africa.—Much is known of Middle Stone Age man in Africa. Important recent discoveries are (1) the Florisbad skull discovered in the Orange Free State in 1934 by Dr. Dreyer. This skull, intermediate to the Rhodesia specimen and the later Boskop type of South Africa, seems to represent a type which is ancestral to Bushman and Hottentot. (2) The remains found at Eyassi, East Africa, in 1935 by Kohl-Larsen. Eyassi man outwardly resembles Solo man, but real affinities are likely to link him to Rhodesian man.

There is no support, so far as anatomical evidence goes, for those who seek to explain resemblances of the stone culture of one continent to that of another continent by postulating intercontinental migration. There may have been diffusion of knowledge, but there is no evidence of diffusion of race.

Dr. F. E. Zeuner.—*The geological evidence* (2.25).

The results of investigations by various workers (D. M. A. Bate, A. C. Blanc, H. Breuil, D. Garrod, H. Kelley, O. Schmidtgen, W. Soergel, F. E. Zeuner) on sections in which the stratigraphical position of Mousterian and Aurignacian can be studied in detail, are summarised.

It is evident that, in the area from Central and West Europe through the Mediterranean to Palestine, the Mousterian, Levalloiso-Mousterian, and the uppermost Levalloisian, probably representing *Homo neanderthalensis*, lasted from the warm interglacial Riss/Würm until the end of the first phase of the Würm glaciation. The Aurignacian (as representative of *Homo sapiens*) everywhere appears before the maximum of the second phase of Würm, in most places already during the interstadial Würm 1/Würm 2.

As lower Aurignacian, however, has been found in a climatically datable position only in Palestine whilst elsewhere deposits identifiable with the interstadial Würm 1/Würm 2 contain developed Aurignacian, it seems that Aurignacian Man did not spread generally in the area under consideration before the middle of this interstadial. It is noteworthy that at least in North France and possibly in parts of middle Italy also, the Middle Palæolithic did not become extinct before the climax of the second phase of the Würm glaciation when the Aurignacian had become well established in the remainder of the area. In North France, the section of St. Pierre-les-Elbeuf near Rouen suggests a recurrence of Levalloisian in early Würm 2 after Upper Palæolithic Man had visited the locality during the preceding interstadial.

Dr. D. A. E. Garrod.—*A note on the lithic industries of Ehringsdorf and Wallertheim* (2.40).

Mr. A. L. Armstrong.—*The Middle Palæolithic in Rhodesia and South Africa* (3.0).

The Middle Palæolithic cultures of South Africa have a wide distribution and in general facies resemble those of Europe, but there is evidence, particularly in Rhodesia, that the earlier phases are linked in close association with the South African Acheulean, and the later ones with the South African Aurignacian.

Dr. K. P. Oakley.—*The earlier Palæolithic flake industries in relation to the Pleistocene sequence in the Thames valley* (3.20).

Since it has been shown that the group of cultures to which the Mousterian belongs extends far back into the Lower Palæolithic the application of the term ‘ Middle ’ Palæolithic has become difficult. This is well illustrated by the cultural sequence in the Thames Valley.

The earliest flake-industry recognised in the Thames Valley is the Early Clactonian ; this is found in the deposits of the Lower Barnfield stage, from which true *bifaces* are absent. It may be shown to pre-date the introduction of Middle Acheulean culture into the area. Evolved Clactonian flake-implements, formerly classed as ‘ Early Mousterian,’ are found associated with a late Middle Acheulean industry in deposits which are earlier than the Main Coombe Rock of the Thames Valley. Recent researches in East Anglia by Mr. Reid Moir seem to confirm the identification of the ‘ Mousterian ’ industry associated with the Main Coombe Rock as *Early* Levalloisian. The industries occurring in the series of deposits which succeed the Coombe Rock (i.e. the Crayford Brickearths, etc.) are of *Middle* Levalloisian facies. Discoveries in the valleys of the Lea and the Wey indicate that the ‘ arctic ’ peats of Ponders End, etc., are *Late* Levalloisian, and not Magdalenian as originally supposed.

Mr. T. T. Paterson.—*The Middle Palæolithic industries in relation to the Pleistocene sequence of S.E. England* (3.40).

Discussion (4.0).

Mrs. A. Bowler-Kelley.—*Middle and Earlier Stone Age industries in south-west Africa* (4.15).

In south-west Africa the surface industry, occasionally imbedded in limestone spring deposits, is an undifferentiated Middle Stone Age usually of quartz, more rarely of quartzite or shale. It consists of smallish Levallois-like cores, flakes, unretouched points and blades and some scrapers reminiscent of Smithfield types.

In 1937, at a new site, Ameib Mine, the author identified in a thin gravel, a typologically older complex of broad flakes and hand-axes of dolerite and quartzite, called by the late Dr. Lebzelter the ' Ohombahe Kultur,' as an ensemble of strong Lower Fauresmith flavour with Victoria West elements.

Further north, at the tin mine of Uis, was discovered a workshop of late Victoria West facies the artifacts of which occur in immense numbers both rolled and unrolled throughout a three metre thick gravel deposit in a now completely dry valley. A study of the *état physique* shows a perfectly chromatic series and with both long and ' horse-hoof ' type cores and flakes, cleavers and crude hand-axes in all degrees of rolling. This find extends the known range of the Victoria West industry for approximately a thousand miles in a north-westerly direction.

Saturday, August 20.

Excursion to Ely, Littleport, Welney, Mildenhall and Newmarket.

Sunday, August 21.

Excursion to Gog and Magog Hills, Ickleton, Saffron Walden and Bartlow.

Monday, August 22.

Presidential Address by Prof. V. Gordon Childe on *The Orient and Europe* (10.0). (See p. 181.)

Prof. Stanley A. Cook.—*The re-discovery of the ancient Orient : its bearing on modern thought* (11.0).

The importance of this area is by no means confined to the part it has played in giving birth to the three great monotheisms : Judaism, Christianity and Islam. Besides the generally recognised significance of the area for the growth of early culture, the fact that its actual history can be traced over so many millennia makes it a unique field for the study of processes of development. In tracing the outlines of pre-history, proto-history, the period covered by the Bible, and the subsequent mediæval and modern ages we can throw light upon a variety of linguistic, social, political and ideological changes. What is often called ' the unchanging East ' provides positive material for reconsidering our conceptions of the processes of human development or evolution. Especially noteworthy are the gradual rise of ethical, humanist, scientific and philosophical interests in the middle centuries of the first millennium B.C., and the question of the changes now taking place over a much wider area.

Miss E. W. GARDNER.—*Pleistocene terraces and palæoliths in south Arabia* (11.30).

Miss G. CATON-THOMPSON.—*Archæological work in the Hadhramaut, south Arabia, 1937–8* (12.0).

The archæological objects of the recent expedition were :

(1) To bridge the gap in palæolithic distribution maps between East Africa and Asia—in particular to test a possible channel of diffusion of blade and burin industries into East Africa. The results may be summarised : (*a*) The bifacial complex of the lower palæolithic appears to be absent in south-west Arabia. Combining present results with those of Dr. Huzayyin, one hand-axe only has been found in Yemen, none in Hadhramaut. (*b*) The dominance of a crude flake industry of Levallois type widely distributed from the sea-board to the Rub-el-Khali. Miss Gardner shows its occurrence in her 10, 5 and 3 m. gravel terraces, as well as in æolian silt. The continuation of this culture during a long geological period is postulated. (*c*) That blade and burin industries did not reach South Arabia in Quaternary times, which retained the flake tradition throughout. (*d*) That no evidence of Stone Age contacts between South Arabia and East Africa was found. In late historic times, however, a small obsidian industry, probably of local Arabian material, was introduced, presumably from East Africa.

(2) To test the possible occurrence in South Arabia of chalcolithic painted pottery, etc., indicating contacts with the Makrau coast, Mesopotamia and India in the third or fourth millennia. No evidence was found in the area visited for such contacts.

(3) To investigate the material culture of pre-Islamic periods of whatever date, and by its help to introduce some dating, absolute or relative, into the history.

The work took place in the Wadi 'Amd, near Hureidha. A temple, domestic dwellings, and tombs were excavated.

Dating.—The earliest possible date is given by import seals, which may be sixth to fourth century B.C. in their probably Achæmenid homeland. On the other hand some beads are current in Egypto-Roman times, and a glass bottle looks equally late. As the temple was reconstructed twice, and the ossuary, from which datable objects were got, probably represents a considerable length of time—200 years at least is suggested—an inclusive date of about 300 to 100 B.C. or later may be a fair estimate, subject, however, to possible further evidence from inscriptions. It is suggested that the temple architecture was influenced more by Persia than by the classical world.

Miss W. LAMB.—*Prehistoric Anatolia : excavations at Kusura, near Afyon* (12.30).

Kusura, situated in the debatable country between the western cultural zone of Anatolia (known to us from Troy, Yortan, etc.), and the central provinces (represented by Alishar and Bogazköy), provides important evidence concerning the extent of the Hittite domination to the west and the contacts of the more primitive peoples during the early age of metal.

The site consists of a town and an extra-mural cemetery. The town was first occupied in the chalcolithic age (period A) by the makers of an excellent monochrome ware whose affinities are somewhat obscure; but their successors, in period B (covering part of the second millennium), definitely belong to the western group. Their wares are marked by a wealth of plastic decoration; their houses, mainly of mud-brick, are often preserved to a considerable height. A transitional period follows, when the potters' wheel was introduced and red-cross bowls recalling those of Troy V were made. In the second millennium (period C), Kusura came under Hittite influence, and there is reason to believe that the citadel, like those of Alishar and Bogazköy, was fortified. The cemetery, which seems to belong to period A, contained pithos-burials and cist-graves.

M. L'ABBÉ BREUIL.—*The thirty metres terrace of the Somme.* (*Taken as read.*)

Altitude: 30 metres above the sunk bed (to the top, including the loess and its loams). At Abbeville it is only a few metres above the marshes; at Menchecourt the Riss-Würm beach is above it.

In the coarse basal gravel, the result of solifluxion, it contains crushed industries coming from the beds of the 40-metre terrace (above the sunk valley): Abbevillian, Clacton I, Acheul I; it has lost the Cromerian fauna of this high terrace (*Elephas meridionalis*, *Rhinocerus etruscus*, *Machaïrodus*).

In favoured sites, such as Bourdon, it is perfectly preserved as far as the edge of the marshes, and has no lower terrace superposed. Elsewhere, the latter has swept it away, leaving only a hanging cornice, though it very often covers lower lying scraps of it. The rocky base, visible at Bourdon, drops at a rather acute angle.

Section from the base upwards:—

(1) Solifluxion at the base: Abbevillian, Clacton I, Acheul I, all crushed.
(2) Sand and fine fluviatile gravel; Acheul II intact at its base. Warm Fauna.
(3) Middle solifluxion, a great part swept away.
(4) Sand and fine fluviatile gravel; Acheul II intact at the base and the top; some few small Levallois flakes.
(5) Marked solifluxion, intact. Evolved Clactonian on the surface.
(6) Sand with warm water shells, weathered into reddish sand above. Acheul IV (? the oldest loess).
(7) Solifluxion of the base of the old loess. Acheul V superposed.
(8) Old loess. On top, signs of peat.
(9) A slight solifluxion destroying this.
(10) Red loam. (Acheul VI and VII.)
(11) Würm I solifluxion, Levallois V on top.
(12) Lower recent loess.
(13) Middle solifluxion, Levallois VI on top.
(14) Middle recent loess.
(15) Upper solifluxion, Levallois VII on top.
(16) Upper recent loess.
(17) Brick earth with gravel coming from a destroyed solifluxion. Upper Palæolithic.

AFTERNOON.

Prof. C. DARYLL FORDE.—*The stability of unilineal kin groups* (2.15).

The fundamental importance of unilineal kin groups in primitive social organisation has long been recognised. Unilineal reckoning permits the maintenance of stable social relations within and between self-perpetuating groups. But stability and persistence have, in schematic formulations, been given too absolute a character. The number, constitution and functions of kin groups in a given society are in fact subject to significant changes as a result of processes of fission, accretion and combination. Population trends are likely to affect the balance of these processes. Their importance in modifying the social structure over relatively short periods of time are demonstrated by field data from communities of declining and of increasing population.

Dr. M. Fortes.—*A religious ' racket ' in the Gold Coast* (2.50).

The Tong Hills ' fetish ' is widely known in the Gold Coast. It shows a primitive, technically backward society exploiting the credulity of people of superior culture already partly westernised. The ' fetish ' is actually one of several, each the cult centre of a group of clans. Esoterically, the cult is a blend of ancestor and earth reverence, initiation ceremonies, calendrical festivals, magic and oracle. Exoterically, it purports to confer fertility and prosperity, and to be an omniscient oracle. The countrymen of the cult custodians scoff at this. But more distant tribes reverence the ' fetishes ' with fear and devotion, making pilgrimages to them with valuable gifts. An elaborate organisation sustains this traffic. In recent years, chiefly through the enterprise of one man, utilising his power as Headman, one ' fetish ' has extended its influence widely through Ashanti and the Coast. All classes of people, including literates and Christians, flock thither, paying large sums to supplicate for children, health, wealth. The Headman's messengers, in the name of the ' fetish,' keep the pilgrims up to scratch by persuasion or threats. This traffic is so lucrative that competition for it has split the Hill clans into hostile factions.

Mrs. A. Hingston Quiggin.—*Primitive money : origin and evolution* (3.25).

The object of this paper is : (1) To draw attention to the collection of about 300 examples of primitive currency in the Museum of Archæology and Ethnology. (2) To show how a study of such a collection upsets economic theories of the origin and evolution of money. The usual theory is that money results from barter in one (or all) of three ways : (1) From the inconvenience of barter—' hungry hatter theory.' (2) From selection of some article wanted by all—' coconut theory.' (3) From substitution of a token for the articles bartered—knives, hoes, etc. Investigation of the present-day use of primitive currency suggests that it is not derived primarily from barter, but from present-giving, ' bride-price ' and wergeld. These fix the standard of value and evolve a recognised medium of exchange (two of the main functions of money) and endow certain objects with a ceremonial value irrespective of their intrinsic worth. The whole process is illustrated in backward societies at the present day.

Sir Richard Paget, Bt.—*The influence of sign language on civilisation* (4.0).

The linguistic sciences can have little or no relation to Anthropology until they study the gestures rather than the sounds of human speech.

The born deaf mute naturally expresses only generalised ideas, viz. by a general pantomime ; his mentality is pre-Aurignacian !

Helen Keller's education only began when she realised that everything had a ' name,' which could be spelt in the hand alphabet.

Civilisation may have begun when man—in Aurignacian times—became an Artist, and thus learnt to symbolise the isolated elements of his environment; characteristic hand gestures then produced corresponding mouth gestures—i.e., words.

The poetic gift enabled man to develop signs for abstract ideas—by analogy to their concrete counterparts.

A ' verbal ' type of sign language would be of great value for the education of deaf children. Their parents could learn it very easily—the child would then think in terms of verbal symbols represented by hand signs. From these, the transition to written words would be relatively easy.

Sign language gives also the clue to the development of grammatical forms, inflexions, etc., in speech, and offers a new field for grammarians.

The writer pleads for the co-operation of anthropologists and linguists, so that human speech may take its legitimate place among the sciences of human behaviour.

(CONCURRENTLY WITH ABOVE SESSION.)

Sir ARTHUR KEITH, F.R.S.—*Re-examination of the Piltdown problem* (2.0).

A prolonged re-examination of the Piltdown fragments in the light thrown on them by the intact bones of the Swanscombe skull has led the speaker to alter his original reconstruction of the Piltdown head. Of all the early forms of humanity known to us, that of Piltdown is the most strange and most misunderstood.

The early types of humanity found in all other parts of the world frankly proclaim their ape-like heritage. It is otherwise with Piltdown man. His forehead is strongly made, but it is upright compared with that of his contemporaries in China, Japan and Africa. His head was high-vaulted and his brain relatively large, but as regards some parts of his face, particularly in the region of the chin and jaw, Piltdown man was the most ape-like of all. This early English representative of humanity blended characters of ancient ape and evolved man and represents one of Nature's vain attempts to produce a new type of mankind, for we have every reason to suppose that Piltdown progeny became extinct before the dawn of modern conditions.

Asymmetry of the brain is believed to be a mark of specialisation and a feature confined to highly evolved modern races of mankind. Now there is no doubt that the brain of Piltdown man was asymmetrical to a degree rarely met with in modern human heads, and in Swanscombe man there is also a high degree of asymmetry. We must alter our conception of the antiquity and meaning of asymmetry of the brain. In the Piltdown breed of humanity Nature was trying a bold experiment of concentrating the higher functions in one hemisphere of the brain. It is only when we accept the existence of cranial and cerebral asymmetry that the Piltdown fragments fit into their appropriate anatomical position.

Modern man has shed many of his ape-like features, which have been replaced by those proper to juvenile stages of growth, and this tendency is called pedomorphism. Piltdown man has several pedomorphic characters in his skull—an unexpected finding in so early a type of humanity but not so surprising when we consider that one of the most primitive of living races, the Bushmen of South Africa, manifests such characters abundantly.

Dr. Harry Campbell.—*The immense range of natural selection* (2.20).

No inherent drive operates in the evolution of species.

Organic evolution results from a discriminating selection of transmissible variations (mutations), and has been in operation from the first dawn of life. Without such selection at every stage of evolution neither advance nor retrogression would occur.

The specific development of a member of any given species, *including its tendency to mutations*, is determined by the *heredity complex* of that species. This is represented by an average zygote of the species, the specific ' germ-plasm ' of which is derived from the germinal epithelium and gametes of the two parents, whereby its continuity is preserved.

The evolution of a species is the evolution of its *heredity complex*, which is built up by natural selection. It follows that the *mutations* and *mutation-trends* of a species are the products of natural selection, which therefore should take precedence of mutations in the evolutional process.

All organic evolution, mental and bodily, takes place by the natural selection of mutations. Progressive mental evolution results from the selection of super-average mental endowment possessing *survival value*. This is favoured by good prehensibility and adequate scope.

Sir Arthur Keith, F.R.S.—*Early Palestinians* (2.55).

To-day we may claim to know more of the peoples who lived in Palestine many tens of thousands of years ago than of their contemporaries in any other land. For this knowledge we are indebted to the British School of Archæology in Jerusalem and the American School of Prehistoric Research, whose expedition, led by Dr. Dorothy Garrod, explored caves on Mount Carmel which are unrivalled for the completeness and the length of time covered by their records of human occupation. The earliest people revealed by the excavations were living on Mount Carmel before the onset of the last Ice Age.

Bones embedded in hard limestone were transported to the Museum of the Royal College of Surgeons, where they were carefully chiselled out and examined by Mr. T. D. McCown and the author. They were taken from two caves, Tabun and Skhul, which, there is reason to believe, were inhabited in the same climatic phase. From preliminary examination of the bones it was at first thought that two distinct races had inhabited the caves—the Tabun people resembling the Neanderthal type of ancient Europe and the Skhul people being more akin to a primitive sort of Caucasian or white man. Later, however, intermediate stages between the two extremes were found and the idea of two races was abandoned. In no modern community could one find such a wide range of individual variability. The evidence suggests that the people of Mount Carmel were in a state of evolutionary instability.

The Palestine discoveries throw light on the possible origin of the Cro-magnon type, which replaced the Neanderthal type in Europe. The Skhul people were Cromagnon in many respects, tall with massive carinated thigh bones, big-headed, with strong jaws and well-developed chins. They seem to give a transition type between Neanderthal and Cromagnon.

Casts made of the interiors of Palestinian skulls reveal brains which in size, shape and pattern are up to modern European standards ; and rearticulation of hands and feet shows that these did not differ in any essential from the hands and feet of modern Europeans.

Mr. R. F. Peel.—*Local intermarriage and the stability of rural population in the English Midlands* (3.30).

Mr. M. M. Rix.—*Prehistoric skulls recently excavated in Cyprus* (4.0).

With the help of a grant from the British Association, I was able to make an expedition to Cyprus in November 1937 to January 1938. One purpose of my visit was to help Mr. James Stuart of Trinity Hall, Cambridge, in the excavation of an early Bronze Age cemetery at Vounous near Kyrenia on the north coast of the island. Sixteen of the skulls have been brought to England, and will be on exhibition at the meeting.

Secondly, my chief object was to study the skulls from Khirokitia, a Neolithic settlement on the south of the island, which is in process of excavation by M. Dikaios ; he is the Curator of the Cyprus Museum, Nicosia, where I spent much of my time reconstructing the Khirokitia material. Although their poor state of preservation made the actual skulls disappointing, the important fact emerged that they had been artificially deformed by means of fronto-occipital flattening. I also spent some time at Khirokitia itself where the skeletons are being preserved *in situ*. As the Neolithic period in Cyprus dates back beyond 3000 B.C. the discovery of artificially deformed skulls from that epoch involves a revision of ideas about the earliest date, and the place of origin, of cranial deformation.

Tuesday, August 23.

Mr. K. Jackson.—*Calendar Customs in the Eastern Counties* (10.0).

In the Eastern Counties the calendar customs, the celebrations on certain festivals and fixed events throughout the year, were as well preserved within living memory as in any part of England. They have mostly died out since the War, but descriptions are available from various sources which give a fairly complete picture of their character. Plough Monday, May Day, St. Valentine's Day, Shrove Tuesday, Ash Wednesday, Mothering Sunday, Good Friday, Oak Apple Day, Guy Fawkes Day, St. Cecilia's Day, St. Clement's Day, St. Catherine's Day, St. Thomas' Day, were all kept up with their appropriate festivities. Village Feasts and the Harvest Home Horkey are also described.

Mr. I. C. Peate.—*Some Welsh light on the development of the chair* (10.35).

It has generally been assumed that the earliest examples of movable chairs in Britain were probably the ' joyned chairs ' of panelled character, ' their structure suggesting that they were evolved from the chest.' The earliest surviving examples of such chairs in this island belong to the beginning of the sixteenth century. But a Welsh manuscript written in the twelfth century has contemporary illustrations of chairs of a very different character. These are discussed and attention drawn to the early literary evidence in Welsh concerning the various forms of chairs and the seating arrangements in the courts of the Welsh princes.

Miss B. Newman and Mr. L. F. Newman.—*Birth customs in East Anglia* (11.10).

Theories of the couvade and other allied customs have been accepted on somewhat incomplete evidence and the subject allowed to rest on the assumptions made. Explanations of the couvade as practised by primitive peoples were applied equally to Europe and Europeans. Some recent investigations on British customs in Eastern England have suggested that present ideas and explanations do not cover all the observed facts.

The Eastern Counties may be considered as a non-fairy district, and stories of the human midwife called in to attend a fairy birth are rare, but they do exist. This also applies to changeling lore and fairy interference with birth and babies.

The use of the caul as a protection against dangers and to confer desirable talents on the owner is essentially English. With disposal of the placenta and inferences to be drawn from it, the caul and membranes provide a considerable part of the birth customs and folk-lore in the Eastern Counties.

Beliefs that midwives have special powers to enable them to be in attendance at the birth is widely spread. Christening customs and observances during lying-in and for the first months of the child's life are interesting and are practised all over the Eastern Counties.

Rt. Hon. LORD RAGLAN.—*Survivals in dress* (11.50).

European society has always contained groups which wore garments that had gone out of fashion ; examples are judges' wigs, military and ecclesiastical uniforms, and peasant costumes.

All these survivals were once the fashionable court costumes of the day, and the reasons for their survival are unknown. It is clear, however, that the widespread belief that peasants invent costumes is quite unfounded, as is the belief that garments were invented to fulfil the functions which they now serve. Wigs, now associated with the majesty of the law, were invented for court dandies, and this and analogous facts show the absurdity of trying to put speculation in the place which can only be occupied by history.

Mr. CHARLES FFOULKES, C.B., O.B.E.—*The equipment of the soldier throughout the ages : its merits and disadvantages* (12.20).

The Greeks and Romans were armed in a more or less practical manner, and what weapons they had were well suited for their purpose.

With the decline of the Roman Empire Europe relapsed into semi-barbarism, and the art of metal working was almost lost. The fighting man was equipped mostly in leather or quilted fabrics, fairly adequate protection against primitive weapons.

With the improvement in metal working, small plates were added to leather and linen, but none of these were great hindrances to a convenient and rapid movement.

The Norman troops were equipped with a pointed helmet, the first attempt at a 'glancing' surface, but were considerably hampered by the skirts of the hauberk and long, unwieldy shields.

With the thirteenth century we find the soldier hopelessly encumbered by his panoply, the useful pointed helmet gives place to the cumbrous flat topped helm and the whole body is covered with chain armour under which is a thick quilted gambeson. Swords are badly constructed, useless for the point and badly balanced for the cut.

By degrees the skill of the metal worker progressed, and plate was added to mail till in 1400 a full suit of plate appears. During the foregoing period the horse was encased in long housings, very protective but entirely preventing any rapid movement.

The long-bow and the lance had become the decisive weapons in battle, and as the shield could only be used for defence in close quarter fighting, the mounted man had to be completely protected with plate which would oppose glancing surfaces to all weapons. Many of these pieces were fastened with straps, the cutting of which rendered the wearer *hors-de-combat*, and the weight of the armour was so great that, when unhorsed, the knight was at the mercy of the foot-soldier. Although the horse was protected entirely with armour on head and body, his legs were uncovered, and therefore a cut with knife or sword brought down man and horse.

With the advent of firearms armour was made bullet-proof and became so heavy that it was eventually discarded.

The formation of the Standing Army brought new trials to officers and men ; wide hats, full skirted coats, stocks, heavy boots, a multitude of leather belts, pouches and powder horns, heavy muskets, bayonets, swords and often hand grenades prevented rapid mobility.

In the nineteenth century the great shako of the infantry, the busby of the Guards, Hussars and Artillery, the slung pelisse of the Light Cavalry, and the useless sabretache, all militated greatly against the efficiency of the soldier.

Though some attempt in practical equipment was introduced in India in the middle of the nineteenth century, it was only in the Boer War that convenient uniform, head-gear and weapons were adopted.

We may therefore say that for nearly 700 years the efficiency of the fighting man was seriously hampered by those responsible for his equipment.

Afternoon.

Dr. R. Broom, F.R.S.—*The Pleistocene anthropoid apes of South Africa.* (Read by Prof. J. T. Wilson, F.R.S.) (2.0).

The fossil anthropoid, nearly related to *Australopithecus africanus* (Dart), and formerly described under the specific name *A. transvaalensis*, is now placed in a distinct genus as *Plesianthropus transvaalensis*, largely on account of differences from the Taungs ape in the form of the symphysial region of the jaw.

The author further records the discovery of the fossil skull of another large anthropoid which he proposes to place in a new genus and species as *Paranthropus robustus*, and refers to as the Kromdraai [pronounced Kromdry] skull.

The palate (practically complete) is relatively short and broad, and, owing to the small size of the canines and incisors, its anterior part is narrowed and the teeth are arranged more as in man than in any living anthropoid. The upper dentary arcade is slightly horse-shoe-shaped : the premolars have rounded crowns without any high well-developed cusps as in living forms, and are thus rather like human premolars but about twice as large.

The relation of the tympanic to the glenoid cavity resembles the hominid, and not the typical anthropoid, condition.

An estimate of the brain volume is given as 600 c.c.

The skull is provisionally regarded as of Middle Pleistocene age and was associated with remains of about a dozen mammals, all extinct except one, the living porcupine.

Symposium on *Ritual* (2.0).

Mr. A. M. Hocart.—*Ritual and emotion.*

Ritual is associated by most students with emotion. This association does not correspond to the facts. If we analyse these we see that ritual has got a logical structure which is the result of working out a problem. Incidentally the solution satisfies the emotions, but if emotion gets the upper hand it destroys that structure. In extreme cases, in hysterical cults, the structure vanishes altogether, and nothing is left but ejaculations and simple movements repeated over and over again.

Ritual is not peculiar in this breakdown. The logical structure of science can be broken up by the emotionalists, as we can see happening in the social sciences. The same can be observed in architecture and even in music.

It is a general law that logical constructions lose their structure under the stress of strong emotion.

Mrs. N. K. Chadwick.—*Ritual and tradition* (2.35).

Ritual and tradition belong properly to oral phases of culture. Where the order of a ritual is committed to writing, this is only for mnemonic purposes, or purposes of reference. Records of ritual and of tradition from the past have generally been committed to writing in the first instance by the intellectual classes, which are largely sacerdotal. Hence such records tend to assume a religious colouring, which conveys a disproportionate idea of the part played by religion in the cultures to which they belong. The study of the traditions and ritual of modern peoples shows that secular traditions and social ritual are quite as important, and that secular learning and education are generally held in high esteem, even among peoples who have no writing. Educational methods among unlettered peoples, e.g. the Polynesians and the Bantu peoples, are deserving of careful study.

Prof. S. H. Hooke.—*Ritual and myth* (3.10).

The paper is based on the examination of some of the early documentary material from Sumerian, Babylonian, Egyptian, Hittite and Canaanite sources. It attempts to establish the following points for the culture area which the texts in question cover.

(i) In these texts a number of very early myths have been preserved. Some of these myths, which might be called *basic* myths, in slightly varying forms, are common to the culture area referred to above. In them the actions of certain persons are described. These persons are gods, divine kings, or semi-divine heroes.

(ii) By comparison of these myths with early ritual texts, and especially with the class of texts called ritual commentaries, it becomes clear that the basic myths are actually the description of ritual situations which spring from, and are characteristic of, the social and religious organisation of our culture area at the earliest stage of which we have documentary knowledge.

(iii) By examination of certain changes which this myth and ritual material undergoes as it passes from one part of this culture area to another, it is possible to trace the emergence of history from the ritual situation.

Prof. H. J. Rose.—*Ritual and magic* (3.45).

Definitions of the words ' ritual ' and ' magic.' The former is a series of actions, generally of religious import (in the wide sense of ' religious ') forming a pattern. The latter is used in a modification of the Frazerian sense, without implying any priority in time to worship. Ritual is of three main kinds ; one is dramatic, setting forth some kind of myth ; another a series of acts of worship, designed to win the favour or avert the anger of a superhuman being. The third is magical, a process or series of connected actions, supposed efficacious in themselves, to secure some desirable end, positive or negative. This may be illustrated by the ritual of the Lupercalia in classical and pre-classical Rome. Description and interpretation of this rite : it was not addressed to any deity, but intended of itself to draw a magic circle about the earliest settlement, putting and keeping in fertility and warding off the quasi-supernatural harm feared from wolves. A few methodological conclusions.

SECTION I.—PHYSIOLOGY.

Owing to the coincidence of the International Physiological Congress (Zürich, August 14–19, 1938), no separate meetings were arranged for Section I (Physiology).

SECTION J.—PSYCHOLOGY.

Thursday, August 18.

Dr. L. S. Penrose.—*Heredity and mental hygiene* (10.0).

A knowledge of the hereditary factors which are partly or wholly responsible for mental diseases can be of preventive value in two ways: firstly, by making it possible to avoid the births of mentally abnormal people and, secondly, by facilitating early and accurate diagnosis so that effective treatment can be applied. Both these methods for controlling mental disease are at present in their infancy because of the lack of precise knowledge in the field of human genetics. The only relevant certainties are the modes of inheritance of some rare mental diseases and defects. Of the more common mental abnormalities, only genetic outlines are known. Some mental defects are recessively determined and also some psychoses with onset early in life: the less severe types of defect and some psychoses with later onset are dominant. A few severe conditions are nearly always sporadic and in some the age of the mother is a determining factor. The biological viewpoint helps in the understanding of nature's own methods of mental hygiene. Though natural selection helps to control the incidence of mental diseases with onset early in life, it has little effect upon the incidence of abnormalities which are not manifested until the end of the period of reproduction.

Prof. G. Humphrey.—*The problem of the direction of thought* (10.45).

Prof. C. W. Valentine.—*Facts and fallacies in the social psychology of early childhood* (11.30).

I. It is much harder to ensure exact knowledge about the social development of children, than about their intellectual abilities. Generalisations are often based on inadequate evidence—sometimes on occasional coincidences.

Highly speculative interpretations of the behaviour of a few abnormal children are apt to be accepted partly because they are so original and interesting.

Examples of such fallacies, in reference to: (*a*) early sex developments and interests; (*b*) thumb-sucking; (*c*) early signs of later neuroses; (*d*) interpretation of some kinds of play in infancy.

II. *Fallacies in some theories as to problem children.*

Sound and fallacious arguments in favour of Nursery Schools and Child Guidance Clinics.

The supposition that a child's character is determined by its early environment and especially by its treatment by parents.

Failure to allow for great innate individual differences in elements of disposition and character.

Many delinquent children come from 'broken homes,' but so do many normal children.

Unjustified assertions as to 'the only child,' etc.

III. *Some facts as to early social development.*

Social play and active sympathy in the first three or four years. Some experiments on the social training of infants by one another.

A critical consideration of the prognostic value of observations on early social development.

Dr. W. Brown.—*Psychological problems of the mature personality* (12.15).

The age of maturity may be regarded as having begun when the individual has finally chosen his mate and is satisfactorily and happily married. (The problems of the unmarried and the homosexual are somewhat different, but have analogous solutions.) Marriage itself involves some deep psychological problems. The necessity for the individual to shoulder his responsibilities, to think for another as well as for himself, to develop the 'binocular' view of life, both masculine and feminine, might give rise to disturbances, if not affecting conduct, at least affecting feeling and outlook. The great danger at every stage of mature life is regression, a retreat to a previously occupied position. It is because of this that deep mental analysis is so helpful, enabling the individual to gain an insight into the persistent effects of his earlier experiences. With the birth of children come new problems of adaptation. The parent must be encouraged to live for his children in an objective way, not in a narcissistic way, as though the children were his possession.

Another problem of maturity is settlement in a profession. Here, in the most favourable circumstances, it is not the man who chooses the profession but the profession which chooses the man. In this respect also there is always the possibility of regression. The occupation not chosen may have had a glamour of its own. It may have attracted him at one time, and have been dismissed for one reason or another, but not completely excluded, and when difficulties are encountered, or other special circumstances arise, such earlier ambitions may reassert themselves, sometimes with very disturbing, even disastrous, effects for the individual.

The fundamental problem before the mature personality is sublimation, the opposite of regression, a movement forward to a fuller development of the personality instead of a stepping back. Sublimation means the direction of the primitive instinctive energy towards ever higher social and spiritual ends. As a rule it is the late forties or early fifties that the greatest ethical demands are made upon the individual. He must consent finally to the surrender of some personal ambitions for himself, he must abandon the last vestiges of narcissism, he must find a philosophy to meet the needs of his advancing years. He is at the parting of the ways, one way being a process of continued sublimation, the other of regression, lost courage, and futile depression. Here again much help can be given by some form of analysis, taking him back into his past, enabling him to talk out his life and to know himself: and then, through the emotional rapport which springs up between himself and the psychotherapist, he is enabled to get a clearer view of his difficulties and courage and incentive to overcome them.

Difficulty of adaptation to the approach of declining bodily vigour, with failure of satisfactory sublimation, may result in injudicious enthusiasm for some special theory or cult in place of a real philosophy of life.

A sound philosophy of life, practical as well as theoretical, harmonising biological and psychological needs and leading up to a supra-temporal and duly co-ordinated system of values and a serene and courageous spiritual outlook, gives the highest degree of unity, vigour, and permanence of the personality, and leads to true happiness.

Afternoon.

Mr. R. Knight.—*The background of the problem child* (2.0).

There have been several attempts to classify the various psychological problems to which children are liable. Such classifications are of some importance to psychological theory, but for three reasons their practical value is limited : (i) in remedial work we are concerned, not with isolable problems, but with problem children ; (ii) different problems, such as stammering, enuresis and pilfering, do not necessarily spring from entirely different causes ; (iii) in practice the nature of the disorder is usually obvious, and it is the cause that it is important to lay bare.

On the other hand, classification of the main *causes* of children's difficulties is of considerable value in helping parents, teachers, doctors and others, in dealing with difficult children, to know what lines of inquiry are likely to be most profitable.

The records of problem children show that the three main causes of psychological difficulties are : subnormal intelligence ; defective physique ; and an unsatisfactory home environment. The third of these accounts for the greatest number of cases. Within the home environment, or background, of the child, the conditions that the author's clinical work has shown to be most productive of difficulties are : (i) parental discord ; (ii) family jealousy ; (iii) illegitimacy ; (iv) conflict with step-parents ; (v) emotional fixation ; (vi) desire on the part of the parents to satisfy their own thwarted ambitions through their children ; (vii) excessive, inadequate, or capricious discipline.

Prof. R. B. Cattell.—*The inheritance of temperament* (2.45).

Mr. E. Farmer.—*Social implications of vocational guidance* (3.30).

The experimental work that has been done in vocational psychology all tends to show that there is a closer relation between psychological tests and occupational success than between scholastic examinations and occupational success. It is, however, clear from the size of the correlations that factors not yet measured by scholastic or psychological tests also play an important part in occupational success.

Certain workers at Cambridge have endeavoured to explore the field of some of the unmeasured factors entering into occupational fitness.

It has been found that fundamental psychological drives play an important part in determining choice of occupation and that satisfaction with work is more dependent on the general conditions of employment and type of work than on immediate financial gain.

The data so far obtained in this investigation all tend to show the importance of social factors and social co-operation in obtaining and retaining good types of employment and indicate the importance, from the point of view of industry, of social development as well as intellectual.

Mr. J. G. W. Davies.—*The place of interests in vocational adjustment* (4.15).

Friday, August 19.

Presidential Address by Dr. R. H. Thouless on *Eye and brain as factors in visual perception* (10.0). (See p. 197.)

Prof. A. Michotte.—*Motor learning and morphology of the responses* (11.0).

Mr. F. B. Kirkman.—*Recent field experiments on birds* (*1937–38*) (12.0).

Subject of experiments : the black-headed gull (*Larus ridibundus*) in its breeding place.

1. Recapitulation of previous experiments : what a gull will incubate as functional equivalents of its eggs ; the range of equivalence ; the response-determining factor common to the equivalents.
2. What a gull will retrieve (i.e. get back into the nest from outside) as functional equivalents of its eggs ; the range of equivalence ; the response-determining factor common to the equivalents.
3. The gull's response when presented with an object that serves two conflicting needs, e.g., hunger, broodiness.
4. The relative strength of two selected opposing needs or drives : broodiness and the territorial impulse.
5. Conclusions.

Illustrated by lantern slides.

Afternoon.

Dr. F. W. Edridge-Green, C.B.E.—*Acquired colour-blindness* (2.0).

Acquired colour-blindness, which may be temporary or permanent, may be in every respect similar to congenital colour-blindness. A person suffering from a septicæmic condition may become partially colour-blind. When examined a year afterwards he may have recovered or become much worse and become a very bad dichromic. Acquired colour-blindness throws light on the nature of the colour sensations in congenital colour-blindness. Red and violet are the two sensations of the dichromic, they are the last to go and the first to reappear on recovery from total colour-blindness.

Mr. K. J. W. Craik.—*Sensory adaptation in vision* (2.45).

It is well known that dark adaptation lowers, and that bright adaptation raises, the absolute brightness threshold of the eye. The present paper describes some investigations into the effect of such adaptation upon brightness discrimination, visual acuity, and the subjective brightness of a uniform field. The eye was adapted by prolonged exposures to fields ranging from darkness to 5,000 or 10,000 equivalent foot candles in brightness, and its acuity and brightness discrimination tested by brief exposures to equal or different illuminations. It was found that brightness discrimination was keenest, and acuity highest, when the eye was adapted to the same illumination at which it was tested ; testing at illuminations far above or below the adapting illumination caused a marked deterioration of these visual functions. The apparent brightness of fields presented to dark and bright adapted eyes has also been investigated by successive comparison between fields presented to the two eyes separately. Some theoretical implications and practical applications of these results are also discussed.

Mr. R. S. Sturdy.—*Sensory adaptation in hearing* (3.30).

The effect of previous stimulation of the ear by pure tones of adequate intensity is to induce a condition of temporary deafness, the extent of which is extremely variable in different individuals and in the same individual on different occasions. This deafness is chiefly to tones of the same pitch, but the octaves above and below are affected to a less degree. That this effect is largely a central nervous manifestation is indicated by the fact that stimulation in one ear induces deafness in the other, and that it is a cortical manifestation is indicated by the fact that the deafness can be abolished by a sudden 'disinhibitory' stimulus, such as sudden darkness in the observation cabinet.

The effect of previous stimulation on intensity discrimination has recently been investigated, and it has been shown that the finest discrimination of intensity is, in general, made when the ear has previously been adapted to the intensity level at which the intensity discrimination is to be tested. The inconstancy of the experimental results suggests that this adaptation is, at least in part, due to central mediation rather than to fatigue of the peripheral organ.

Monday, August 22.

Prof. C. Spearman, F.R.S.—*A curious pitfall in factor psychology* (10.0).

Two or three years ago the theory of mental factors entered into a new era with what has been called multiple factor analysis. And a few months ago the leader of this new movement, Prof. Thurstone, produced its first great application to actual research. Here, 240 students have been submitted to an elaborate system of 57 tests. The result has been more than revolutionary. In place of the very heavily weighted single general factor which has hitherto always revealed itself in some form or other, there are now disclosed no less than twelve primary factors whose weights are all almost exactly equal. But such a result would appear to be unreasonable. Moreover, the principle here adopted for the reduction to factors can be shown to break down altogether under certain defective conditions of procedure, which happen to have prevailed in the present case. On the other hand, Thurstone's results do prove to be quite amenable to treatment by the older procedure known as that of 'Two Factors.' And in this fashion the fruits of his valuable work are saved from the threatened irrationality and brought instead into perfect accord with the general results of accredited investigation elsewhere.

Prof. H. S. Langfield.—*Present trends in American psychology* (10.45).

Dr. L. F. Richardson, F.R.S.—*Generalised foreign politics* (11.30).

Love and hate are alike in this: that the chief stimulus to either is any sign of the same feeling in the opposite person, or nation. The simplest mathematical expression of this mutual instinctive stimulation is

$$dx/dt = ky, \qquad dy/dt = kx \tag{1}$$

where k is a positive constant, t is time and x and y, when positive, are the intensities of hate in the two persons and, when negative, the intensities of love. The solution of (1) is

$$x = Ae^{kt} + Be^{-kt}, \qquad y = Ae^{kt} - Be^{-kt} \qquad (2)$$

where A and B are constants; so that, as t becomes large, x and y both increase with the same sign. The point $x = 0, y = 0$ is a point of balance; but the balance is unstable. The instinctive drift may lead in one or other of two opposite directions.

The expression can be made more lifelike by the introduction of positive fatigue-and-expense coefficients α and of positive or negative grievances g so that

$$dx/dt = k_{12}y - \alpha_1 x + g_1, \qquad dy/dt = k_{21}x - \alpha_2 y + g_2 \qquad (3)$$

The terms in α have a stabilising effect; so that instability only occurs if

$$k_{12} \, . \, k_{21} > \alpha_1 \, \alpha_2 \qquad (4)$$

There are still two opposite kinds of drift. For nations, the positive infinity may be called ' war '; and we have to find a name for the negative infinity, which is obviously not tranquil exclusiveness, but may be called ' united organisation,' or ' close co-operation.'

Foreign trade is the commonest form of co-operation between nations. Alfred Marshall in his discussion of foreign trade, when near to zero, gave an argument closely related to equations (3). Accordingly, on looking for something objectively measureable which might serve to represent x and y, the author has formed two statistics of ' threats minus co-operation.' One is

$$\psi = \text{(population)} \log_{10} \left\{ \frac{\text{(warlike expenditure)}}{\text{(foreign trade) . (constant)}} \right\}$$

the constant being adjusted to make ψ zero for an average nation in the year 1926.

So far it appears that assumptions of type (3) are a credible approximation to actuality. Accordingly they have been generalised for n nations thus

$$\frac{dx_i}{dt} = g_i + \sum_{j=1}^{j=n} k_{ij} \, x_j \qquad (i = 1, 2, 3 \, . \, . \, . \, n)$$

and the criterion of stability has been deduced. In many cases there is an instinctive barrier separating two regions of opposite drift; the barrier goes through a point of balance, the position of which depends on the grievances g.

The relation between g and objective facts is very peculiar. A halting apology may be received as though it were a fresh insult.

The practical conclusion is that the traditional policy of the balance of power is now futile, because the balance is unstable. To bring the point (x, y) into the region where the instinctive drift goes towards more co-operation, two actions can be taken: (i) the barrier can be heaved to the positive side by abolishing grievances g, and (ii) the point (x, y) can be heaved to the negative side by decreasing threats. Neither of these actions is instinctive; both would require national efforts of will.

Miss D. Gandine-Stanton.—*An examination of behaviour in attempting difficult tasks* (12.15).

Difficult tests, problems and puzzles have been presented to several groups of children. About six hundred individuals have been examined, all of them twice and most of them three times over a period of three years. The groups can be differentiated as

Christian	Mentally superior
Jewish	Neurotic
Mentally subnormal	Physically defective

There are observable differences in the behaviour manifested by the members of some of the different groups, and it seems that further study of this kind should result in the development of a useful diagnostic test.

Afternoon.

Excursion to Royal Eastern Counties Institution, Colchester.

Visit to works of the Cambridge Instrument Co., Ltd.

Tuesday, August 23.

Prof. J. C. Flugel.—*The Hormic Theory* (10.0).

If, with most modern psychologists, we distinguish between feeling and conation as separable aspects of orectic experience, it behoves us to determine the relation between them. The doctrine of psychological hedonism, widely held in the nineteenth century, maintains that conation is determined by feeling, while the hormic theory, which is most in favour to-day, reverses this relationship. Many contemporary psychologists, however, do not hold the hormic theory consistently, and indeed it is pretty generally admitted that, in particular, the pleasures and unpleasures connected with sensory experience are at first sight difficult to reconcile with this theory. McDougall has endeavoured to champion the hormic theory even in this sphere, but there is a dearth of experimental evidence directly bearing on the problem. A great many researches, however, have indirectly produced strong evidence in favour of the theory, including those on will, choice, involuntary movement, æsthetic attitude, as well as those more specifically dealing with feeling experience. Washburn and Grose, in a limited field, attacked the problem more directly with the same result, and recently Yuan Pan, though in general confirming the hormic theory, has shown that the nature of sensory feeling and its relation to conation are more complicated than might at first appear.

Joint Discussion with Section L (Education) on *The educational significance of the cinema and wireless* (11.0). See under Section L.

Mr. R. C. Steele.

Dr. P. B. Ballard.

Dr. S. J. F. Philpott.

Miss L. M. Holt.

Afternoon.

Dr. Tom A. Williams.—*Assisting mental hygiene by literature* (2.0).

The case method to convey principles is used through inner situations of characters chiefly fictional, e.g.:

Conditioning : *Henri Brulard, Linda.*

Reconditioning and social adjustment : *Old Man's Birthday, Clayhanger, Visiting Moon, Kim* in *New Wine.*

Acute conflict in women : *Mary Lavelle, The Bridge, Tops and Bottoms.*

Acute conflict in men : *Long Tunnel, Quatrain, The Fountain.*

Scruples are transcended by D'Eath in *Processional, The Rector's Daughter.* Shame is got rid of by Enid in *Old Man's Birthday.* Props fail the inadequate Forrest in *Full House*, Lola in *Invisible Event.* The harm of domineering is shown in *Galaxy*, and by Tillinglast, who blamed others, in *Never Go Back.*

That self-deception is unwise we see in *The Wildings, Laughter in Heaven.*

The futile see themselves in *Dusty Answer, The Hotel.*

There are many examples also of spite, envy, jealousy in all disguises.

Hopeless misfits may take courage from *The Odd Job Man, Peter Homunculus, Jacob Stahl, Mary Lee.*

Amateur meddling is shown up in *Chip of the Block, Progress to the Lake, The Balliols, A Room with a View.*

Skilful guidance is found in *Green Light, New Wine, Antony, Dr. Sam.*

A psychological pharmacopœia on these lines proved valuable to teachers and college professors in a course at Duke University.

Mr. R. C. Oldfield.—*Some verbal problems connected with the definition of personal qualities* (2.45).

Miss S. M. Harvey.—*Some factors affecting the reliability of the interview as a method of obtaining personal information* (3.30).

In an investigation at the Cambridge Psychological Laboratory, each of 8 interviewers saw in turn each of 11 candidates. The interviews were directed towards the reasons why the latter had taken up the study of psychology. A report was written by each interviewer upon the reasons for each candidate's choice. Conversation during the interviews was recorded. Quantitative treatment of the results shows : (*a*) variation both in amount and in content of the statements obtained by different interviewers from the same candidates ; (*b*) a closer correspondence between the statements obtained by some interviewers than between those obtained by others ; (*c*) variation between the length and content of reports made by different interviewers. To some extent this follows the same trends as the information obtained from candidates, but (*d*) the proportion of candidates' statements ' accepted ' and reported varies more between interviewers than it does between candidates ; (*e*) the interviewers' inferences and the structure of the reports also tend to differ. These differences suggest the operation of selective factors, different as between interviewers, (1) during the interview, and (2) before or during the writing of the reports. Qualitative analysis reveals that these were related : the first, to the ' general approach ' of the interviewer, type of question and items emphasised during conversation ; the second, to the ' attitude ' of the interviewer to the information to be obtained ; these two had some determinants in common. Such factors depend not only upon preconceived interpretative hypotheses about such interest determination, but also upon less formulated attitudes, founded in the interviewers' experience, but operative at a less conscious level.

SECTION K.—BOTANY.

Thursday, August 18.

PRESIDENTIAL ADDRESS by Prof. W. STILES, F.R.S., on *The general physiology of the plant cell and its importance for pure and applied botany* (10.0). (See p. 213.)

JOINT DISCUSSION with Section C (Geology) on *The postglacial history of Fenland* (11.15). See under Section C.

AFTERNOON.

Excursion to Roman Road and Gog Magog Hills.

Friday, August 19.

JOINT DISCUSSION with Section D (Zoology) on *The mechanism of evolution* (10.0). See under Section D.

(CONCURRENTLY WITH ABOVE SESSION.)

Prof. F. J. LEWIS.—*The physical nature of the outer surface of the cell walls of the mesophyll of the leaf* (10.0).

The outer surface of the mesophyll cell walls is unwettable by water but wettable by hydrocarbons. Infiltration of the leaf with dye solutions shows that dyes with an acid chromophore pass in with the water and fill the interspaces. Infiltration with dyes having a basic chromophore results in the adsorption of the dye on the surface of the walls at the point of entry while the water alone passes on and fills the interspaces. On exposure of the mesophyll the dye adsorbed on the wall is found to be insoluble in water. The effect of a fatty acid such as sodium taurocholate is to render the surface of the walls wettable, but this action only takes place within certain *p*H limits.

Dr. W. R. G. ATKINS.—*The measurement of light in relation to plant growth and distribution* (10.30).

Attention is drawn to the necessity of a thorough study of the instruments used in measuring light, so that the relation between current and intensity is known. The optical properties of diffusing surfaces and colour filters must also be considered, as well as the angular distribution of the light. The daily and seasonal changes in vertical illumination have been measured, and the variations from year to year noted for eight years. Shaded habitats are best rated by the 'daylight factor,' though the 'colour factor' also deserves consideration. Penetration of light into the sea results in an alteration both in intensity and colour. The effect on diatom cultures has been studied and reveals an optimum depth for photosynthesis. The explanation of the disappearance of *Zostera marina* which attributes this to lack of sunshine resulting in attack by parasites is disproved by an adequate study of the meteorological records.

Prof. T. A. Bennet-Clark and Miss D. Bexon.—*The rôles of osmotic and electrosmotic pressures in the regulation of cell turgor* (11.0).

The view that cell turgor is maintained solely by the osmotic pressure difference between the vacuole and outside solution is shown to be untenable. For example (*a*) it is shown that when a protoplast is plasmolysed in potassium chloride of 28 atmospheres and is suddenly transferred to sucrose 28 atmospheres, rapid entry of water into the vacuole occurs. Further, when a protoplast plasmolysed in 28 atmospheres is transferred suddenly into 53 atmospheres sucrose there is still a rapid passage of water from the *stronger* external to the *weaker* internal solution during a period of approximately 10 to 20 minutes, followed by a passage of water out of the cell again in the 'normal' or expected direction. (*b*) The data obtained enable us to calculate that in addition to the ordinary osmotic flow of water expected on the classical theory water movement at pressures of over 50 atmospheres may be encountered under certain circumstances. (*c*) In general when a tissue is transferred from an electrolyte solution to one of a non-electrolyte there is a flow of water into the vacuoles which is not due to and may act in the opposite direction to osmotic pressure differences. Similarly if the transfer is from a non-electrolyte to an electrolyte, a non-osmotic flow of water out of the vacuoles is brought about. (*d*) Study of the behaviour members of the lyotropic series Na—K—Ca—La suggested that these water movements are due to electrostatic forces set up by the ions of the electrolyte. (*e*) The fact that considerable pressures (possibly electrosmotic) can be generated by a cell of which the osmotic pressure is only about 15 atmospheres suggests that these 'electrosmotic' pressures may be of great importance in the many turgor reactions of plant tissues.

Dr. T. G. Mason and Dr. E. Phillis.—*Observations on the effects of pressure on the properties of protoplasm* (11.30).

Exposure of cotton leaves to *direct* pressures of 14,000 lb. per sq. in. in a hydraulic press leads to the expression of a clear sap which is believed to come from the vacuole through fissures in the protoplasm. Only about one-third of the total water of the leaf can be expressed in this way. The rest of the water can be expressed by relatively low pressures provided that the residue is gently rubbed between the fingers and the thumb. It is suggested that protoplasm possesses a gross structure that is destroyed by small shearing forces but which can withstand relatively large direct pressures. The residue from the hydraulic press shows approximately normal rates of respiration and can imbibe water till turgor is fully restored. The tenacity with which protoplasm retains its water under direct pressures is contrasted with the ease with which water can be separated from gelatin gels under pressure and it is suggested that the continuous medium of protoplasm cannot be aqueous.

Dr. Winifred E. Brenchley.—*The comparative toxicity of inorganic plant poisons* (12.0).

The majority of elements may exert a harmful action on the growth of higher plants, but the relative proportions necessary vary considerably. Boron and manganese, for instance, may be harmless in quantities at which copper and zinc are intensely poisonous.

The toxicity of any element may depend upon the compound in which it is presented—e.g. arsenious acid and arsenite proved fatal to barley at concentrations at which arsenic acid and arsenates were innocuous. With copper, cobalt and nickel the sulphates and chlorides have shown relatively slight differences in action, though these were sometimes significant with regard to the concentration at which marked toxicity occurred.

Individual species respond according to the poison used. With barley, copper was more poisonous than either nickel or cobalt, but the differences were relatively small. With broad beans, however, cobalt was much more toxic than nickel or copper, particularly as sulphate. Earlier experiments had shown that peas were more sensitive than barley to copper and arsenic, whereas barley was less able to withstand zinc, boron and manganese. The morphological response to poisoning depends on the element concerned, root and shoot growth being variously affected. Individuality of plant response is frequently shown by great variation in growth in borderline concentrations just below those causing marked depression of growth.

AFTERNOON.

JOINT DISCUSSION on *The mechanism of evolution* (*continued*) (2.15). See under Section D.

(CONCURRENTLY WITH ABOVE.)

Dr. J. BARKER.—*Temperature and the starch/sugar balance in potatoes* (2.15).

Since the researches of Muller-Thurgau (1882 and 1885) it has been generally recognised that sugar accumulates in potatoes which are stored at low temperatures and that the accumulated sugar disappears when the potatoes are returned to higher temperatures. These changes in the sugar content were attributed by Muller-Thurgau to differences in the temperature-coefficients of the three reactions ; (*a*) hydrolysis of starch to sugar, (*b*) condensation of sugar to starch, (*c*) consumption of sugar in respiration. Recent experiments suggest, however, that the influence of temperature on the balance between these three reactions cannot be interpreted as due solely to differences in their temperature-coefficients.

Evidence has been obtained showing that the accumulation of sugar at low temperatures is associated with a change in the metabolic state which persists for a short time when the temperature is increased again ; this change is provisionally conceived as an activation of the hydrolytic system.

The changes in sugar content are shown to be dependent not only on the actual temperature of storage (a characteristic already well recognised) but to be markedly influenced by the previous temperature-history.

Mr. R. S. DE ROPP and Prof. F. G. GREGORY.—*The hormone system of the rye grain* (2.45).

The embryo of the rye grain receives from the endosperm, during the first few hours of soaking, hormones which influence growth of roots and coleoptile and the rate of response of these organs to gravity and light. A factor influencing the response of the embryo to added hetero-auxin has been noted.

The aleurone layer of the grain appears to be the source from which the scutellum receives the stimulus for diastase production.

The aleurone and endosperm form with the embryo a complex interacting system. Removal of the aleurone layer after the grain has been soaked for an hour causes growth of the embryo to be inhibited. Subsequent growth of such embryos is abnormal.

A similar effect is produced by removing the endosperm from grain soaked for one hour leaving the aleurone in contact with the embryo.

Breaking the connection between aleurone layer and the endosperm before the grain is soaked has the effect of reducing the growth rate of embryo, while breaking the connection after the grain has been soaked has no effect on the embryo.

Mr. E. K. Woodford and Prof. F. G. Gregory.—*The relation of oxygen supply and respiration rate to anion and cation absorption by barley plants at varying nutrient levels* (3.15).

An apparatus is described for measuring simultaneously nutrient absorption and root respiration rate, at varying oxygen tensions and under aseptic conditions.

The results of an experiment are presented in which sixteen combinations of oxygen tensions and nutrient concentrations were studied. Special care was taken in the design of the experiment so that the interaction of the factors could be statistically estimated.

A considerable absorption under completely anærobic conditions was found for nitrogen, phosphorus, and potassium. With phosphorus, absorption in pure nitrogen was considerably greater than in 20 % oxygen. Nutrient concentration was the chief factor in absorption, irrespective of oxygen concentration. Respiration was scarcely affected by nutrient concentration but was greatly increased by increasing the oxygen tension. Oxygen supply scarcely affected absorption at low nutrient levels but had a large effect at high concentrations. This is the chief interaction noted.

Respiration has no direct relation to nutrient absorption in experiments of short duration, where root growth plays no part.

In excised root systems the rapid fall in absorption precedes the fall in respiration rate.

The relation between absorption and nutrient concentration is different for anions and cations, and specific effects of ions were observed also in the relation to oxygen tension.

Mr. G. J. Boswell and Mr. G. C. Whiting.—*The catechol oxidase system* (3.45).

By the use of an oxidation product of catechol as an inhibitor of cell respiration it has been possible to show that the catechol oxidase system controls not less than 65 % of the total oxygen uptake and carbon dioxide output of thin slices of the potato tuber. The amount of the residual respiration is at a maximum, 33 %, in potatoes collected and used during September and October and decreases to a minimum value of about 10 % in potatoes stored in the autumn and used experimentally in the following April and May.

Semi-popular Lecture by Prof. A. H. R. Buller, F.R.S., on *The sexual process in the rust fungi* (5.0).

During the past twelve years J. H. Craigie, A. M. Brown, and other workers in the Dominion Rust Research Laboratory at Winnipeg have investigated the sexual process in the rust fungi by means of experiment. As a result of their labours we now know that, in long-cycled heterothallic rust species, there are two ways in which the sexual process is normally initiated : (1) by the fusion of a (+) mycelium with a (−) mycelium, as first observed by Craigie (1927) in compound pustules of *Puccinia helianthi* and of *P. graminis* and as confirmed by A. M. Brown (1935) in compound pustules of *P. helianthi* in which a mixing of the pycnidial nectar was prevented; and (2) by the union of (+) pycnidiospore with a (−) flexuous hypha, or of a (−) pycnidiospore with a (+) flexuous hypha, as observed by Craigie (1927 and 1933) in *P. helianthi* and by Buller (1938) in *P. graminis*. Flexuous hyphæ in pycnidia have been observed: by Craigie in *Puccinia helianthi* and *P. graminis*; by Pierson in *Cronartium ribicola*; by Miss Hunter in species of Melampsora, Milesia, Pucciniastrum, and Cronartium; and by Buller in twenty rust species belonging to the following genera: *Puccinia*, *Uromyces*, *Gymnoconia*, *Phragmidium* and *Gymnosporangium*. It seems likely that flexuous hyphæ are present in the pycnidia of the rust fungi in general.

Saturday, August 20.

Excursion to Wicken Fen.

Sunday, August 21.

Excursion to Forestry Commission Plantations in Thetford Forest.

Monday, August 22.

JOINT DISCUSSION with Department K* (Forestry) on *The ecological aspects of afforestation.* (10.0). (See under Department K*.)

(CONCURRENTLY WITH ABOVE SESSION.)

Sir ALBERT SEWARD, F.R.S., Mr. W. N. EDWARDS and Dr. J. B. SIMPSON.—*The vegetation of the Inner Hebrides in the early Tertiary period* (10.0).

More than fifty years ago John Starkie Gardner obtained a large collection of fossil plants from sedimentary beds near the base of the basaltic lava flows at Ardtun Head in the island of Mull. He described several of the plants but did not publish a complete account of the flora as a whole. In the present communication the authors contribute a preliminary survey both of the Mull flora, as represented by the Starkie Gardner collection in the British Museum supplemented by specimens kindly lent by other institutions, and of material from the island of Skye and a few other localities. The results are expected to be published in the near future as an official British Museum Catalogue.

The present contribution includes: (i) an account of the flora based on impressions, almost exclusively leaves; and (ii) an account of pollen investigations by Dr. J. B. Simpson. Special attention is drawn to contrasts in geographical distribution illustrated by comparison of recent and extinct species of some of the genera represented in the Mull leaf-beds, e.g., *Onoclea*, *Amentotaxus*, *Sequoia*, *Cephalotaxus*, *Ginkgo*, *Cercidiphyllum*, *Platanus*, and several others. The examination of the pollen which is abundant in the lignites of Mull and Ardnamurchan confirms the predominance of forms whose habitat is now Eastern Asia. This is evidenced not only by the presence of genera such as *Ginkgo* and *Bucklandia*

(Hamamelidaceæ) now exclusively Asiatic, but also by certain fossil pollen forms belonging to widely distributed genera such as *Alnus*, *Corylus* and *Acer*, which are most nearly related to the Asiatic species of these genera. Similarly leaf-impressions referred to *Vitis*, *Celtis* and *Quercus* can be most closely matched with the leaves of living Far Eastern forms. Reference is also made to the climatic implications of the flora and its relations to other early Tertiary floras in Europe, North America, the Arctic regions, and the Far East.

Dr. R. Florin.—*The morphology of the female cone in palæozoic conifers* (10.45).

The old and important problem of the morphology of the female cone in the Conifers may profitably be considered in the light of the oldest known remains.

Instead of ovuliferous scales the cones of *Walchia piniformis* had radially built short shoots bearing spirally arranged scale-like appendages and placed in the axils of bifurcated bract scales. One of the appendages on the inner side of the short shoot bore one single erect and somewhat flattened ovule in a terminal position. The ovules had two archegonia and a single integument, which probably originated from a division of the scale-like appendage just below the ovule.

In other species of the same group (Stephanian—Lower Permian) the fertile short shoots were more or less flattened, and showed various stages of transition towards the ovuliferous scales of *Ernestiodendron* (Lower Permian), *Pseudovoltzia* (Upper Permian), and *Voltzia* (Triassic).

The female cone of *Walchia piniformis* was built essentially on the same plan as that of *Cordaites*. In this genus, however, a still more primitive structure has now been found, characterised by the short shoots having fertile appendages with a repeatedly divided apex, and one or two seeds each.

The structure of the female cones in *Cordaites* and in the conifers is most readily interpreted in the light of the telome theory.

Dr. H. S. Holden.—*The structure of the rachis in Rachiopteris cylindrica* (11.15).

Prof. W. T. Gordon.—*On Tetrastichia bupatides, a primitive Pteridosperm of Lower Carboniferous Age* (11.45).

Theoretical considerations indicated that surfaces of minor unconformity in bedded volcanic ashes might prove useful levels along which to search for examples of the flora, and perhaps the fauna, coeval with the periods of eruption of the volcano. Plant remains in particular might be expected to occur occasionally in a petrified state. In rocks of Lower Carboniferous age at Oxroad Bay, North Berwick, the theory was borne out, and, among the plants, a new pteridosperm, to which the name *Tetrastichia bupatides* has been given, was fairly common.

The internal structure of this form is simpler than in any other of its class. The stele of the axis is cruciform in section, and entirely composed of xylem elements. The inner cortex contains sclerotic nests, and the outer cortex many secretory cells and a marked hypodermal meshwork of groups of elongated fibrous elements. The mesh is long and narrow, but becomes dilated when secondary thickening of the stem takes place. A smooth epidermis, containing stomata in which the guard-cells are flush with the surface, encloses the other tissues.

Petioles of *Lyginorachis* type are inserted in the stem almost opposite one another ; and each pair is set at right angles to the preceding and the succeeding pairs. A pulvinus occurs at the base of the petiole ; and the petiole bifurcates at a distance of some $5\frac{1}{2}$ in. from its insertion into the stem. Primary pinnæ are developed, in an alternating series, in each branch of the petiole. No laminar structure has been found so far.

No fructifications have yet been discovered in actual continuity with the petioles, though synangia that may possibly belong to the plant occur in association.

Correlation is suggested with *Telangium affine*, L. & H. sp., mainly on the ground of the characters of the cortex, but such correlation is not stressed meanwhile.

An interesting mineralogical feature is the presence of analcime as a petrifying medium. Analcime has been recorded as a sedimentary deposit in what were ancient saline lakes, but never before as the petrifying material of fossil plants. Another point of mineralogical import is that the unweathered character of the adventitious felspar grains in the ashes, as compared with the weathered condition of the felspars of the ash fragments, appears to indicate that the climate in the neighbourhood of the volcanoes was semi-arid and this is confirmed by the plant types in the ashes.

The geological horizon is low down in the Calciferous Sandstone Series, perhaps in the Cementstone Group, but certainly low in the Oil Shale Group.

A full account will be found in the *Transactions of the Royal Society of Edinburgh.*

Mr. N. W. Radforth.—*An analysis and comparison of the structural features of Dactylotheca plumosa Artis and Senftenbergia ophiodermatica Göpp sp.* (12.15).

Of these two carboniferous fern-like compressions, the former has been regarded as a true fern with annulate sporangia, while the latter was believed to be a Pteridosperm from the nature of its apparently exannulate sporangia. However, this analysis has shown that the fructifications of *Dactylotheca plumosa* are definitely annulate and identical with those of *Senftenbergia.* Moreover, it is clear that these two fossils are merely different parts of the same plant, and can no longer be regarded as belonging to separate genera. Since the name *Senftenbergia* has priority, this genus must now embrace the forms previously described as *Dactylotheca plumosa.* The spores, which hitherto had not been revealed, have been isolated from the sporangia in their various developmental stages. Both the sporangia and the mature spores resemble so closely those of the living fern *Aneimia* that the view of a possible relationship between *Senftenbergia* and the living schizæaceous ferns to which *Aneimia* belongs, is now greatly strengthened.

Afternoon.

Dr. Ralph Emerson.—*Life cycles in the Blastocladiales* (2.15).

The phycomycetous order Blastocladiales embraces three genera of aquatic fungi, *Allomyces*, *Blastocladia*, and *Blastiocladiella.* The types of sexual reproduction and the life cycles which have been discovered in certain members of this group are unique in the filamentous Phycomycetes.

They are of particular interest because they have an important bearing on general problems of sexuality, alternation of generations, fungus phylogeny, etc.

The following life histories are described :

I. *Allomyces.*

(*a*) *Euallomyces*—alternation of equal sporophyte and gametophyte generations ; discovered in *A. javanicus* by Kniep (1929, 1930) and confirmed in *A. arbuscula* by Hatch (1933), Sörgel (1937) and others.

(*b*) *Brachyallomyces*—without alternation of generations, the sexual stage apparently entirely lacking ; found by the writer to be the regular life cycle in certain isolates and noted by Sörgel (1937) as a departure from the usual cycle in *A. arbuscula.*

(*c*) *Cystogenes*—without obvious alternation of generations but differing clearly from (*b*) in the regular encystment of swarmers from resistant sporangia ; discovered in four of his isolates and in *A. moniliformis* by Emerson (1938).

II. *Blastocladia.*

Complete life cycle not yet demonstrated in any of the species ; importance of recent work on germination of resistant sporangia (Blackwell (1937), and growth of *Blastocladia* in pure culture (the writer, Sörgel 1938) is emphasized.

III. *Blastocladiella.*

(*a*) Short cycle, corresponding with *Brachyallomyces*, discovered by Matthews (1937) in *B. simplex.*

(*b*) Long cycle, corresponding with *Euallomyces*, discovered by Harder and Sörgel (1938) in *Rhopalomyces variabilis* (probably a species of *Blastocladiella*).

The similarities and probable relationships between members of the three genera are briefly discussed.

Dr. W. R. Ivimey Cook.—*The phycomycete flora of Glamorgan ; general scheme* (2.45).

A study of the Phycomycete Flora of the British Isles is a matter which has not so far received much attention at the hands of mycologists. Isolated accounts of individual members of certain groups exist, but with certain exceptions, no complete monographical account, such as has been made in Europe and America, has been attempted. With a view to improving this situation, a start was made in 1932 to compile a Phycomycete Flora of the county of Glamorgan. During the subsequent six years, with the aid of various research students, the work has been continued, and although much still needs to be completed, sufficient information is available to justify a report on the progress of the work.

In 1936 the first volume, 'Natural History,' of the *Glamorgan County History* appeared and in it the author published the first list of the Phycomycetes known to occur in the county. Since then the number has been increased and at the present time 176 species have been recorded. In the course of the work 42 species new to the country have been found, while 9 species new to Science have been discovered.

The following table gives a summary of the distribution of the species so far studied :

Order.	No. of species found.	No. of new British records.	No. of new species.
*Acrasiales	1	1	0
Mycetozoa	58	0	0
Plasmodiophorales	5	0	0
*Chytridiales	18	5	2
Ancylistales	2	1	0
Pythiales	12	4	1
Saprolegniales	38	22	2
*Peronosporales	10	0	0
*Blastocladiales	1	0	0
Monoblepharidales	7	4	0
Mucorales	23	5	4
*Entomophthorales	1	0	0
	176	42	9

The Mycetozoa have not been personally studied and the records are taken from the list published by Miss G. Lister in the *Glamorgan County History*. Those groups marked with an asterisk have not yet been critically studied and the numbers given represent only incidental records.

Miss E. Morgan.—*The phycomycete flora of Glamorgan ; the Saprolegniales, especially the terrestrial forms* (3.0).

During 1934–1936, members of the Saprolegniaceæ were isolated from soil samples taken from stations in wet pasture, dry pasture and gardens, in two districts of Glamorganshire.

Nine species of *Saprolegnia* were identified, and four species of *Isoachlya*. These species were on the whole more frequent in wet pasture than in dry pasture or garden soils.

Nine species of *Achlya* were found. These were more frequently isolated from soils of dry pasture than the species of *Saprolegnia*.

Certain species with monoplanetic spores, e.g., *Pythiopsis Humphreyana*, *Thraustotheca clavata*, were more common in dry than in wet pasture. These results suggest a connection between spore behaviour and the distribution of species.

Temperature also seems to be a contributing factor in distribution. Some species are characteristic of spring months, others of summer and others of autumn or winter months.

There are indications that water and soil may be alternative media for certain species, e.g. *Saprolegnia lapponica* was abundant in water in July, but reached its highest frequency in soil in April and May.

Neither humus content nor acidity is apparently as important in determining species distribution as the two factors, temperature and water content.

Miss P. E. Thomas.—*The phycomycete flora of Glamorgan; the Monoblepharidales* (3.20).

In 1935 an investigation of the species occurring in the vicinity of Cardiff, Glamorgan, was begun and all the six species of which the full life history has been described were found growing together in one locality. Subsequent collections from this and other localities in the same area have also yielded these six species.

The six species described are: *M. sphærica*, *M. macranda*, *M. polymorpha*, *M. brachyandra*, *M. fasciculata* and *M. insignis*. Particular attention has been paid to *M. fasciculata* and *M. insignis* since these two species have not been recorded since they were originally described by Thaxter in 1895. *M. polymorpha* and *M. brachyandra*, although new British records, have previously been described from other European countries and from America.

Investigations have shown that the type of substratum on which the organisms grow is of comparatively little importance, for although it had previously been suggested that they occur most commonly on ash and birch twigs, this has not been found to be true and species have grown on a variety of woods, the consistency of the wood being apparently of greater importance than the species. Temperature is an important factor in the cultivation of the fungus and a period of cold immediately following collection appears to be essential for the production of active growth. Under suitable control of temperature it had been possible to grow the species under laboratory conditions and investigate their life-histories.

Dr. J. Caldwell and Mr. A. L. James.—*Investigations into stripe disease of the Narcissus* (3.50).

In the spring of 1936 a study of the "Stripe" disease of Narcissus was begun at Exeter. Diseased plants of different varieties were collected and examination revealed the fact that our main types of symptoms can be recognised in different varieties. Briefly, the main symptoms are either a mosaic or an hypertrophy of the plant tissues leading to the formation of proliferations on the surfaces of leaves and of flower stalks. A study of the diseased tissues has shown that the two types of symptoms are probably closely related histologically.

In the spring of 1937 preliminary inoculations were made and this year symptoms of the disease were well developed on the inoculated plants. This spring, further inoculations were made which indicate that the causative agent is a virus and that the different symptom-pictures are conditioned by the variety rather than by the virus.

Experiments are being carried out on the identity of the virus and on the method of spread in the field.

Tuesday, August 23.

Discussion on *Present aspects of plant virus research* (10.0).

Dr. K. M. Smith and Mr. W. D. MacClement.—*On the natural modes of dissemination of certain plant viruses.*

Until recently it has been generally assumed that most, if not all, plant viruses are dependent upon one or more insect vectors for their dissemination in the field. The fact that the mode of spread of a number of viruses was unknown was explained by the suggestion that the particular insect vector concerned was rare, difficult to find, or nocturnal in habit. This theory seems insufficient, however, to account for the fact that there are no fewer than fifty-two plant viruses which spread without insect agency or by unknown means. For some of these viruses, no doubt, insect vectors will be found in due course. Nevertheless, it is now known that other means of natural spread exist and some of these are discussed. In the case of *Nicotiana Virus XI* (tobacco necrosis virus) it has already been stated that this virus is both water- and air-borne and further experiments on these lines are outlined. Murphy and Loughnane have recently expressed the opinion that *Solanum Virus I* (potato virus X) spreads by mechanical contact alone and this statement is examined in the light of field experiments on the spread of this virus carried out at Cambridge during the past five years.

Dr. R. N. Salaman, F.R.S.—*Protection against virus diseases of the potato* (10.30).

The solution will vary with the character of the district. The factors to be considered are: (*a*) The virus content of our seed potatoes; (*b*) the conditions favouring the insect vectors; (*c*) the existence of infective foci.

Methods of Attack.

(1) *The breeding of virus-resistant varieties.*—The only success so far is the production of variety No. 41956, immune to the X virus, in U.S.A. It is hoped to breed immune varieties from wild species but no virus-resistant species have so far been determined; to convey the immunity when found to an economically valuable potato is a long and difficult task.

(2) *The maintenance of stocks tolerant to virus diseases.*—Unconsciously we have been following this course for the last hundred years. The results are not impressive. We have no varieties completely tolerant to either Leaf Roll or the Y virus, though Great Scot is semi-tolerant to both. A few show some degree of tolerance to Y. All our popular stocks are highly susceptible to both. Progress along these lines with our existing material is unlikely.

(3) *Protection by means of vaccination.*—In America all varieties are infected with some mild strain and in consequence are protected against the virulent strains of X. In England such is not so general, hence our varieties are liable to acute X infection. We have isolated a non-virulent strain of the X virus which, used as a vaccine, protects against other forms of the X virus. With the Y virus, weak forms protect against the severer, but the former are not yet weak enough to be of practical use.

(4) *The raising and growing of virus-free stocks.*—The Potato Virus Research Station at Cambridge has isolated and maintained stocks of our chief varieties in a state of freedom from virus disease or, where that is impossible, such as are infected by only a virus of low virulence. These return the highest yields. If isolated from infected stocks, they will continue to do so indefinitely.

Their multiplication is intended to follow a rotation beginning in the Hebrides and/or coastal districts of West Scotland; thence to the large seed-growing areas of East Scotland and, finally, to the potato-raising districts of England. Each year virus-free seed would leave Cambridge for the 'islands,' and seed from these would pass to the mainland. The cycle of rotation would be five or six years from the liberation of virus-free Cambridge seed to its growth in England. Combined with this scheme should go a nation-wide eradication of bad and infected stocks.

Mr. F. C. Bawden.—*The isolation and properties of some plant viruses* (11.0).

Tobacco mosaic virus, cucumber virus 3, potato virus X and Bushy stunt virus have been isolated and found to be nucleoproteins. The analytical figures are all similar except that Bushy stunt virus contains twice as much nucleic acid as the others, and the apparent absence of water and any constituents other than nucleic acid and protein sharply separates these viruses from bacteria and other organisms. Tobacco mosaic virus, cucumber virus 3 and potato virus X have rod-shaped particles ; they form birefringent gels, dilute solutions show anisotropy of flow and concentrated solutions are liquid crystalline. Precipitates of the first two with acid or ammonium sulphate are paracrystalline, but those of potato virus X are amorphous. The particles of Bushy stunt virus are spherical and all preparations of this virus are isotropic ; when precipitated with ammonium sulphate at 0° C. it forms true cubic system crystals. The proteins are infective at dilutions of from 10^{-8} to 10^{-10} and give serological titres of from 10^{-6} to 10^{-7}. The type of specific precipitate with antiserum varies with the shape of the virus particles. The rod-shaped viruses give flocculent precipitates similar to those of bacterial flagellar antigens, whereas Bushy stunt virus gives a dense precipitate similar to those given by bacterial somatic antigens.

Dr. R. W. G. Dennis.—*The virus content of some Peruvian potatoes* (11.30).

In December 1937 a consignment of fifty-nine Peruvian potato varieties, collected at Puño by the Percy Sladen Expedition to Lake Titicaca, was received at the Potato Virus Research Station Cambridge. During the present season their virus content has been under investigation by the writer. The virus most frequently isolated is X, which is present in thirty-four varieties ; several strains occur, some of which are carried without symptoms by tobacco whilst others induce an interveinal mottle with yellow rings. A necrotic or ringspot type capable of inducing local lesions on tobacco or Datura has never been isolated. One variety has yielded an X the symptoms of which on European potatoes recall those of Foliar Necrosis, virus D. Associated with X in at least nine varieties is a ' streak ' virus probably identical with Up To Date Streak, virus B.

Nine other varieties contain viruses of the Aucuba Mosaic type. It is probable that these mostly resemble the virus of Tuber Blotch (F) rather than true Aucuba (G). In twelve varieties no virus has been detected whilst in four there is evidence of the presence of viruses differing from any known in this country. Infection of some of the twelve healthy varieties with European viruses has shown that they react to them as do varieties of the domestic potato ; thus two react to X by top necrosis whilst others exhibit leaf-drop-streak with Y.

Discussion (11.50).

(concurrently with above session.)

Miss E. R. Saunders.—*The neglect of anatomical evidence in the current solutions of problems in systematic botany* (10.0).

The neglect of anatomical evidence in the analysis of the angiosperm flower has led to the acceptance of many erroneous interpretations. Some justification for this neglect existed at a time when little knowledge of floral anatomy was available and when reliance had to be placed upon the general rule of the alternation of successive whorls as the sole guide to the solution of the problems of floral morphology.

The inadequacy of the basis of many of the solutions adopted is revealed increasingly by a study of floral anatomy. It has become evident that the traditional interpretation of that complex organ, the syncarpous gynæceum, does not accord with the evidence, and that, even respecting the perianth and andrœcium, accepted views must be revised, as in Cistaceæ, Amarantaceæ, *Hypericum*, *Soldanella*, *Saraca*, *Cucurbita* and many other types.

Descriptions of the flower in the more detailed accounts have mostly been written before the advent of this new knowledge. Hence they depict, but they rarely analyse. They describe the visible, but seldom indicate how the variations observed have been effected. In order that accounts which may appear in the future should present a more complete picture of the flower and of its history, it is now greatly to be desired that the workers in the two fields of systematic botany and anatomy should co-operate in framing and adopting a terminology which embraces the known facts.

Prof. F. E. Weiss, F.R.S.—*Apparent reversions in variegated hollies* (10.40).

White and green margined holly leaves are the foliage of periclinal chimæras in which the core or centre of the leaf is surrounded usually by two layers of cells of different colouring. Thus in the white margined leaves the two peripheral layers of cells are devoid of chlorophyll, while central portions of the mesophyll contain abundant green colour. From the general laws of leaf development we may assume that three layers of cells of the stem apex are concerned in the development of the holly leaf, and that in the white margined leaves the bulk of the leaf tissue arises from the innermost of these three layers which is potentially green, while in the thinner margins of the leaf the tissues have arisen entirely from the two outermost colourless layers of the stem apex.

On holly-trees with white margined leaves certain branches may be produced at the base of the plant which bear completely green leaves. Such branches or suckers arise from the normally green stock upon which the variegated holly has been grafted. More commonly twigs bearing completely white leaves are observed on the branches. These have been considered to be reversions to the colourless component of the chimæra.

A microscopic examination of such white branches has, however, shown that they are not colourless throughout, but that they have a central green core, as can be seen from the inner cortical cells which contain abundant chlorophyll. The colourless mantle by which the green core is surrounded may be six to ten layers in thickness in the mature twig, and we may assume that these have arisen by division of the second layer of colourless cells. We are dealing therefore with a periclinal chimæra in which the mantle consists of more than two layers of cells. It may be trichlamydeous or, as indicated, have an even greater number of peripheral cells. There is indeed often a considerable irregularity in the number of layers of colourless cells, and in some cases white margined and completely white leaves are found on the same twig, the former arising from dichlamydeous, the latter from trichlamydeous portions of the stem apex. The arrangement of the number of colourless cells in the stem may tend to become somewhat sectorial.

What has been said of the white margined hollies is true *mutatis mutandis* of the green margined forms. It is also true of other plants with periclinally variegated leaves such as *Euonymus*, *Eleagnus*, etc.

Probably some of the apparent reversions in periclinal graft hybrids may be merely modifications of the periclinal arrangement with an increased development of the superficial component. Some investigations seem to point to this being the case.

Mr. G. O. Searle.—*The Sandringham flax experiment* (11.10).

The linen industry of the British Isles depends largely on foreign supplies of flax fibre, although flax may be grown in most parts of this country.

The main drawbacks have been the difficulty of ensuring high yielding crops, and the excessive labour required for fibre processing.

The Linen Research Association has concentrated on the production of improved varieties of fibre flax, and on the mechanising of flax fibre production.

Largely owing to the interest shown by the late King George V in a linen research exhibition, a small crop of flax was grown at Sandringham in 1931, followed by a much larger crop in 1933.

In 1934–35 this led to the erection of a small flax factory for the experimental processing of the crop from 250 acres each year.

The experiment has already demonstrated that excellent pedigree flax seed, and fibre of the standard of the best machine scutched continental flax, can be produced in Norfolk. It is hoped that further work will demonstrate the possibility of reducing processing costs to such a level as to encourage the cultivation of this crop on a large scale.

Miss D. Joan Heppell.—*Some contributions to the cytology of the genus Narcissus* (11.40).

An attempt has been made to ascertain the chromosome numbers of members of the genus *Narcissus* not hitherto investigated. Indication of the parentage of certain garden forms is apparent from their chromosome behaviour. Meiosis and mitosis have been followed in a number of types. A study of pollen grain germination has been made and a record compiled both of the per cent. germination and of the duration of time between the commencement of growth and the various stages in nuclear division. The dates on which meiosis and mitosis take place in the pollen have been ascertained in all the types investigated.

Mrs. E. R. Sansome and Dr. F. W. Sansome.—*Genetical experiments with garden peas* (12.0).

Since Mendel's work on peas forty new genes have been discovered. Nevertheless the arrangement of these genes into the expected linkage groups has not been satisfactorily accomplished. One important reason is that different races of peas have different arrangements of chromosome segments. When these races are crossed, structural hybridity and accompanying semi-sterility are found. The relationship between genes and translocations in peas is described.

Afternoon.

Exhibits (2.15–5.0).

Prof. F. T. Brooks, F.R.S., and Dr. Y. A. S. el Alaily.—Die-back and canker of roses caused by *Griphosphæria corticola.*

Dr. M. R. Brown.—Physiologic races of *Puccinia coronata-avenæ.*

Dr. A. Burges.—Sporotrichium infection by rats.

Dr. D. G. Catcheside.—Genetics of *Oenothera.*

Dr. V. J. Chapman.—Marine algæ.

Dr. C. G. C. Chesters.—A simple method of producing photomicrographs.

Dr. W. R. Ivimey Cook.—Species of Plasmodiophorales.

Dr. C. G. Dobbs and Mr. A. M. Acock.—Plants of Spitzbergen.

Dr. W. J. Dowson.—1. Some bacterial diseases of plants. 2. Cultures of bacterial plant pathogens arranged in group-genera.

Dr. R. Emerson.—Methods of reproduction in the Blastocladiales.

Dr. R. Florin.—Female fructifications in *Cordaites* and conifers of Palæozoic Age.

Dr. H. Godwin.—Bog peat.

Prof. W. T. Gordon.—*Tetrastichia bupatides.*

Dr. B. J. Grieve.—Investigations of stimulation phenomena in plants induced by parasitic infection.

Miss E. M. Halley.—Myxomycetes.

Miss D. Joan Heppell.—The cytology of *Narcissus.*

Herbarium, Cambridge.—1. Local floras. 2. Collections of special interest. 3. Plants from localities to be visited on the excursions.

Prof. B. J. Luyet.—Vitrification of colloids and of protoplasm.

Mr. G. Metcalfe.—Watermarked willow wood.

Miss E. Morgan.—Saprolegniaceæ.

Miss Helen M. Muggoch.—Dispersal of spermatocytes in *Mnium hornum.*

Prof. W. Neilson Jones and Mr. A. G. Morton.—Material illustrating some features of soil and growth on Wareham heaths.

Dr. M. C. Rayner and Dr. I. Levisohn.—Pure cultures of fungi associated with roots of *Pinus* ; Mycorrhizal relations in Sitka Spruce and *Chamæcyparis Lawsoniana.*

Dr. P. W. Richards.—Mosses with ‘ dwarf males.’

Miss E. R. Saunders.—A century’s challenge to orthodoxy.

Mr. J. K. Scott.—A modification of the katharometer.

Mr. G. O. Searle.—The Sandringham flax experiment.

Sir Albert Seward, F.R.S., Mr. W. N. Edwards and Dr. J. B. Simpson.—Interbasaltic plant beds of Mull.

Dr. J. B. Simpson.—Fossil pollen of dicotyledons of Jurassic age.

Mr. K. V. Srinath.—Nutrition of embryo sac in *Calceolaria*.

Sub-Department of Plant Physiology, Botany School, Cambridge.—Apparatus.

Dr. H. Hamshaw Thomas, F.R.S.—The works of Richard Bradley, the first professor of botany in Cambridge, 1724–1732.

Dr. H. Hamshaw Thomas, F.R.S., and Mr. H. Andrews.—*Pachyderphyllum* and *Sarcostrobus*, Jurassic plant remains of problematical affinities.

Miss P. E. Thomas.—Monoblepharidales.

Mr. T. G. Tutin.—(*a*) The flora of Lake Titicaca; (*b*) Species of *Hydrodictyon* from Peru.

Dr. E. F. Warburg.—British forms of *Sorbus*.

Mr. P. Warnock.—Parasitic fungi on *Rosa*.

Dr. A. S. Watt.—The morphology of the bracken.

Dr. D. H. Valentine. Forms of *Viola silvestris* and *Viola Riviniana* and of the hybrid between them.

Material illustrating virus diseases of plants exhibited throughout the meeting by:—

Dr. R. N. Salaman, F.R.S., in the Potato Virus Research Station.

Dr. Kenneth M. Smith, in the Potato Virus Research Station.

Mr. N. W. Pirie and Mr. F. C. Bawden, in the Sir William Dunn Institute, School of Biochemistry.

Wednesday, August 24.

Dr. E. F. Warburg.—*The origin and distribution of the British forms of Sorbus* (10.0).

Three diploid species ($n = 17$) occur in Britain, *S. Aucuparia* widespread over the whole, *S. torminalis* in England only where it is widespread but local, and *S. Aria* ranging from Kent to the Wye Valley and the Mendips. Hybrids of *S. Aria* with each of the other species occur. The remaining British forms are triploid or tetraploid. They may be divided into three groups as follows: A—species allied to *S. Aria*, B—species intermediate between A and *S. Aucuparia*, C—species intermediate between A and *S. torminalis*. These species are mostly confined to small areas on the limestone in the West and North, where they entirely replace *S. Aria*, with which they grow only in the Wye Valley and in Somerset. They are all apparently endemic except for *S. rupicola*, the most widespread in Britain which also occurs in Scandinavia, and possibly for *S. porrigens*. Some of these forms are almost certainly apomictic. Groups B and C are presumed to have originated by hybridisation between Group A and *S. Aucuparia* and *S. torminalis* respectively. Group A is represented by about half a dozen forms of which some may be pre- or inter-glacial derivatives of *S. Aria* or allied species, the others being perhaps derived from them by hybridisation.

Dr. C. G. Dobbs.—*The vegetation of Cape Napier, Spitsbergen* (10.30).

Cape Napier is a shingle promontory situated in latitude 78° 39′ N., on an innermost branch of Ice Fjord, which penetrates the west coast of West Spitsbergen to a depth of over fifty miles. Behind the main shingle beach lies a small salt marsh and a larger area of pools and bogs, crossed by shingle laterals. The area was described by J. Walton in 1921.

The object of this investigation has been to record the vegetation in greater detail, and to establish any changes which have occurred during the intervening fifteen years. For this purpose a vegetation map was made, and the frequencies of plants along certain lines were expressed by means of graphs based upon ring sampling.

Three main associations were found on the small area studied : on the shingle—*Dryas* fjaeldmark with lichens ; on the tidal mudflats—*Cyanophyceæ*, the grass *Puccinellia phryganodes*, the moss *Swartzia inclinata*, and *Carex subspathacea* ; in the bog—mosses, *Carex subspathacea* and *Salix polaris*. The succession from bare shingle to fjaeldmark, and bare mudflat to bog, was traced.

Changes since 1921 were slight, indicating a slow rate of development. *Puccinellia*, however, had spread some distance by means of broken shoots washed along the tidal channels. *Swartzia* and *Carex subspathacea* had spread much more slowly on the mudflats, and a pool in the boggy area had become filled with vegetation.

Mr. A. M. Acock.—*Observations on vegetation and associated soil phenomena in Spitsbergen* (10.50).

The severity of environmental conditions in Arctic regions is such that any mitigating influence has a profound effect on vegetation. Small variations in soil characters and micro-relief are reflected in changes in floristic composition and density.

Apparently associated with the frozen sub-soil and seasonal thawing and freezing of the surface is the peculiar organisation of the soil into the structures known as ‘ polygons.’ A variety of types was observed near Bruce City, Spitsbergen :

(i) Fissure polygons in silt or clay.
(ii) Stone circles with clay centres.
(iii) Polygons in stony material.
(iv) Elongated forms on slopes.

A number of mechanisms of formation has been suggested by various workers. The botanical interest centres in the rearrangement of the mechanical contituents of the soil and in local differences in exposure, duration of snow lie, and moisture, as a result of doming of the polygon centres. There is generally a concentration of vegetation on the margins of polygons, either rooted between stones or in humus - filled fissures between polygons.

Mr. K. V. Srinath.—*Nutrition of the embryo sac in Calceolaria* (11.30).

The disintegration of the scanty nucellus in the ovules of the Sympetalæ is now well known. As a result the innermost layer of the massive single integument comes to lie in immediate contact with the outer wall of the embryo sac. In the genera *Calceolaria* and *Herpestris* of the Scrophulariaceæ, this layer of the integument becomes very prominent and the cells become elongated and palisade-like. The process of differentiation starts early, even when the megaspore mother cell is in the meiotic prophase. At this stage the nucellus is a single layer of cells forming a loose envelope around the embryo sac. As development proceeds the nucellus completely breaks down, its cell outlines are lost and it finally disappears. This breakdown of the nucellus is nearly coincident with the formation of the eight nucleate sac.

During the changes in the megaspore, the integumentary cells become very conspicuous with prominent nuclei and densely staining cytoplasm. After fertilisation these cells become less conspicuous. Their behaviour strongly suggests that they function as a nutritive ' tapetum ' investing the embryo sac and perhaps operative during the changes in the megaspore leading up to the formation of the mature embryo sac.

DEPARTMENT OF FORESTRY (K*).

Thursday, August 18.

Symposium on *The cultivation of British hardwoods* (11.15).

Mr. D. W. Young.—*The position of hardwoods in British forestry.*

Mr. A. P. Long.—*The Oak* (11.45).

Mr. A. L. Felton.—*The Beech* (12.15).

Afternoon.

Sir Roy L. Robinson, O.B.E.—*The home-grown timber supply* (2.30).

Symposium on *British hardwoods, continued* (3.0).

Mr. J. Macdonald.—*Ash and Sycamore.*

Mr. A. H. Popert.—*Alder and Birch* (3.20).

Mr. W. H. Guillebaud.—*Progress of experimental work on hardwoods* (3.40).

(*Continued on Friday*)

Friday, August 19.

Symposium on *The cultivation of British hardwoods* (*continued from Thursday*) (10.0).

Mr. W. A. Robertson.—*Some remarks on the utilisation of home-grown hardwoods.*

The paper lays stress on the relation of silvicultural treatment of the crop to the money value of the final yield, and indicates how the defects in home-grown hardwoods are largely due to silvicultural mistakes or neglect. Reference is made to particular defects in oak, beech, ash, birch, etc. Attention is drawn to the importance of plywood as an outlet for home grown timber and the need for careful silviculture. Charcoal as a means of disposing of cordwood is touched on.

Discussion (10.20).

Afternoon.

Excursion to Cambridge Experimental Area and Conington Hall.

Saturday, August 20.

Excursion to Mr. W. J. Burton's Chip Basket Factory at Wisbech; Ryston Estate; and Forestry Commission's Nursery, Mundford.

Sunday, August 21.

Excursion to Forestry Commissions Plantations in Thetford Forest.

Monday, August 22.

Joint Discussion with Section K (Botany) on *The ecological aspects of afforestation* (10.0).

Dr. A. S. Watt.—*The significance of Breckland in British forestry.*

The climate, soil and vegetation are considered in formulation of general principles governing the practice of forestry in Breckland.

Dr. H. M. Steven.—*The ecological aspects of afforestation in hill country* (10.45).

Mr. R. Ross.—*The colonisation of abandoned agricultural land in south-west Cambridgeshire* (11.15).

The Chalky Boulder Clay of south-west Cambridgeshire gives rise to very heavy clay soils, the natural vegetation of which is ash-oak woodland. On this formation considerable areas of agricultural land have gone out of cultivation during the last hundred years. These have become covered by a scrub dominated by *Cratægus* in the early stages of which a few seedlings of *Quercus* and *Fraxinus* develop under the bushes and survive. Animal, probably rabbit, attack severely limits the numbers. *Ulmus minor* becomes established freely by suckers at this time close to hedges containing the species. At a later stage, when the scrub becomes denser, further establishment of trees becomes impossible and almost all the ground vegetation is killed. The *Cratægus* is not regenerating, apparently owing to caterpillar attack of the seedlings, and will therefore die out in time. By then the soil will have been considerably altered, having acquired much humus and an open crumb structure. It appears likely that trees, particularly *Ulmus minor*, will then be able to establish themselves, and it is suggested that certain copses in the area, consisting almost entirely of *Ulmus* and lacking the characteristic woodland undergrowth of the neighbourhood, are a further stage in the succession.

DISCUSSION (11.30).

AFTERNOON.

Prof. W. NEILSON JONES and Mr. A. G. MORTON.—*Some features of soil and growth on the Wareham heaths* (2.30).

Mr. C. H. THOMPSON.—*The present position of forestry at Cambridge University* (3.15).

Mr. G. METCALFE.—*The morbid histology of watermarked willows* (3.40).

From stained wood four bacterial species can be isolated—*Bacterium salicis*, a white organism resembling *B. ærogenes*, a yellow organism and a member of the *fluorescens* group. These form a mixed population in the vessels. In recently diseased wood (brown stain) *B. salicis* predominates, in later stages (black stain) the secondary organisms predominate. Bacteria form occluding masses in the vessels ; tyloses appear and form a pseudo-parenchymatous tissue. Small diagonal cracks, joining infected elements, are formed by the solution of the pectic middle lamellæ by the fluorescent organism. Tannins are abundant in the degenerating ray cells ; the brown stain is due to the oxidation of their breakdown products. The bacterial oxidase systems may bring this about as the brown stain is correlated with the presence of occluded vessels. The black stain sometimes present is due to the formation of melanin in the ray cells. Bacteria colonise ray cells after degeneration of protoplasm ; the bacteria spread radially in this way. Longitudinal spread is along vessel lumina. The bacteria become actively motile following spring hydrolysis of starch and spread rapidly about the tree. The bacteria are non-motile during the summer and autumn. There is a slow radial and longitudinal spread during winter, resulting in the formation of thin layers of bacteria around the walls of vessels in the current year's wood.

Tuesday, August 23.

Excursion to Weasenham Wood.

SECTION L.—EDUCATIONAL SCIENCE.

Thursday, August 18.

Sir RICHARD GREGORY, Bart., F.R.S., and Mr. H. G. WELLS.—*Report of Committee on the content of school curricula* (10.0).

DISCUSSION on *Tendencies in the design of schools* (10.30).

Mr. S. E. URWIN.

The School.—A place of learning and training of culture and appreciation. *The Building.*—Temporary or permanent—Construction—should be of good taste in design—attractive—carefully considered planning—located in pleasant surroundings—no longer the institution of old.

Accommodation.—To provide for the child from the cradle to old age—a community and health centre—aspect—rooms for teaching and practical instruction and special subjects—for physical fitness and entertainment—maximum light and air.

Internal Finishings.—Colour and furniture.

Mr. W. G. Newton (11.0).

Educational theory reflected in plans: theories fluctuate, buildings remain. To-day an emphasis on decentralisation, light and air, sunshine, gardens, practical work, play: both here and abroad. Also school as a cultural centre for neighbourhood. Consequent plan-types. Research. The *News-Chronicle* competition. Concentration and dispersion. Expansion vertical and horizontal. Façades: encampments. Theory fluctuates: materials, obsolescence.

Mr. Denis Clarke-Hall (11.30).

Theoretical approach to the designing of a school building rather than a description of existing practice.

Analysis of Problem in designing a Modern Building.

A building must include to a varying degree of importance according to its type:

1. *Function* (fulfil in a satisfactory manner the purpose for which it is built).
2. *Human Association* (the physical and mental contact of the individual with scale surface texture and detail).
3. *Æsthetic Value* (the refinement of 1 and 2 by the science of proportions, perspective and logic, giving intellectual satisfaction over emotional).

Buildings whose functions are of major importance and dictate the whole conception and bear no human association:

1. Large industrial work. 2. Hospitals. 3. Engineering work.

Buildings whose human associations are of major importance:

1. Domestic work. 2. Small hotels, etc.

Buildings in which it is essential to have incorporated to an approximate equal degree both Function and Human Association:

1. Schools. 2. Flats. 3. Larger hotels and any other buildings where large numbers of people gather and have direct contact with the building itself.

On the æsthetic value of a building depends its architectural importance.

Architectural criticism can be based on a muddled conception of these points:

1. Function distorted by the forced application of Human Association and Sentiment.
2. Human Association destroyed by an excessive degree of function.
3. Aesthetic value destroyed by misuse of 1 and 2.

Approach to the problem of designing schools based on these points:

1. *General.*
   (*a*) What is the school for. (*b*) The best method of achieving this purpose. (*c*) Division into specialised sections.

2. *Function.*

(*a*) Weatherproof. (*b*) Purpose of units. (*c*) Size of units. (*d*) Ventilation and air-space. (*e*) Heating. (*f*) Lighting. (*g*) Circulation. (*h*) Detail (furniture, etc.).

3. *Human Association.*

(*a*) Reaction of the mind to environment. (*b*) Scale. (*c*) Surface texture of materials. (*d*) Detail. (*e*) Colour. (*f*) General atmosphere.

4. *Æsthetic Value.*

(*a*) General massing of proportion. (*b*) Perspective. (*c*) Proportion of detail. (*d*) Relationship of 2 and 3. (*e*) Final adjustments.

In no way must æsthetic value distort function, but must be a direct expression of this and also of Human Association. All must grow naturally from one to the other.

Description of a school designed in this way.

Mr. W. D. Seymour (11.55).

The change in attitude towards problems of heating and ventilation, which dates from the beginning of the present century, has resulted in a great deal of research in applying the new principles.

During the past ten years many experiments have been carried out on the heating and ventilation of schools, first by the Industrial Health Research Board, and more recently by the National Institute of Industrial Psychology. In studies undertaken by investigators of the latter body, an attempt has been made to determine the most suitable conditions for school children at work and, in a school equipped specially for the purpose, experiments have been made to compare the suitability of different types of heating equipment.

The paper deals with these experiments, and with corresponding ones on the natural and artificial lighting of schools. In addition, some account is given of subsidiary experiments on the design of equipment.

Friday, August 19.

Presidential Address by Mr. John Sargent on *The proper function of administration in public education* (10.0). (See p. 235.)

Discussion on *Education for a changing society* (11.0).

(*a*) Senior Schools.

Mr. W. H. Robinson (11.0).

The Senior School is the school of the people—the football crowd, the cheap cinema audience, the factory ' hands,' the shop assistants, the trades-unionists, the vast majority of the voters ; in short, ' democracy.' The majority of the Senior School pupils will receive no further organised education as long as they live but will be ' educated ' by their environment —physical and mental.

There is no lack of guidance for the teacher as to *what* they should be taught, *how* they should be taught and what should be expected as the result of this schooling.

The growing tendency is to insist that the educational environment in which these pupils grow and the experiences through which the daily school routine will lead them, are more important even than the knowledge which they gain, and educational technique is being devised to fit in with this tendency. In the revolt from attempts at training the powers of abstract thinking in bookish ways divorced from the actual practice of the art of living in a modern community, there has developed an over-emphasis on 'practical' work which is often not a 'realistic' and 'practical' approach to education, but is merely a substitute for education.

Only as the pupil understands what it is all about and is led consciously (not merely indirectly) to co-operate in a process of real education for life, will the system succeed. And this cannot be left to chance, or merely be *expected* to develop in the right atmosphere, but must become the dominant note in the new technique of the Senior School, especially in the additional year which is soon to be added to that life.

Miss Ruth Dawson (11.20).

I. *The Modern Child and his Environment.*

(*a*) The standard of living is higher.
(*b*) There is less home life.
(*c*) The influence of wireless and the cinema is considerable.
(*d*) There are greater facilities for travel.
(*e*) The age is one of speed and noise.
(*f*) The age is one of mass production.
(*g*) International relationships are strained.

II. *The Aims of the Senior Schools*—as stated in the Hadow Report.

(*a*) To form and strengthen character—individual and national.
(*b*) To train tastes which will fill and dignify leisure.
(*c*) To awaken and guide intelligence—especially on its practical side.

III. *The Senior School.*

(*a*) Its constitution.
(*b*) Its atmosphere.
(*c*) Its curriculum.
(*d*) The value—or otherwise—of specialisation.

(*b*) Secondary Schools.

Dr. P. T. Freeman (11.40).

Has society changed in fundamentals to an extent which would justify abandonment of 'training' subjects ? Need to cultivate willingness to make mental effort, and respect for knowledge and truth. Some changes in society call for changes in content and method in such subjects as Divinity, Biology (including matters concerning sex, heredity, etc.), Civics, Physical Training. Education in the use of leisure. Longer school life.

Miss Muriel Davies (12.0).

Our educational system and political bias. Need for unification in ranks of teachers and taught. Education for social progress rather than for individual advancement. The development of self-discipline through freedom. Co-operation to replace competition : activity and initiative of pupils further to be encouraged. Group system. Manual work. Distinctions between home work and school work should be abolished. School as club for past and present pupils.

(*Discussion continued on Monday*)

Monday, August 22.

DISCUSSION on *Education for a changing society.* (*Continued from Friday*) (10.0.)

(*a*) Technical.

Mr. J. PALEY YORKE (10.25).

Dr. W. A. RICHARDSON (10.50).

The modern world has awakened to its responsibility to youth, and unfortunately many countries exploit it. There is increased thought and expenditure for the recreation, physical culture and education, both general and vocational, of young people. In work of this kind, technical colleges have been pioneers and take still an active part not only in making actual provision, but in supplying ideas and inspiration. There are two ideals, not necessarily antagonistic, which functioned in the founding of technical institutions. One is exemplified fully in the movement which led to the growth of the London Polytechnics, and is essentially social. They started as clubs for young people, and their educational provision was in a measure supplementary to their main social purpose. The other conception, namely, vocational or further education, inspired the development of provincial colleges, which in many instances arose out of the Mechanics Institutes. Colleges to-day do not normally receive students until the age of sixteen, and since their accommodation is limited, have to decide upon their policy. The social ideal means admitting all youths to their activities, whether they engage in education or not. On the whole the idea of the local college is gaining ground. Such a college is a community of *students*, whose main purpose in attending is educational, but for whom the authorities must provide facilities for social and physical development in the fullest sense. For education to be effective it must be continued until the age of mental maturity—at least until eighteen. The full benefit of educational effort and expenditure will only be secured if continued education is compulsory, at least until this age. And in this continuation technical colleges have a great function in providing young people with wide educational opportunities for all phases of life, although such institutions have, and must ever have, a vocational and industrial bias.

Mr. F. PICK (11.20).

Education has been conservative. It has not given to the newer subjects bred of an industrial civilisation the breadth and quality which was given to the older subjects bred in the classic ages.

A liberal education usually means education in and through classic literature. There is a liberal education to be got through the discipline of tool and material, differing in kind but the same in substance. There should be a liberal education to be secured from trade and industry conceived as elements in an ideal life.

Art and technology have been largely divorced and the liberal education of the master craftsman destroyed. This must be restored. The sickness of industrial production may be attributed to this lack. A fresh conception of a school embracing both art and technology is needed.

The practical and pragmatical classes in subjects related to trade and industry must be infused with a philosophic content. The trained and broadened mind must be applied to the building up out of the mass of knowledge which they embody, human sciences to rank alongside ethics, politics, economics, sociology and so forth. In fact these human sciences need reconstituting to take cognisance of the revolutions in affairs that have occurred since the opening of the nineteenth century.

The English common law suggests an approach and a method for this purpose. It has survived change and is still growing.

The enormous scale of modern business can only be supported if an education can be devised which will give quickly the understanding requisite for its conduct.

Modern business absorbs at least two-thirds of the energy and time of the people, it must therefore be developed to constitute a satisfying life. For this purpose it must have a creative interpretation. There can be little hope for current civilisation until its major occupation is liberalised. The task confronts education.

(*b*) University.

Prof. Winifred Cullis, C.B.E. (11.50).

Much attention is being given to-day to the study of ways in which two aspects of University education, the training for livelihood and the training for living, can be combined. It is generally agreed that specifically vocational training is given very efficiently. The discussion is mainly concerned, therefore, with ways of helping the Universities to provide leaders in conduct of social, commercial and political administration.

Afternoon.

Excursion to Bottisham and Linton Village Colleges.

Tuesday, August 23.

Discussion on the Presidential Address by Mr. John Sargent (10.0).

Joint Discussion with Section J (Psychology) on *The educational significance of the cinema and wireless* (11.0).

Mr. R. C. Steele.

Some of the chief problems discussed are :—The educational value of broadcasts—Broadcasts as compared with other mechanical aids to education—Learning to listen — Broadcasts and further education—Listening powers of different types of children—Such problems as 'How much can children visualise from mere words ?'—The effect of personality in a broadcast—The contrasted effects of novelty and familiarity of subject matter—Education by means of entertainment—Broadcast as compared with classroom technique.

Dr. P. B. Ballard.

The educational film as part of the school programme is still in the early experimental stage. There is, however, clear indication that it should be brief, should be repeated, and should be full of action and human interest. The ordinary cinema often has a bad influence on children. The broadcast lesson, having had a run of ten years, is more developed and more firmly established. Neither the film nor the radio talk is a complete educational unit; it has to be supplemented by other teaching devices.

Dr. S. J. F. Philpott.

Film teaching is sometimes said to reduce gap between dull and bright children, the gain to the dull being seen in increased vividness or liveliness in descriptions (verbal or written). The argument is probably fallacious on the statistical side, being due to reasoning from a regression instead of from corresponding correlation. There are also difficulties on the qualitative side. Vividness can be measured in terms of the percentage of particular (as distinct from general) statements in the essay or verbal description. It can be shown experimentally that (*a*) the more 'particular' the child, the more he tends to put in descriptions of films relative to what he does in more ordinary circumstances, and that (*b*) the bright child is naturally less 'particular' than the dull. There is consequent danger of confusion in the results, and figures quoted show that no decision can yet be made on the data.

Miss L. M. Holt.

An account is given of the comparison of a series of lessons in which the wireless was used with another series in which it was not, for the purpose of obtaining some indication as to the specific contributions and functions of this new teaching aid as distinct from others available.

The top class of an elementary school was divided into two approximately equal groups by means of the Simplex Group Test. Each group had both normal and wireless lessons in cyclic order over a period of eight weeks. The same main facts on Empire Geography were taught in each corresponding pair of lessons. After each lesson the children both wrote free essays and answered a Questionnaire. The results of these were evaluated, but it was necessary to use a method of evaluation that was in no way dependent upon individual judgment. The essays were marked for facts which were classified under various categories. This is discussed, together with the distinctive differences apparent when material is presented visually (as in the cinema), orally (as on the wireless) and when it is presented by the teacher without either of these two aids.

At the end of the series it was felt desirable to obtain some indication as to the children's attitude towards these periods. A scaled series of statements was presented to them and they indicated their acceptance or rejection of each of the items and their position has been assessed.

SECTION M.—AGRICULTURE.

Thursday, August 18.

Discussion on *Agriculture in relation to national employment* (10.0).

Mr. C. S. Orwin.—*The demands for labour in agriculture.*

This paper treats the subject historically, covering approximately the last hundred years. It shows how the demand for labour for the land, as reflected in wages, has always been exceeded by the supply, right up to the present day. There have been times when temporarily there was a shortage of workers in agriculture, but they have not lasted long. The present time is one of them, and the question is whether the more attractive alternatives offered by the labour market, combined with the mitigation of the consequences of unemployment provided by the social legislation of the times, are going to alter the experience of a century and drive farmers to find ways by which to meet a more permanent withdrawal of labour.

These might take the forms of (1) an acceptance of the situation and a further extensification of farm practice; (2) an attempt to carry on with a reduced staff and a further resort to labour-saving machinery, or (3) an intensification of farming, combined with high wages, in an attempt to reproduce industrial conditions.

Prof. J. A. Scott Watson.—*Systems of farming* (10.30).

Mr. S. J. Wright.—*Men and machines (including transport on the farm)* (11.0).

Mechanisation is neither a serious menace to our rural amenities nor a royal road to prosperity. It has two phases which, in principle, are quite independent of one another: the elimination of hand labour by such machines as the reaper-binder or the combine harvester; and the replacement of animal by mechanical power. The first of these phases came into being once and for all during the last century, when the forerunners of all modern machines were invented, and a discussion of its desirability to-day can have only an academic interest. The second phase—the introduction of mechanical power—can be discussed from either of two points of view: as a factor which makes it possible to reconcile two general features of modern life, cheaper food and higher wages; or quite independently of either of these features as a common-sense matter of using the most efficient source of power. Whatever the levels of prices and wages, the farmer who grows food for his own horses could, in theory at any rate, use the same products as fuel for a mechanical heat-engine and get something like three times as much energy from it. Most of the changes for which mechanisation has been blamed are due to purely economic causes, and in the long run, agriculture can absorb mechanisation without prejudice to its own interests. Moreover, under present conditions, only the machine can give the agricultural worker the leisure and amenities which he is entitled to demand.

Dr. F. Kidd.—*Preservation, storage and transport of farm produce* (11.30).

General Discussion opened by Prof. R. G. White (12.0).

Afternoon.

Visit to University Farm Plant Breeding Institute, Potato Virus Research Station.

Friday, August 19.

Presidential Address by Prof. R. G. Stapledon, C.B.E., on *Ley farming and a long term agricultural policy* (10.0). (See p. 245.)

Dr. W. G. Ogg.—*Problems of marginal and waste land* (11.0).

A great deal of land which was at one time under cultivation has been allowed to revert to semi-waste, and every agricultural depression sees additional areas of marginal land go into this condition. In order to check the shrinkage of our agricultural land it is desirable that we should examine the possibilities of re-conditioning the marginal farms and of reclaiming land which has gone out of cultivation.

In many cases the deterioration is chiefly due to lack of lime and of manures, particularly phosphates, but re-conditioning would involve cultivation and usually some draining and renovation of buildings and fencing.

Special equipment and workers with some experience are necessary if this work is to be done in the most economical manner. But even if the equipment could be hired many of the owners of this class of land are unable or unwilling to invest the necessary capital in re-conditioning it. In some ways the problem of our marginal land resembles that of afforestation, but if the State were to purchase the land and undertake the re-conditioning the farms could then be rented, with suitable safeguards, to private individuals. Unless something of this kind is done, or unless special facilities and inducements are offered to private owners, it seems likely that still more of our agricultural land will revert to its original condition.

Experiments on the re-conditioning of land have been carried out in recent years at various centres and experiments on the reclamation of moorland are in progress in Lewis and in Lanarkshire.

Dr. E. M. Crowther.—*The maintenance of soil fertility* (11.30).

The old view that the maintenance of fertility in arable land requires merely the return of the plant foods removed in the crops is now known to be totally inadequate. Among other things it neglects the losses of calcium and nitrate in drainage, and of nitrogen as gas, the conversion of added phosphates and potassium into unavailable forms, and the steady loss of soil organic matter. It fails to direct attention to the deterioration of physical properties under continued cultivation through the destruction of root channels and soil crumbs. Under extreme conditions of torrential rain or prolonged drought, this may lead to soil erosion on a devastating scale. The most effective measure of soil conservation is an active cover of vegetation, the living and decaying roots of which granulate the soil and make it permeable to air and water. Most stable systems of arable cultivation have involved either an alternation with grass, scrub, or forest or the production of large quantities of bulky rotted manure.

The possible chemical, physical and biological effects of farmyard manure are so manifold that their precise analysis has rarely been attempted. The traditional methods often prove unduly expensive under modern conditions and attempts to dispense with them provide severe tests of technical skill. On the other hand uncritical advocacy of old ways may merely retard progress. It is known that lime, the major plant foods, and some of the minor plant foods can be supplied far more economically in inorganic forms, where the principal soil deficiencies are known. Modern methods of soil survey, field experiment and soil analysis can do much to reveal the requirements and potentialities of individual soils, but far more active co-operation between official bodies, farmers and technical workers is needed to develop these methods and extend their use. There is an especial need for field trials over a term of years on the residual effects on soil fertility of different systems of land management.

General Discussion opened by Sir Daniel Hall, K.C.B., F.R.S. (12.0).

AFTERNOON.

Visit to Department of Animal Pathology, National Institute of Agricultural Botany and Animal Research Station.

Saturday, August 20.

Excursion to Cressing Temple, Braintree; Lord Rayleigh's Farms, Hatfield Peveril; Henry Ford Institute of Agricultural Engineering; Little Hallingbury Park, Bishop's Stortford.

Monday, August 22.

DISCUSSION on *The practical problems of crop production* (10.0).

Mr. J. A. McMILLAN.—*Crop husbandry.*

At heart the farmer is a scientist. Many of the broad principles in crop husbandry have been established through the cumulative experience of successive generations of tillers of the soil. The scientist has discovered the reasons underlying many of these principles, often after long and patient investigation, and in doing so has demonstrated improved methods of crop production. It is only comparatively recently that he has turned his attention to problems that appear beyond the solution of the farmer or that have arisen with changing economic conditions.

More recent tendencies are towards a system of sound rotational farming. The value of animal manure has been rediscovered and the place and balance of artificial fertilisers in the rotation, rather than for the individual crop, are the subjects of new investigations. The older accepted methods of cultivation are being called into question; the much wider use of mechanical power holds out new possibilities.

Many important problems await solution, for example, in the fields of drainage, cultivation and weed control. It has become increasingly evident of late, however, that the translation into practice of the many contributions of science in the above and other directions is dependent largely on the economic condition of the farming industry.

Prof. F. L. ENGLEDOW.—*The place of plant physiology and of plant breeding in the advancement of British agriculture* (10.30).

In growing any crop the two main biological considerations are which variety to use and how to grow it. These respectively link agriculture with plant breeding and with physiology. Of the controllable factors in crop growing, crop rotations and soil cultivations are the chief. Our knowledge of these is traditional and no longer adequate to modern circumstances. The secret of the whole matter is to discover not merely the extent to which treatment affects yield, but how in terms of plant growth treatment influences yield. Thus the great task of physiology in agriculture is to equip the experimenter in husbandry with tests or indices of plant growth in relation to yield.

The objective of breeding (in Britain) is to produce new varieties which bring the farmer a greater net cash return than existing ones. Breeding is still pre-eminently an art and the grand problem of applied genetics is to elevate this art into a systematic science. This involves the interpretation of the economic characteristics of plants in genic terms and the solution of such problems as hybrid vigour ; and as self-sterility and cross-compatibility, with their implications in productivity.

Farmers have an important part to play in a greater discrimination of varietal merit and a firmer insistence upon proofs of it. General policy in plant breeding must inevitably be of very long range and its nature and soundness are completely dominated by the national policy adopted by the country for its agriculture as a whole.

Mr. C. T. Gimingham.—*Crop pests and diseases* (11.0).

The fact that insect pests and fungus diseases are important factors in crop production is generally accepted, but the continuous toll taken by them, and the extent of the losses sustained, are perhaps less commonly realised. In recent years, practical and economic means of dealing with several important pests and diseases, previously very difficult to control, have been found, but, on the other hand, there are many unsolved or only partially solved problems requiring investigation. Some examples are given. Control measures need to be considered strictly in relation to the value of the crop, and the problems are thus often most difficult where the less valuable agricultural crops are concerned. Some of the factors affecting the natural limitation and artificial control of plant pests and diseases are shortly discussed, particularly in relation to cultural conditions.

General Discussion opened by Sir John Russell, F.R.S. (11.30).

Afternoon.

Visit to farms of Messrs. Chivers & Sons, Ltd., Histon.

Tuesday, August 23.

Discussion on *The practical problems of animal production* (10.0).

Prof. R. Rae.—*Animal husbandry.*

Prof. F. A. E. Crew.—*Animal breeding* (10.30).

Dr. E. L. Taylor.—*Parasitic diseases of animals* (11.0).

Parasitic worms are responsible for the most economically important diseases of grazing animals throughout the world. There is little doubt, however, that in the original primitive state they were relatively harmless, forming part of a well-balanced ecological unit in which the parasites and parasitised animals lived harmoniously together. To a very considerable extent the diseases which parasitic worms now cause are man made, having been brought about by the enclosure of grazing land and the improvement of pasture. The particular types of parasitic worm which are of the greatest concern are ubiquitous in their distribution, a light infestation being regarded as normal and causing no disturbance of health. A heavy infestation is, however, harmful, and as the number of parasites tends to increase with the square of the concentration of grazing animals the effect of the unnatural crowding of the animals increases progressively as pasture land is improved and severe losses from worm disease frequently occur.

The successful development of the individual worms depends, however, upon the suitability of a variety of conditions concerning the environment of their parasitic life in the grazing animal and that of their free life in the pastures. Their control will continue to be an urgent problem in animal nutrition until we understand the detail of their development so well that we are able to make suitable readjustments in the environments that will compensate for the departure from the original scattered condition of grazing which our grassland improvement has brought about.

General Discussion opened by Dr. J. Hammond, F.R.S. (11.30)

CONFERENCE OF DELEGATES OF CORRESPONDING SOCIETIES

The Conference was held in the Department of Mineralogy on Friday, August 19, and Monday, August 22, 1938. Sixty-eight Corresponding Societies were represented, the Rt. Hon. the Earl of Onslow presiding.

Friday, August 19.

Dr. Tierney, Secretary of the Conference, reported that since the last meeting the Midland Naturalists' Union had been inaugurated, with headquarters at Birmingham, ' to further the advancement of natural history in the Midland Counties by every practicable means and for the purpose of affording facilities for co-operation and co-ordination between local natural history societies, field clubs and individual workers in the counties of Monmouth, Hereford, Worcester, Warwick, Leicester, Northampton, Rutland, Nottingham and Lincoln.' The Hon. Secretary of the Union is G. Brian Hindle, B.Sc., Avebury House, 55 Newhall Street, Birmingham, 3, from whom full information can be obtained.

The delegates nominated the following to fill vacancies on the Corresponding Societies' Committee for the ensuing year: The Earl of Onslow, Mr. N. B. Kinnear.

THE IMPORTANCE OF NATIONAL PARKS IN THE PRESERVATION OF THE FAUNA OF GREAT BRITAIN

ADDRESS BY

The Rt. Hon. the Earl of Onslow, G.B.E., P.C.,

President of the Conference.

I have been asked to say a word or two on the subject of the use of National Parks in Great Britain for the preservation of our fauna—or what is left of it.

In the first place I think it is necessary to distinguish carefully between what is meant by a National Park in this country and in other countries. I believe that the term ' National Park ' occurs in only one public instrument and that is in the documents connected with the Convention for the Protection of the Fauna and Flora of Africa which was concluded in London in 1934. So perhaps for the sake of clarity I may be allowed to quote Article 2 of the Convention which defines what is meant by a National Park considered internationally, if I may so describe it.

Article 2.

1. The expression ' national park ' shall denote an area (*a*) placed under public control, the boundaries of which shall not be altered or any portion be capable of alienation except by the competent legislative

authority, (*b*) set aside for the propagation, protection and preservation of wild animal life and wild vegetation, and for the preservation of objects of æsthetic, geological, prehistoric, historical, archæological, or other scientific interest for the benefit, advantage, and enjoyment of the general public, (*c*) in which the hunting, killing or capturing of fauna and the destruction or collection of flora is prohibited except by or under the direction or control of the park authorities.

In accordance with the above provisions facilities shall, so far as possible, be given to the general public for observing the fauna and flora in national parks.

2. The term ' strict natural reserve ' shall denote an area placed under public control, throughout which any form of hunting or fishing, any undertakings connected with forestry, agriculture, or mining, any excavations or prospecting, drilling, levelling of the ground, or construction, any work involving the alteration of the configuration of the soil or the character of the vegetation, any act likely to harm or disturb the fauna or flora, and the introduction of any species of fauna and flora, whether indigenous or imported, wild or domesticated, shall be strictly forbidden ; which it shall be forbidden to enter, traverse, or camp in without a special written permit from the competent authorities ; and in which scientific investigations may only be undertaken by permission of those authorities.

3. The expression ' animal ' or ' species ' shall denote all vertebrates and invertebrates (including non-edible fish, but not including edible fish except in a national park or strict natural reserve), their nests, eggs, egg-shells, skins, and plumage.

You will see that not only do the African Powers contemplate the creation of National Parks but also of another type of reserve which they denominate a strict natural reserve. This was put in at the instance of the French Government, who were anxious to provide for the creation of areas for the preservation of fauna and flora to which the public should not have access except under very definite restrictions, that is to say, they were to be created for purely scientific purposes, while the National Parks are to afford as much access to the general public as is possible compatible with their reasons for existence. Now in England we are apt to be rather more loose in our terminology and National Parks cover a very wide field—in fact, they cover any natural reserve or open space to which the public have access regardless as to whether they are to be devoted to the species of fauna and flora or not, and to-day I propose to devote myself to the methods of utilising the National Parks of this country on the lines contemplated in the African Convention. I may say that there are a number of National Parks throughout the world devoted to fauna preservation. In Africa, for example, there are the Parc National Albert in the Belgian Congo and the Kruger National Park in South Africa. The success that has attended the creation of these parks might, I think, tempt us to try and do something of the kind in this country. We have not the rich fauna of South Africa or of the Congo, but we have a very interesting native fauna which, if it is not protected, must in time gradually disappear, and I feel that now that there is this strong movement in favour of creating national parks in this country we should take the opportunity of creating one at least which is mainly devoted to the preservation of fauna and flora.

It will be within the knowledge of this meeting that a Standing Committee on National Parks, of the Council for the Preservation of Rural England and Wales, under the Chairmanship of Mr. Norman Birkett, is in existence, and a preliminary Group is being formed of members of both Houses of

Parliament with the same object in view. The policy of this Committee is that the Government should :

(*a*) Declare that the establishment of National Parks is an essential national service.
(*b*) Set up, as chief and central agents, two National Parks Commissions (one for England and Wales and one for Scotland, with a joint committee co-ordinating the two).
(*c*) Provide funds.

But if, as seems to me to be the case, the only suitable place for a Park such as I am dealing with to-day is in Scotland the consideration of its establishment would be for a Scottish committee. The Association for the Preservation of Rural Scotland agrees with the proposal for the establishment of National Parks, but is making a separate study of the case for National Parks in Scotland and their special requirements. Scottish National Parks policy presents a different problem and calls for different treatment as compared with that required in England and Wales, so that it is to the Scottish organisations that we look for the realisation of any scheme for the establishment of a National Park devoted mainly to the preservation of our fauna.

I may say that the principle has been adopted by a number of authoritative societies. Recently a meeting took place of all those societies who were interested in the matter for the preservation of fauna and flora and for the preservation of open spaces, and general agreement was reached as to the desirability of action such as I have described. If therefore we agree that a National Park of this description should be created in Great Britain the first consideration is where to put it. I do not myself think that there is anywhere in England or Wales which would prove suitable. I do not think that it would be easy to get a sufficiently extensive area and I also fear that even if such were available the cost would be prohibitive. But the West Coast of Scotland seems to be an ideal spot for such a venture. In those counties there exist thousands of acres of deer forest, and I believe that deer forest land lends itself most readily to the creation of a National Park. In the first place a forest already contains a considerable number of the animals which it is sought to preserve and it may be hoped that others could be acclimatised there. Of course the ideal spot would be an island such as I remember many years ago in the Gulf of Hauraki in New Zealand, which was acquired and stocked by Sir George Grey. But an island would not be a convenient place for the public to visit and obviously it is desirable to have land as accessible to the public as possible even though there are other disadvantages. A National Park must be in forest country not contiguous to grouse ground, for a number of the animals which it is sought to preserve are detrimental to the preservation of grouse and would be very unwelcome neighbours to the owners of grouse moors. On the other hand, if the Park were surrounded by deer forests vermin would be welcome since nothing is so detrimental to stalking as the presence of grouse An old cock grouse getting up just in front of the stalker will scare the deer just as he is approaching his shot. Probably the most attractive animals in a park will be the deer. We have in this country three species of deer—red deer, roe deer, fallow deer. Whether the latter is actually indigenous or was imported at some remote period has not been decided, but if they are not really wild animals they exist all over the country in a feral state. It has been said that red deer are such mischievous animals that you would have to fence your park in order to keep them away from cultivation to keep them from doing damage to crops.

But I think this is hardly necessary. The deer would cross the march into the park and out again as they do now between forests. Moreover, it would not be necessary to keep anything like such a stock of deer in a National Park as proprietors of forests like to see on their ground. Naturally people who take a forest for stalking want to get as much sport as possible and like to see a good head of deer, but for the purpose of preservation this is not necessary nor indeed does it seem particularly desirable. All you want is sufficient deer to be visible to the public in their native haunts and it would be necessary to shoot them fairly hard in order to keep their numbers within limits and prevent them from becoming a nuisance. But they would be shot on rather different lines from those pursued by owners of forests. Naturally a stalker wants to get a big beast with a good head. The heavier the beast and the finer the head the better—but in a Park one would spare the big stags with the good heads and confine shooting to the poorer animals and older stags with going-back heads. The hinds would probably have to be shot a good deal harder than is done on forests at present and in the winter a considerable number of yeld hinds, old beasts and poor animals would want shooting, possibly also the calves would have to be kept down to a certain extent. Another reason for fairly hard shooting would be that otherwise the deer, finding themselves unmolested, might crowd into the Park from the neighbouring forest and not only would you get your ground overstocked but there would be complaints from your neighbours. The same remarks apply also of course to fallow deer who frequent rather lower ground than red deer. As regards the roe deer it would be necessary that there should be a certain amount of woodland on the ground and a careful eye kept upon the bucks. Old roe bucks are ferocious beasts and are liable to drive the other bucks away unless they are strictly controlled.

Other beasts which I think might well be added to the stock of a National Park are the so-called park cattle. These, as is well known, exist in a wild state at Lord Tankerville's place at Chillingham. I feel certain that a herd of these cattle allowed to exist in a wild state would prove a valuable addition to a National Park. I do not know whether they have been actually tried in Scotland, but I do not see why they should not do there since they resemble fairly closely the native Scottish cattle.

Then in Scotland we also have a few wild goats. These are probably the descendants of ordinary goats run wild and they are therefore feral animals and not true wild ones, but they are well known in Scotland and like the cattle might well be preserved in a National Park. Perhaps the most interesting animals would be the carnivorous animals. Foxes, badgers, stoats and weasels are common enough and exist in Scotland as well as in England and Wales. There would be no difficulty about them nor would there be about otters, provided, of course, that the Park had streams, rivers and burns to provide fish. But there are three species which are becoming very scarce indeed and deserve every effort being made to retain them. These are the wild cat, the pine marten and the pole cat. Wild cats only exist now in the North and West of Scotland and unfortunately they cross with the common tame cat, but they are beautiful beasts, very fierce and, it is said, quite untamable. We had some at Whipsnade, but unfortunately they died of cat distemper. They are not easily preserved in captivity. There are some in the forests in the North where they are preserved because they kill grouse and other hindrances to stalking and they do well and increase. Much the same may be said of the pole cat, which is also becoming rare. But with preservation there is no reason why these species should not also increase, if preserved. Perhaps the rarest of all our fauna is the pine marten, which as its name implies, does best in places where pine trees exist, so that the woodland country which is necessary for roe deer would be equally valuable for the pine marten. Unfortunately they are great travellers and might wander off the ground and disappear. But if they could be induced to remain in an area which is favourable to them they would be an interesting and valuable addition to the fauna of a National Park. Perhaps the most difficult beasts to acclimatise in Scotland would be the rarer bats, most of which have only been found in the South of England. Possibly they might do on the West coast, but I should imagine that their acclimatisation would present considerable difficulties. Rodents are common enough and in some places too common, so their preservation would present no difficulties. Plenty of rabbits would be necessary.

Turning from mammals to the birds one may say that if birds are unmolested generally speaking they will be present, at least those which are suitable to the district. All that can really be said is that the National Park should contain a strict bird sanctuary such as exists in so many parts of the country at present, and if possible part of the Park should be near the sea so that sea birds could be encouraged to breed there.

The question as to whether it would be desirable in a National Park to attempt to acclimatise animals which have become extinct in this country is one which will occur to everybody. Of course they would be few, for most of those which have died out and are extant in other countries are dangerous and, I think, undesirable, but perhaps would form an attraction to a National Park. The elk which is found in a good many parts of Europe died out here very many years ago in prehistoric times, and I doubt whether an attempt to re-introduce it would be worth while, since it does not seem to do very well in this country. But the reindeer, wild pig and beaver have become extinct in England only within recent times, comparatively speaking—that is to say, within the last few centuries, and there seems no reason at all why they should not be re-introduced. There is a record that reindeer were hunted by the Norse Jarls of the Orkneys in the twelfth (?) century. They seem to have been pushed out by the red deer.

Of course the question of feeding them is difficult and we might not be able to get the right food for them in Scotland and that would make them difficult to acclimatise. Wild pigs in Surrey existed up to the end of the seventeenth century, and they seem to have become extinct less owing to their having been killed than from their having been domesticated. Charles I introduced wild boars into the New Forest, and the half-wild pigs which used to roam in the forest until recently probably had this blood in them, as they are very hairy. Wild boars exist in Northern France. There is a story that the Commander-in-Chief went out one day and suddenly saw a wild pig and immediately snatched the lance which his escort was carrying and proceeded to ride him. I have seen them near Hesdin, near Crecy and several other places, so there would be no difficulty in introducing them into England and they would form a very interesting addition. Some people think they might be dangerous, but I do not think so. A pig will fight if he is attacked, but otherwise he is harmless.

As regards beavers, they were common in England until a few hundred years ago and still exist on the Rhone and the Rhine in small numbers. On the eastern rivers of Europe they are fairly plentiful and they would do well on the West Coast of Scotland. A beaver dam and a number of beavers on a river in a National Park would be a great attraction for the public, but of course the whole question of introduction of species which have become extinct would require careful consideration.

There is one animal which is a denizen of the British Isles, but only occurs in Ireland, namely the Irish stoat. I think it should be introduced into a National Park if one is provided in Scotland, as it is really a British animal. Other rodents such as lemmings might be introduced. But we have to be very careful of the introduction of foreign animals into England or they may become a terrible nuisance, such as the musk rat and the grey squirrel. So much then for the *inhabitants* of a National Park in this country.

We now come to the very difficult questions of *finance* and *management*. As regards finance, forest country in Scotland is now, I believe, a good deal cheaper than it used to be and land may be acquired at a reasonable figure. People are apt to be frightened at the cost of buying a large area of land and maintaining it, but in the first place private individuals acquire deer forests and what is possible for a private individual should not be impossible for the public generally either under the Government or by means of public subscription. It would be costly but not necessarily ruinous.

Then as regards maintenance. Probably a deer forest is the most expensive luxury in which a rich man can indulge himself. But a National Park would not be so expensive as a deer forest. In the first place there would be money coming in as in the case of the National Kruger Park, which makes quite a handsome income. There would have to be, of course, an hotel or rest house or something of that kind, and roads and footpaths would have to be made so that people could get about and see the animals. I do not think that so many people would require to be employed on a National Park as there are on a deer forest. A number of keepers would be needed corresponding with the stalkers on a forest, probably rather less than on a forest. But there would not have to be nearly as many ghillies, pony men, dog men, and so forth. A few to act as watchers and keep off poachers and a few to keep sightseers from disturbing the sanctuary would be all that is necessary. Indeed, I do not believe that the number of people employed would be as great as in a forest, so that I do not think we need be unduly terrified either by the cost of acquisition or of management.

And now I come to the last point, namely the method of management, and here I would like to refer you to the ideas which have been put forward by Sir Peter Chalmers Mitchell. Sir Peter is a Scot and can speak with authority. Not being a Scot I do not dare to offer hints as to the management of property in Scotland.

He advocates a scheme whereby the arrangements for the popular functions of a National Park would be entrusted to delegates appointed by Edinburgh, Glasgow, Dundee and Aberdeen, working with delegates appointed by the Council or Councils of the County or Counties in which the Park was situated. He would add to the Governing Body of the National Park a panel of persons selected for their special knowledge of wild nature in all its aspects. At least one botanist, one zoologist, one geologist, and two 'field naturalists,' one with special knowledge of plants, the other an ornithologist. He thinks these might be selected by the Principals of the four Universities, the President of the Royal Society of Edinburgh, and of the Highland and Agricultural Society. Moreover, apart from the staff concerned with the general regulation of the Park, there should be one warden or ranger selected by the Naturalist panel, whose sole duty should be the constant study of wild life in the Park and all its fluctuations.

If I were to be asked whether such a system of control would be desirable in England I should hesitate perhaps to agree. I rather think that in England control by a number of delegates responsible to different Local Authorities would be difficult and I would rather prefer a Trust, a Society under Royal Charter or a Public Utility Society. However, as I do not think that a National Park devoted to the preservation of indigenous animals is a practical possibility in England the matter is of no importance.

As regards facilities for the public to see the Park and the animals, one would of course wish that these should be as comprehensive as possible, but naturally certain restrictions would be necessary. As in a deer forest there must be a considerable area well in the middle of the Park preserved as a strict sanctuary to which no one must be allowed access. Once a year probably it would be necessary to 'move' the sanctuary or it would tend to get over-stocked with deer, but the 'moving' should be done very quietly, just a few men walking in line through the sanctuary with the wind, so as to move the deer over the march—no more than this or the deer would be unduly disturbed and the other beasts as well.

There should be rough paths about the Park and it should be possible to overlook the sanctuary and study the place with a glass. For a short time in the year it would be necessary to close the Park to the public in order that the surplus stock can be kept down. It would be impossible to allow people to wander about if rifles were being used. But this would not need to be long—perhaps a fortnight in October to destroy superfluous stags. As hinds are not shot till late in December, there would be no difficulty about closing the Park then.

There should be convenient camping places, but near the roads and on no account in the middle of the Park—a hotel would be needed and good roads leading to various points in the Park, as it would be impossible to see it all from one base.

As the object of the Park would be to interest the public in fauna preservation, I should like to see a small zoo attached to the hotel containing paddocks where the beasts preserved wild in the park could be seen close at hand. People are much more likely to see things at a distance and through a glass if they know what they look like. Lastly, it should be remembered that the Park is for the animals as well as for the public, and all behaviour likely to disturb the beasts must be vigorously suppressed.

The views I have ventured to express are my own and I take all responsibility for them. My objects in advocating this scheme are the presentation of such of our fauna as still exists in comfort and in a natural and wild state: perhaps the re-introduction of certain harmless extinct species and their preservation in a feral state, and lastly the education and amusement of the public.

Mr. N. B. Kinnear and Dr. D. H. Valentine.—*Wicken Fen, and what the National Trust has done for East Anglia.*

Mr. N. B. Kinnear.

Few parts of England have so many places of historical interest, beautiful old houses and such varied country as East Anglia. Unfortunately the National Trust owns very little property in either Norfolk or Suffolk, indeed in the latter county it can only claim one small place, namely, Kyson Hill, a haunt of Edward Fitzgerald's near Woodbridge, presented by Mr. R. O. Norcott in 1934.

In Norfolk the position is better, and there, four properties are held by the Trust. Between Fakenham and Holt is Bulfer Grove, a woodland area of about eight acres. At West Runton there is a valuable stretch of some hundred acres of wood and hill land on one of the highest points of the county. On the crest of the hill are some earthworks, which have given rise to the local name 'the Roman Camp,' though there is no reason to suppose that the Romans had anything to do with their construction. It is a great resort for picnic parties on account of the very fine view of the coast-line. In addition they hold two coastal sandhill properties of exceptional interest, namely Blakeney Point and Scolt Head. Of these two places Professor Oliver wrote—over 14 years ago—'With the whole world to choose from it would be difficult to find two better examples of the encumbered coast-line type than those provided by Scolt Head and Blakeney Point respectively, two reservations held under the National Trust. As they lie within easy reach of one another on the coast of Norfolk they can be visited conveniently on successive days, and their features compared before the details have faded from the mind.' Not only are these shingle beaches, sand dunes and salt marshes of very great interest from the view of plant ecology, but they are breeding grounds of several species of terns, and also a landing place for migrants arriving in this country from overseas.

Besides the National Trust the Norfolk Naturalists' Trust own certain properties in the county, namely, Cley Marshes, a hill on the foreshore at Salthouse, and Alderfen Broad at Irstead.

But there is another type of country in East Anglia of which neither of these Trusts own any part, I refer to the Breck area, which botanically and zoologically is of the very greatest interest. Some of this area has already been taken up and planted by the Forestry Commission, but good tracks are still to be secured if the necessary money is forthcoming. There are many wealthy people in the British Isles ; can they not be induced to help in securing a portion of this unique type of country confined in the British Isles to the Breck area ? There are many interesting historical country houses in East Anglia and probably owners have a difficulty in keeping them up owing to the present-day taxation. The National Trust has a scheme for saving some of these historical homes which, briefly, is as follows :

An owner transfers his house and grounds to the Trust and is relieved of the accumulative burden of death duties and income tax upon himself and his heirs. In return for this exemption the nation will benefit in two ways : by the permanent and proper maintenance and repair of the property transferred, and by facilities for visiting at certain times of the year.

Dr. D. H. Valentine.

At Wicken is a small area of the English Fenland which has never been under cultivation, and which bears a rich and characteristic flora and fauna. Its main features are its peaty, alkaline soil and its high water-table.

Studies have been made on the variation throughout the year of such physical features of the fen as height of water-table, soil temperature and soil aeration. An important task of the future is to study the life-history of the fen plants and animals in relation to these physical features. This has been begun for *Cladium Mariscus*.

Cladium (known as sedge) is one of the most important plants on the fen and has been, in the past, regularly cut as a crop. If all cutting is stopped, bushes, particularly two species of Rhamnus, cover the ground and eventually form a scrub to the exclusion of the 'litter' and sedge communities. As these disappear, most of the rare and characteristic species of the fen, both plant and animal, disappear too. Regular cutting of sedge and 'litter' and uprooting of bushes has therefore to be maintained.

Of the two species of Rhamnus, *R. Frangula* is much the more important in the early stages of succession, but it dies out and is replaced in mature scrub by *R. cathartica*. The causes underlying this replacement are as yet unknown. This problem is typical of the fen, which has been and will remain a storehouse of problems for every kind of naturalist.

Monday, August 22.

Dr. H. W. Parker.—*The co-operation of Corresponding Societies in the study of systematics in relation to general biology.*

The existence of the then newly formed Association for the Study of Systematics in Relation to General Biology was brought to the notice of the Corresponding Societies a year ago, and the delegates on that occasion showed their interest in its aims by recommending the Council of the British Association to co-operate. That co-operation has been fruitful of results ; but the work is only just beginning and the experience of the last twelve months makes it evident that some modifications in the original plans are desirable.

Specialisation is leading to an increasing isolation of the various branches of biology from one another with deplorable results. Systematists are so overburdened that they are often unable to devote the time necessary to experiment with new methods, and faulty or inadequate identification of the raw material is ruining much otherwise valuable work in other branches. It is this which has led to the formation of the Systematics Association with its two complementary ideals—encouragement of the use of genetical, cytological, ecological and other data in taxonomy and the provision of better systematic information for workers in these other fields. The realisation of these ideals is a matter which affects the Corresponding Societies profoundly and it is hoped that they will continue to take part in the various projects which the Systematics Association has in hand.

As a first step towards the provision of that reliable systematic information which has already been mentioned, it is proposed to try to issue a series of handbooks on the British fauna and flora. The information necessary for the compilation of such a series of hand-books is practically non-existent for many groups and surprisingly incomplete in many others which are usually regarded as well known. The obvious first step is, therefore, to discover what information is lacking and then try to fill the deficiencies. With this end in view a questionnaire has been submitted to experts in various groups to discover the extent of the existing literature and its suitability for modern requirements. The answers to these questions are now almost complete and a summary is being prepared for publication. This list should be of considerable value to the amateur naturalist, enabling him to discover the literature necessary for the determination of his material in any group. But its greatest value will be in indicating those fields which are fallow. The growth of systematic knowledge has been disproportionate ; inevitably those groups of direct economic importance and those whose study does not require special technical equipment have received the most attention. But other branches of biology cannot reasonably be expected to restrict themselves to those fields which are taxonomically well covered, and in such sciences as ecology and biogeography a uniform knowledge of the whole fauna and flora is absolutely essential.

It was at first thought that the lacunæ could be filled to some extent by the organisation of national panels of referees and recorders for the whole animal and plant kingdoms, who could identify material collected by the

Natural History Societies and thus gather together the requisite information. But it has become apparent that there are not sufficient competent workers to provide the personnel for such a comprehensive scheme, and modifications have become necessary. In the more ' popular ' groups, with many amateur workers, there are already local panels of referees and recorders appointed by the individual societies or unions working in restricted areas. The Systematics Association is anxious to act as the clearing house for the information obtained by these local bodies, and, at the same time, would like to see them extend their range of activity so that the whole country is covered. Unfortunately, at the moment, it is only the areas of greatest population-density which are being adequately surveyed by the societies, and this in itself is liable to lead us to wholly fictitious conclusions as to distribution and so forth. In the other groups the solution appears to lie in individuals specialising in biological groups of limited size and undertaking a distributional survey for the whole country. The Systematics Association can undertake to assist such workers with technical advice in taxonomic difficulties and by acting as the clearing-house through which information collected by other societies can be transmitted to the right person. Already a certain number of volunteers are engaged along these lines, but many more are urgently wanted.

The subject of distributional surveys has been mentioned, and considerable importance is attached to these. We are still far from an understanding of the factors which may limit the distribution of animals and plants and a comparision of the distribution of allied species and subspecies may be expected to yield some valuable information. It is apparent that it would be a matter of great convenience if all distributional records were presented in such a way that they are directly comparable, and to this end it is suggested that for terrestrial animals the vice-county system be used (see *Journ. Conchology*, **16**, 1921, pp. 168–169, and *Entom. Mon. Mag.*, **67**, 1931, pp. 183–193). Any attempt to use geological or ecological subdivisions would appear to be impracticable, except in special circumstances, and the wholly arbitrary vice-county units have already been widely used with success, so that new recordings will be directly comparable with the old. There are some minor discrepancies between the different published accounts of the vice-county limits and the Association has prepared a new list which tries to harmonise these ; maps will be obtainable from the Association. For marine organisms it is recommended that the Fishery divisions as used by the Ministry of Agriculture and Fisheries (see ' Chart for locating Fishing Grounds,' No. A.208/F.G., published by the Ministry) would form convenient geographical units. Brackish water, intertidal and estuarine organisms which may enter into the ecological chains of both land and sea would necessarily have to be recorded on both systems.

What has been said above is concerned chiefly with the accumulation of the data necessary for the provision of better systematic information for the use of biologists in general. There remains to be considered the question of utilising ecological and other data in the solution of systematic problems. In this work, too, the Corresponding Societies are already giving valued help. The Systematics Association maintains a large and constantly changing list of taxonomic problems for whose solution ecological, genetical, physiological, geographical or experimental research is needed. Some of these problems, such as those which require simultaneous observations in different districts or observations in a limited area over a lengthy period of time, are eminently suitable for the combined attack of all the Natural History Societies of the country. A number of these problems have already been circulated to the Corresponding Societies and a generous response has been forthcoming ; more than a hundred workers have offered their help in the attack on some twenty-one problems. But this does not exhaust the possibilities. The Systematics Association will be glad to offer what help it can to any individual or society with a systematic problem. Wherever it seems probable that additional material or information from other districts may help towards a solution, steps will be taken to obtain the necessary assistance. Thanks to the resolution passed at this Conference a year ago the British Association will inform all its Corresponding Societies of what is needed and for this co-operation we of the Systematics Association wish to express our gratitude.

Dr. VAUGHAN CORNISH.—*The preservation of Crown lands adjacent to Camberley, Bracknell and Ascot as an open space.*

Between Camberley, Bracknell and Ascot is an area of wild heaths and woods about nine square miles in extent, which Norden's map of A.D 1607 shows to be part of the Forest of Windsor, as it was in Stuart times.[1] The area now comes under the category of Crown Lands, the term applied to properties of which the revenues were surrendered by King George III in exchange for payments out of the Civil List, while the title to the freehold, if that expression may be applied, remains with the Sovereign. Subject to this ultimate right it appears to be for the nation to decide what use shall be made of this land during their tenancy, which has already lasted for more than a hundred years.

Throughout the lifetime of the present generation these heaths and woods have been open as a pleasant place for walking, riding and driving to the residents of the towns and villages in the neighbouring parts of Berkshire and Surrey. Since the introduction of the motor this great fresh-air space has also come within reach of Londoners, for London is less than thirty miles away. At the annual meeting of the South-Eastern Union of Scientific Societies [2] in 1935 I urged that this great area of wild country should be preserved as an open space for the benefit of the nation. But now the Commissioners of Crown Lands have enclosed the area with barbed wire and blocked the rough roads or drives with locked gates. Smaller gates, usable only by foot passengers, are left open, but here notices are put up saying ' No footpath.' The Commissioners state that they are within their rights in so acting, and in the policy of building development ; that nearly the whole of this area was bought by the Commissioners for development ; that Parliament has always been told firmly that the Commissioners must retain the right to develop at their discretion land bought for the purpose ; and that any land beyond one acre required by local authorities as a public open space must be paid for.

In the provisional planning scheme of the Frimley and Camberley Urban District Council it was proposed that some five acres (more or less) of this area should be purchased out of the rates as a public recreation ground. Ratepayers being invited to send in objections to the planning scheme, I suggested that the better way would be to apply for the reservation of the whole area, several square miles in extent, for the benefit of the nation without further payment, and I also submitted my memorandum on the subject to the Surrey County Council, Berkshire County Council, the Council

[1] Norden was surveyor of the Crown Woods and Forests.

[2] Presidential Address, Regional Survey Section, since embodied in the author's book on *The Preservation of our Scenery* (Cambridge University Press).

for the Preservation of Rural England and other authorities and societies interested in such matters. My purpose was not to question the legal rights of the Commissioners of Crown Lands under the existing Act of Parliament, but to contend that their policy is socially out of date, and to urge that this great area of wild land within thirty miles of London is of national importance and should be maintained as an open space for the preservation of its natural beauty, for the protection of its ancient monuments, for the encouragement of outdoor life, and for the promotion of physical fitness. When I proposed this matter for discussion at the Conference of Delegates of Corresponding Societies it was my intention to move a resolution to be sent to the Council of the British Association, but I am happy to report that sufficient progress has been made in the interval to make this unnecessary.

The Council for the Preservation of Rural England have appointed a sub-Committee to enquire into the question of the general position of the Crown Land Commissioners in regard to their administration of Crown Lands from the point of view of rural preservation and what action if any can be taken in regard to the Crown Lands round Camberley.

At a conference of representatives of the Berkshire and Surrey County Councils and the District Councils concerned, at which I submitted my proposals, it was decided that the Berkshire County Council should appoint a small delegation to interview the Commissioners of Crown Lands to ascertain their proposals in connection with this land and their views on the proposal which I had put to the meeting.

You will see therefore that action is being taken by important and influential bodies, and I am clear that it is best to leave matters in their hands for the present. The subject is extremely complicated and there are some aspects of the matter which need tactful handling. I am therefore content to report progress to the Delegates without moving a resolution, which it would be difficult to frame in words which might not in one way or other hamper the procedure of the C.P.R.E. and the Berkshire County Council.

I feel sure, however, that this Conference will wish all success to the C.P.R.E. and the Berkshire County Council in their efforts for the preservation of the natural scenery of the Crown Lands adjacent to Camberley, Bracknell and Ascot.

At the conclusion of the proceedings the delegates attended a garden party at the Botanic Garden as the guests of Prof. F. T. Brooks, F.R.S.

EVENING DISCOURSES.

FIRST EVENING DISCOURSE
FRIDAY, AUGUST 19, 1938.

HISTORY OF THE FENLAND

BY

DR. H. GODWIN.

THE Discourse illustrated the great advantages which follow from co-operation between workers in different sciences converging on some common problem. The Fenland Research Committee, which was founded in 1932 under the presidency of Sir Albert Seward, F.R.S., includes in its members botanists, geologists, archæologists, geographers and professional experts in fen drainage. With the help of small grants from the British Association, the Percy Sladen Trust and the Department of Scientific and Industrial Research, they have established the rough outline of fenland history in the period since the last Ice Age.

This history was begun when the North Sea had not yet been formed and peat fens covered the flat country between the east coast of England and the continental coast. By the recently developed technique of pollen analysis it has been possible to recover from the submerged peats of the Dogger Bank the pollen grains of trees growing in that former period. These grains are still recognisable by their size, shape, pores and surface markings as readily as grains dispersed to-day. From extensive counts of these sub-fossil tree pollens, it has proved possible to reconstruct the post-glacial forest history of Europe.

The Dogger Bank peats belong to a period of sparse birch-pine forests, which is known on the continent to be at least as old as 8000 B.C. As the North Sea formed during the following centuries more recent peats were restricted to the shallower coastal areas. The considerable age of the deeper peats was confirmed by the discovery of a bone fish spear of Mesolithic type from a lump of peat dredged by fishing boats from the Leman and Ower Banks off the Norfolk coast.

The deposits of the fenland itself are mostly younger than this. The lecturer showed illustrations of sections established by lines of borings from the fen margin towards the sea. The typical section showed continuous peat formation at the landward margin, but towards the sea this was split by a bed of soft fen clay, which its content of microscopic plants and animals (diatoms and foraminifera) showed to have been formed in brackish lagoon conditions. On the seaward side of the fens the uppermost deposit is a thick silt which was laid down under semi-marine conditions : it is the fertile potato-, fruit- and bulb-growing land of the Wisbech-Spalding-Holbeach district.

The excavations and enquiries of the Fenland Research Committee have always sought to determine both the date and the conditions under which these major types of deposit were laid down, and they have met with a good deal of success. Of particular interest was the excavation of an ancient river channel at Shippea Hill between Ely and Mildenhall. Here no fewer than three archæological horizons were discovered stratified into the fen deposits.

Broadly speaking the fenland history has shown alternating phases of marine invasion and of fresh-water conditions. The first fresh-water phase in the present fens extended through the Mesolithic and Neolithic periods, and for much of this time the fens were covered with alder-birch fen woods. It was probably about the end of the Neolithic period that marine invasion caused formation of the fen clay. In the Bronze Age which followed, fen woods grew extensively, but these must have been dry enough for occupation by prehistoric man, since Bronze Age remains are found abundantly in the fen peats. It is probable that the fens became too wet for occupation in the Iron Age.

In the Roman period marine invasion once again dominated the fenland history. All the silt of the Wisbech-Spalding area was laid down and its surface intensively cultivated. Along the tidal rivers silt banks were built up, and stood above the surrounding peat land as habitable areas.

After the Romans left Britain the fens were not exploited until the drainage which began seriously in the seventeenth century. As it became effective the shallower lakes such as Whittlesea Mere, Soham Mere and Benwick Mere disappeared. The ground level sank by shrinkage and wastage of the peat, often by as much as one inch per year, and the silt banks of the Romano-British watercourses began to appear as raised banks, or ' roddons ' crossing the peatland.

Dr. Godwin suggested that there was no reason to suppose that the land and sea movement which had played so much part in former fenland history should now have ceased entirely, and said that there was some evidence that recent drainage troubles in the fens were due in part to sinking of the coast. He suggested that the subsidisation of work like that of the Fenland Research Committee would be of value not only to science, but to such scientific applications as the drainage of our fenlands.

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SECOND EVENING DISCOURSE

Monday, August 22, 1938.

THE CONTRIBUTION OF THE ELECTRICAL ENGINEER TO MODERN PHYSICS

BY

PROF. M. L. OLIPHANT, F.R.S.

The Discourse was reported in *Engineering*, 319–20, Sept. 16, 1938, and in *Nature*, **142**, 3592, 444–5, Sept. 3, 1938.

REFERENCES TO PUBLICATION OF COMMUNICATIONS TO THE SECTIONS

AND OTHER REFERENCES SUPPLIED BY AUTHORS.

The titles of discussions, or the names of readers of papers in the Sections (pp. 381–522), as to which publication notes have been supplied, are given below in alphabetical order under each Section.

References indicated by 'cf.' are to appropriate works quoted by the authors of papers, not to the papers themselves.

General reference may be made to the issues of *Nature* (weekly) during and subsequent to the meeting.

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Blackett, Prof. P. M. S. *Nature*, **142**, No. 3598, 692, Oct. 15, 1938.

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Feather, Dr. N. Cf. 1938 *Proc. Roy. Soc.*, A, **165**, 530; *Proc. Roy. Soc.*, A (in preparation).

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Stoneley, Dr. R. Cf. 1931 *M.N., R.A.S., Geoph. Suppt.* **2**, 430.

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Guillebaud, C. W. Expected to appear early in 1939 as a small book entitled 'The economic recovery of Germany : from 1933 to the incorporation of Austria in March 1938' (probably Macmillan).

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THE EIGHTH

ALEXANDER PEDLER LECTURE.*

HUMANITY IN GEOLOGICAL PERSPECTIVE.

BY

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It is a curious corollary to our system of education that a large part of the population should be almost completely ignorant of geological science. This ignorance is common to all classes, not least among those who have suffered intensive mental cultivation. Without unduly stressing the sentimental consideration that ordinary people might be expected to take an interest in the nature and history of their mother, we must marvel at the lack of curiosity of those who use and enjoy the material amenities of civilisation. In an age of petrol engines and ferro-concrete, an intelligent interest in the nature and origin of essential raw materials would be expected to extend beyond the few whose business it is to locate and exploit them.

A bare catalogue of the necessities of life to-day or at any time in the past, under any form of civilisation or none, is but a list of materials that are directly or indirectly the concern of geological research. For Geology is the science of the Earth and all that it contains, inanimate or animate, past and present. Fuel, metal, stone, water and soil are necessary to our various activities and for our very lives ; so that the practising geologist (whether called by that name or not) is and must always be at the back of every enterprise.

No intelligent person can fail to realise the immense importance of Applied Geology in such matters as Mining or Civil Engineering ; but the uninitiated may be forgiven for doubting the utility of some branches of geological research. The character and evolution of extinct micro-organisms seems a topic that can serve little useful purpose save to keep some crank out of worse mischief ; while the molecular and atomic readjustments of minerals subjected to violent treatment far underground appear suitable to be dismissed as 'academic,' a word often considered synonymous with 'useless.' Nevertheless, petroleum companies find it advantageous to employ experts on the evolution of the Foraminifera ; and the discovery and exploitation of mineral wealth depends on knowledge of the processes involved in its production. There is, indeed, no such thing as 'useless knowledge' ; for knowledge is a tool ready to the craftsman's hand, always effective if skilfully used.

In addition to, and transcending, the material contribution of geological

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science to civilisation, is the aesthetic influence of the study; for Geology is a stimulant to the imagination. Without intelligence man is but an unsatisfactory animal; intelligent but unimaginative he is a dangerous nuisance; imaginative and unintelligent he is futile; but with imagination controlled by intelligence he is truly human. The glories of Nature, whether expressed in a landscape or a sand-grain, are wasted on a mind that fails to respond with intelligent curiosity. There is better and more inspiring entertainment to be derived from the works of Nature than was ever provided by the art of man. Boredom and disillusionment, those ravaging diseases that kill body and mind, can never approach a man trained to appreciate his environment. No very profound geological knowledge is needed to transform a country walk from mere exercise of the legs into an adventure of the mind. Everywhere in this world is a happy hunting-ground for a geologist. The average expectation of life among geologists is such that it has fostered the superstition that Geology, like bowls, is a pastime of senility; it is due to the perpetual interest that keeps life worth living.

My purpose to-night, however, is neither to extol the study of Geology as a gateway to long and happy life, nor as the basic factor in the material aspect of modern civilisation. I wish to direct your thoughts rather to the reaction on our philosophy of life of such geological facts as can be claimed to be established. Man's place in Nature, his whereabouts in time and space, is, and has always been, his fundamental problem. Early and mediæval attempts to solve that problem were foredoomed to failure, for next to nothing was known of Nature, and philosophical speculation savoured of vacuous bombination. We still know very little about the material Universe, but we do know something; and our few established data afford a solid basis for theoretical deductions that are as worthy of serious consideration as some of the older speculations are of ridicule.

Most psychologists, and all parents, will agree that a young child, as soon as he acquires independent consciousness, is in his own estimation the centre of the Universe. All phenomena that he experiences are aimed, benevolently or maliciously, at him and at him only. He is, in his own conceit, the only pebble on the beach. Experience and training will in time tend to modify this attitude; and indeed, if and when wisdom comes, egotism will be banished. But knowledge is usually in advance of wisdom, and there is often a regrettable stage in childish development when budding knowledge is mistaken for omniscience. This phase can also be modified by experience. After the disappointment and humiliation have subsided, the adolescent is in a position to find his place in the scheme of things, and to adapt himself to it. The clever animal may become transmuted into a man. His success in that sphere may be measured in direct proportion to the reversal of his childish instincts.

It is not surprising that the earliest philosophers, the first thinkers in the childhood of the race, should have fallen into childish errors. Scarcely removed from the supreme egotism of animals, but capable of correlation and imagination, they saw themselves as the ultimate climax of creation, for whose especial accommodation the whole Universe was designed. They could not conceive of any reason for the existence of the world apart from themselves; so that, for them, the world and the

Universe were made expressly for their habitation, scarcely antedating their arrival. By precisely similar reasoning, the only habitable part of the world, perhaps all the world there was, centred around their homes and extended not many days' journey beyond their horizon. Early voyagers must have experienced exceptional thrills from excursions into regions that did not even exist; doubtless their tales were given no more credence than the reports of geologists who described terrestrial events that preceded the creation of the world.

It is surprising to realise that less than two thousand years ago our predecessors had scarcely any reliable knowledge of world-geography, and less of the configuration of the globe. An interesting study could be made of the influence on philosophical ideas of the vast increase in the conception of space that resulted from mediæval exploration. Our modern ideas of cosmic space, whether curved or infinite, are in some sense but a sequel to the revolutionary discovery that there was anything of the sort to discover.

Realisation of the immensity of geological time is relatively recent, and it is far from universal even to-day. Whereas a conception of the size, and even of the cosmic relations, of the world is subject to daily experience and confirmation, that of past time is more subtle to obtain. Modern transport and other inventions enable us to span in a day distances greater than early conceptions of the size of the Universe; but we are still time-bound by the threescore years and ten of our earthly experience. It may be doubted if anyone, even a geologist or a historian, can form a clear idea of the significance of a thousand years of time; while it is probable that the four or five thousand years canonically ascribed to the earth's existence seemed an almost infinite period to those who decided upon it. And yet to-day we know that an interval of, say, 100,000 years represents an infinitesimal part of world history, and does not cover even the duration of mankind. We know, thanks to archæological research, of complex human civilisations antedating the official creation of the world; and we know, through geological research, of animals and plants that populated the earth in eras a thousand times more remote. We know, but we cannot truly comprehend.

Although we must stand bewildered before the actual figures of geological time, more hopelessly than before those of cosmic space, there is no serious difficulty in appraising relative time values. A million years may be inconceivable, but they are obviously fewer than ten million or a hundred million. We can mentally dispense with the cyphers, and reduce the totals within the limits of our understanding. So that if we estimate the duration of mankind at one million years, and that of the Cainozoic era (the 'age of Mammals') up to the present at sixty million years, the ratio of one to sixty is a true and intelligible expression of the data. Whether we give credence to the estimates of the length of preceding eras or not, we can readily understand that they were collectively vastly longer than the Cainozoic. And who shall say what vistas of time are behind and beyond the mists of the pre-Cambrian? Without pretending to ascribe to geologists an abnormal share of the attributes of Deity, it is within the truth to say that they think in terms of time where 'a thousand ages' are lost in the total.

The calculations of astronomers and physicists have, of course, a profound interest for geologists, and we may be gratified if their results accord with ours. But we must be forgiven if we regard them as giving but uncertain confirmation, or negligible denial, of our own deductions. Too often in the past century did the physicists attempt to limit the duration of the world, and of the solar system, within impossibly small scope, basing their conclusions on the elusive and superficially convincing principles of mathematics. Doubtless their arithmetic was beyond cavil; but the premisses were inevitably incomplete and even inaccurate. The hoary imposture of the accuracy of the ' exact ' sciences still deludes mankind, through the wildly illogical belief that a rigidly logical argument must reach a correct result whatever errors may have existed in the premisses on which it is based. Nevertheless, however askance we may look at the current theories of Astrophysics, we can recognise with satisfaction, that their bearing on time is consistent with the conception of the world's duration deduced from geological facts.

Three considerations that are inspired by our present knowledge of geological history may be emphasised here. In the first place, the human race, though of far greater antiquity than our forbears taught, has existed for a minute fraction of the time during which the world has been essentially like it is to-day. Indeed, the ' human period ' requires the myopic vision of an archæologist; it is too near and too small to focus clearly on a geologist's retina. Secondly, the world was a ' going concern,' with successive waves of prolific population, for vast periods of time before the appearance of mankind. Thirdly, and perhaps most significantly, throughout the whole sequence of these incalculable ages, physical, chemical and biological laws have remained the same. A rhythmic orderliness pervades the trivial and ephemeral details of the earth's history—without it all scientific endeavour would be in vain.

This is a very different view from that prevalent but a few generations ago; and since it is, as far as it goes, demonstrably true, the old ideas must be wrong. They served their purpose in the childhood of mankind; but now that we are growing into intelligent adolescents, with a glorious prospect of new truths to be learned, they are out of date and must be put away. It is grievously hard to discard a disproved theory, but much more difficult to get free from the philosophical deductions that sprang from it. When the theory is based on self-esteem, and the philosophy designed to justify conceit, conversion becomes painful in the extreme. Some, blinded by prejudice, employ the childish ruse of denying the truth offhand, in the pathetic hope that truth is destructible. Others, more circumspect but even less respectable, ignore the facts even when they see them, or pretend that they have no bearing on their philosophy of life. Others again, intellectually convinced but emotionally hide-bound, strive to force the old beliefs into the new container.

Most of us believe, and almost all pretend, that the world, and indeed the Universe, was devised expressly for our convenience. Perhaps a thief may consider that the trinkets he purloins were made for him to steal; but was that the jeweller's original intention? One can but gasp at the effrontery of a person who considers, for example, that the Coal Measures were laid down in far-seeing preparation for human needs.

If for a moment one grants that preposterously egotistic assumption, what is to be said of the millions of tons of coal that were destroyed by denudation long before their rightful owner was ready to use them? Indeed, when arguments of this sort are employed (and they are usually the stock-in-trade of those most seriously anxious to give reverence where it is due) the result is a dilemma from which blasphemy affords the sole escape. Philosophy is the clothing of truth: a baby's vest is inadequate, and indecent, on an adolescent.

We must reconcile ourselves, and our philosophies, to the fact that from the world's standpoint we have only just arrived. Although during our brief career we have made an unconscionable mess of parts of its surface, the globe continues to revolve unperturbed, and we cannot imagine that our disappearance would cause it a passing tremor. To those who have grown up in the belief that the world was made solely for their occupation and benefit, this conclusion seems humiliating; but only the conceited can experience humiliation. Moreover, the third scriptural criterion for a satisfactory and moral life involves humility. There can be no incentive to progress for those who think that they have already arrived, and there is no prospect but a fall for the arrogant. But to those who are not blinded by conceit there is stimulation in the thought that they are playing a part, however humble, in a vast drama; and elation in the knowledge that they, alone of the actors, can be more than puppets in the show.

If the first two of our considerations tend to induce humility, the third surely inspires confidence. The constancy of natural laws, the reiteration of cause and effect, the simplicity of the outline of history, show that there are some principles at least in which we can trust. There is an orderliness in Nature that we can appreciate without knowing its origin or aim. One has but to read some of the cosmogonies of the last few centuries, when the catastrophic school was trying to compress the gallon of geological facts into the pint pot of canonical time, to realise how profoundly our views are altered. These earnest attempts to reconcile fiction with truth led to a conception of the world staggering from one supernatural cataclysm to another, and make ludicrous reading to-day. They evoke a picture of a Creator learning by trial and error, with no set plan and very little patience—surely the butt of ribaldry rather than the inspirer of reverence. There could be no security under so fickle a tyrant, and no point in trying to understand a policy that might be reversed at any time.

Just laws must bind the legislator no less than his subjects; and it is a heartening thought to realise that even in Cambrian times the sun shone and the rain fell with the same sort of effects as they produce to-day. It gives confidence to know that, come what may, effect follows cause as day follows night, and that in a world of seeming change and decay there are principles and processes that are eternal. In the material world at least we can know where we are, and what to expect. There are laws that neither time nor circumstance can alter. We can discover their gist, learn to obey them, and so acquire power beyond imagination; and on the other hand we can ignore them or defy them, and perish.

The geological record shows that we have but a small, perhaps transient, part to play in the world-drama ; but it also reveals the grandeur of the theatre and the impartiality of the management. It induces humility, but gives security. The establishment we have so recently entered is soundly constructed and consistently managed ; with reasonable observation we can learn the way to our own rooms and the sure results of our actions. But valuable and salutary as this knowledge may be, it leaves us completely at the mercy of our environment, like passengers in a train going they know not whither with almost ominous smoothness. Our surroundings are impersonal, insensitive and inevitable ; it is for us to make the best we can of them. And here another aspect of geological history, Palæontology, is available to give warning and advice. The story of life through the ages of the earth's history touches us more nearly than does that of the inanimate fabric ; for we are living creatures, and our bodily lives are held on the same terms as those of the rest of the animate world.

Before attempting to discuss the influence of the palæontological record on our own case, it is necessary to meet certain objections that may be raised. The same childish conceit that supposes the Universe to be a playground made for mankind alone automatically believes that man is so far superior to all other creatures that the episodes of their obscure lives have no bearing on the problems of his exalted existence. It is of course true that man has certain attributes and capacities that are scarcely developed among other animals ; but so have all other types, else how could we distinguish and classify them ?

For convenience we may admit that a man consists of two parts, commonly called body and soul, and that these two parts are largely antithetic. All respectable religions have always stressed the conflict between the carnal and the spiritual ; and yet many most earnest enthusiasts insist that their enemy the body must be as peculiar and sacred as their friend the soul. Confusion of thought such as this is not only strange, but disastrous, for it is the beginning and end of materialism. The human body, in its anatomical and physiological characters, is an animal's body ; as such it is strictly comparable with that of any other animal, and subject to the same laws. Anyone who believes otherwise, and lives in accordance with his beliefs, will be dead within an hour.

Such evidence as is available to show the history of living creatures during the course of geological time will, therefore, have at least a partial bearing on the problems of our own lives. It will be apt for comparison with our bodily and racial lives, and our reactions to our physical environment, whatever complications may be introduced by our special human attributes.

A comprehensive survey of the palæontological record shows conclusively that there, as in the physical history of the earth, inviolable laws are in continuous operation. Paramount among these laws is that of cause and effect ; which, in its biological aspect, is called the law of Evolution.

In many cases, and for various reasons, our apprehension of the causes of evolutionary change is far less complete than in the case of physical processes ; but the constant repetition of similar effects gives presumptive evidence of oft-recurring cause. The chief difficulty in appraising the determinants of evolution lies in the dual nature of life, expressed in the legacy of heredity and the impact of environment. Opinions differ widely as to the relative importance of these twin influences, but there is no room for doubt that both exist, and that they may often prove incompatible.

An unfortunate but inevitable weakness of palæontological evidence enables it to show very little of the early history of groups of organisms, although its record of their decline and fall is often clearly displayed. We are far from knowing how or why new types appear; but on the other hand we have plentiful illustration of how they disappear, and convincing indication as to the way in which Nemesis overtakes them.

The record of Evolution is, in essentials, the same for all groups of organisms. Indeed, it is the same when expressed in the changes that befall the several organs of which organisms are built. Phylogeny and morphogeny are mutually dependent, for the whole, though greater than the parts, consists of them and is directly affected by their condition. Hyperbolic though it may sound, it is a bare fact of experience that the life-story of an individual, or of a single cell in its body, is a précis of that of a phylum, or of any taxonomic grade. Families and orders, like species and individuals, may possess the contrasted qualities of ' perennials ' or ' annuals '; but the general trend of their lives is the same. They have their youth, a stage of growth and adaptation; their maturity, when equilibrium has been attained; and their senility, when persistent development beyond perfection leads to decline and death.

In the youthful stage groups or individuals are plastic, producing much diversity by the reaction of their intrinsic vitality with the moulding influence of environment. In the senile stage their characters have become stereotyped, and their reaction to an ever-changing environment is extinction. The same inexorable range of variation in physical surroundings acts as a tonic to the young and a poison to the old. For life is a competition between the mysterious quality called ' vitality ' and the insensitive environment that encompasses it. The struggle is exhilarating, creative, and usually successful, in youth; but old age fights a losing battle. The secret of perpetual youth is no mystery, for all that is needed is perpetual plasticity, giving ready adaptation to environment. But in the nature of things this is impossible. It is true that simple forms of life can adjust themselves and their needs to varying conditions more readily than more complex forms; they have a greater expectation of racial life; but there is a term to their duration. Life itself, transmitted from one generation to another, may be everlasting; but all living things are mortal.

This sounds like a somewhat morbid summary of the course of an ordinary human life; but actually it is a description of the evolution of every large or small group of organisms of which we have adequate palæontological knowledge.

There are two harmonies essential for successful living, one internal and the other external; both must be kept consonant. The several organs of an organism must maintain their proper proportions, and the organism as a whole must conform to its surroundings. Internal discord, due to the modulation of one ingredient independently of the rest, cannot fail to produce inefficiency and final collapse; while external discord brings the individual into mortal conflict with an invincible opponent. One or the other of these disasters is in store for every living thing, be it a cell, a body, a race or a species. To be alive is to be changing, and there is a limit to the range of possible harmonies.

Recognition of the orderliness of animate nature, and of the inevitable sequence of change, decay and replacement, does not engender optimism when we think of ourselves, our institutions, and our species. A complicated mammalian mechanism with an over-developed nervous system seems like a diagnosis of a very short-lived race.

If we despairingly claim that our wits have enabled us to reduce the risks of environment, the records of our history are open to show that the internal dangers develop none the less. Diseases of disproportionate development, such as cancer, attack individuals ; and civilisations crumble through over-complexity and dissension. Our cleverness may make our success spectacular, but it speeds on the ensuing collapse.

By virtue of our over-developed intelligence we accelerate the processes of evolution, especially in our social relations ; and whatever hope evolution may hold for the unborn, a tomb is all that it can offer to the living. The history of the decline and fall of empires makes familiar reading for a palæontologist ; it illustrates in a condensed and diagrammatic form the late phases of evolution in other creatures that are the normal subjects of his study. Regrettable though it may be, the human animal seems to a palæontologist superior to a Dinosaur or an Ammonite merely in the speed with which it rushes towards extinction.

This is a tragic outlook ; but there is nothing unfamiliar about it. All individuals realise, when they choose to think, that they are not immortal ; every philosophy and religion lays emphasis on the transient nature of ' man's earthly hopes.' It is not only the palæontologist who knows that the prize awaiting the winners in the struggle for existence is death. Nor need we be morbid in our outlook ; a man who has made his will can still enjoy life. ' The play's the thing,' not to be spoilt by regrets that the actors will not hold the stage for ever. But whether or not we can derive comfort from such considerations, the fact remains that all available evidence, palæontological and historical, racial and personal, indicates the inevitable doom of man the animal, and of all his works.

Must we then reconcile ourselves to the belief that we are such stuff as palæontological collections are made of, and that in the geologically near future a few fossil relics will be all that remains of our species ? A creed so desperate would demand extinction as an escape from a farcically hopeless existence. Before finally abandoning ourselves to utter pessimism, we may try to review our position from another angle.

Once, very long ago even as a geologist reckons time, a strange thing happened. We do not know why or how ; but a certain combination of substances acquired the quality that we call life. In many ways the first organisms, doubtless unicellular and microscopic, defied the ordinary laws of physical nature. Especially was this the case in their capacity for sexual reproduction and its consequent succession of ever-changing individuality ; in other words, in their quality of evolution. The organic world, surrounded, influenced and in no small measure controlled, by the inorganic, started on an adventure that led it ever further from the mechanical principles of insensate forces. To-day that same 'life,' spread among a myriad of individuals, is still flourishing, and shows no signs of decline. It is an important, though superficial, part of the economy of the globe. Its more progressive exponents, elaborating their structural and mechanical diversities, have acquired an enhanced sensitiveness that has become concentrated into a definite nervous system, and has gradually attained the faculty of intelligence. Being mammals ourselves, we can recognise in our fellow mammals mental capacity and consequent behaviour that appeal to us as comparable with our own ; it is not possible to appreciate the mentality of creatures utterly unlike ourselves, even if such mentality exists. Nevertheless, it seems evident that reception of sensations and response to them become more acute and intelligent with improving brain-structure. Increasing faculties of locomotion stimulate perception, and life becomes less automatic and more emotional. The brain comes to dominate the organism.

The earliest forms of life must have striven against their physical surroundings, for life is an irritating alien in the inorganic world. But when, by virtue of the faculty of multiplication, living things came to exist in great quantity and congestion, internecine competition was added to environmental problems, and the complicated anarchy of the 'struggle for existence' began. Structural advantages, or (in later stages) mental superiority, help to bring success to their possessors ; but the struggle is ultimately unavailing. For although the winners may crush, and perhaps even exterminate, the losers in any particular rivalry, the factors that gave them victory ensure their collapse. Unobtrusive types endure, but aggressive and domineering types achieve success and disaster in direct proportion. We can liken the course of evolution to the use of a cylinder of gas. If the gas is allowed to escape slowly under control, it may burn steadily, giving a feeble light, for a long time ; if it all ignites at once, there is a brilliant flash, a crash, and then darkness. Such were the records of *Lingula* and *Productus*, or of the Turtles and Dinosaurs.

Mental acumen is a better means to success than mere structural advantage. The rapid rise and fall of hosts of mammalian types, contrasting as it does with the considerable stability of the invertebrate fauna during the Cainozoic era, seems a clear illustration of the paradox of the struggle for existence, where the prize is death.

And so once more we reach the depressing view of the human species, surely the most spectacular and record-breaking winners yet evolved, hastening towards the reward of victory. In so far as it has entered the lists, matching its capacity for selfish greed against the individualism of other animals or of its fellows, the human race is bound by the rules of the competition, and the prize is within its grasp. Man is a supremely successful animal ; such success, whether involving murder or not, is the precursor of suicide.

There can be no doubt that the introduction of life marked a crisis in the earth's history. Perhaps its significance can be best expressed by the suggestion that to the eternal changelessness of physical laws there was added the eternal changefulness of organic evolution. Bound by the insensitive chains of material environment, living organisms possessed a sort of individuality of which they became increasingly conscious with the improvement of their nervous mechanism. Sensation and reaction were limited to physical and material phenomena, and so were ultimately subject to the inexorable rules that propelled their possessors from birth to death.

But the nervous mechanism of mankind can transcend the sensuality of the animal brain. It is perhaps not too extravagant to claim that the faculty of imagination is an acquisition as far advanced beyond that of sensitiveness as life is beyond non-life. When an abstract conception was formulated for the first time, a man was born, and a marvellous new quality introduced into the world. For imagination, though expressed through the medium of material and ephemeral apparatus, can break the bonds of physical restraint, finding freedom and immortality among the eternal verities. Imagination is the gateway to wisdom, and an antidote to cleverness.

The growth of the imaginative faculty has produced, or perhaps can produce, a remarkable revolution ; for its most obvious result has been a complete inversion of the technique of life. The quality of a man is measured by his recognition and exposition of such qualities as honesty, sympathy and unselfishness, rather than by his skill in ruthless self-aggrandisement. Truth, chivalry and kindness are inconsistent with the struggle for existence ; but they are recognised as desirable attributes even by those under-developed minds that class them as impracticable ideals. A ' realist,' who boasts that he ' faces facts,' denies his humanity and takes pride in beastliness ; an ' idealist,' who faces noble thoughts, is a man.

The human race is very young, and few of its members have as yet shown enough precocity to visualise, let alone to attain, the ideal that is humanity. To mankind in the mass a real man is a sort of ' foreign devil,' to be treated as animals treat aliens in their preserves. Prophets are stoned by their generation, even though they are sentimentally canonised by the next. Philosophers are ' such stuff as dreams are made on,' and therefore unintelligible and irritating to animals, however clever. But men can learn ; their capacity for appreciating wisdom shows that its acquisition is not beyond their powers. And wisdom, which makes men human, is better than the rubies of material success that may leave him bestial.

The contrast between the attitude of imaginative insight and that of animal instinct is nowhere more clearly seen than in the realm of ethics and morality. Every action that savours of the struggle for existence is a sin, and every effort in the reverse direction is a virtue. There could be no clearer illustration of the power of the imagination to see beyond knowledge than the pronouncement that ' the wages of sin is death,' made centuries before the laws of evolution were suspected.

The exponents of religion, in spite of the Laodicean spirit of compromise that lessens their effectiveness, give more than lip-service to the creed that man should be different from the other animals. A multitude of organisations directly or indirectly sponsored by the Churches attempt to translate this pious belief into practical service. Science, especially in its medical branches, caters for all sorts and conditions of men with selfless devotion. Some enactments of legislation definitely encourage humanity as an alternative to brutality, and extol principles above opportunism. Perhaps some day even financiers and statesmen may discover that their choice is between the Mammon of deceit and animal avarice and the God of truth and human sympathy, and that there is no middle course. Until then they will continue to lead their dependents and subjects along the well-worn track that opens before all the ' beasts that perish.'

The alternative is a great adventure, whose end none can foresee, along the trail blazed by martyred pioneers who have had the courage to be men.

THE ATMOSPHERES OF THE PLANETS.

BY

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During the last few decades the main interests of astronomical research have shifted rapidly from the solar system outwards. The application of the spectroscope to the study of the stars and nebulæ, the use of photography, facilitating the study of faint objects, shortening the time of observation at the telescope, and providing permanent records, and the construction of larger and larger telescopes have made it possible for the astronomer to study objects at greater and greater distances. Whole new fields of research have been opened up, and the exploration of these has proved so attractive and has been so productive of results that the planets of the solar system have received much less attention than was formerly given to them. Nevertheless, the planets have not been entirely neglected in recent research. The great light-gathering power of large modern telescopes has enabled spectrographs of very high dispersion to be used for the more detailed study of the spectra of the planets, and the great advances in the manufacture of plates sensitive to the infra-red region of the spectrum have made possible the investigation of a region of the spectrum whose importance arises from the fact that the selective absorptions by planetary atmospheres lie mainly in this region. It is my purpose this afternoon to summarise some of the conclusions about the physical conditions on the planets derived from the investigations of recent years.

From theoretical considerations it is possible to decide whether or not any planet may be expected to possess an atmosphere. The natural tendency of an atmosphere is to diffuse away into space. The molecules of the atmosphere are flying about in all directions at high speeds, continually colliding with one another and rebounding. In the upper layers of the atmosphere, the preponderant tendency is for them to be pushed outwards. They are prevented from escaping only by the gravitational pull of the planet.

In order to overcome this pull and to fly away into space, any particle, whether large or small, must acquire a velocity greater than a certain minimum value, determined by the mass and radius of the planet. If the radial component of the outward velocity is greater than this minimum value, the particle will escape from the planet, provided its motion is not impeded by collision with another particle.

In a simple gas, at a uniform temperature, the velocities of the molecules are distributed according to a law, discovered by Maxwell, which he first announced at the meeting of the British Association in 1859. The mean velocity of the molecules increases with the temperature, being proportional to $\sqrt{T}$; the number of fast-moving molecules with velocities much in excess of the mean velocity falls off very rapidly with increase of velocity. In a mixture of gases, the average energy of each type of molecule is the same; the lighter the molecules the faster they move in the mean.

There are definite proportions of molecules with speeds of 10, 20 or 100 times the mean speed, so that there must be a progressive loss of fast moving molecules from the upper layers of the atmosphere of any planet. The rate at which this loss takes place depends upon the relative magnitudes of the velocity of escape and of the mean velocity of the molecules. The rates of escape were calculated by Jeans. He found that if the velocity of escape is four times the mean molecular velocity, the atmosphere would be practically completely lost in fifty thousand years; if the velocity of escape is four and a half times the mean molecular velocity, the atmosphere would be lost in thirty million years; whilst if the velocity of escape is five times the mean molecular velocity, twenty-five thousand million years would be required for the loss to be almost complete. The age of the planets is believed to be of the order of three or four thousand million years, so that if the velocity of escape is as great as five times the mean molecular velocity of hydrogen the atmosphere will be practically immune from loss.

The mean molecular velocity of hydrogen at 0° C. is 1·84 km./sec. At the observed maximum temperature of the Moon, 120° C., it is 2·21 km./sec. The escape velocity from the Moon is only 2·4 km./sec., so that an atmosphere of hydrogen would be lost from the Moon almost instantly. Similarly for Mercury; the temperature of the sunlit face is found by measurement to be about 400° C. and at this temperature the mean molecular velocity of hydrogen is 2·9 km./sec., whilst the escape velocity from Mercury is 3·6 km./sec.; a hydrogen atmosphere would again be lost almost instantly. It appears that the Moon, if it had never been hotter than at present, would have lost water-vapour, nitrogen, and oxygen completely, but would have retained carbon dioxide and heavier gases; Mercury, under the same supposition, would have lost almost all its water-vapour and nitrogen and most of its oxygen, but would have retained heavier gases to a large extent. The rates of loss are likely to be underestimated because, as we shall see later, it is probable that when these bodies were young and had temperatures much higher than they now have, the loss of atmosphere during the period of rapid cooling must have been considerable. It is certain that the Moon has no atmosphere now and this is fully in accordance with expectations. The evidence of an atmosphere on Mercury is not fully conclusive, but faint and transient shadings on the planet have been interpreted by Antoniadi as indications of an atmosphere. The observations are naturally difficult, but the conclusion of Antoniadi that Mercury may possess a very tenuous atmosphere is not in conflict with the theoretical evidence. It is certain, however, that most of the original atmosphere must have been lost.

Coming to the Earth, the escape velocity is 11·2 km./sec., which is almost exactly six times the mean molecular velocity of hydrogen at 0° C. Hence the atmosphere of the Earth should be immune from loss of hydrogen and all other gases.

At the present time the amounts of hydrogen and of helium in the Earth's atmosphere are very small. The spectrum of the aurora does not contain the lines of helium, an indication that the high regions of the atmosphere cannot contain very much helium. The total helium content of the atmosphere has been estimated to be about five parts in a million. The supply is being gradually replenished by the weathering of the igneous rocks of the earth's crust, which contain uranium and thorium and also, consequently, helium. Yet the atmosphere does not now contain more than a fraction of the amount of helium that it has gained in geological times in the process of the formation of sedimentary rocks as a result of the weathering of the igneous rocks. We may therefore say that there is direct observational evidence that helium is being lost from the atmosphere at the present time. It is believed that there may be a state of equilibrium between the rate of supply and the rate of loss.

Even if the Earth had remained hot, in the early stages of its existence, for a sufficient time for the hydrogen and helium then present in its atmosphere to escape entirely, it still remains to explain how helium continues to be lost when, according to the theoretical results, which are based on the accepted principles of the kinetic theory of gases, it should be immune from loss. There is one process by which the escape of helium can be brought about. It is well known that the night sky is faintly luminous. In addition to the light from the stars there is a faint luminescence from the upper atmosphere, whose brightness seems to vary with the sun-spot cycle, being greater at sun-spot maximum than at sun-spot minimum. Lord Rayleigh has termed this the non-polar aurora. In the spectrum of the night sky the characteristic green auroral line, as well as the two red lines, are always present. These lines are emitted by oxygen atoms that are in what the physicists term a metastable state. An atom, when excited or loaded up with energy, usually unloads its energy, with the emission of radiation, within a short interval of time of the order of one hundred-millionth of a second. But a metastable state is characterised by the peculiarity that the atoms in that state have a very slight tendency to unload their energy. They may remain for an average time of a second or longer in that state before emitting their energy in the form of radiation. There is a high probability that before this occurs the atom will have collided with another atom. When a collision of a metastable oxygen atom with another atom occurs, the energy of the oxygen atom will be unloaded and converted into kinetic energy. The two atoms will rebound with a greatly increased speed. By such a collision an atom of helium could acquire a speed of more than 12 km./sec., which is greater than the velocity of escape from the Earth. Hydrogen atoms would acquire a still higher speed, but heavier atoms, such as those of nitrogen or oxygen, would not by this process acquire sufficient speed to escape. They would receive an equal amount of energy but, being heavier, they would not move so fast. The loss of hydrogen and helium from the atmosphere of the Earth is thus made possible by the fact that free oxygen is present in the atmosphere.

It appears probable that the primitive Earth must have remained hot sufficiently long for most of its initial atmosphere to have been lost. It was pointed out by Russell and Menzel that in the stars and the nebulæ neon is as abundant as argon, whereas in the Earth's atmosphere argon is five hundred times more abundant than neon. Nitrogen is far less abundant on the Earth than in the stars ; it is ten thousand times more abundant in the Sun than on the Earth. These large differences in relative terrestrial and solar abundance demand explanation, because in general the relative abundance of elements on the Earth is in close agreement with their relative abundance in the Sun and other stars. These facts can be accounted for on the supposition that the rate of loss of atmosphere was very rapid when the Earth was hot. When the cooling had proceeded sufficiently far for the escape of the atmosphere to cease, neon had been depleted to a much greater extent than the heavier argon. If this supposition is correct, much of the original oxygen, nitrogen, and water-vapour and all the original helium and free hydrogen must have been lost. As the molten Earth cooled, great quantities of water-vapour, carbon dioxide and other gases must have been evolved from the solidifying magma ; these, with the residual gases from the initial atmosphere, formed the new atmosphere which, as the Earth was then relatively cool, could not escape.

It has been recognised for more than a century that the presence of free oxygen in the atmosphere of the Earth, which we are apt to take for granted without a thought, needs explanation. Oxygen is an element that is chemically active and processes are in continual operation that are depleting the store of oxygen in the atmosphere. One of the principal sources of depletion arises from the weathering of the igneous rocks to form sedimentary deposits—sand, clay and mud. The iron contained in the igneous rocks is not completely oxidised. The greyish hue of these rocks results from the iron being present mainly in the form of ferrous oxide. During the process of weathering, much of the ferrous oxide is oxidised into ferric oxide, which gives the red or brown tints to the weathered deposits. The amount of oxygen that is withdrawn from the atmosphere by this process is very considerable and it has been estimated that during geological times the amount of oxygen thus depleted from the atmosphere is about twice the quantity now present. It is clear that some process must be in operation which replenishes the oxygen in the atmosphere. The vegetation over the Earth's surface provides the means for this replenishment. The green plant absorbs carbon dioxide from the air and uses energy from sunlight to decompose it, the energy-transformer being the green colouring matter, called chlorophyll, contained in the plant cells. The carbon is used to build up the complex organic substances found in living plants, the oxygen being returned to the atmosphere as a by-product.

The supply of carbon dioxide is in turn renewed by the decay of vegetable matter and other organic materials. During the decay of such matter, oxygen is absorbed and carbon dioxide is liberated. This carbon dioxide is again available for building up new plant cells. Whenever organic matter is buried, as in coal measures and oil deposits, so that it cannot become oxidised and decay, there is a net gain of oxygen to the atmosphere. It seems probable that the present abundance of oxygen in the atmosphere has been provided in this way and that if the coal, oil and other organic deposits could be unburied and completely burned, the whole of the oxygen in the atmosphere would be used up.

The atmosphere of Venus is in marked contrast to that of the Earth. Venus is the planet which, of all the planets, most closely resembles the Earth in size, in mass, and in mean density. It is a little smaller than the Earth, a little less massive and has a slightly lower mean density. The velocity of escape from Venus is 10·2 km./sec., a little smaller than the corresponding velocity from the Earth. This velocity is about five times the mean molecular velocity of hydrogen, and it may therefore be expected that Venus will have an atmosphere comparable with that of the Earth in extent and density. The presence of an extensive atmosphere is confirmed by observation. Her disk shows faint ill-defined transient markings, which are evidently cloud phenomena. No surface details are shown, even on photographs with infra-red sensitive plates. Photographs in ultra-violet light record cloudy markings which rapidly change their form and are of short duration.

The permanent cloud layer over Venus makes the determination of the period of rotation difficult; the cloud formations are not sufficiently long-lived to give any more definite information than that the rotation is not rapid. The spectroscopic determination of the period is difficult, but the evidence is in favour of a period of not less than 20 or 30 days. On the other hand, it is likely that the period is considerably shorter than 225 days, the period of revolution of Venus round the Sun, because it has been found by measurement that considerable heat is radiated from the dark side of the planet. Although the measurements show that the bright side sends us more heat than the dark side, the difference can be to a large extent explained by the reflection of sunlight from the cloud layer over the bright side. The small difference in temperature between the bright and dark sides is to be expected on a planet that is densely cloud-covered, the clouds acting as a blanket at night, provided that the length of the day is not too great. If the planet turned always the same face to the Sun, the difference in temperature between the bright and dark faces would be greater than is found by observation. Hence a rotation-period of several weeks seems probable.

The method used for the determination of the temperatures of the planets may be briefly described. The radiation received from the planet, or from a portion of its surface, is measured with a sensitive thermocouple or bolometer. This radiation consists of two portions: reflected sunlight and long wave-length infra-red radiation from the planet. By placing a small transparent vessel containing water in the path of the rays the true planetary radiation of long wave-length is absorbed and the amount of the radiation that is merely reflected sunlight can be determined. Knowing, in this way, the amount of the true planetary radiation, the temperature of the planet may be estimated approximately; this temperature refers to the radiating surface, and if the planet has much atmosphere the actual surface temperature may be considerably higher.

The measured mean temperatures of the planets are in close general agreement with the temperatures calculated on the assumption that for each planet there is a balance between the radiation received from the Sun and the radiation re-emitted into space. The temperature differences from one part of the surface to another depend very much, however, upon the extent and nature of the atmosphere. A dense atmosphere greatly reduces the variations of temperature across the surface and the range of temperature between day and night. The Moon provides an extreme example of rapid variations. At the lunar eclipse of January 14, 1927, Pettit and Nicholson found that the temperature of the surface dropped from $+70°$ C. to $-80°$ C. in a little more than an hour, as the result of the radiation from the Sun being cut off by the interposition of the Earth. During $2\frac{1}{2}$ hours of totality, the temperature dropped a further 40° C. But after totality had ended, the temperature rose to almost its initial value in about an hour. Venus, on the other hand, despite its long day, shows only a moderate range of temperature.

To determine the composition of the atmosphere of Venus, or of any other planet, recourse must be had to the spectroscope. Absorption in the atmosphere of the Earth is a complicating and troublesome factor. Ozone, though present in the Earth's atmosphere in very small amount, with an equivalent thickness of but a few millimetres, completely cuts off the whole spectrum below λ 2900, so that the extreme ultra-violet region is completely inaccessible to observation. Oxygen reveals itself by some strong absorptions in the near infra-red and red regions, including the A and B bands of Fraunhofer and some weaker absorptions in the visible spectrum. Water-vapour has some extremely strong absorptions in the infra-red. The terrestrial origin of these various absorptions can be established in two ways. First, by observing the spectrum of the Sun at different altitudes, the terrestrial absorptions become stronger the lower the altitude, because the air-path is correspondingly increased. Secondly, if the spectra of light from the east and west limbs of the Sun are compared, the absorptions of solar origin show a slight relative displacement caused by the solar rotation, whilst the absorptions of terrestrial origin are undisplaced.

The absorptions of terrestrial origin in the spectrum of the Sun having been identified, the absorptions produced in the atmosphere of a planet can be investigated by photographing the spectra of the planet and the Moon on the same night and at the same altitude. An absorption present in the spectrum of the planet and not in that of the Moon, or much stronger in the spectrum of the planet than in that of the Moon, must originate in the atmosphere of the planet. Another, and more delicate, method of investigation is to photograph the spectrum of the planet at a time when it is approaching or receding from us most rapidly. The motion will displace the absorptions due to the planet's atmosphere with respect to those due to our own atmosphere, and in this way the planetary absorptions may be revealed.

Complete information about the constitution of any planetary atmosphere is not obtainable, however, because many possible constituents of the atmosphere show no absorptions in the region accessible to study. Amongst such undetectable constituents are hydrogen, nitrogen, helium, neon and argon.

The investigation of the atmosphere of Venus has given no certain evidence of the presence of oxygen. Observations with the 100-inch telescope, in conjunction with the high-dispersion coudé spectrograph, have led to the conclusion that the amount of oxygen must be less than one-thousandth part of that above an equal area of the Earth. It must be remembered, however, that the observations refer only to the portion of the atmosphere above the permanent layer of cloud and this layer may be at a considerable height above the surface of Venus. More surprising, perhaps, than the failure to detect oxygen is the failure to detect the presence of water-vapour, even though the tests for water-vapour are less sensitive than those for oxygen. It would seem that the clouds on Venus must be clouds of water droplets, similar to the clouds in the Earth's atmosphere; the explanation of the apparent absence of water-vapour may be that the atmosphere above the clouds is extremely dry.

The most interesting fact about the atmosphere of Venus is the great abundance of carbon dioxide. In 1932 Adams and Dunham discovered three well-defined bands in the infra-red region of the spectrum of Venus which are not found in the spectrum of the Sun, even when setting. They were evidently produced by absorption in the atmosphere of Venus. These bands had not at that time been observed in any terrestrial spectrum. Theoretical investigations indicated that they might be due to carbon dioxide; this was confirmed when Dunham succeeded in obtaining a faint absorption, corresponding with the strongest of the bands, by passing light through 40 metres of carbon dioxide at a pressure of 10 atmospheres. Later, Adel and Slipher reproduced the three bands by passing light through 45 metres of carbon dioxide at a pressure of 47 atmospheres; the absorptions so produced were less intense than the corresponding absorptions in the spectrum of Venus. Adel and Slipher concluded that the amount of carbon dioxide above the surface of Venus is equivalent to a layer two miles in thickness at standard atmospheric pressure and temperature. For comparison, it may be mentioned that the whole atmosphere of the Earth is equivalent to a thickness of five miles at standard pressure and temperature and that the amount of carbon dioxide present in the path of sunlight, when the Sun is setting, is equivalent to a thickness of only about thirty feet. Further confirmation is thus obtained of an abundant atmosphere on Venus.

The carbon dioxide will have a powerful blanketing effect, the escape of the long wave-length radiations being greatly impeded by the absorption by the carbon dioxide. It is not improbable that the temperature at the surface of Venus may be as high as, or higher than, that of boiling water. The high temperature, the lack of oxygen and the abundance of carbon dioxide can be interpreted as indications that there cannot be any great amount of vegetation on Venus and suggest that the planet is not the abode of life.

Mars occupies a position between Mercury on the one hand and Venus and the Earth on the other, as regards size, mass and velocity of escape. The velocity of escape is 5·0 km./sec., about one-half of the velocity of escape from Venus. It may be expected that Mars will have a much thinner atmosphere than Venus or the Earth. The presence of an atmosphere on Mars can be proved by photographing the planet in light of different colours. Photographs in the infra-red show permanent markings, which are evidently surface features, whereas photographs in the ultra-violet show none of these. By photographing through filters which pass a narrow spectral region, it is found that the surface details become more and more distinct as the wave-length of the light increases. The atmosphere is extensive enough to scatter ultra-violet light to such an extent that the light cannot penetrate to the surface and out again.

The images obtained with ultra-violet light are larger than those obtained with infra-red light and the difference in size indicates that the atmosphere extends to a height of fully fifty miles above the surface.

The polar caps provide additional evidence of an atmosphere on Mars. As the summer advances over one hemisphere the polar cap gradually shrinks and disappears whilst the opposite cap, with the advance of winter, forms and grows. These changes are to be explained by the melting or deposition of ice, snow or hoar-frost, for the temperature is not low enough for the caps to consist of solid carbon dioxide. From the rate at which the caps decrease as summer advances it can be calculated that they are not more than a few inches thick, so that the whole quantity of water contained in them would be sufficient to make a lake of only moderate size. The caps are more prominent in ultra-violet than in infra-red photographs and are therefore partially atmospheric; in winter, there is a permanent cloud layer above the pole.

Photographs by Wright, at the Lick Observatory, in light of different colours have given further confirmation of an atmosphere in the occurrence of clouds. The clouds are of two different types. One type of cloud is most prominent in the ultra-violet photographs. Such clouds must occur fairly high up in the atmosphere and must be sufficiently thin to allow the infra-red light to pass through; these clouds have a tendency to begin to form at about Martian noon and to grow during the afternoon. It is probable that they are produced by the condensation of water-vapour, with the fall of temperature that begins at noon. The second type of cloud is seen on the infra-red, but not on the ultra-violet photographs. Such clouds appear yellowish to the eye. They must be at a fairly low level in the atmosphere and the yellowish hue is no doubt caused by atmospheric absorption.

All attempts to detect oxygen in the atmosphere of Mars have been unsuccessful. It can be concluded that the amount of oxygen is not more than one-thousandth part of the amount in the Earth's atmosphere. The red colour of Mars, which is unique among the heavenly bodies, provides indirect evidence of oxygen, suggesting rocks that have been completely oxidised. We may contrast the colour of Mars with the grey or brownish rocks of the Moon, which have not been oxidised. It appears probable that Mars may be a planet where the weathering of the rocks, followed by their oxidation, has resulted in the almost complete depletion of oxygen from the atmosphere.

The amount of water-vapour in the atmosphere of Mars is so small that it can be detected only under the most favourable conditions. At the Lowell Observatory, which is at an altitude of 7,250 feet, Slipher, in

1908, by comparing the spectra of Mars and the Moon when at the same altitude under conditions of exceptional atmospheric dryness in the winter, found that the water-vapour absorptions were slightly stronger in the spectrum of Mars than in that of the Moon.

Carbon dioxide has not been detected in the Martian atmosphere, which is not surprising since carbon dioxide must be present in large quantity before the absorptions in the region of the infra-red available for investigation can be detected. There is some evidence of the existence of vegetation on Mars. Seasonal changes in form and coloration of the dark areas, light green changing to a darker green, and then to yellow and brown, seem to be reasonably well established. The interpretation of these changes as due to the seasonal growth of vegetation is plausible. The presence of some carbon dioxide in the atmosphere may therefore be inferred. Mars appears to be a world in the state that the Earth will ultimately reach when the oxygen in the atmosphere will have been almost entirely exhausted by the progressive weathering and oxidation of the rocks.

The major planets, Jupiter, Saturn, Uranus and Neptune, may be considered together. They are large massive planets, of low mean density, whose visible disks are considerably oblate. Their masses range from 317 times the mass of the Earth, in the case of Jupiter, to 15 times the mass of the Earth, in the case of Uranus. The mean densities of Jupiter, Uranus and Neptune are not greatly different from that of the Sun, which is 1·4 times the density of water; Saturn has the lowest mean density of any of the planets, only seven-tenths that of water. The velocities of escape from all the major planets are so high, from 21 km./sec. to 60 km./sec., that extensive atmospheres are to be expected containing an abundance of the light constituents, hydrogen and helium, which have been lost from the atmospheres of the medium-sized planets.

The telescopic appearance of Jupiter and Saturn confirms the existence of dense atmospheres. Markings in the form of belts parallel to the equator may be seen; these are of complex structure and their details are continually changing. Photographs in the infra-red show many differences from those in the ultra-violet, due to the greater penetration of the long-wave radiations into the atmosphere, but again the recorded features are continually changing, so that the infra-red light does not penetrate to the surface. Uranus and Neptune are too distant for detailed study of their surfaces, though faint belts parallel to the equator may be seen on Uranus.

Some theoretical results of interest have been obtained from the oblateness of these planets and the changes in the orbits of their satellites produced by the equatorial bulges of the parent planet. From investigations of this nature, Jeffreys concluded that these planets consist of a core of rock, generally similar to the inner planets in its constitution and of about the same mean density, surrounded by ice-coatings of great depth, above which are very extensive atmospheres. If these conclusions are accepted, some inferences may be derived about the thickness of the ice-coating and the depth of the atmosphere.

According to the calculations by Wildt, the rocky core of Jupiter has a radius of about 22,000 miles, so that it occupies only one-eighth of the whole volume corresponding to the visible disk; the ice-coating is 16,000 miles in thickness and the depth of the atmosphere is about 6,000 miles. The rocky core of Saturn is about 14,000 miles in radius; it is covered with a layer of ice some 6,000 miles thick, over which is an atmosphere extending to a height of 16,000 miles. The total weight of the atmosphere of Saturn is about equal to that of the rocky core. Saturn has the most extensive atmosphere of any of the planets, which explains why it has the lowest mean density and the most flattened disk of any planet.

The pressures of these extensive atmospheres are very great; at the bottom of Jupiter's atmosphere, for instance, the pressure is fully a million times the pressure at the bottom of the Earth's atmosphere. At a relatively small depth in the atmosphere, the pressure is great enough to compress the gas to a density nearly equal to that of the corresponding liquid. It is stated by Wildt that at the bottom of the atmospheres the pressure is great enough to solidify even the permanent gases.

The densities of the atmospheres are low; according to Wildt's calculations they are 0·78 for Jupiter and 0·41 for Saturn. This enables most of the possible constituents to be excluded, for all known gases, in the liquid or solid state, have densities exceeding 0·3, with the exceptions of hydrogen and helium. Frozen oxygen, for instance, has a density of 1·45; nitrogen, 1·02; ammonia, 0·82. In addition to helium and hydrogen, the only gases whose densities in the liquid or solid state are less than the density of the greater portion of the atmosphere of Jupiter are the hydrocarbons, methane and ethane. There seems to be no escape from the conclusion that the atmospheres of the major planets must contain large quantities of free hydrogen and helium. This conclusion is in accordance with expectation. The planets are believed to have been formed in some way or other from the Sun, which is known to contain a large amount of hydrogen, to the extent of about one-third part by weight. Helium, oxygen, carbon and nitrogen are abundant in its outer layers. Massive planets, like the four major planets, would retain their light constituents; hydrogen and helium are therefore to be expected to be present in large amount in their atmospheres.

The spectra of the major planets are of great interest. In the early days of spectroscopy Huggins discovered visually a strong absorption band in the orange and several weaker bands in the green in the spectrum of Jupiter. These bands appear more strongly in the spectrum of Saturn, but are not found in the spectrum of the rings—a conclusive proof that they originate in the atmosphere of Saturn. Uranus and Neptune show for the most part the same bands with still greater intensity, together with some additional ones. The great increase in the selective absorption from the yellow into the red and infra-red from Jupiter to Neptune accounts for the green colour of Uranus and Neptune; most of the red and yellow regions of their spectra are lost by absorption. The investigations of Slipher during recent years have extended the spectra far into the infra-red to beyond λ 10,500 and have revealed several intense bands in that region.

The origin of these bands remained unknown until a few years ago. They had never been observed in the laboratory. Then Wildt succeeded in proving from theoretical investigations that certain of the bands agreed in position with bands of ammonia and that others agreed in position with bands of methane or marsh-gas. These theoretical conclusions were confirmed by Dunham, who, with the 100-inch telescope using much higher dispersion than had been available to Slipher, was able to obtain a more complete resolution of the bands into their component lines and found a complete coincidence. Dunham estimated that the quantity of ammonia gas producing the absorptions in the spectrum of Jupiter is equivalent to a layer 30 feet thick under standard conditions. The amount is less in Saturn. The ammonia absorptions are not detected in the spectra of Uranus and Neptune.

Methane is present in much larger amount. Adel and Slipher, in 1935, found that a 45-metre path of methane, at a pressure of 40 atmospheres, gave bands intermediate in intensity between those of Jupiter and Saturn. The much greater strength of the methane absorptions in Uranus and Neptune is probably accounted for by the lower temperatures of these planets. The ammonia must be frozen out of their atmospheres, making it possible to see through them to a greater depth. Adel and Slipher estimated that 25 miles of methane at atmospheric pressure would be required to give absorptions as strong as those of Neptune.

The higher gaseous hydrocarbons, ethane, ethylene and acetylene, have been looked for in vain in the spectra of the outer planets. All the absorption bands appear to be accounted for by ammonia and methane. It is a grand slam.

The presence of ammonia and methane in the atmospheres of the large planets is not surprising. It is to be expected as a consequence of the large amount of hydrogen in the atmospheres. The picture, as painted by Russell, of the successive developments is as follows. When the major planets were hot, the hydrogen and helium was mixed with water-vapour, nitrogen and carbon dioxide. When the temperature fell below about 300° C., the carbon dioxide reacted with some of the hydrogen to produce methane and water-vapour, the partially reduced oxides of iron on the rocky surface exposed to hot hydrogen acting as a catalytic agent. With further cooling, at about the temperature at which the moisture began to condense, the free nitrogen would react with hydrogen to produce ammonia. There would then be an atmosphere of hydrogen, helium and other inert gases, mixed with methane, ammonia and water-vapour, but with little or no carbon dioxide or free nitrogen. Below this there would be a deep ocean, strongly alkaline from the ammonia in solution. As the temperature fell still further, the ocean would freeze. It may be mentioned that an ocean consisting of one part of ammonia to two parts of water would freeze at − 100° C.; all the four major planets are colder than this. The only constituents in the atmospheres that are capable of detection are ammonia and methane.

It used to be thought that the rapid changes shown by the markings on Jupiter were indications that the planet was hot. It was believed that it still retained a great amount of its original heat. The theoretical considerations of Jeffreys and the direct measurement of the temperature of Jupiter—which give a value of about − 135° C.—have shown that Jupiter must be intensely cold. The presence of ammonia and methane in its atmosphere provides further confirmation, if any lingering doubt remains, for, if Jupiter were hot, these gases would be dissociated. The ultra-violet radiation from the Sun gradually breaks up the molecules both of ammonia and of methane even at low temperatures. In the absence of oxygen, the break-up is followed by a natural recombination. From the quantity of ammonia observed to be present in the atmosphere of Jupiter, Dunham has concluded that the temperature cannot be lower than about — 120° C., if there is a large excess of hydrogen in the atmosphere. This is in close agreement with the directly observed value.

The ammonia in the atmospheres of Jupiter and Saturn must be nearly on the point of condensation and the clouds over these planets may consist of droplets of liquid ammonia or even small crystals of frozen ammonia.

The mean temperatures of Uranus and Neptune due to solar radiation alone are about — 200° C. and — 220° C. respectively. At the temperature of Neptune the methane must be nearly ready to condense.

The nature of the planetary atmospheres, about which so little was known until recently, seems now to have been solved in its broad outlines. There are many details still not understood, such as the nature of the disturbances that continually occur in the atmosphere of Jupiter, and the cause of the colorations; and it still remains a puzzle whether there is water or water-vapour on Venus. As a brief summary we find that we can divide the planets and their satellites into three groups: the small ones, entirely devoid of atmospheres; the middle-sized ones, with atmospheres of moderate extent, devoid of hydrogen or hydrogen compounds but containing oxygen or compounds of oxygen; and the large ones, with very extensive atmospheres, devoid of oxygen or compounds of oxygen but containing hydrogen and compounds of hydrogen.

APPENDIX

A
SCIENTIFIC SURVEY
OF
CAMBRIDGE
AND DISTRICT

PREPARED FOR
THE CAMBRIDGE MEETING
1938

BY VARIOUS AUTHORS

EDITED BY
H. C. DARBY, M.A., Ph.D.

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MAPS AND DIAGRAMS

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CHAPTER ONE

THE GEOLOGY AND PHYSIOGRAPHY OF THE CAMBRIDGE DISTRICT

Edited by O. T. Jones, F.R.S.

(With contributions by W. G. V. Balchin, A. G. Brighton, E. C. Bullard, H. Godwin, O. T. Jones, W. V. Lewis, and T. T. Paterson)

In Cambridgeshire and the surrounding country (Fig. 1), three broadly contrasted areas can be readily distinguished: (i) the Chalk escarpment to the east and south-east; (ii) the western plateau; and (iii) the Fenland occupying the northern part of the district. The area is drained mainly by the Cam and the Ouse, which flow from the upland, through the Fenland to the outfall at King's Lynn. In the Fens they are joined from the east by the Lark, the Little Ouse, and the Waveney, which drain the Chalk region east of Mildenhall, Brandon, and Stoke Ferry.

(i) *The Chalk Escarpment*, in the south and east, reaches its greatest height (549 ft. above O.D.) near Therfield, south-west of Royston; it declines north-eastward (to 400 ft. and below) towards Bury St Edmunds, and descends to still lower levels farther north-east. This watershed is crossed by three main depressions. One of these is followed by the Cambridge-Liverpool Street branch of the L.N.E.R. from Chesterford to Newport; the other by the Cambridge-King's Cross branch between Hitchin and Stevenage; while the third lies some miles to the east, and joins the valley of the Little Ouse with that of the Waveney.

The escarpment is determined largely by the Chalk Rock which outcrops near its brow; the overlying Upper Chalk leads down to Eocene beds on the fringe of the London Basin and is almost wholly covered by glacial drift.

From the low ground occupied by the Gault around Cambridge, the Chalk rises in gentle undulations to the brow of the escarpment (Fig. 4). Among these undulations the effect of certain hard bands in the Chalk, such as the Totternhoe Stone (or Burwell Rock) and the Melbourn Rock, can be distinguished by minor escarpments and dip slopes. The general character of this area is that of rounded ridges with intervening hollows carrying, at the present time, little surface drainage. It is traversed by shallow coombes (mainly dry valleys) which trend in a general north-west–south-east direction. The Gogmagog ridge (rising to 222 ft.) is a prominent feature near Cambridge (see Fig. 8).

Around Mildenhall, Brandon, Thetford and Lakenheath, the characteristic features of the Chalk upland are modified by a covering of gravels and sands that occupy the area known as Breckland. In places, deep circular depressions formed by solution reveal the presence of the underlying Chalk. Some of these depressions are very large and contain water more or less permanently, e.g. the Devil's Punch Bowl, 4 miles north of Thetford. Other depressions, which are probably also solution hollows, hold the well-known meres of the Thetford district, remarkable for the fluctuations of their water levels, and even for their complete desiccation at certain periods.

(ii) *The Western Plateau* lies south of Madingley along the Cambridge-Bedford road. From Eltisley, the plateau extends southwards for about 7 miles and occupies a considerable area; the surface stands between 200 and 250 ft., and, when viewed from a distance, looks remarkably even. Originally, it had a much wider extent both eastwards towards the lower part of the Chalk scarp, and westwards: its present limits are the result of dissection by the streams of the Ouse and Cam drainage systems. Large areas have, however, escaped dissection. The plateau is due in the main to a covering of Chalky Boulder Clay which overlies rocks ranging from Oxford Clay to Chalk. These rocks are exposed only on the dissected slopes of the plateau.

(iii) *The Fenland* occupies the northern part of the district. At one time the Chalk uplands of Norfolk were continuous with those of Lincolnshire, stretching across what is now the Wash. This ridge of Chalk was worn away by the action of rivers and the sea, and behind the ridge the Fenland was carved out of soft Jurassic clays—the Oxford, Ampthill and Kimeridge Clays. The surface of this Jurassic plain was uneven, and its higher portions projected above the general level to become the "islands" of the historical period. The whole region emerged from the various phases of the Ice Age with its islands capped with Boulder Clay, but with its basin nature unchanged. Subsequent time has witnessed the filling up of this basin.

The modern surface, therefore, is composed of post-glacial deposits consisting of alternating layers of peat and silt or clay (Buttery Clay), which rest on a foundation of formations ranging from Oxford Clay to glacial or post-glacial sands and gravels (March Gravels, etc.).

RIVER SYSTEM

Broadly, the Cambridge district may be regarded as an immature peneplain, in which the influence of the varying resistance to erosion of the formations has not yet been obliterated. The area was invaded more than once by ice which has left behind a cover of Chalky Boulder Clay. The removal of this drift cover has re-exposed the rock formations and led to renewed differential erosion. During the retreat of the ice, deposits of

Fig. 1.

The relief of Cambridgeshire and the surrounding country.

coarse gravels and sands were laid down probably in glacial lakes. These now occur as ridges, which were formerly attributed to a system of river valleys older than the present Cam drainage. Gravels and sands near the eastern end of the Gogmagog ridge have been explained as outwash products from an ice sheet with an ice-contact slope along the south-west margin (Linton and Hildersham).

In the south of the area, the drainage consists of the tributaries of the River Cam which join to form the main river above Cambridge (Fig. 3). The Rhee, also known as Cam or Rhee, rises in the group of powerful springs which issue below the road at Ashwell (Herts), and flows as a strike stream mainly on the Gault to Trumpington, where it is joined by the Cam (also known as Cam or Granta), which rises about 4 miles south of Newport, and flows northward along the Chesterford-Newport depression.[1] This depression is underlain by a deep, narrow channel eroded into the Chalk to a depth of at least 20 ft., and possibly even to 100 ft., below O.D. It is filled largely by loams and sands, apparently laid down in water. A third tributary, known as the Granta, descends across the face of the Chalk escarpment from the neighbourhood of Bartlow and Linton. Of these, the Rhee and the Granta are related to the dip and strike of the rocks, whereas the Cam has been determined by the events that formed the above-named depression. The Bourn brook, which drains the western plateau, rises near Eltisley and enters the Cam above Cambridge, just below its junction with the Rhee (see Fig. 3).

It is not improbable that the three major depressions which traverse the Chalk escarpment were eroded by streams flowing from the north and west as consequent streams down the general dip slope of the Chalk, and that the headwaters of these streams were captured by the development of consequent or strike streams (e.g. the Rhee), leaving these depressions as wind gaps. The Chesterford-Newport deep channel was probably eroded further by an overflow from a glacial lake occupying the ground near Cambridge which was hemmed in between the ice on the north and the Chalk escarpment on the south. The Little Ouse-Waveney gap may also have been an overflow channel. In various parts of the district borings have revealed the existence of channels or holes eroded to considerable depths below O.D., but the origin of these is as yet unexplained. The valleys of the main rivers Cam and Ouse are occupied by river gravels in which certain well-defined terraces can be observed.

UNDERGROUND STRUCTURE[2]

Although no rock older than part of the Great Oolite Series immediately beneath the Cornbrash outcrops within the area, older rocks are known from borings, particularly near Methwold where Middle and Lower Lias

[1] The Upper Cam has apparently brought about the capture of streams which formerly flowed southwards to the London Basin.

[2] By E. C. Bullard, Ph.D.

were proved; the latter to a depth of 660 ft. below O.D. Eastward, however, at Culford and near Harwich, Cretaceous rocks rest directly upon much older strata assumed to be Lower Palaeozoic; the Jurassic system has disappeared. It would be of considerable interest to know the depth and nature of this Palaeozoic floor. To this end, Bullard, Kerr-Grant, and Gaskell have applied the refraction seismic method with some success, and have obtained the results which are summarised briefly in Fig. 2.

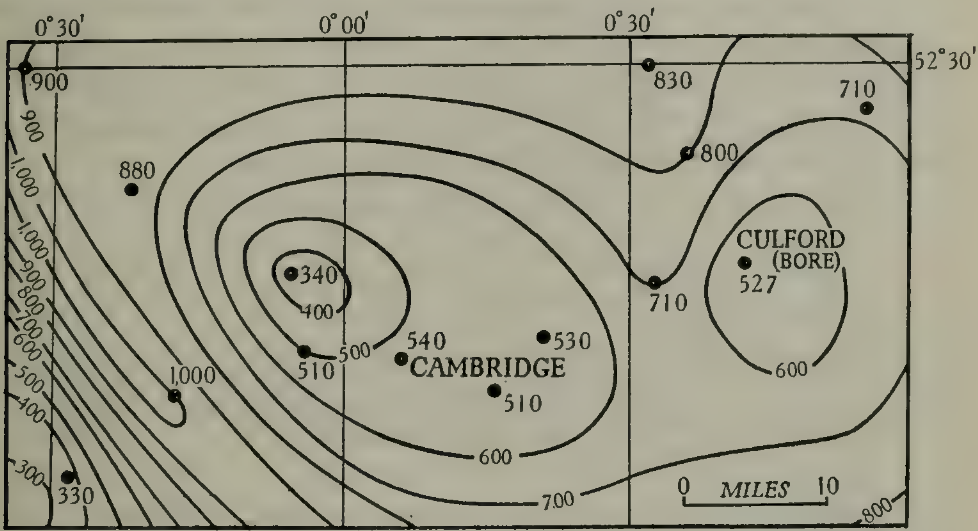


Fig. 2.

Depth of the Palaeozoic Floor in feet below O.D. Observations at further stations show that the valley to the west is less deep than is shown by the contouring above.

The stations shown above are as follows:

Cambridge and to the west		East of Cambridge	
Benefield	900	Bridgeham	710
Bourn (Cambs)	510	Culford (bore)	527
Cambridge	540	Feltwell	830
Fenstanton	340	Fulbourn	510
Houghton Conquest	330	Kentford	710
Leighton	880	Lakenheath	800
Tempsford	1000	Swaffham Prior	530

(Figures in feet below O.D.)

These results are provisional and may be slightly different from the values finally published. The experimental error, due to inaccuracies of measurement and difficulties of interpretation, is of the order of 50–100 ft. The form of the contours round the margins of the map is based to some extent on a number of seismic stations and bores which lie outside the area.

THE MESOZOIC ROCKS OF CAMBRIDGESHIRE[1]

The Mesozoic rocks of Cambridge strike roughly north-east and south-west, with a very gentle dip to the south-east.[2] The older beds (Jurassic) thus occupy the north and west of the County,[3] the younger beds (Cretaceous) the south-east. A few outliers of Cretaceous beds interrupt the Jurassic outcrops, as at Haddenham and Ely; and an anticlinal fold produces a Jurassic inlier surrounded by Cretaceous at Upware. The Cretaceous-Jurassic boundary is an unconformity; the base of the Cretaceous rests on the Kimeridge Clay at Ely, but, when traced to the south-west, this base oversteps the Kimeridge and Corallian in turn on to the Oxford Clay. These Jurassic rocks form the northern limb of an anticline with its axis (in the region of Sandy, Beds) pitching south-east.

The formations in the County may be summarised as follows:

Cretaceous	Chalk	
	Cambridge Greensand	
	Gault	
	Lower Greensand	
Jurassic	Kimeridge Clay	
	Corallian	Ampthill Clay, Coral Rag, Coralline Oolite
		Elsworth Rock Series
	Oxford Clay	

The Jurassic rocks are covered by drift in the Fens, except when they protrude to form the "islands" of Haddenham and Ely. The Cretaceous beds are largely covered by glacial deposits, both in the south-west and in the south-east (see Figs. 4 and 29).

OXFORD CLAY

Oxford Clay is found in the west of the County and beyond, but it is badly exposed. A little beyond the County boundary at Forty Feet Bridge, north-east of Ramsey (Hunts), the Geological Survey have recently collected ammonites identified by Dr Spath as *Scarburgiceras scarburgense* (Young and Bird); but the best exposure is to the south-east of Ramsey at Warboys, where the same ammonite is plentiful. The Oxford Clay is dark blue or grey, with a few thin argillaceous limestones, bands of septarian nodules, selenite and pyrites (the fossils are often pyritised). Dr Arkell has suggested that the Warboys exposure is of the *mariae*-zone;

[1] By A. G. Brighton, M.A.

[2] To the north, in Norfolk, the strike changes to approximately north and south.

[3] I am indebted to the Director of the Geological Survey for permission to include some unpublished information about the Jurassic Clays in the Fenland. Part of the area is now undergoing revision by the Geological Survey.

lower zones of the Oxford Clay are, however, worked in the well-known brick pits around Peterborough.

The relation of the Oxford Clay to the Corallian requires further research. Dr Morley Davies has recognised a non-sequence just over the boundary at Sandy (Beds), where Corallian *Exogyra nana* beds rest on the *renggeri*-zone. The Elsworth Rock Series at Upware rest on the *mariae*-zone, as does the Corallian Limestone at Warboys.

CORALLIAN

The correlation table given below is based on the work of Dr Arkell.

Zone	Main outcrop	Upware inlier
pseudocordata	Upper Ampthill Clay (Long Stanton)	
"*variocostatus*"	Middle Ampthill Clay with Boxworth Rock	? Coral Rag
plicatilis	Lower Ampthill Clay (Gamlingay) Upper Elsworth Rock Series	Coral Rag and Coralline Oolite
cordatum	Lower Elsworth Rock Series	Elsworth Rock Series

The *Elsworth Rock* is a hard ferruginous calcareous mudstone, blue in colour when fresh, and brown when weathered. It is associated with clays and sandy beds, and all the rock-types contain limonite ooliths. It is diachronic. At Upware, it is 16 ft. thick, rests on Oxford Clay (*mariae*-zone), and belongs to the *cordatum*-zone, corresponding in age to the iron-shot clays which form the lower part of the series at Elsworth. The upper part of the series at Elsworth is about 12 ft. thick; from the abundance of ammonites, the masses of Serpulae and encrusting oysters, and the ironshot lithology, it is usually regarded as a condensed deposit. To the north, at Warboys, the base of the Corallian is represented by 3 ft. of hard shelly limestone with *Exogyra nana*, and a few, as yet undescribed ammonites; this is probably equivalent to part of the Elsworth Rock Series. To the south, at Sandy, the series is replaced by clays with limestone bands.

The *Ampthill Clay* is not well exposed. The pits at Gamlingay are overgrown; the excavation at Long Stanton, described by Dr Arkell, was temporary. New pits have been opened near Manea and Mepal (where the clay includes a limestone band, possibly the Boxworth Rock). Ampthill Clay fossils have been collected by the Geological Survey at Horseway on the Forty-Foot Drain, and at Honey Bridge on the Sixteen-Foot Drain. The Ampthill Clay is darker than the Oxford Clay, and contains phosphatic

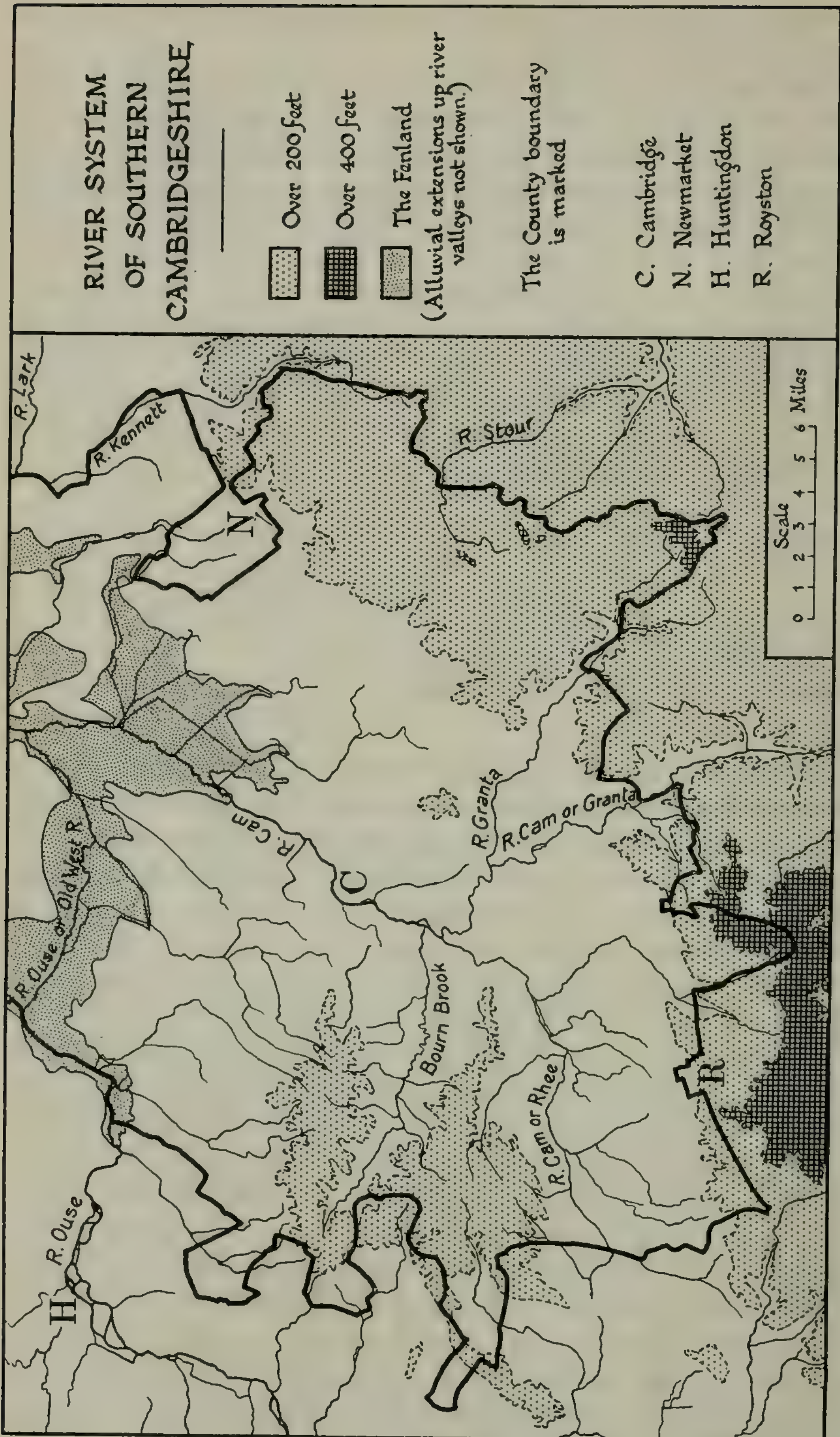


Fig. 3.
The River System of southern Cambridgeshire.

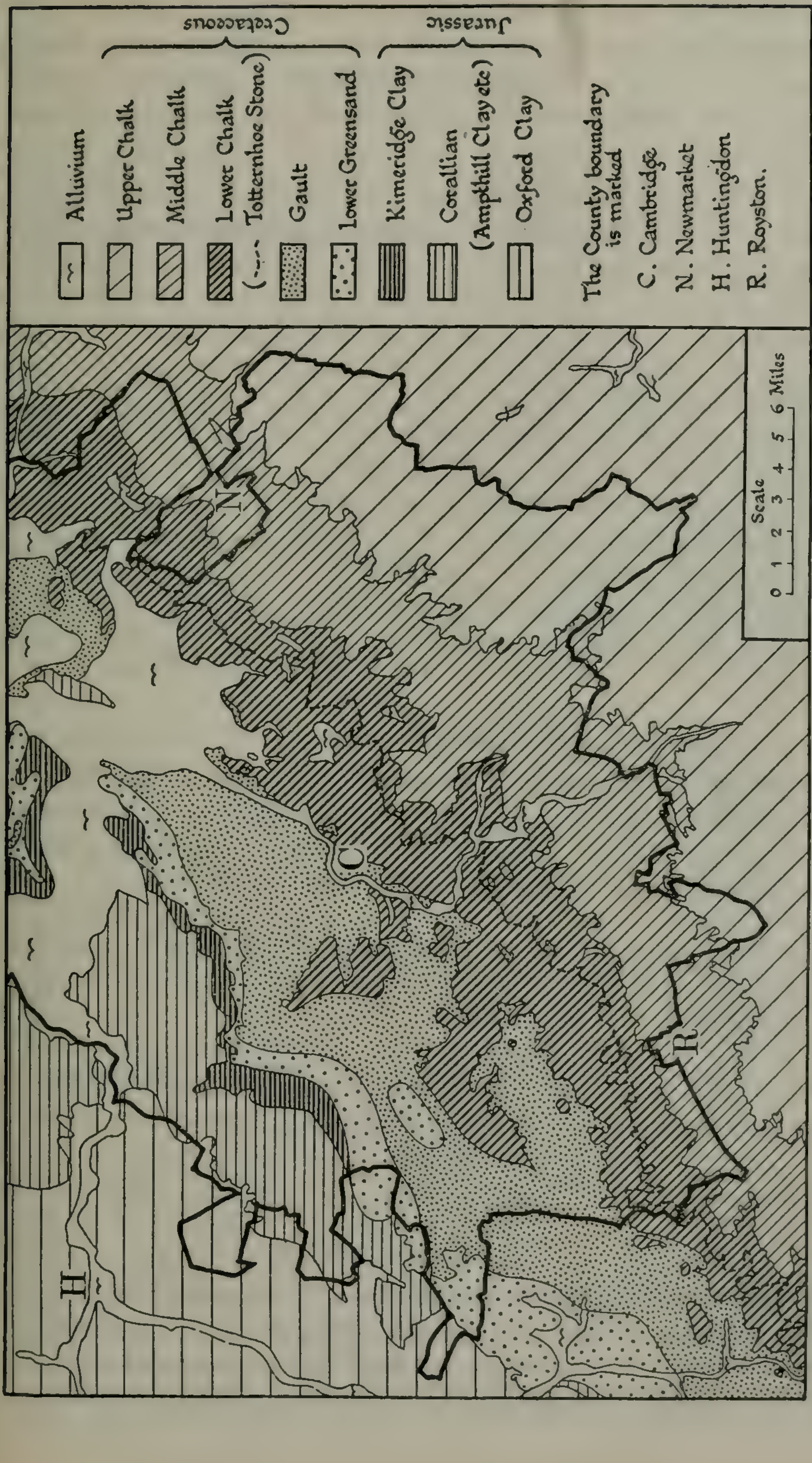


Fig. 4.
The solid Geology of southern Cambridgeshire (from information supplied by the Geological Survey).

nodules but little or no pyrites; limestone bands occur, some made up almost entirely of *Serpula intestinalis*. The fauna is muddy-water molluscan in character, with very rare echinoids in the limestone bands. To the east, in borings near Southery in West Norfolk, the Ampthill Clay is about 70–80 ft. thick.

Coral Rag and *Coralline Oolite* are known only at Upware, where Corallian limestone protrudes through the Cretaceous and extends over an area of about 3 miles by 1 mile. The Coralline Oolite is a cream-coloured limestone, full of small cavities, with large, irregularly shaped ooliths. Lamellibranchs and gastropods are common, usually as casts; the echinoids are all irregular. The Coral Rag is in places a hard compact limestone, with many lenticular colonies of reef-building corals. The characteristic fossils include thick-shelled forms, both of regular echinoids and lamellibranchs. The coral colonies are usually a few inches thick, and from 2 to 3 ft. in diameter; they are often separated and surrounded by oolitic limestones in which thin-shelled lamellibranchs, gastropods, and brachiopods occur. Attempts have been made to explain the position of the various exposures of the Rag and Oolite by hypotheses involving folding and faulting, but it is at least equally likely that these facies interdigitate.

KIMERIDGE CLAY

The *Kimeridge Clay* was described in detail by Roberts in 1892; Kitchen and Pringle have since classified some of the horizons in terms of a more modern zoning, and their results, summarised by Dr Arkell, are given in the right-hand column of the following correlation table:

Zones (Roberts, 1892)	Lithology	
Discina latissima [*Orbiculoidea*]	7 ft. papery shale	
Exogyra virgula	3 ft. grey-black shale	*Aulacostephanus*
Ammonites alternans	24 ft. clays with sandy and papery shales	
Astarte supracorallina [*extensa*]	9 in. fissile sandy clay	*mutabilis*

This succession is seen in the Roslyn (Roswell) Pit at Ely, where a band of large septarian nodules separates the two upper zones. Roberts considered beds exposed at Littleport to be older, and proposed a zone of *Ostrea deltoidea* [*delta*] to include the base of the Kimeridge as exposed around Haddenham. The upper part of the Kimeridge Clay is absent, the Lower Greensand resting unconformably on it. At Upware, a few feet of Kimeridge Clay (exact age unknown) wedges out under the Lower Greensand against the Corallian ridge. Lithologically, the Kimeridge Clay is distinguished from the other Jurassic Clays by the presence of paper shales. Near Southery in West Norfolk, Dr Arkell records, from borings, thicknesses of about 145 ft. of Kimeridge Clay, but the faunal succession is incomplete.

THE LOWER GREENSAND

The *Lower Greensand*, largely covered by drift, extends from Gamlingay in a north-easterly direction to the Fenland, where it caps the ridges at Haddenham and Ely; outcrops are also found upon the flank of the Upware-Wicken ridge. It is an important water-bearing formation. The best exposure is to the south-west of the County boundary at Sandy (Beds), where strongly false-bedded, yellow sands occur; and, about 100 ft. above the base, a pebble bed has been recorded containing derived ammonites (*Pavlovia* spp.) characteristic of the Hartwell Clay. At Upware, the Lower Greensand is represented by about 12 ft. of yellow sand, with a basal conglomerate with layers of phosphatic nodules; this oversteps the Kimeridge Clay on to the Corallian. The rare Aptian ammonites at Upware include phosphatised fine-grained internal casts, which Keeping suggested were derived; they are identified by Dr Spath as species of *Deshayesites*, characteristic of the upper Lower Aptian. There are also a few forms, with the shell preserved round a sandstone matrix, which are possibly indigenous. Unfortunately these cannot be determined with certainty, but Dr Spath compares them with *Columbiceras* which is said to occur elsewhere in the lower Upper Aptian. A badly preserved internal cast of ?*Tropaeum* sp., identified by Keeping as *Ancyloceras hillsi*, contains a similar matrix.

Keeping attempted to identify the various derived fossils found in the Lower Greensand of Upware, Potton and Sandy, and he claimed that Wealden, Neocomian, Portlandian, Kimeridgian, Corallian and Oxfordian forms are to be found. His identifications, especially those of the ammonites, are, however, in need of revision. Since the Lower Greensand rests in different places on Oxford Clay, Ampthill Clay, the Upware Corallian and Kimeridge Clay, fossils derived from these formations are to be expected. A Carboniferous trilobite has been found at Sandy, and fossils identified as of Lower Palaeozoic age at Potton (Beds).

The indigenous fossils at Upware included abundant porifera, polyzoa, lamellibranchs, and especially brachiopods. The brachiopods of Brickhill at Upware were the subject of a detailed study by Keeping, who in 1883, from examination of over 15,000 specimens, arranged the species in three morphological series, which are preserved in the Sedgwick Museum.

GAULT

The *Gault* is summarised in the table below, based on the work (some unpublished) of Dr Spath, whose identifications of the ammonoid faunas of Cambridgeshire have established their horizons with greater precision than was hitherto possible.

	Age	Zone	Beds at Folkestone	Exposures in Cambridgeshire
Upper Gault	Pleurohoplitan	*dispar*		
		substuderi	XIII	[Cambridge Greensand]
	Pervinquierian	*aequatorialis*	XII	
		auritus	XI	Burwell, Barnwell
		varicosum	X	Wicken, Barnwell
		orbignyi	IX	Wicken
Lower Gault	Dipoloceratan	*cristatum* to *intermedius*	I–VIII	Not known
	Hoplitan	*dentatus*	I	Upware, Oakington
		benettianus to *mammillatus*	—	Not known

The Lower Gault is exposed, although very badly, at Upware, where a fairly large ammonite fauna typical of the *dentatus*-zone has been found; the beds are a dull tenaceous clay, but the lowest layers are glauconitic and sandy. A boring at Chapel Lane, Wicken passed through 59 ft. of Gault Clay; the top 18 ft. were calcareous clay, below which was a bed 5 ft. thick with ammonites characteristic of the *orbignyi-varicosum* zones. Ammonites were not recorded in the lower 36 ft., the base of which was sandy with a few pebbles. This section illustrates a point which must be borne in mind when reading the above table. These records are based entirely on ammonites, which are rarely found throughout the whole of a section. Further, the Gault is badly exposed. Lack of records from the upper Lower Gault, for instance, may possibly be due to non-exposure. Ammonites typical of the base of the Upper Gault have been found at Landbeach;

those of the lower part of the Upper Gault in an ice-transported boulder at Ely; and those of the *auritus*-zone in a pit one mile west of Burwell. The section at Barnwell revealed over 50 ft. of grey and blue calcareous clays, with an inconstant limestone band near the bottom. The thickness of the Gault varies between 100 ft. and 200 ft.; to the north-east it is reduced to 60 ft. at Methwold, Norfolk. Here, the rock passes laterally into a white calcareous clay, and finally into Red Chalk at Hunstanton on the Norfolk coast.

CAMBRIDGE GREENSAND

The *Cambridge Greensand* is a thin bed of calcareous clay with glauconite grains and phosphatic nodules, resting on a well-defined surface of Gault Clay, but passing gradually into the Chalk Marl above. The glauconitic grains may be foraminiferal casts; they are commonest at the base, and disappear as they are traced upwards into the Chalk Marl. The phosphatic nodules occur usually within a layer about one foot thick. They are sometimes very rare, and are commonest in the deeper depressions in the Gault surface. They, too, become fewer and smaller when traced upwards, and usually disappear earlier than the glauconitic grains. The nodules are often black or dark brown, but there are light brown examples, and all intermediate stages are to be found. Many include fossils or are internal casts of fossils; often they have adherent lamellibranchs (such as *Dimyodon nilssoni*). More rare, are pebbles of igneous and sedimentary rocks; similar pebbles have been recorded throughout the English Chalk, but are sporadic. The sudden change of lithology between the Gault and Cambridge Greensand makes the junction quite distinct. The upper surface of the Gault is irregular, and is obviously an erosion surface; small irregular tubes filled with a matrix of the Cambridge Greensand penetrate downwards into it. The passage from Cambridge Greensand into Chalk Marl is gradual, so that the thickness of the Cambridge Greensand cannot be given accurately; it is, however, approximately one foot thick, but may be more in the deeper hollows of the Gault. Lithologically, the Cambridge Greensand is the basal pebble-bed of the Chalk Marl.

The fauna recorded from the Cambridge Greensand is very large, partly because it was once extensively worked for its phosphatic nodules and therefore offered abundant opportunities to collectors, partly because the phosphatisation of its fossils increased their chances of survival. Some of the species, such as *Terebratulina triangularis*, are always unphosphatised; but the majority are internal phosphatised casts. Dr Spath considers that all the ammonites may have come from the *aequatorialis*- to *substuderi*-zones of the Upper Gault.

THE CHALK

The *Chalk Marl*, about 80 ft. thick, is a compact bluish argillaceous limestone, weathering brown. Gasteropod and ammonoid casts are characteristic. It is exposed at the Norman Cement Works, just south-east of Cambridge, and near Barrington.

The *Burwell Rock* (15 ft.–20 ft.), seen at Burwell, is well jointed, brownish in colour, and contains small brown phosphatic nodules, and an abnormal proportion of small shell-fragments. It passes upwards into the *Grey Chalk* (70 ft.), which is distinguished by curvilinear jointing, often almost horizontal and simulating bedding. *Holaster subglobosus* is common in the lower part, but is replaced by *H. gregoryi* in the upper (seen in the pit on the Golf Course on the Gogs). At the top is a variable series with yellowish laminated marl seams in which *Actinocamax plenus* reaches its greatest size: this is the attenuated representative of the Belemnite Marls.

The subdivisions of the Chalk are summarised in the following table:

	Zone	Rock-bands and Lithology
Upper Chalk	*Micraster coranguinum*	White Chalk with flints
	Micraster cortestudinarium	
	Sternotaxis planus	Top Rock
		Chalk with flints
		Chalk Rock
Middle Chalk	*Terebratulina lata*	Chalk with flints
	Inoceramus labiatus	White Chalk
		Melbourn Rock
Lower Chalk	*Holaster subglobosus*	Belemnite Marls
		Grey Chalk
		Totternhoe Stone or Burwell Rock
	Schloenbachia varians	Chalk Marl (passing down into Cambridge Greensand)

The *Melbourn Rock* (about 10 ft. thick) is a hard limestone; on weathered surfaces it is seen to be nodular. Greenish marl seams occur, and sometimes the marl wraps round isolated nodules. *Inoceramus labiatus*, *Rhynchonella cuvieri* and *Discoidea dixoni* are characteristic. Above the Melbourn Rock is white chalk with some nodular bands. The junction between the *labiatus*- and *Terebratulina*-zones is difficult to define, partly owing to lack of exposures; the total thickness of the Middle Chalk, however, is about 200 ft., the upper part consisting of white chalk with marl seams and flints.[1] Characteristic fossils of the *Terebratulina*-zone are *Terebratulina lata, Sternotaxis planus, Micraster corbovis* and a variety of *Echinocorys scutatus*, a species elsewhere diagnostic of the Upper Chalk. There are exposures near Dullingham Station, at the Linton Whiting Works, and east of Great Chesterford.

As in the Thetford district, the *Chalk Rock* of the Cambridge district occurs above the base of the Upper Chalk. At the best exposure (Underwood Hall) about 6 ft. of white blocky chalk with tabular flints underlies the Chalk Rock, which consists of hard patches of chalk embedded in soft white chalk, often with no clear-cut demarcation. Green-coated nodules are absent. Ammonoids and gasteropods are common (they are almost absent between the Chalk Rock and the Chalk Marl), and a typical *Hyphantoceras reussianum* fauna occurs. The lithology of the Chalk Rock reappears towards the top of the *planus*-zone in the *Top Rock*. This is distinguished from the Chalk Rock by the occurrence at its upper surface of a hard limestone with pinkish brown nodules, about one foot thick, with a definite top crowded with green-coated nodules, some of which are internal casts of *Micraster cortestudinarium*. The best exposure of the Top Rock is near Westley Waterless; it can also be seen south of Higham in West Suffolk.

The *Micraster*-zones of the Chalk are largely concealed under Boulder Clay; the *cortestudinarium*-zone is seen north-west of West Wratting, and the *coranguinum*-zone near Shudy Camps and Saffron Walden. The Chalk is white and flints are plentiful.

THE PLEISTOCENE DEPOSITS OF THE CAMBRIDGE DISTRICT[2]

In Cambridgeshire no exposures have been found of beds comparable in age with the Crag and Early Pleistocene deposits of the eastern parts of Norfolk and Suffolk. Coarse gravels and sands, fan-wash fingering out from the *Lower Chalky Boulder Clay* ice sheet, can be seen underlying that Boulder Clay on the high ground south-east of Cambridge—on the Gogmagog Hills and at Haverhill in Suffolk. The ice sheet advanced over a fairly deeply dissected landscape as far south as the London Basin.

[1] H. Dixon Hewitt has shown that part at least of the famous Brandon Flint Series (Suffolk) belongs to the *Terebratulina*-zone.

[2] By T. T. Paterson, M.A.

Rubbling and thrusting of the Chalk took place where the ice met the Chalk escarpment, and the consequent structures are well exposed south of Royston and on Chalk Hill, north-east of Newmarket. The Boulder Clay is generally confined to the high ground and upper slopes. It is characterised by a blue colour, and by erratics of chalk, Lincolnshire flint, Yorkshire sandstones and coals, and Scottish quartz dolerites, quartzites and granites, as well as by rocks of Scandinavian origin.

Upon the retreat of the ice, the land, several hundred feet higher than to-day, was strongly eroded. Deep, steep-walled valleys were formed, and these were subsequently filled up either by glacial drift or during a period

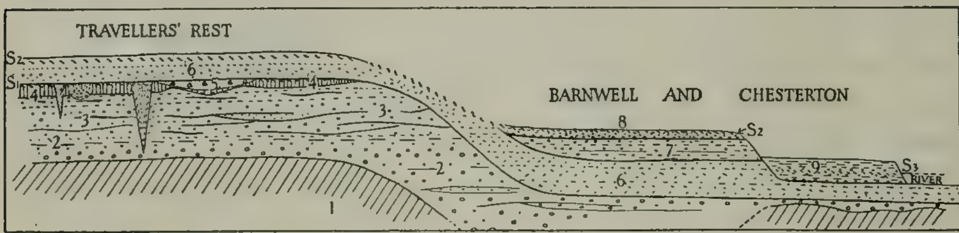


Fig. 5.

Diagrammatic Composite Section of the Terraces around Cambridge.

1. Gault with surface rucked by sludging.
2. Lower even-bedded series of Travellers' Rest Pit, with erratics derived from Lower Chalky Boulder Clay.
3. Uneven-bedded series of Travellers' Rest Pit, with rolled Lower Palaeolithic tools and cold fauna.
4. Loess-loam.
5. Upper Chalky Boulder Clay in lenses.

S_1 Solifluxion band with frost cracks and polygonal soil forms of Upper Chalky Boulder Clay age.

6. Succeeding interglacial aggradation gravels.
7. Middle Terrace gravels with warm fauna and Late Clacton-Levallois-Acheul industry.
8. Loams and gravels with cold fauna and associated solifluxion band (S_2).
9. Lower Terrace; fine gravel and silt with poorly marked solifluxion band (S_3).

of rapid aggradation and change of base level. Conglomerates and coarse gravels were deposited on the north of the main Chalk escarpment between bosses of Chalk (Barnham), Boulder Clay and outwash fans; and the finer facies were laid down as a flattened spread in the Fen region, determining the essential features of the present-day landscape (Shrubhill, near Feltwell). A warm fauna has been found in a gravel of a late stage of aggradation (Fakenham), and it is probable that the Barrington gravels are of this age. During a subsequent wet period the surface of the gravels carrying the warm fauna was sludged, and, on this sludged surface, during a drier time, brown loess-like loams accumulated (Brandon; Travellers' Rest Pit, Cambridge).

A few rolled Early and Middle Acheulean bifaces and Early Clactonian flakes have been found in the warm gravels; while *in situ* at the top and on the surface, there is a series of Middle Clactonian industries. In the brown loams, a Middle to Upper Acheulean industry occurs in many localities.

The brown loam is capped with outwash gravels heralding the onset of the *Upper Chalky Boulder Clay* ice sheet which advanced along the valleys, partially enveloping the hill slopes and penetrating almost to the London Basin at Hertford. This Boulder Clay is distinct from the earlier deposit in its brown colour, due to the included brown interglacial loam and to the large quantity of Bunter erratics. The earlier glacial and interglacial deposits are folded and overthrust. There were two advances of the ice and, during the intra-glacial period, some sands and loess-loams were deposited in isolated pools on the surface of the Boulder Clay of the first advance; there is an exposure at West Stow (Suffolk). It is probable that the High Lodge Late Clactonian industry belongs to this period, because, along with earlier forms, tools of that age appear in the gravels of Warren Hill which is an outwash deposit formed during a halt in the retreat of the ice sheet. Decalcification of Chalky Boulder Clay has given rise to a great part of the sands of the Breckland.

The three *terraces* of the river gravels are composite, and are cut out of the earliest interglacial gravels and Upper Chalky Boulder Clay. After the deposition of the latter, rapid erosion cut channels over 30 ft. deep. Subsequent aggradation filled these, and this gravel filling caps the Upper Terrace. During the succeeding interglacial phase, 20 to 30 ft. channels were cut and the Middle Terrace was formed of fine gravel carrying a warm fauna and an industry with the very latest Clactonian technique conjoined with Levallois and Acheulean (St Neots; Milton Road, Cambridge). A solifluxion layer on these gravels of the Upper and Middle Terraces, and a deposit with a cold flora, indicate a third cold period (Barnwell and Chesterton). The Lower Terrace is cut out of all the preceding deposits and is composed of fine gravel and silt with a poorly marked solifluxion level in the gravel.

THE POST-GLACIAL DEPOSITS OF FENLAND[1]

The post-glacial deposits of the Fenland occupy a shallow basin centring upon the Wash. They are of two types. On the landward side, they are composed largely or wholly of peat, which has formed as a result of an accumulation of fresh water augmented by the numerous large river

[1] By H. Godwin, M.A., Ph.D.

systems that enter the fens from the surrounding upland, and been maintained by poor drainage gradients towards the sea. On the seaward side, the fenland deposits are silts and clays laid down under conditions of greater or less salinity (see Fig. 47). Alterations in the former relative levels of land and sea have left their trace in the disposition of the two types of deposit. During a phase of marine transgression the silts and clays extended inland above the peat beds, and in the ensuing phase of regression, peat extended seawards over the silts and clays. In this way, the silts and clays from the seaward side, and the peats from the landward side, interdigitate with one another.

A much simplified scheme showing the relations of the chief fen-beds to one another is shown in Fig. 6. The history of formation of the fen deposits can be very briefly outlined as follows:

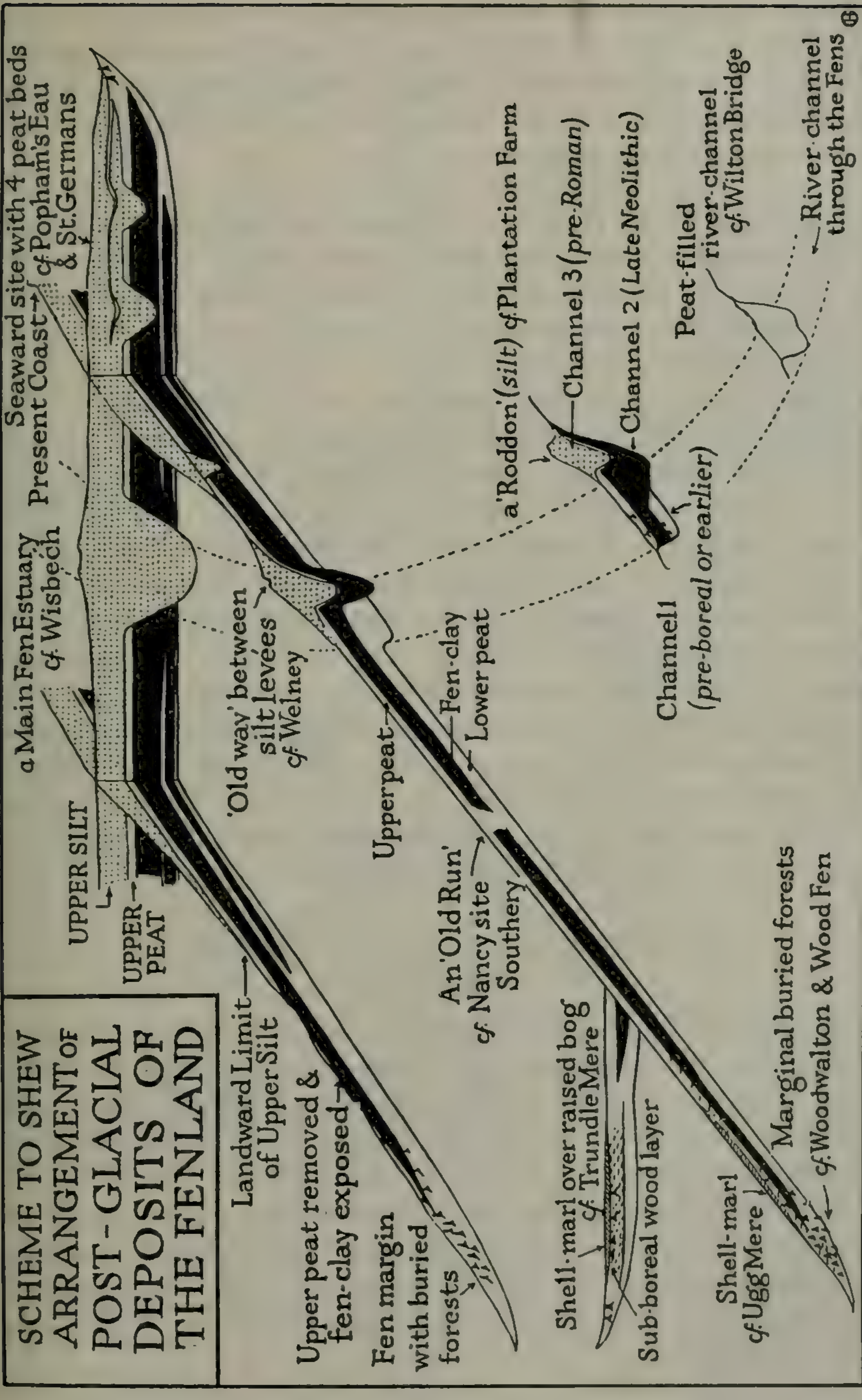
The Pre-Boreal Period. Say before 7500 B.C., a period of sparse birch-pine. Peat was forming on the present floor of the North Sea, the coast of which is now about 200 ft. below its former level. No deposits of this age are yet known from the Fenland.

The Boreal Period. From about 7500 to 5500 B.C., a period of birch-pine woods, but with oak and elm and hazel in increasing importance. In the deep river valleys of the Fenland peat-formation began during this period (e.g. in Little Ouse Valley), and at a few sites with local water supply. During this time the North Sea reached most of its present extent, but it did not directly affect the Fenland.

The Atlantic Period. From about 5500 to 2000 B.C., a warm wet climatic period marked by the sudden onset and subsequent importance of the alder and, to a smaller extent, of the lime.

At the beginning of this period, Mesolithic (Tardenois) man occupied local sand hills in the fens, but, very soon, peat-formation became widespread throughout the fen basin, and thick beds of fen sedge-peat and fen brushwood-peat were formed. Towards the end of this time the peat surface dried and became increasingly wood-covered, partly due, no doubt, to marine recession and partly to climatic dryness. There is some trace of Neolithic man at this time.

The Sub-Boreal Period. About 2000 to 500 B.C. At the end of the Neolithic or in the early part of the Bronze Age, an extensive but shallow marine invasion caused silt and clay to spread far inland over peat. Foraminifera and diatom analyses suggest shallow brackish lagoon conditions; this was the stage of formation of the Fen Clay.



SCHEME TO SHEW ARRANGEMENT OF POST-GLACIAL DEPOSITS OF THE FENLAND
UPPER SILT
UPPER PEAT
a Main Fen Estuary cf. Wisbech
Present Coast
Seaward site with 4 peat beds cf. Popham's Eau & St. Germans
Landward Limit of Upper Silt
'Old way' between silt levées cf. Welney
Upper peat removed & fen-clay exposed
Fen margin with buried forests
Upperpeat
Fen-clay
Lower peat
An 'Old Run' cf. Nancy site Southery
a 'Roddon' (silt) cf. Plantation Farm
Channel 3 (pre-Roman)
Channel 2 (Late Neolithic)
Channel 1 (pre-boreal or earlier)
Peat-filled river-channel cf. Wilton Bridge
River-channel through the Fens
Shell-marl over raised bog cf. Trundle Mere
Sub-boreal wood layer
Shell-marl cf. Ugg Mere
Marginal buried forests cf. Woodwalton & Wood Fen

Fig. 6.

In the middle and later phases of the Sub-Boreal period, semi-marine conditions were replaced by those of peat-formation. Locally, pine and birch woods developed (e.g. Wood Fen at Ely, and Woodwalton), and the fen surfaces became dry enough to have encouraged dense occupation by Bronze Age man. Along the fen margins, freedom from flooding allowed sphagnum peat to form incipient raised bogs.

The Sub-Atlantic Period. From 500 B.C. This period is generally recognised as colder and wetter than the Sub-Boreal. At this time the Fenland became very inhospitable, and was, apparently, shunned by Iron Age man. The period is marked by two stratigraphical events. In the peat fens were formed the shallow lakes which persisted into the last century (e.g. Whittlesey Mere, Ugg Mere). Their sites are still recognisable by the deposits of lake marl that formed on their beds. The seaward side of the fens was built up during the Roman times by the deposition of fine silt above the upper peat. These silts formed a broad belt round the Wash and they extended to high-tide level. They were densely occupied in Romano-British times,[1] especially in the last stages of their formation, and this is true also of the raised banks of the tidal rivers which form landward extensions of the silt country. These levées are now recognisable as "roddons", raised banks standing above the peat fens. They become increasingly evident as drainage causes the wasting of peat which formed over their flanks after the Roman period. The courses of the extinct waterways of the Fenland have been mapped by Major Gordon Fowler, and Fig. 7 summarises the available information about their extent and distribution. They are mostly of Romano-British age.

THE CHALK WATER TABLE SOUTH-EAST OF CAMBRIDGE[2]

The Fenland and much of Cambridgeshire which lies below the 50 ft. contour line are not well adapted for a water-table survey. On the Chalk uplands in the south-east of the County, however, a considerable amount of work has been done since 1935 by undergraduates of the University Department of Geography. The water levels in about 120 wells, within the region covered by Fig. 8, have been measured three times a year; and fortnightly observations have been taken (during term) at about fifteen reliable and widely distributed wells. Previous to 1929, similar measurements had been made by Prof. W. B. R. King and also by officers of the Royal Engineers.[3]

[1] See p. 92 below. [2] By W. G. V. Balchin, B.A., and W. V. Lewis, M.A.
[3] See H. J. O. White, "The Geology of the Country near Saffron Walden" (*Mem. Geol. Surv.* 1932), p. 109.

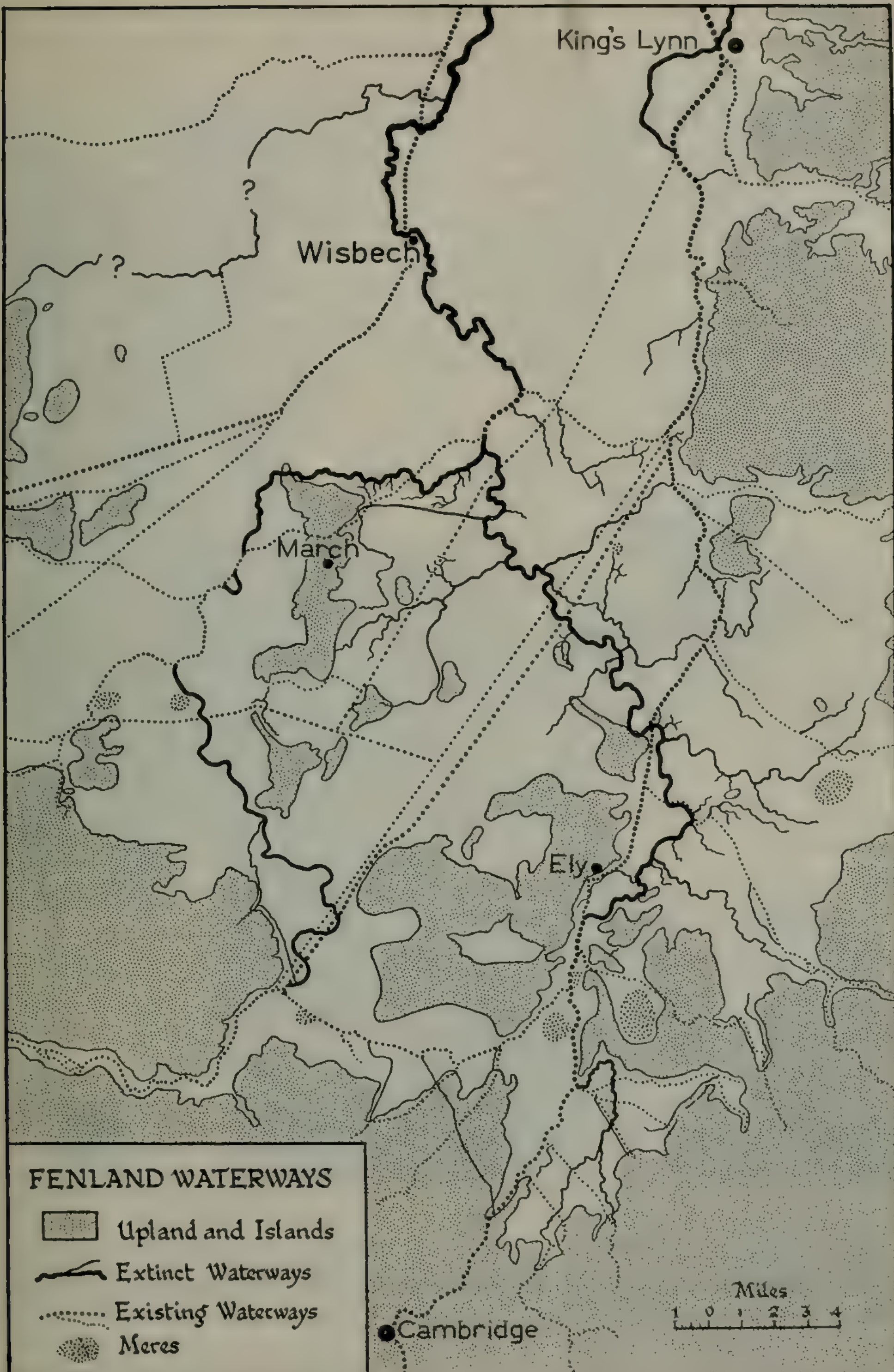


Fig. 7.

Based upon (1) Gordon Fowler, "The Extinct Waterways of the Fens", *Geog. Jour.* lxxxiii, 32 (1934); (2) additional information supplied personally by Major Fowler.

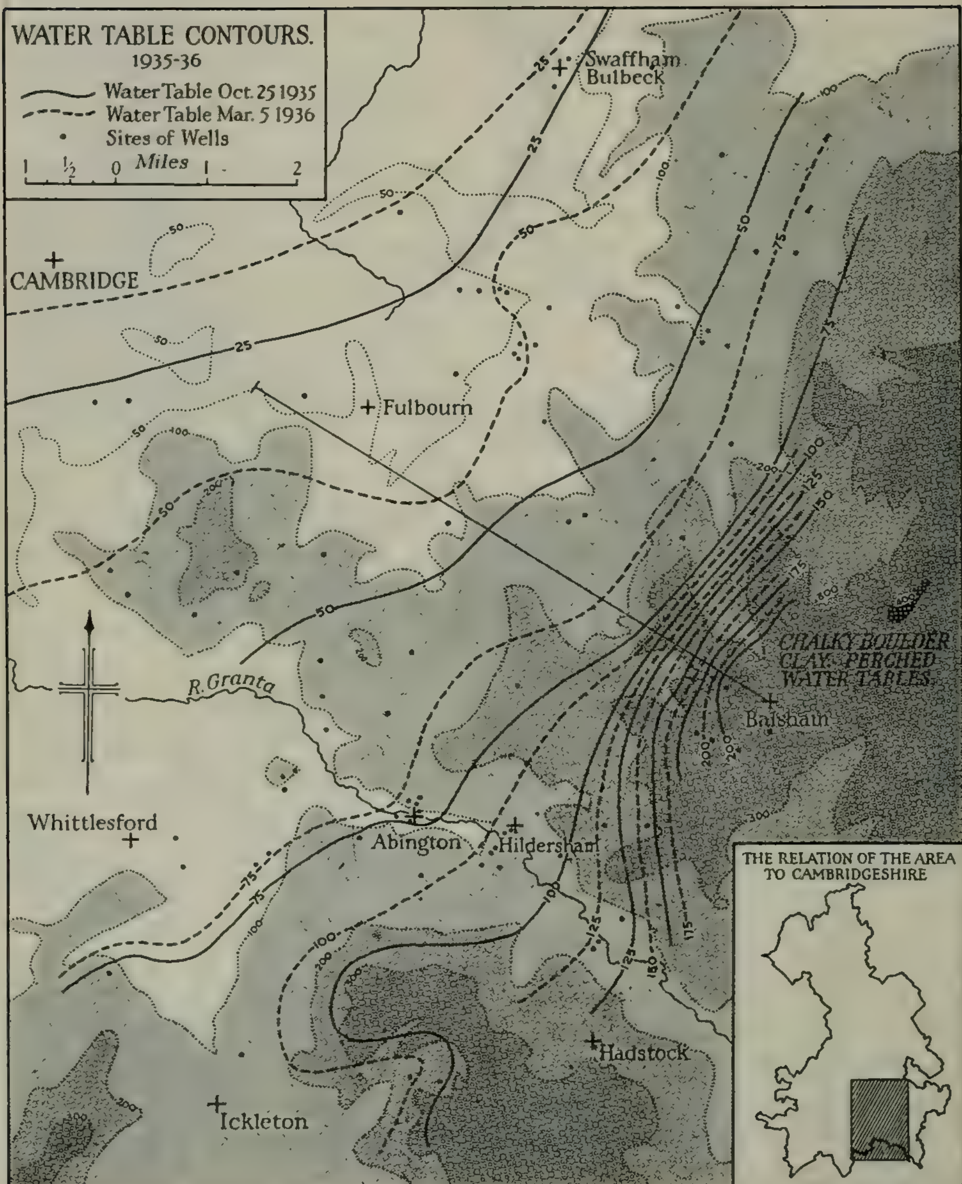


Fig. 8.

Relief Map of south-east Cambridgeshire showing Water Table contours subsequent to the drought of 1934–35. The autumn minimum and spring maximum levels are shown.

The post-1935 work has enabled water-table contours to be constructed for three different dates in each year (October, March and June), and Fig. 8 is an example of the results obtained. The October level of 1935 was selected as it came after the drought of 1934–35 and represents, therefore, an exceptionally low level. The March level of 1936 represents the spring maximum of that year, and although this was lower than the maxima for 1937 and 1938, the two readings chosen represent the greatest range within any of the three years. Although the best wells have been levelled from bench-marks, too great reliance should not be placed on the details of the contours. The errors inevitable with a party of nearly eighty students are partly counterbalanced by the large number of observations taken; but

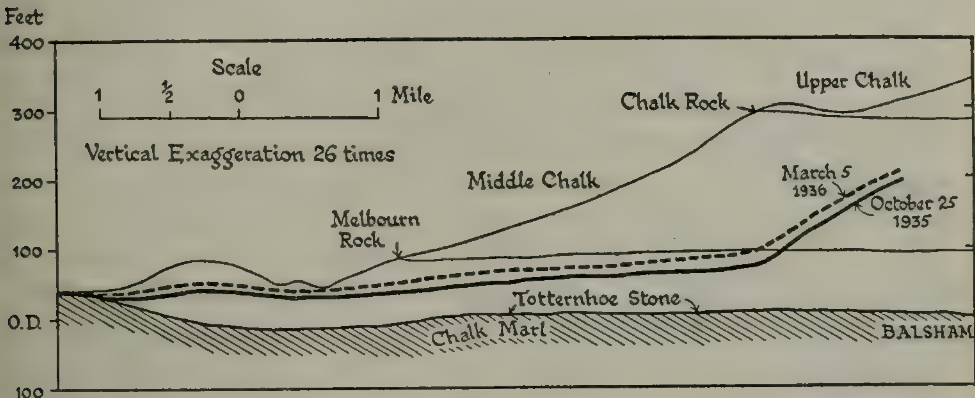


Fig. 9.

Water Table: Section along a line running north-west from Balsham (see Fig. 8).

on the other hand, the wells are irregularly distributed, and pumping, together with the recent closing of many wells, prevent great accuracy. This applies particularly to the neighbourhood of the town of Cambridge. A feature of note is the fall of the water table below the level of the Granta near Abington and Linton during October 1935; an impervious bed of alluvium presumably enabled the river to flow perched above the neighbouring Chalk water table.

The section (Fig. 9) covers the same period as the map and shows the geological formations that influence the water table. The relation between surface relief and the water table is clear, particularly in the sudden rise in level north-west of Balsham. The seasonal range is also seen to increase with the depth of the water table. The section is terminated on the west at the outcrop of the Totternhoe Stone near Cambridge, and on the east at the occurrence of several perched water tables in the Boulder Clay near Balsham. In this latter area, wells, within 40 yards of each other, differ by more than a hundred feet in their water levels.

Fig. 10 shows (1) representative rainfall figures for the county, and (2) the fluctuations in a shallow well at Great Abington. The low rainfall for the spring and summer of 1935 is reflected in the exceptionally low autumn water table. Further, a strong seasonal rhythm can be seen in the water table which is not evident in the rainfall. It is clear, particularly in 1936, that summer rainfall has little or no influence on the water table. On the

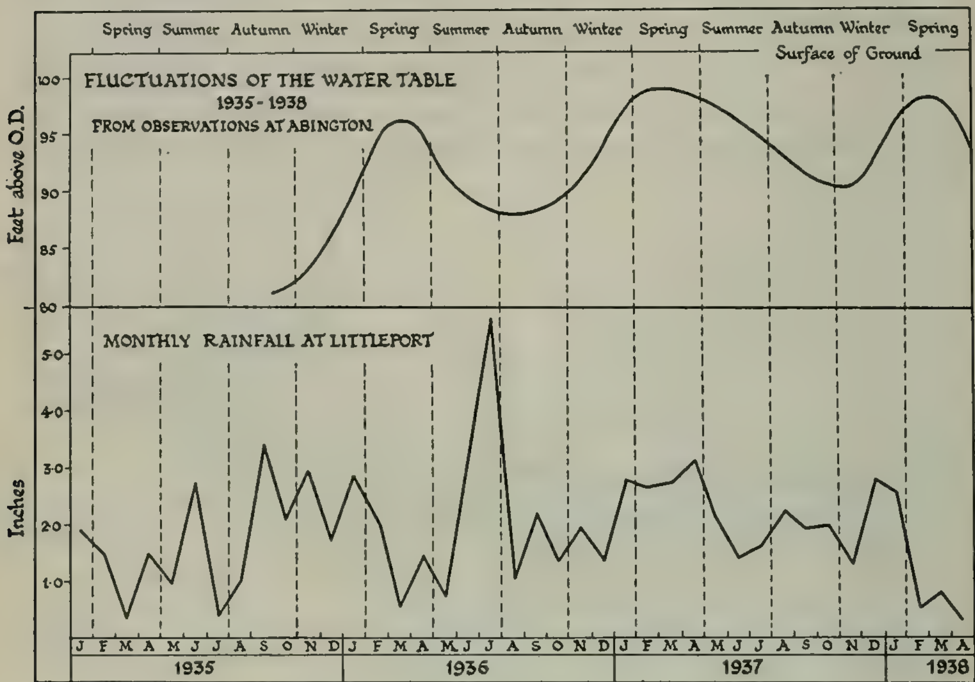


Fig. 10.

Fluctuations of water level at Great Abington in relation to the rainfall of the County, 1935–38. The rainfall for the period 1935–38 was abnormal in amount and distribution through the year. Fig. 13 below gives more representative figures for the County.

other hand, the heavy autumn and winter rainfall of 1935 probably accounts for the rapid rise in level after the drought, while the heavy winter and spring rainfall of 1936–37 explains the high and extended 1937 maximum. The drought at the beginning of 1938 is also reflected in an unusually early lowering of the water table.

CHAPTER TWO

THE SOILS OF CAMBRIDGESHIRE

By H. H. Nicholson, M.A., and F. Hanley, M.A.

WITHIN THE COMPARATIVELY LIMITED AREA OF Cambridgeshire, circumstances have combined to produce a large variety of soil conditions.[1] To begin with, the Cretaceous and Upper Jurassic outcrops from the Upper Chalk down to the Oxford Clay provide a goodly range of parent materials, and superimposed upon these are large areas of drift deposits of all sorts, from recent Alluvium to Glacial Gravel (see Figs. 4 and 29). Some of these drift deposits are so thin and diffuse as to escape notice in the Drift Maps but they are of considerable importance from the soil point of view. Furthermore, the topography of the area varies from the comparatively high hills of the Chalk escarpment to the flat expanses of Fenland, so that every type of drainage condition is encountered. The types of soil parent material and the varying drainage conditions are summarised in the adjoining table.

While this table summarises drainage conditions from the point of view of soil formation and leaching, it does not fully depict land-drainage conditions. The greater part of the Fenland is dependent on artificial drainage and the use of pumps to keep the water table at a reasonable level. Outside the fen area, the upper rivers, Cam, Granta, and Rhee, are bordered by belts of gravel and alluvium which, because of the higher and more permeable areas flanking them, are characterised by high water tables and liability to flooding. The area of Gault and Chalk Marl in the Rhee Valley is peculiarly circumstanced for drainage. An outlying ridge of chalk, north of Barrington, makes this almost a land-locked basin which, although of an altitude 70 ft. or more, is afflicted with drainage conditions not unlike those of the Fenland itself. With the exception of the Boulder Clay areas, all the clay lands of the County are both impermeable and low lying, though some of them are favoured with enough fall to make field drainage moderately simple. In the Boulder Clay country, the heavy land has the advantage of fair altitude and for the most part useful

[1] Fuller descriptions, together with analytical details and profile descriptions, are contained in Bulletin No. 98 of the Ministry of Agriculture, *The Soils of Cambridgeshire* (1936), by H. H. Nicholson and F. Hanley. Detailed information concerning the soils of fruit-growing areas in West Cambridgeshire, and in the Isle of Ely, is available in Bulletin No. 61 of the Ministry of Agriculture, *West Cambridgeshire Fruit-Growing Area* (1933), by J. F. Ward; and in Research Monograph No. 6 of the Ministry of Agriculture, *A Survey of the Soils and Fruit of the Wisbech Area* (1929), by C. Wright and J. F. Ward.

slope. The ditches are often natural watercourses, and impermeability of the formation is the only obstacle to its satisfactory drainage.

PARENT MATERIALS AND SOIL-DRAINAGE CONDITIONS

(1) WITH FREE DRAINAGE. (Parent material permeable, excess water draining to depth)

A. *Drainage Excessive.* (Considerable leaching; moisture-holding capacity poor)

LIMESTONES	GRAVELS AND SANDS ON CHALK	SANDS
Upper Chalk	Plateau Gravels	Lower Greensand
Middle Chalk	Glacial Gravels	Breckland
	Taele Gravels	

B. *Drainage Free.* (Moisture-holding power satisfactory for plant growth)

LIMESTONES	SANDS
Lower Chalk	Lower Greensand (Mid-Cambs)
Coral Rag	

(2) WITH IMPEDED DRAINAGE. (Subject to continuous or seasonal water-logging)

A. *Drainage Imperfect.* (Subsoil and parent material impermeable)

CLAYS

Boulder Clay

Gault	Ampthill Clay
Kimeridge Clay	Oxford Clay

B. *Drainage Impeded.* (High water table due to topography; underlying stratum impermeable, or low permeability)

LIMESTONES	GRAVELS	ALLUVIUM
Chalk Marl	Valley Gravels	Peat
Lower Chalk (base)	Old River Gravels	Silt
Middle Chalk (base)		

Most of the soils reflect to a marked degree the characteristics of their parent materials or the geological outcrop on which they lie, but this is not invariably the case. Textures vary from the loose sands of Breckland to the heaviest Gault, Boulder Clay or Oxford Clay soils. In general, the lime status of the soils is satisfactory or good, the only serious exception being the soils derived from the Lower Greensand (especially those in the west of the County), and some of the Gravels. This is to be expected of soils formed from cretaceous rocks, or drifts originating in the neighbourhood of cretaceous exposures, or associated with rivers of calcareous waters.

Boulder Clay soils occupy two substantial areas, one in the south-east along the Essex border, and one in western Cambridgeshire on the Huntingdonshire border. They are buff-coloured clays or clay loams, lying on clay subsoils, containing from 1 to 10 per cent calcium carbonate, much of it as rounded chalk pebbles. The depth to the grey Chalky Boulder Clay varies considerably. In some places, it can be reached within 2 ft. of the surface, but, on the lower slopes and in the smaller valley bottoms, there is often almost this depth of colluvial heavy loam material before a buff clay subsoil is reached, and this in its turn gives place to grey unweathered clay. The drainage of these soils is naturally poor, although the two areas are in the highest parts of the County. The western area is the most impermeable. On high ground, where land is flat or only gently sloping, run-off is hindered, and water-logging or surface pools are of common occurrence in a wet winter. Smaller areas of Boulder Clay soils are also found in the "islands" of the Fens.

The *Upper Chalk* formation in Cambridgeshire is covered, for the most part, by Boulder Clay. The soils on the limited exposed areas vary from thin white or grey chalky soils to brownish grey loams, depending on the proximity of the Boulder Clay and the extent of downwash from it.

The exposure of the *Middle Chalk* occupies a big proportion of Cambridgeshire, and is covered by two main classes of soil. One is a thin grey or brownish grey chalky loam, consisting mostly of fragments of chalk, and lying directly on raw chalk. This chalky, or "whiteland", type occupies the higher slopes and summits especially along the flanks of the exposure. The other is a warm brown or reddish brown loamy sand, up to 20 in. deep, lying on the chalk. This "redland" type consists chiefly of coarse sand with only a small percentage of calcium carbonate. It fills the lower slopes and flatter areas, especially around the numerous patches of *Gravel* scattered along the outcrop. The soils on these Gravels are dissimilar only in respect of their greater content of flint pebbles. All the soils on the Middle Chalk are, with the exception of hollows and limited areas on the north-west edge of the outcrop, characterised by very free drainage. This constitutes their chief drawback, and the farmer's great difficulty is to conserve sufficient moisture in the soil to carry the crops through the growing season.

The *Lower Chalk* soils differ from those of the Middle Chalk, chiefly in that they are less deep but heavier in texture and have a narrower range of colour. They are free draining but the water table, in general, comes nearer the surface, and, in places, field drainage is necessary. Both in the Lower Chalk soils and in a few of the low-lying parts of the Middle Chalk, yellow mottling, the characteristic sign of impeded drainage, is to be found in the subsoil chalk within 18 in. of the surface.

The *Chalk Marl* soils occupy low-lying ground, much of it bordering on the Fens. Both colour and texture vary as a result of surface admixture with drift materials. The soils are generally brownish grey, marly, medium to heavy, loams lying on yellowish grey marly subsoils, often with yellow or orange mottling due to high water tables. Coprolites are plentiful in the surface soil,[1] especially in those parts of the outcrop bordering that of the Gault.

The *Gault* soils are dark brownish grey in colour, and form the heaviest of the clay soils. They lie on a buff-coloured clay subsoil, which merges into blue-grey clay with orange mottlings. The formation is impermeable, as are the soils which lie on its surface, except in so far as they are opened up by tillage or by admixture with sand and gravel from neighbouring formations. Consequently, the land tends to lie wet or water-logged in its natural condition, except during dry seasons. The soils, however, have the advantage of being calcareous (2 to 10 per cent of calcium carbonate, increasing to 30 per cent or more within 3 ft. of the surface). This calcium carbonate is not present as lumps or pebbles, but is finely disseminated through the soil and so assists the formation of good tilths.

There is not a very big outcrop of *Lower Greensand* but it is important, being associated with intensive market gardening in the western part of the County, and with fruit and flower culture in the centre of the County. The soils are rich brown, loamy sands in the west, with more mellow sandy loams to the centre. The parent material is a coarse quartz sand, highly permeable, so that the chief features of the soils are their coarse open character, very free drainage, low content of organic matter and bases, frequently acidic reaction with signs of leaching and the formation of iron-pan in the subsoils. This pan in some cases, and the proximity of underlying clay in others, gives rise to localised patches where drainage is impeded and the subsoil is mottled.

The *Kimeridge Clay* gives rise to dark grey-brown clays and heavy loams, often with a poor reserve of lime. They occupy low-lying flat areas, and their chief handicaps are their poor tilth potentialities due partly to difficulties of draining and partly to their low content of calcium carbonate.

The incidence of thin washes of drift materials gives to the other Jurassic clay soils (*Oxford Clay* and *Ampthill Clay*) a character of their own. The surface textures are varied and there are wide variations in the soil profile. They are all on the heavy side, but the top soil is frequently much lighter than the subsoil. Their poor reserve of calcium carbonate, however, is a factor against the easy production of good tilths. Low lying and adjacent to the Fenland, their lack of fall and lack of internal structure make them difficult to drain.

[1] See p. 13 above, and p. 126 below.

Scattered along the existing rivers and around the southern edge of the Fens, and constituting some of the "islands", are considerable areas of *Valley and River Gravels*, of varying constitution. They give rise to soils that are gravelly, brownish grey to grey-black in colour, and loamy sands to medium loams in texture. The soils are free draining, but many of them, through their position, have high water tables except in so far as these are lowered by field drainage. Much of the material of the Gravel deposits is calcareous, but leaching has reduced the percentage of calcium carbonate in the top soils to very low values. The ground water is calcareous, however, and seriously acid soils are rare.

The *Fen Alluvial* deposits cover about half the area of Cambridgeshire. The soils which are characteristic of Fenland are of four main kinds—peat, silt, shell marl and skirt. All these soils have been formed from materials laid down in association with a river system containing calcareous waters, a fact which has had an important result on the fertility of the soils; they are almost all rich in calcium in one form or another. The surface material, of varying constitution, is the result of the deposition of inorganic particles by the rivers and estuaries in their meanderings and frequent floodings, and of inorganic material by the growth of vegetation in swamp and marginal conditions. The variation of these main factors from time to time, and from point to point, has resulted (*a*) in a "profile" of great complexity at any point, and (*b*) in considerable variations in existing soil conditions from place to place.

The *Peat* soils are composed mostly of organic matter derived from swamp vegetation. They are black or nearly so, light, spongy, and crumbly. The organic matter contains considerable amounts of exchangeable calcium and is frequently associated with a small amount of free calcium carbonate. The depth of the peat deposit varies considerably from place to place, from a few inches to 10–15 ft.; but it is not always in an uninterrupted layer. Occasionally, as many as five separate bands have been proved, interlayered with "buttery clay", silt, or sandy deposits. Much of the peat does not lie directly on the older formations but on the characteristic clay, a dark blue-grey greasy material, entirely unlike the older clay formations. The depth of the peat[1] and the nature of the underlying material are points of major importance in determining the agricultural value of the land.[2]

The *Silt* soils are associated chiefly with areas of marine deposits of fine rounded quartz grains with small flakes of muscovite, occurring at the seaward or northern end of the County. They show a big range of texture

[1] For the shrinkage and wastage of the peat, see p. 186 below.
[2] For the practice of "claying" the peat, see pp. 120–1 and 152 below.

from place to place but are deep and uniform. The physical make-up of all these soils is peculiar, and unlike anything commonly encountered elsewhere. Coarse sand particles are generally absent, but fine sand is present in quantity, especially in the lighter silts. The silt fraction is more prominent in the heavier silts. The *Shell Marl* soils, derived from material formed in the clear water of shallow meres, occur farther south as patches among the true black peats. They are white or grey in colour, highly calcareous, with numerous small freshwater shells or shell fragments present. The chief shell marl areas are on the sites of Stretham Mere and Soham Mere. *Skirt* soils, from their location and constitution, appear to be the result of conditions where both the accumulation of peat and the deposition of silt or drift have alternately held sway. They might be described as mineral soils with a rather higher content of organic matter than usual and a darker or black colour.

Representative data of these typical soils are summarised in the following table:

CONSTITUTION OF MAIN TYPES OF TOP SOILS

The figures show the percentages of carbonates and of each of the four usual mineral-particle size-groups in the air-dry soil after passing through a 2 mm. sieve

Type	Coarse sand	Fine sand	Silt	Clay	$CaCO_3$
South-west Boulder Clay	8·8	18·0	15·5	32·0	8·2
South-east Boulder Clay	18·4	26·0	11·8	24·7	10·6
Middle Chalk (Redland)	46·6	23·3	7·4	9·2	4·6
Middle Chalk (Whiteland)	22·3	13·4	6·0	6·7	38·0
Lower Chalk	32·2	14·1	8·0	9·5	27·3
Chalk Marl	21·4	17·7	10·0	22·5	23·7
Gault	3·9	7·4	17·7	61·1	7·9
Lower Greensand (West Cambs)	74·5	7·6	2·8	8·0	0·0
Lower Greensand (Mid-Cambs)	46·2	20·2	8·3	15·2	0·15
Kimeridge Clay	20·0	24·4	15·8	34·0	0·0
Ampthill Clay	22·1	14·9	8·8	46·7	0·06
Old River Gravel	54·0	19·0	8·6	13·1	0·5
Valley Gravel	45·8	25·5	10·7	9·8	0·5
Fen Peat	1·6	10·5	21·8	15·9	5·6
Fen Silt (Light)	0·8	64·5	14·4	7·6	2·6
Fen Silt (Heavy)	0·5	34·2	19·2	30·9	1·6

CHAPTER THREE

THE CLIMATE OF CAMBRIDGESHIRE

By A. S. Watt, PH.D.

THE MAIN FEATURE OF THE CLIMATE OF THE NEIGHBOURHOOD of Cambridge lies in the definite approach it makes to the continental type.[1] Its latitude and position on the western side of a continent enable it, of course, to share with other parts of the British Isles in the equable climate associated with oceanic conditions. But its position in relation to the continental mainland also allows it to share in continental characteristics. The climate of Cambridge, in fact, may be described as transitional. Just how far it departs from oceanity and how near it approaches continentality is the main theme of this chapter.

To throw into relief the essential features of the Cambridge climate comparison is made, in the account that follows, between meteorological data from selected stations lying approximately in the same latitude across Europe from west to east. Valentia, Cambridge, Berlin, and Orenburg represent a transition from the oceanic to the continental type of climate.

TEMPERATURE

The data given in Table 1, and graphically presented in Fig. 11, show a slightly decreasing mean annual temperature from west to east.

At Valentia, the maximum falls in August, but the value is only slightly above that for July when the maximum occurs at the other three places: the minimum, on the other hand, falls in February at Valentia, but in January at the other three stations. The range of the monthly means increases from 14·8° F. at Valentia, through 22·4° F. at Cambridge, and 33·7° F. at Berlin, to 67·3° F. at Orenburg, an increase partly due to higher summer temperatures but mainly to lower winter temperatures.

The departure from oceanity, seen in the increasing range of monthly means, is emphasised by the monthly extremes of normal maxima and minima for Valentia, Cambridge, and Berlin (Table 2; Fig. 12), which show (*a*) an increasing annual range of monthly extremes (Table 2, last

[1] It is a pleasure to acknowledge my indebtedness to Dr G. C. Simpson, Director of the Meteorological Office, for placing the services of his department at my disposal. Mr E. G. Bilham of that department has been particularly kind in elucidating points of detail. His book on *The Climate of the British Isles* (1938) appeared during the preparation of this chapter and fuller information will be found there. For the use made of the data I must accept responsibility.

Table 1. *Mean annual and mean monthly Temperatures in °F.*

	Jan.	Feb.	Mar.	Apr.	May	June	July	Aug.	Sept.	Oct.	Nov.	Dec.	Mean annual temp.
Valentia (1906–35)	44·9	44·3	45·2	47·3	52·4	56·3	59·0	59·1	56·3	52·2	47·0	45·4	50·7
Cambridge (1906–35)	39·3	39·7	42·3	46·3	53·5	58·0	61·7	61·3	56·9	50·3	42·9	39·9	49·3
Berlin	30·7	32·9	37·8	45·7	55·8	62·1	64·4	62·6	56·8	47·8	38·8	33·3	47·4
Orenburg	4·3	7·7	18·5	39·2	58·6	67·5	71·6	67·5	55·4	39·6	23·7	12·2	38·8

Table 2. *Monthly extreme Temperatures in °F.*

	Jan.	Feb.	Mar.	Apr.	May	June	July	Aug.	Sept.	Oct.	Nov.	Dec.	Range
Valentia:													
Max.	53·1	54·0	55·9	61·0	68·0	72·0	71·1	71·1	69·1	62·1	57·0	55·0	18·9
Min.	28·9	30·0	30·9	34·0	37·9	43·0	46·0	46·0	42·1	35·1	32·0	30·0	17·1
Cambridge:													
Max.	54·0	55·9	63·0	69·1	75·0	81·0	83·8	82·9	78·1	68·0	59·0	55·0	29·8
Min.	19·9	21·0	23·0	26·2	30·0	37·9	43·0	42·1	36·0	28·9	24·1	21·1	23·1
Berlin:													
Max.	46·6	49·3	60·4	69·8	84·2	88·0	88·9	85·5	79·3	67·6	54·0	48·0	42·3
Min.	10·6	16·0	21·4	30·6	37·6	45·9	50·2	48·7	41·9	31·8	23·5	16·3	39·6

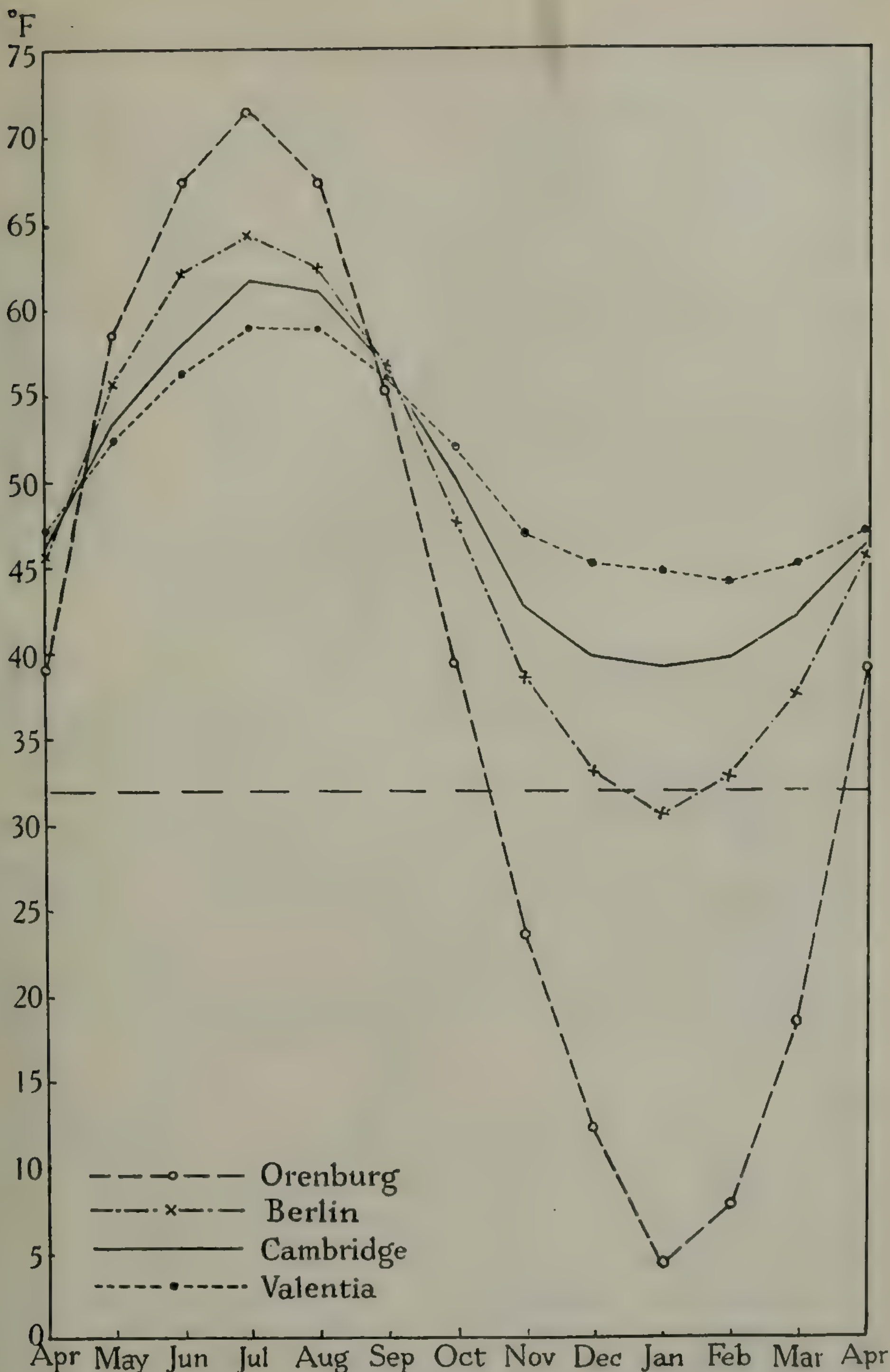


Fig. 11.

Mean monthly temperatures at Valentia, Cambridge, Berlin and Orenburg.

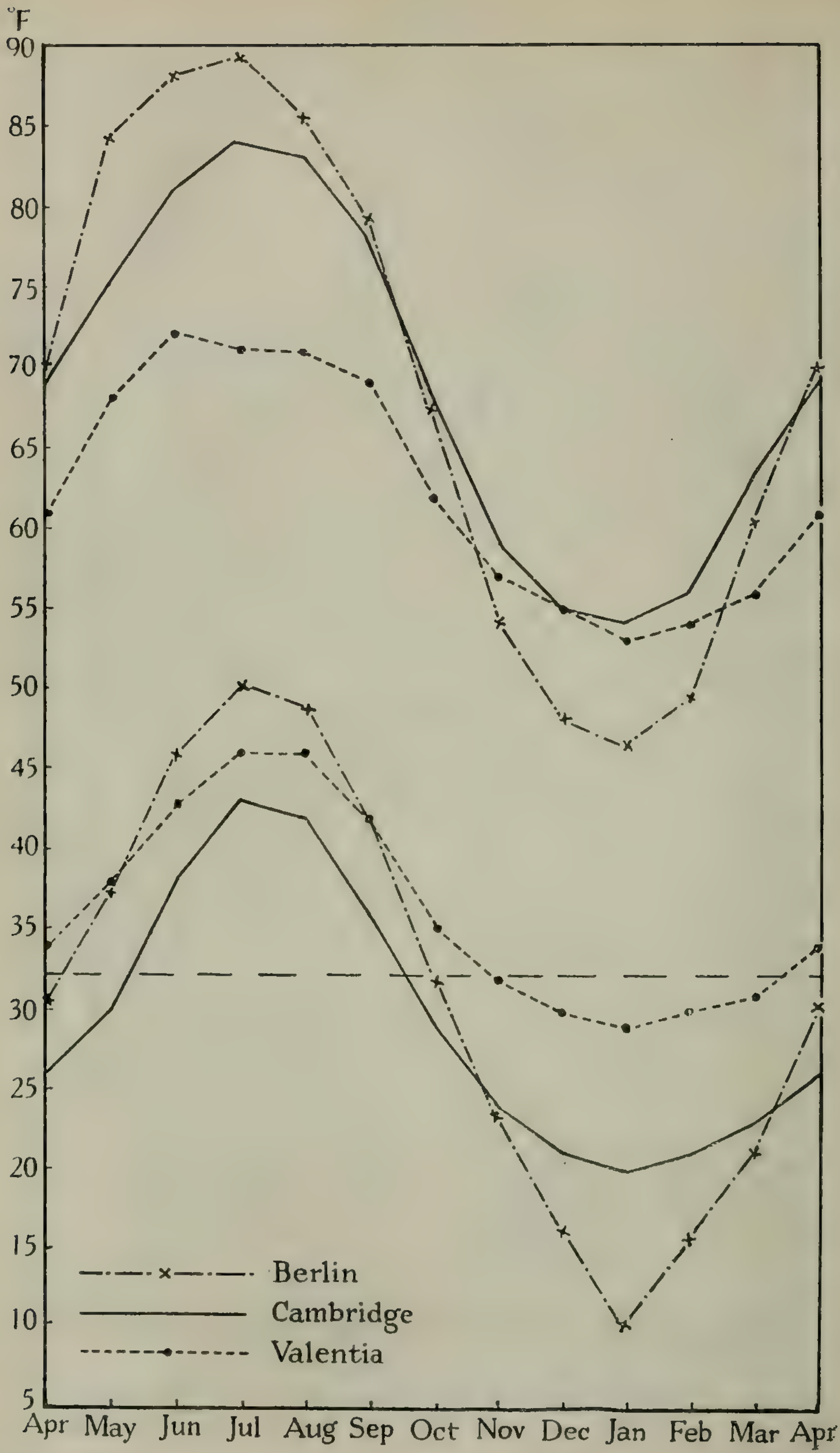


Fig. 12.

Mean monthly extremes of temperature at Valentia, Cambridge and Berlin.

column); (*b*) an increasing range of temperature in any month; and (*c*) increasing differences between the summer maxima and between the winter minima. The values for the winter maxima and summer minima approximate. But Cambridge is peculiar in having the highest winter maxima and the lowest summer minima of all three stations.

These low summer minima around Cambridge are particularly important, for they indicate the frequency of frosts (Table 3). Extreme minima below 32° F. are recorded for Valentia from December to March; for Berlin from October to April; but for Cambridge from October to May. Indeed, at Cambridge serious frosts quite often occur in the beginning of June and the only month really free from frost is July. Winter frosts also are severe, and the damage they do is likely to be increased because snow affords an efficient protection only on a few days of the year: in an average year snow lies in the morning for 12 days only. Skating is enjoyed every other year, and the last occasion when the Cam was converted into a highway was February 1929.

RAINFALL

Both in the amount of rainfall and in its distribution throughout the year, Cambridge again shows a distinct approach to the continental type of climate (Tables 4 and 5; Fig. 13).

Like the continental stations, Cambridge has a low yearly total: Valentia has over twice as much rain as Cambridge, and its lowest monthly value is much greater than the highest at Cambridge.

The total for Cambridge itself is a fair sample of the annual rainfall of the County: the average for twenty-eight stations within the County, at altitudes varying from 6 ft. O.D. to 286 ft. O.D., is 22·28 in. The range is narrow—from 20·6 in. at Upwell to 24·7 in. at Conington. To the west, in Huntingdon and Bedford, and to the east, in Suffolk, the rainfall is slightly higher.

The distribution of the rainfall throughout the year is particularly interesting. The typical oceanic climate shows a summer minimum and a winter maximum, the typical continental the reverse. Throughout the greater part of the British Isles, the rainfall of the winter half of the year is greater than that of the summer half, a feature which the country shares with a strip of Atlantic seaboard of the continent. But a relatively small area in east-central England, including Cambridge, shows the reverse, namely, a greater fall during the summer half than during the winter half year. This is a continental feature that is further emphasised by a consideration of the details. A glance at the graph for Orenburg shows the typical feature of a continental climate, namely, the high peak in late

Table 3. *Normal number of days with ground frost for each calendar month*

	Jan.	Feb.	Mar.	Apr.	May	June	July	Aug.	Sept.	Oct.	Nov.	Dec.	Year
Cambridge (1908–20)	18·6	17·5	18·4	12·8	4·1	0·6	0·0	0·0	2·3	7·4	14·7	15·5	111·9
Falmouth (1914–20)	7·6	8·3	10·0	5·4	0·1	0·0	0·0	0·0	0·3	1·3	6·2	9·1	48·3

Table 4. *Annual and monthly rainfall in inches*

	Jan.	Feb.	Mar.	Apr.	May	June	July	Aug.	Sept.	Oct.	Nov.	Dec.	Year
Valentia (1885–1915)	5·49	5·20	4·54	3·67	3·17	3·20	3·78	4·79	4·14	5·57	5·46	6·64	55·65
Cambridge (1885–1915)	1·50	1·28	1·47	1·36	1·76	2·11	2·16	2·35	1·61	2·36	1·93	1·93	21·82
Berlin	1·89	1·34	1·50	1·65	1·93	2·17	2·99	2·21	2·01	1·54	1·50	2·05	22·78
Orenburg	1·1	0·8	1·0	0·9	1·4	2·0	1·7	1·3	1·3	1·2	1·2	1·2	15·1
Wisbech	1·68	1·39	1·60	1·32	1·64	1·99	2·27	2·42	1·91	2·66	2·06	2·05	22·99
March	1·60	1·29	1·58	1·32	1·73	1·97	2·37	2·39	1·80	2·60	2·05	2·11	22·81
Stretham Engine	1·34	1·10	1·27	1·19	1·73	2·01	2·45	2·42	1·71	2·33	1·83	1·82	21·20

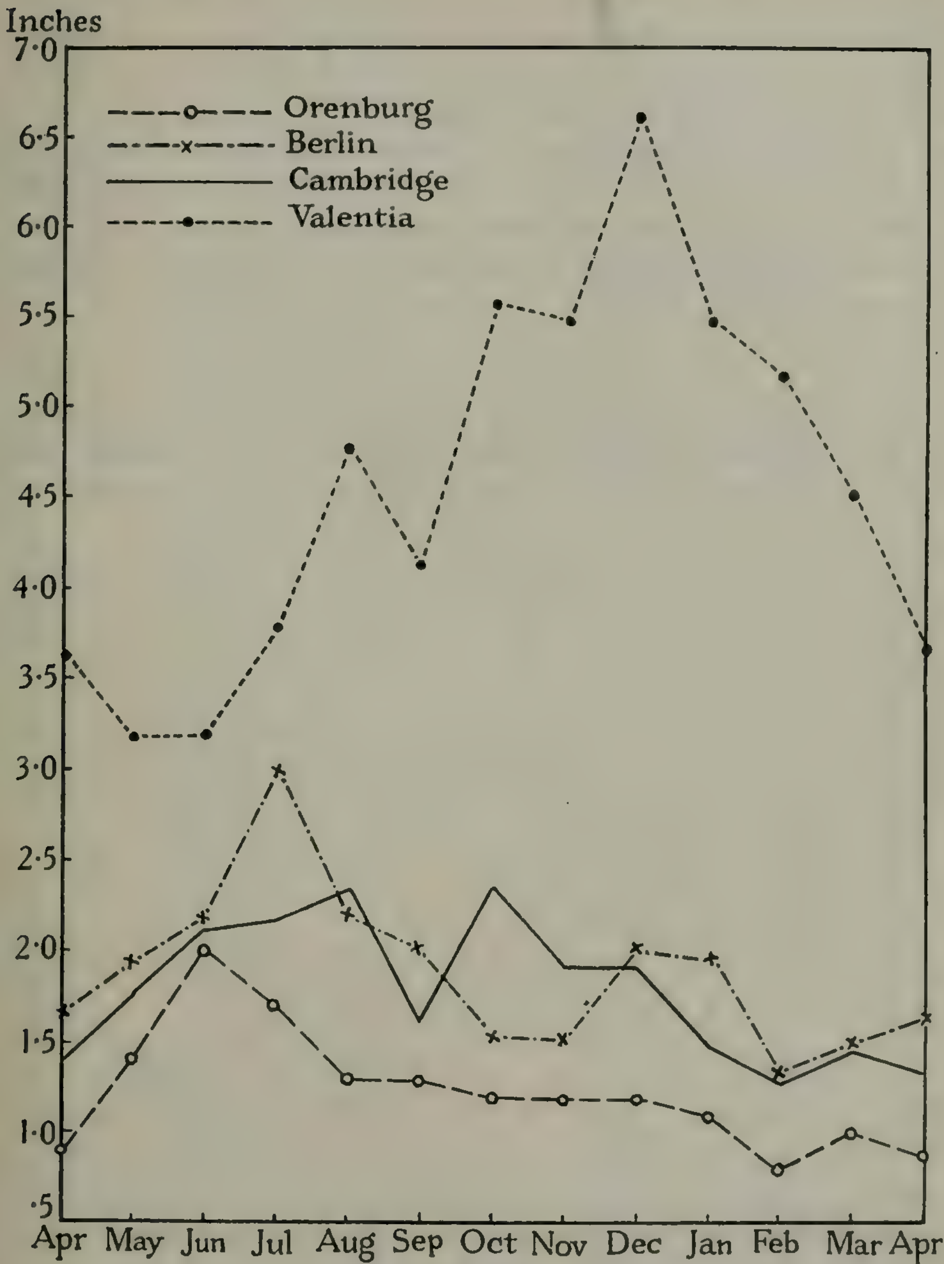


Fig. 13.

Mean monthly rainfall at Valentia, Cambridge, Berlin and Orenburg.

spring or early summer. Like the curves for the continental stations, the curve for Cambridge shows a rapid rise from April to June, but the rate of increase is not maintained, and a further slow rise leads to a maximum in August (or July–August at some other stations in the County). Thereafter, there is a fall in September followed by the assertion of a stronger oceanic influence giving a second maximum in October. This oceanic influence extends as far east as Berlin where it causes a subsidiary maximum in December which does not appear in the station farther east. After October, in Cambridge, there is a general fall to a minimum in February, the month with least rainfall in all the stations except Valentia.

RELATIVE HUMIDITY

Cambridge is situated in one of the lowest rainfall areas in this country and the combination of a high summer temperature with a rainfall that is low must be critical for many species of plants unless there are com-

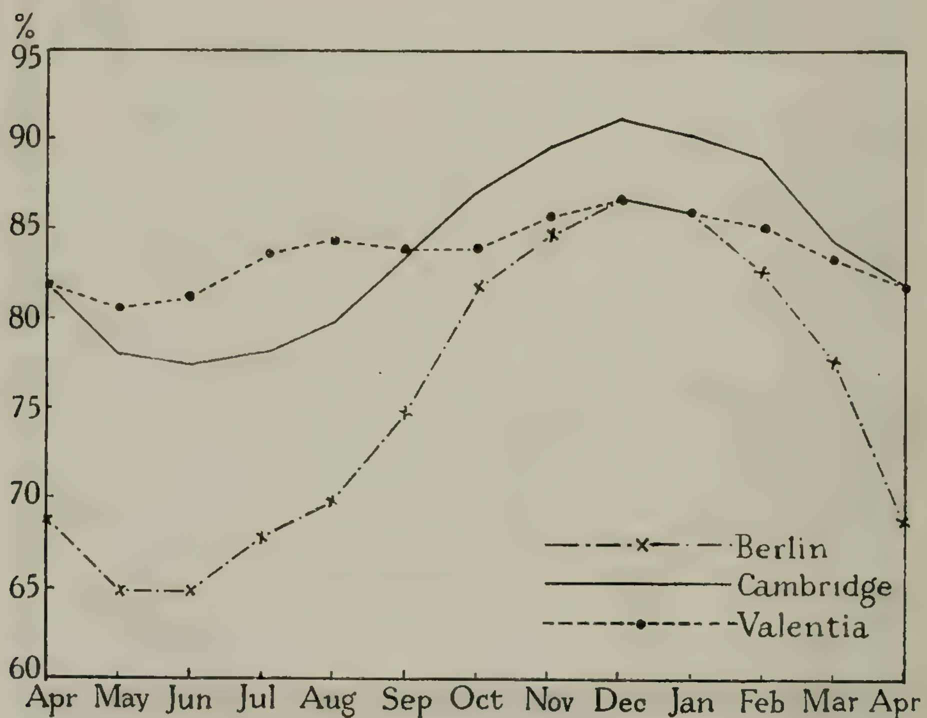


Fig. 14.

Mean monthly relative humidity at Valentia, Cambridge and Berlin.

pensating advantages. One of these is the relatively high humidity. The data are not strictly comparable; the Valentia data are for the period 1886–1910, the Cambridge data for 1924–34 (see Table 6; Fig. 14). The sustained high monthly values for Valentia are in keeping with its oceanic

Table 5. *Number of days with rain (over 0·01 in.) in each calendar month*

	Jan.	Feb.	Mar.	Apr.	May	June	July	Aug.	Sept.	Oct.	Nov.	Dec.	Year
Valentia	24	21	21	19	18	17	21	22	18	22	23	26	252
Cambridge	15	13	14	13	13	12	13	14	11	15	14	16	163
Berlin	15	15	15	13	13	13	15	14	13	14	14	15	169

Table 6. *Relative Humidity*

	Jan.	Feb.	Mar.	Apr.	May	June	July	Aug.	Sept.	Oct.	Nov.	Dec.	Year
Valentia (1886–1910)	86·3	85·5	83·7	82·0	81·0	81·5	83·7	84·6	84·3	84·3	86·0	87·2	84·2
Cambridge (1924–34)	90·6	89·2	84·7	82·1	78·2	77·7	78·3	79·8	83·6	87·4	90·0	91·4	84·4
Berlin	86·0	83·0	78·0	69·0	65·0	65·0	68·0	70·0	75·0	82·0	85·0	87·0	76·1

climate, but the average annual value for Cambridge is much the same, and the range of monthly means, although double that for Valentia, shows both a higher maximum and a lower minimum: but even this minimum is high. Berlin, on the other hand, shows a much wider range, with a maximum in December and a minimum in May–June. The depressing, enervating effect of the Cambridge climate is doubtless due to its high humidity, and even Indians complain of the heat during a hot spell in the summer. It seems likely, however, that the high humidity is due in part to local causes, but chiefly to the "continentality" of the neighbourhood. This arises from the large diurnal range of temperature with values at night and early morning falling to dew point. The result is that the readings at 9 h. and 21 h. tend to be high, particularly in later autumn and winter.

SUNSHINE AND CLOUDINESS

The variations for bright sunshine (Table 7; Fig. 15) show relatively small differences from October to April for Valentia, Cambridge, and Berlin. From May to September, however, the differences are appreciable; Cambridge then occupies an intermediate position. Although the total number of hours of sunshine is less than that enjoyed by parts of the south coast, yet the Cambridge neighbourhood is sunny compared with the west.

The data for cloudiness (Table 8) show the nearer approach of Cambridge to Berlin during April to September: from September to April, Cambridge has the least cloudiness of all these places and also the lowest average for the year.

The preceding data show that while the climate of Cambridge is not continental, it has a number of continental features, the fuller expression of which is checked by a high humidity.

GENERAL CONSIDERATIONS

The weather varies much from year to year and from place to place. Even minor differences in topography are significant to plant life and to man: a few feet may raise them above an accumulation of cold air or of fog. But the major variations are determined by major causes—namely, the position of Cambridge in relation to the centres of low- and high-pressure systems in north-western Europe. In general, the air moves from south-west to north-east, due to the frequency of cyclones centred (1) to the north-west of the country, or (2) directly over the British Isles or over the Channel; the former bring "orographic" rain to the west, the latter "cyclonic" rain to all parts of the country. The rainfall of Cambridge is

Table 7. *Number of hours of bright sunshine daily for each calendar month*

	Jan.	Feb.	Mar.	Apr.	May	June	July	Aug.	Sept.	Oct.	Nov.	Dec.	Year
Valentia (1906–35)	1·4	2·3	3·7	5·4	5·9	5·8	5·1	4·8	4·2	2·9	2·1	1·3	3·7
Cambridge (1906–35)	1·7	2·5	3·9	4·9	6·4	6·8	6·2	6·0	4·9	3·5	2·0	1·3	4·2
Berlin	1·3	2·2	3·3	5·6	7·4	8·2	7·4	6·9	4·8	3·1	1·7	1·1	4·4

Table 8. *Number of hours of cloudiness daily for each calendar month*

	Jan.	Feb.	Mar.	Apr.	May	June	July	Aug.	Sept.	Oct.	Nov.	Dec.	Year
Valentia	7·8	7·7	7·0	6·8	7·2	7·4	7·5	7·5	7·2	7·3	7·2	7·8	7·4
Cambridge	6·6	6·6	6·0	5·9	5·9	5·9	6·3	6·0	5·3	5·7	6·2	6·4	6·1
Berlin	7·3	7·2	6·6	6·0	5·6	5·6	6·1	5·8	5·5	6·5	7·3	7·7	6·4

essentially cyclonic. At certain times of the year, particularly in spring and early summer, bitterly cold weather may be experienced: the centres of high-pressure systems are situated to the north-west and north, and cold north and north-east winds blow over East Anglia as, for example, during the destructive frost of May 1935. These winds may bring rain to our eastern shores when the west is dry. Occasionally, too, cold winds

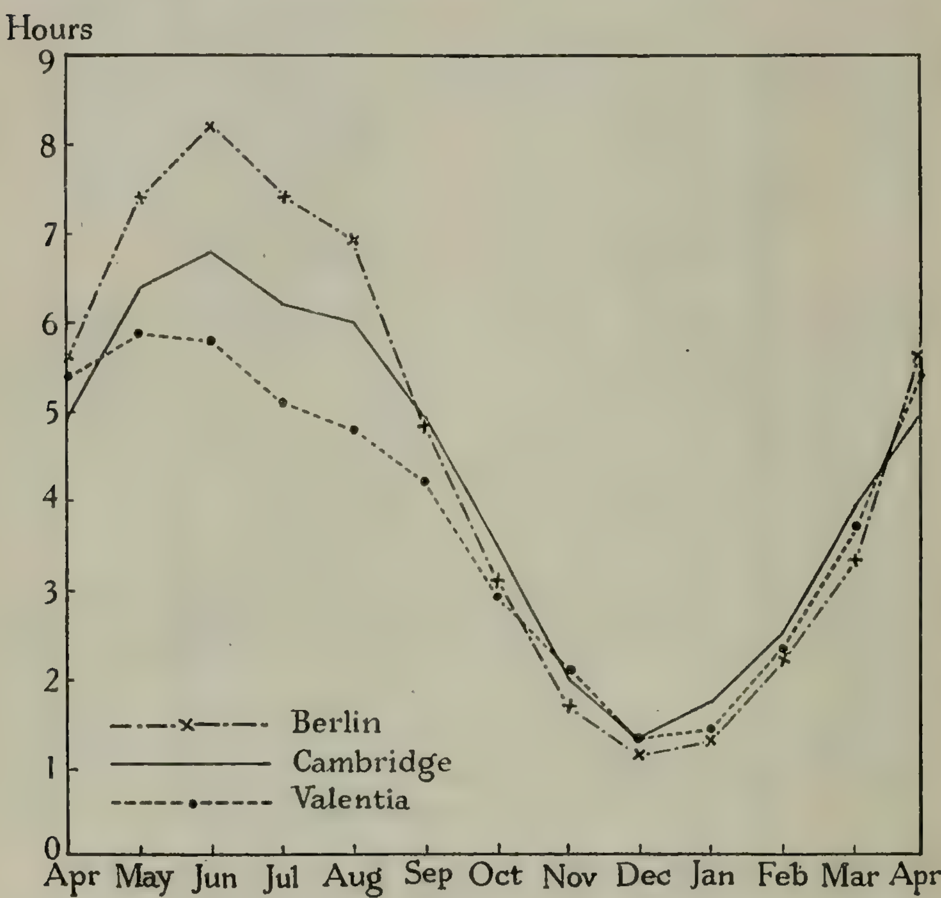


Fig. 15.

Average number of hours of bright sunshine per day at Valentia, Cambridge and Berlin.

blow from the south-east. Since, in general, these cold winds prevail during the spring, and the westerlies during the autumn, spring is colder than the autumn. At different times of the year Cambridge may experience anticyclonic conditions, but whether the weather is warm or cold depends on the origin of the anticyclone and on its position. If it is an extension of the high-pressure belt of latitude 30–35° N., then the air is warm, and long spells of dry warm weather may be enjoyed; if it is an offshoot of a winter continental anticyclone, the weather may be very cold.

These data, or the conditions they represent, are significant for plant life and agricultural practice. The rainfall, as we have seen, is low. The results of deficiency and drought would be more apparent than they are, but for the high water table of much of the neighbourhood and for the prevalence of soils with a high capacity to hold water. Where, as in Breckland, the soil is porous and non-retentive the rainfall deficiency is accentuated.[1] Thus because of the dry conditions, plants of the oceanic west may fail or grow poorly. Among the planted conifers in and around Cambridge only the xerophytic pines do at all well: spruces, hemlocks, silver firs do badly.

On the other hand, flooding may occur, and the floods of the early months of 1937 are still fresh in the memory. They were due to an exceptional run of five very wet months, January to May, when the water draining into the low Fenland depression found its normal flow to the sea checked by a combination of wind and tide.[2]

Again, although the neighbourhood of Cambridge is an area of intensive cultivation, the low temperatures of winter preclude competition with the milder south-west in the production of spring flowers. The extension into spring of cold wintry conditions and the frequency of cold winds and recurrent spring frosts until the beginning of June mean the crippling of frost-sensitive species of plants and in agricultural practice inability to grow early potatoes. Fruit-tree blossom, too, is often severely damaged. On the other hand, the greater number of hours of sunshine allows a proper maturation of wheat, and the relatively high number of hours of sunshine during September doubtless contributes to the percentage of sugar in sugar beet. For native plants, in particular for the "continental" element in our flora, sunshine is critical for the ripening of seed.[3]

[1] See p. 208 below. [2] See p. 193 below.

[3] See A. S. Watt, "Studies in the Ecology of Breckland. I. Climate, Soil and Vegetation", *Jour. Ecol.* xxiv, 117 (1936).

CHAPTER FOUR

THE BOTANY OF CAMBRIDGESHIRE

By H. Godwin, M.A., PH.D.

THE FOLLOWING ACCOUNT OF THE BOTANY OF CAMBRIDGESHIRE is written primarily from the standpoint of the ecologist, showing the vegetation of the area in relation to geology, topography, and climate.[1] The chief vegetation types are considered in turn according to the geological formations on which they occur. As this part of England is particularly heavily cultivated, stress is specially laid on those fragmentary communities still present in a fairly natural state, such as the woodlands on Boulder Clay, the Chalk grassland, and the sedge or scrub in the undrained parts of the Fenland. Since, moreover, no other county contains so much of the old peat fens, the account deals at length with what remains of fen vegetation upon their surface.[2] Although a portion of the Breckland, with its typical soil and vegetation, comes just within the County boundary west of the River Kennett, it will not be considered here as it has been dealt with separately by Dr Watt.[3]

THE FENLAND

The English Fenland within historic times stretched over the greater part of the area to the west and south of the Wash, extending as far north as Lincoln and as far south as Huntingdon and Cambridge (see Fig. 47). On the seaward side, the surface deposits are semi-marine silt, laid down, and afterwards occupied, during the Romano-British period.[4] On the landward side, the upper layers are peat. This peat was produced by discharge of the floodwaters of the Rivers Witham, Welland, Nene, and Ouse into the extensive shallow basin of the fens. This water entering the fens has a high mineral content, particularly that from the tributaries of the Ouse, which drain the chalk escarpment to the east. The fen peats are therefore alkaline in reaction, and support a vegetation of true "fen" type—the "Niedermoor" of German botanists. Such fens are dominated by grass-like monocotyledons of the Gramineae, Cyperaceae, and Juncaceae, and, in their drier stages, by shrubs and trees such as willow, alder, and birch.

[1] I am indebted to the Editor of the *Victoria County History* (Mr L. F. Salzman) for permission to use material prepared for a more extensive account of the vegetation of the County.

[2] See also p. 17 above for a summary of the vegetational history revealed by investigation of the successive layers of peat deposit in the Fenland.

[3] See p. 221 below.

[4] See p. 20 above, and p. 92 below.

Extremely little trace remains to-day of the original vegetation of the peat fen. Almost the whole area has been drained and brought under cultivation: its character can be recognised only by the black peaty soil, the uniform flatness, and by the deep ditches full of reeds (*Phragmites communis*) that separate fields of potatoes, cereals, and sugar beet. The continuous hawthorn hedges of the neighbouring land thin out abruptly at the fen border. Rows of planted willows, and scattered clumps of shelter trees, or small coverts, remain the only woody plants on the cultivated fen.

I. Wicken Fen

One of the largest and best known areas of fen still uncultivated is Wicken Fen, covering about one square mile, now in the hands of the National Trust, and lying about 10 miles to the north-east of Cambridge on the very margin of the Fenland.

The lodes, or main drainage channels traversing the area, converge at Upware, and there they communicate through sluicegates with the River Cam. The surrounding cultivated land has an entirely separate drainage system at a much lower level. The water in Wicken Fen itself is conserved in summer by the Upware sluices, and, in winter, excess water is run off whenever possible. Thus the water level in the fen changes comparatively little through the year, although the fen surface is about +7 ft. O.D. and the surrounding land has become much lower through peat wastage following draining.[1]

Not only are the soil and water-level relations in the fens thus altered from natural conditions, but peat-cutting has removed much of the fen surface, and the fen vegetation has suffered a traditional system of crop-taking maintained to some extent at the present day. Other human activities also in less degree affect the vegetational cover: these include the cutting and clearing of steep-sided lodes and drains, the consolidation and mowing of "droves" and walks, the felling of old fen scrub, and, to a small extent, propagation of rare species of plants.

1. *The Primary Succession.* In accordance with the accepted laws of the succession of plant communities, shallow open water should progress by natural accumulation of peat to shallower conditions, to a soil surface first at water level and then above it, steadily moving towards a final stable community, the climax. This succession is known as the primary hydrosere, and the climax in East Anglia may be supposed to be deciduous woodland. Human activities have obscured the original simple hydrosere relations at Wicken, but it seems clear that it probably differed little from

[1] See p. 186 below.

that of parts of the Norfolk Broads. This primary succession may be summarised as follows:

(*a*) *Aquatics*. There are no large open areas of water in the fen, and nowhere can we see zonations indicating all the early stages of the hydrosere, like those described by Miss Pallis on the Norfolk Broads. We may indeed judge the lode and ditch flora in the light of the work of Miss Pallis, and find, in appropriate depths of water, various aquatic plants, which are more or less widespread in the rest of England. The abundant forms include: *Chara*, *Elodea canadensis*, *Myriophyllum spicatum*, *Hippuris vulgaris*, *Potamogeton lucens*, *P. pectinatus*, *P. perfoliatus*, *P. crispus*, *P. densus*, *Scirpus acicularis*, *Sparganium simplex*, *Oenanthe phellandrium*, *Hottonia palustris*, *Castalia alba*, *Nymphaea lutea*, *Sparganium natans*, *Sagittaria sagittifolia*, *Butomus umbellatus*, *Alisma plantago*, *A. ranunculoides*, *Ranunculus lingua*, *Polygonum amphibium*, etc.

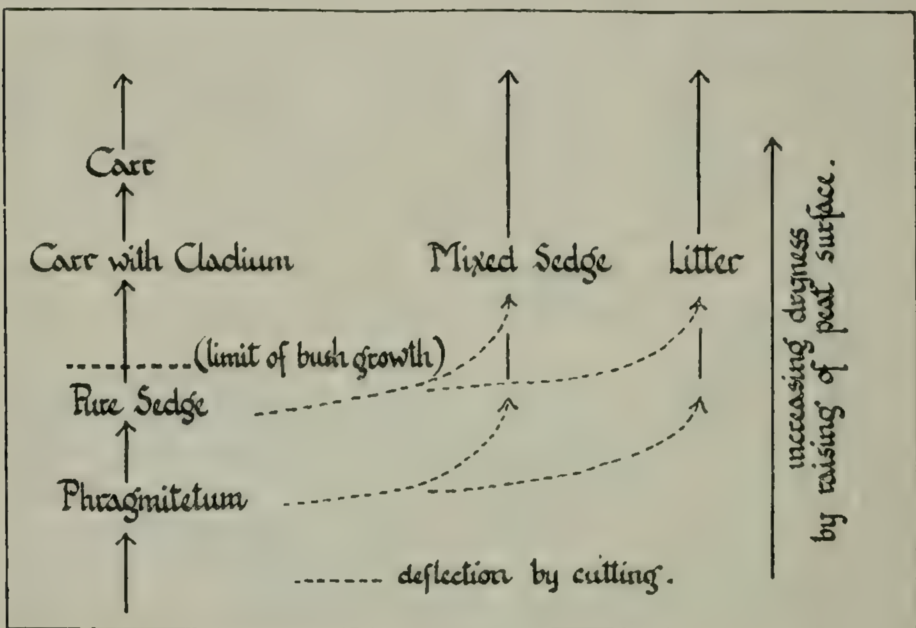


Fig. 16.

Vegetation Successions at Wicken Fen.

From H. Godwin and F. R. Bharucha, "Studies in the Ecology of Wicken Fen", *Journ. Ecology*, xx, 185 (1932).

(*b*) *Reed-Swamp* (*Phragmitetum*). The borders of lodes and ponds and the shallower drains throughout the fen show a reed-swamp of *Phragmites communis* well developed, but only at the eastern end, where the fen peat abuts on the gault clay, do *Scirpus lacustris*, *Typha angustifolia*, *Glyceria aquatica* or *Phalaris arundinacea* occupy similar marginal positions by the water side.

(*c*) *Pure Sedge* (*Cladietum*). Throughout the hydrarch succession we may reckon that development follows the gradual raising of the peat level up to and above the water level, so that we can utilise the difference between the two levels as a criterion of successional phase. It then becomes apparent that the artificially steepened banks of lodes and drains, and their artificially raised edges, give no opportunity for the successional stage next following reed-swamp to become easily evident as a zonal community. Drains and trenches, made by removing peat, are now often choked up to form habitats only little drier than those of the reed-swamp: such places support the community we have called "pure sedge". This is a closed community dominated very completely by the prickly sedge, *Cladium mariscus*. The sedge leaves grow up to 3 m. long, bending over horizontally at about 1·5 m. The luxuriance, the evergreen habit, and the thick "mattress" of dead leaves deposited between the growing shoots of *Cladium*, prevent the growth of all other plants save infrequent individuals of *Phragmites*, *Lysimachia vulgaris*, and *Salix repens* var. *fusca*.

Recent studies on the autecology of *Cladium mariscus* at Wicken have shown many features of great interest in the plant itself and the conditions under which it grows. The meristem of the plant lies below ground at the apex of a vertical stock: it is very frost-sensitive, but the wet fen peat has such a small temperature diffusivity constant, that frost seldom or never penetrates deep enough to be harmful. The meristems also offer a barrier to gas diffusion from the growing leaves to the stock and roots, but aeration of these organs takes place through the bases of dead or mature leaves. The drained upper layers of peat show strong seasonal drift in composition of the soil atmosphere determined by changing soil temperature. Values of 5 to 6 per cent of carbon dioxide at only 20 cm. depth are reached in summer with correspondingly lowered oxygen values. The soil water below the water table is apparently devoid of dissolved oxygen.

(*d*) *Bush Colonisation*. As the peat level is gradually raised, the pure sedge is invaded by bushes, which are dispersed with great rapidity by seeds. The most abundant species is *Rhamnus frangula* (*Frangula alnus*); next come *Rhamnus catharticus*, *Salix cinerea* and *Viburnum opulus*, and lastly there is a very small percentage of *Crataegus monogyna*, *Prunus spinosa*, *Ligustrum vulgare* and *Myrica gale*. Both dissemination and establishment are irregular, and early stages of bush colonisation have a very heterogeneous structure, which disappears, however, as the sedge patches are invaded marginally or over their whole extent, and the bushes come to form a complete and uniform cover over the whole area. Birds are probably the most important agents of dispersal of the *Rhamnus* bushes, especially the large flocks of migratory fieldfares which visit the fen in autumn as the drupes ripen. On the other hand, a very large part of the fruit crop falls to the ground, and, as the stones are exposed by the drying of the fruit, they are taken by fieldmice, which often gather them into stores, where, if forgotten or abandoned, they may germinate.

(*e*) *Carr* (*Franguletum*). As the canopy of the young buckthorn becomes closer there follows, especially where colonisation has been dense, a well-marked phase in which *Cladium* and associated species of the previous stage are killed out; their dead leaves still hang in the crotches of the branches when no living plants persist. The ground becomes almost bare beneath the bushes and a characteristic shade-tolerant flora enters. This usually includes the marsh ferns, *Dryopteris thelypteris, Agrostis stolonifera, Urtica dioica,* and non-flowering shoots of *Lysimachia vulgaris, Symphytum officinale* and *Iris pseudacorus. Convolvulus sepium* is often a conspicuous feature of early phases of scrub formation, twining round the "drawn-up" reed stems, and the sallow, *Salix cinerea,* is a typical pioneer shrub, succumbing early to the competition of other bushes.

As the carr ages, the bush density diminishes and it seems certain that dominance passes from *Rhamnus frangula* to *R. catharticus.* The mechanism of this displacement is uncertain, but it probably involves an extensive "die-back" disease caused by the fungi *Nectria cinnabarina* and *Fusarium* sp. To this, the older *R. frangula* bushes seem very susceptible; the fungi gain entrance by snags and rapidly kill the bushes. *R. catharticus,* which is not so attacked, increases greatly in the later stages of carr development. At this stage, also, the bushes take on a tree form with central trunk and branches limited to a close crown—a strong contrast to the earlier scrub in which each bush stool has many trunks, and branching is extremely diffuse and extensive. In later stages of development, *Viburnum opulus* may be of importance: it straggles extensively under the shade of the other bushes, rooting at the places where shoots touch the soil, to form quite impenetrable tangles.

There is no deciduous fen woodland on Wicken Fen, but in many places birch (*Betula alba*) is spreading from seed; there are also a few good-sized oaks (*Quercus robur*), a colony of grey poplar (*Populus canescens*), and many scattered ashes. It is a remarkable fact that, although the fen peat contains pollen and wood of alder (*Alnus glutinosa*) in great quantity, the few planted trees now growing on the fen are not able to spread, although they produce abundant viable seed.

2. *Deflected Successions.* Over the greater part of the fen the natural succession already described does not take place, for the fen vegetation is cut at intervals, as a rule, of either one year or four years, and the crop is used for litter (cattle-bedding) or for thatching. On these lines, cutting

has probably gone on for centuries. It is evident that although bush colonisation will be prevented by this practice, yet the peat level will continue to rise, and new successions, still subject to the cutting factor, will take place. These will, however, differ from the primary succession and give rise to communities not found therein. Such are the "mixed sedge" and "litter" which cover almost 50 per cent of the fen. These two communities occur on peat often much higher above water level than even mature carr, and as soon as the "deflecting factor" of cutting is removed, they very rapidly become colonised by bushes and give rise to carr. It is quite likely that the carr development stages we have already mentioned as part of the prisere may more properly be referred to these shortened successions.

(*f*) *Mixed sedge* (*Cladio-Molinietum*). Vegetation in which *Cladium mariscus* and *Molinia cœrulea* are more or less co-dominant has long been cut by fenmen for thatch or for fuel. In it, the sedge (*Cladium*) is rather less vigorous than in the pure sedge, a reflection of the influence of the drier habitat upon its summer growth rates. It is scattered through with *Phragmites*; while flowering plants such as *Eupatorium cannabinum*, *Angelica sylvestris*, *Peucedanum palustre*, *Lysimachia vulgaris*, *Hydrocotyle vulgaris* and *Salix repens* var. *fusca*, occur sparsely throughout, though they are much more conspicuous in the season following a sedge crop than afterwards.

It is clear that this vegetation has arisen by persistent cropping of the vegetation, which serves to exclude bushes, but which allows continued peat growth and the ingress of plants characteristic of drier habitats but not susceptible to cutting. *Molinia* is, of course, the most important of these, for it produces annual photosynthetic shoots, and its tuberised stem bases are unharmed by winter scything: in this, it contrasts greatly with *Cladium*, the leaves of which are evergreen, and the loss of which seriously damages the plant. Bush invasion, especially by *Rhamnus frangula*, is very rapid when cutting is discontinued.

(*g*) *Litter* (*Molinietum*). When fen owners have cut the vegetation at short intervals, such as one year, the sedge has been rapidly killed out. The Molinietum, thus formed, like the Cladio-Molinietum, contains much *Phragmites*. In it, however, *Carex panicea* and *Juncus obtusiflorus* are sub-dominant, and several smaller plants, encouraged by the removal of taller dominants, also appear. These include *Succisa pratensis*, *Thalictrum flavum*, *Cirsium anglicum*, *Valeriana dioica*, and *Orchis incarnata*. The yearly crops of litter are used for cattle-bedding or for coarse chaff; the straight boundaries of the litter communities coincide with the limits of different owners' plots, and betray their origin.

It has been demonstrated experimentally that a mixed sedge community can be altered, in ten years by annual cutting, to something that fairly closely resembles litter. Conversely, cessation of cutting will cause the disappearance of such species as *Carex panicea* from late stages of Cladio-Molinietum.

There are, in fact, any number of inter-grades between the communities we now describe, and they lie upon a series of deflected successions suffering cutting of different intensities; when left uncut, they rapidly form carr.

The droves in the fen, which are cut twice or thrice a year, might be considered to show successions still further deflected from the prisere. Certainly, the main drove, which has been in existence for at least two centuries, has a most characteristic flora, very rich in species.

From time to time, areas of mature carr are cleared of bushes, and very striking vegetational changes follow. Innumerable seedlings of a great number of species rapidly establish themselves, and among them are shoots of *Iris*, *Rubus caesius*, *Dryopteris thelypteris* and other relicts of the previous shade phase. The very rich herbaceous vegetation, if undisturbed, quickly reverts to carr.

II. Other Remnants of Fen Vegetation

Around the fen margins there still exist a few much-modified areas of fen vegetation. After Wicken, the largest of these is Chippenham Fen, which lies 4 to 5 miles north of Newmarket, where the Breckland sands come down to the fens. Over most of this fen, peat-cutting has left deep trenches, now filled densely with *Cladium mariscus*. Associated with it, but especially on the ridges, *Molinia* is abundant, while the following species are more or less frequent: *Schoenus nigricans*, *Juncus obtusiflorus*, *Angelica silvestris*, *Eupatorium cannabinum*, *Lythrum salicaria*, *Urtica dioica* *Valeriana officinalis*, *Serratula tinctoria*, and *Scrophularia nodosa*. *Phragmites* is abundant throughout, and there is close general resemblance to the Cladio-Molinietum at Wicken, though bush colonisation is much sparser. Finally, Chippenham Fen is still the home of *Pinguicula vulgaris*, *Aquilegia vulgaris*, *Selinum carvifolium* and *Carex pulicaris*.

Other remnants of fen vegetation occur in the Cam Valley at Dernford Fen, now rapidly drying up, at Quy Waters and Quy Fen. The peat is generally very shallow, and the fen species persist precariously in ditches and pools.

The County flora also includes species such as *Stratiotes aloides*, *Teucrium scordium*, *Villarsia nymphaeoides* hanging on in these sites: while at Wicken still persist *Liparis loeselii*, *Ranunculus lingua*, *Peucedanum palustre*, *Lathyrus palustris* and *Myrica gale*. *Viola stagnina* has not been seen for some years, and it has probably followed *Senecio paludosus*, *S. palustris*, *Sonchus palustris* and *Cicuta virosa* into the list of species now extinct. It is possible that *Typha minima* grew quite recently at Wicken. Throughout the County the fen lodes and their margins naturally still carry an abundant selection of the old fen species, although few of these are rare.

Somewhat different in character from these true relics of the fens, are a few sites on the Gault or Chalk, where local conditions formerly led to the growth of small fens or even of acidic bogs. These sites have now been drained, but from Hinton, Teversham, and Sawston Moors, the following have been recorded: *Drosera rotundifolia*, *D. anglica*, *D. intermedia*, *Pinguicula vulgaris*, *Malaxis paludosa*, *Scirpus caespitosus*, *Eriophorum angustifolium*, *Carex dioica*, *Molinia cœrulea*, *Sphagnum cymbifolium* and *Splachnum ampullaceum*. Triplow Holes is a site on the chalk where fen species still survive, and where *Cladium* has a local dominance.

III. The Silt Fens

The silt which forms the fen soil in the northern part of the County round Wisbech (see Fig. 29) has probably been cultivated, apart perhaps from the Saxon period, ever since its deposition in Romano-British times.[1] Though "natural" vegetation is absent from it, it would be of great interest to work out the progress of invasion and establishment of species in the area during the quite definite period since its origin in brackish water. So far this has not been done.

At Foulanchor, near Wisbech, an area of reclaimed salt marsh falling within the County boundary brings a number of maritime species into the County flora. The tidal influence which formerly extended far inland up the fen rivers has no doubt been responsible for inland records of the more tolerant maritime species, such as *Scirpus maritimus* at Littleport, Sutton and Upware, and *S. tabernaemontani* at Littleport.

THE LOWER GREENSAND AREA

The outcrop of Lower Greensand in the County is not extensive. Some of it supports much of the market gardening and orchard area near Cambridge, and the fields show typical psammophilous weeds. Only at Gamlingay, in the extreme south-west, does heath develop on it, and even here extensive tree-planting, felling and pig-keeping have greatly altered the natural vegetation. Of the former heath dominants *Calluna vulgaris* and *Deschampsia flexuósa* are still abundant. The following also occur: *Teesdalia nudicaulis*, *Galium saxatile*, *Ulex europaeus*, *Cytisus scoparius*, *Luzula multi-*

[1] See p. 92 below.

flora, *Nardus stricta*, *Aira praecox*, *Anthoxanthum odoratum*. Equally typical of heath conditions are the mosses, *Polytrichum piliferum*, *P. juniperinum*, *Bryum roseum*, *Dicranum scoparium* var. *orthophyllum*, *Hypnum schreberi*, *Brachythecium albicans* and *B. purum*. Until recently *Tilia cordata* and *Quercus sessiliflora* apparently grew naturally here.[1]

The Greensand outcrop at Gamlingay is also noteworthy because until 1855 it carried a large acidic peat bog yielding species quite characteristic of the surfaces of raised bogs. The moors at Hinton, Teversham, and Sawston, already mentioned, may have been more or less similar, but on a large scale they are rare in the east of England. Peat investigations in the Woodwalton and Yaxley areas of fen, south of Peterborough, show that the peat fens locally passed into the condition of raised bogs, and that limited marginal areas of fen surface retained this character and typical flora until quite recent times. Most of the raised bog species from Gamlingay are now extinct, but the records and herbarium specimens are sufficiently convincing. They include the following:

Hypericum elodes
Drosera rotundifolia
Oxycoccus quadripetalus
Littorella lacustris
Malaxis paludosa
Narthecium ossifragum
Potamogeton polygonifolius
Scirpus pauciflorus
Eriophorum angustifolium
Rynchospora alba
Carex dioica
C. stellulata

Lycopodium clavatum
L. inundatum
Sphagnum angustifolium
S. cuspidatum
Archidium alternifolium
Splachnum ampullaceum
Aulacomnium palustre
Odontoschisma sphagni
Aneura pinguis
Hypnum revolvens
H. stellatum
Philonotis fontana
Polytrichum commune

THE BOULDER CLAY

I. Woodland

The Boulder Clay in Cambridgeshire lies in two large patches, on the eastern and western sides of the County (see Fig. 29). Place-name evidence shows that these were the wooded districts of the County in the Anglo-Saxon period.[2] In Domesday times also, the distribution of woodland varied sympathetically with that of the clay (see Fig. 17). To-day, by far the greater part of the sparse woodland of the County is to be found in these two areas. These woods are indeed almost the only semi-natural vegetation

[1] Boulder Clay overlying Greensand in some places, and Gault in others, also occurs at Gamlingay, and supports woodland. See p. 54 below.

[2] See p. 103 below.

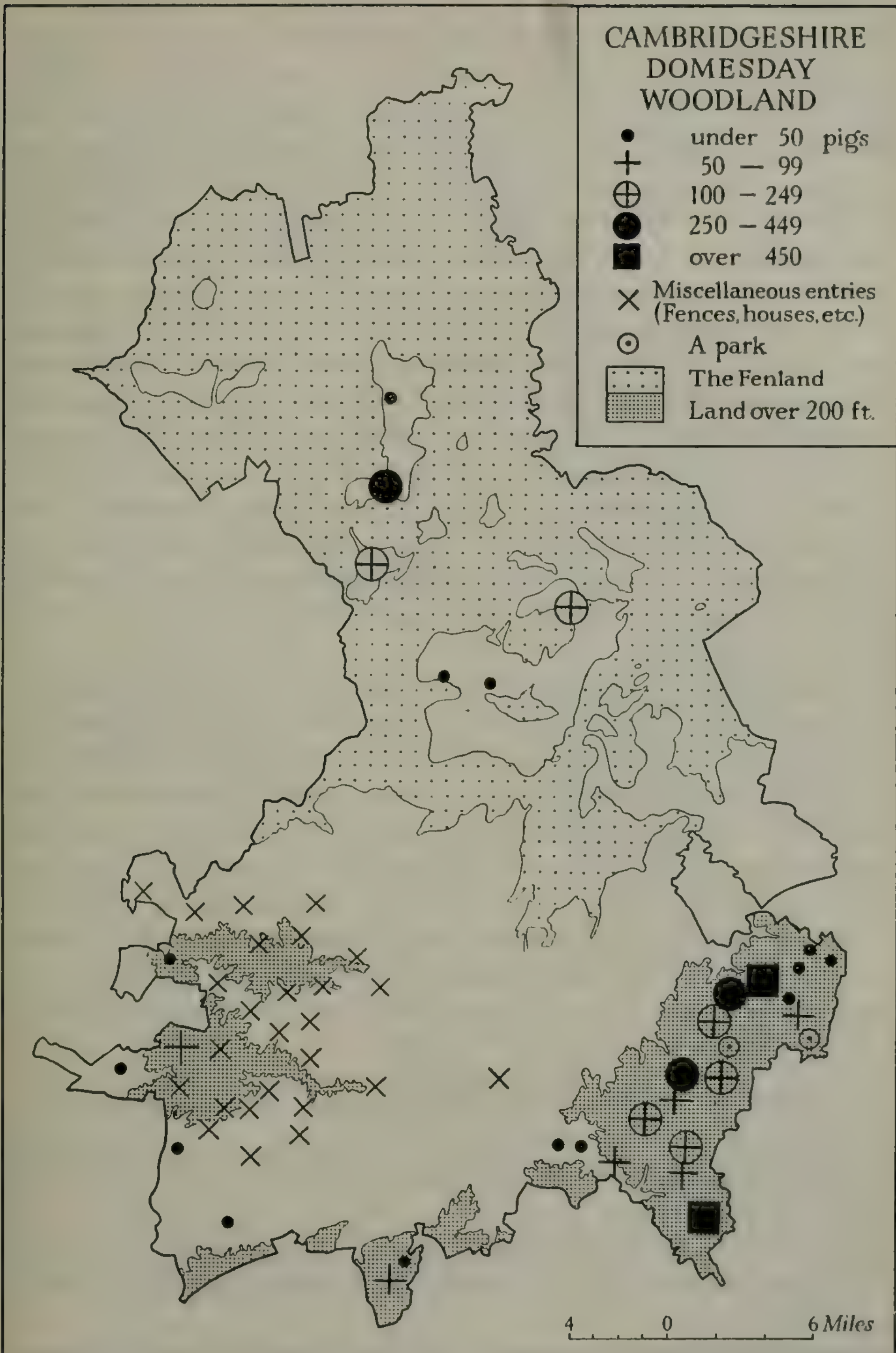


Fig. 17.

From H. C. Darby, "The Domesday Geography of Cambridgeshire", *Proc. Camb. Antiq. Soc.* xxxvi, 49 (1936).

on the Boulder Clay, and merit special consideration. They are almost all of the type known as "coppice with standards", in which standard oak trees (*Quercus robur*) project from a dense shrub layer consisting mainly of hazel (*Corylus avellana*), which is coppiced at rather irregular intervals. They were classified by Adamson many years ago as of the (ash)-oak-hazel type, derived by exploitation from the natural woods by suppression of the ash, which, however, by its strong regenerative powers still gives clear evidence of its natural status. Affinities with the ash woods of calcareous soils are shown by the frequency of calcicole shrubs such as the spindle tree, *Euonymus europaeus* and the wayfaring tree, *Viburnum lantana*, and by herbs in the undergrowth such as *Mercurialis perennis*, *Viola silvestris*, and *Hypericum hirsutum*. The bush species are numerous and include *Acer campestre*, the maple, which is often coppiced with the hazel; both species of hawthorn; the *Crataegus monogyna*, much less frequent as shrub undergrowth than *C. oxyacanthoides*; privet (*Ligustrum vulgare*); dogwood (*Cornus sanguinea*); and blackthorn (*Prunus spinosa*). The maple, privet and dogwood are more frequent than in pure *Quercetum roburis*. Some woods also contain *Viburnum opulus*, *Salix caprea*, *S. cinerea*, *Prunus cerasifera* and *Daphne laureola*.

The early work of Adamson on Gamlingay Wood shows most clearly the dependence of woodland characters upon soil. Most of the wood has a calcareous marl soil where the Boulder Clay overlies Gault, but there is a smaller inland area with a loam soil where the Boulder Clay is above Greensand. The two regions differed strikingly from one another. Abundant coppiced species on the calcareous clay were the ash, maple and hazel, but these were infrequent on the non-calcareous loam. Conversely, the two birches (*Betula alba* and *B. pubescens*) were frequent on the loam but absent from the clay. Similar wide divergences were recognisable between the undergrowth communities. Adamson recognised on the clay soil the four following societies:

(1) *Filipendula ulmaria* society—high summer water content and low light intensity.

(2) *Filipendula ulmaria-Deschampsia caespitosa* society—with high water content but lighter than (1).

(3) *Mercurialis perennis* society—on drier soils and with a wide light range.

(4) *Fragaria vesca* society—in conditions intermediate between those of (2) and (3).

On the loam soil, he recognised two societies, a *Pteridium aquilinum-Holcus mollis* society on the heavier loam, and a *Holcus mollis* society on the sandier loam.

Through the Boulder Clay woods of the rest of the County, Adamson recognised the same communities, the drier, such as the *Mercurialis* society, especially in soils over the Chalk, and the wetter, such as the *Filipendula* society, in soils over the Gault. In general, however, the impermeability of all the Boulder Clay soils leads to winter water-logging which has a marked local influence on the ground flora.

A generalised idea of the ground flora can be gathered from the following lists of species in Hardwick Wood:

Dense old coppice:

Primula elatior
Viola riviniana
V. silvestris
Circaea lutetiana
Sanicula europaea
Geum urbanum
Hedera helix
Ajuga reptans
Mercurialis perennis
Scilla non-scripta
Arum maculatum
Listera ovata
Orchis maculata
Neottia nidus-avis
Habenaria bifolia

Recently coppiced areas show in addition:

Anemone nemorosa
Ranunculus ficaria
Viola hirta
Hypericum hirsutum
Lathyrus silvestris
Filipendula ulmaria
Rubus caesius et spp.
Epilobium angustifolium
E. hirsutum
E. montanum
Angelica silvestris
Galium aparine
Arctium minus
Cirsium palustre
Primula vulgaris
Solanum dulcamara
Scrophularia nodosa
Prunella vulgaris
Lamium galeobdolon
Stachys silvatica
Rumex viridis
Tamus communis
Juncus effusus
Carex silvatica
C. glauca
Deschampsia caespitosa
Brachypodium silvaticum

Further interesting species present in other woods of the same type are *Paris quadrifolia, Helleborus viridis, H. foetidus, Conopodium denudatum, Geum intermedium*, and *Melampyrum cristatum*. On the other hand, it is remarkable that *Oxalis acetosella, Adoxa moschatellina*, and *Allium ursinum*, should be extraordinarily infrequent: the foxglove (*Digitalis purpurea*) is quite absent. The true oxlip, *Primula elatior*, was shown by Miller Christie to be confined in this country to the Boulder Clay areas of East Anglia; and in Cambridgeshire this restriction is very clear,[1] but the status of the

[1] Just as the oxlip and other woodland species like *Paris quadrifolia* and *Daphne laureola* are confined to this formation, so in pastures are *Genista tinctoria* and *Trifolium ochroleucum*; in wood-margins and hedgerows, *Melampyrum cristatum*; and in cultivated fields *Linaria elatine, L. spuria*, and the very rare *Bupleurum rotundifolium* and *Euphorbia platyphyllos*.

plant in our woods and its relation to the primrose (*Primula vulgaris*) are still extremely uncertain, though the hybrids are both abundant and fertile.

The moss flora is not extensive, the commonest species being *Thuidium tamariscinum*, *Brachythecium rutabulum*, *Catharinea undulata*, *Hylocomium triquetrum*, *H. squarrosum*, *Fissidens taxifolius*, *Eurhynchium praelongum*, and *Porotrichum alopecurum*. Elm woods on the Boulder Clay, mostly *Ulmus minor*, can probably be taken as plantations, such as that at Knapwell in which a field system is still recognisable.

II. Scrub

In the south-west of the County, the Boulder Clay cover over Gault has proved so intractable, on account of its very high clay content and deficiency in phosphate, that large areas were allowed to go out of cultivation.[1] Extensive areas of hawthorn scrub of different ages, in consequence, now occupy the ground, and it is possible to make out the main stages of a secondary succession towards woodland. Besides the dominant *Crataegus monogyna*, the young scrub shows frequent *Rosa canina* and *Prunus spinosa*; while the following are either occasional or rare: *Rosa arvensis*, *Rubus fruticosus*, *Ligustrum vulgare*, *Rosa micrantha*, *Rubus caesius*, *Acer campestre*, *Rhamnus catharticus* and *Viburnum lantana*. Along with these, scattered trees of oak or ash are found, and *Ulmus minor* often extends by suckering from nearby hedges. The early stages of bush growth show a remarkable flora of ruderal and pasture species, strongly influenced by very heavy rabbit-grazing and by the local water-logging that follows clogging of the field drains. As the bush canopy closes, this ground flora becomes sparser, and internal competition between the bushes grows, until there is produced a dense scrub of pure *Crataegus monogyna* bushes 5 to 6 m. high, and well spaced apart. The ground is practically bare, but there may be present a very few weakly plants of *Viola hirta*, *Mercurialis perennis*, *Urtica dioica*, *Brachythecium purum*, *Eurhynchium praelongum*, *Fissidens taxifolius*, *Hylocomium triquetrum* and *Mnium undulatum*. It has recently been demonstrated that this scrub shows stages in the development of a new natural soil profile from the old puddled clay surface, and, with further thinning of the old hawthorns, further entry of trees might be expected.

Throughout the succession, animal factors seem to be of great importance: rabbits, mice, woodpigeons, and magpies are present in very great density, while caterpillars wreak great havoc at times below the hawthorn canopy.

[1] See pp. 131 and 150 below.

THE CHALK FORMATIONS

I. Grassland

The wide stretches of Chalk grassland on the North and South Downs and on Salisbury Plain are such a uniform and well-characterised community that the Chalk grassland of Cambridgeshire cannot fail to have special interest. Comparatively little of it remains untouched by cultivation, but parts of Newmarket Heath, the Gogmagog Hills, and Royston Heath (just outside the County) are still more or less natural. The old Roman road (the *Via Devana*), the Devil's Dyke, and the Fleam Dyke (see Figs. 20 and 21), are now also clothed with grass communities and bear most of the typical and some of the rare species of chalk grassland.

A rough indication of the composition of the plant community can be gained from the following group of species collected in an area of a few hundred square yards on Royston Heath. It was obtained by a student class, and represents the results of twenty-nine random throws of a quadrat of one decimetre square. The species are listed in order of the frequency with which they occur in the twenty-nine samples; the figure after each species shows the number of quadrats in which it appeared; the letter before it shows its life-form in Raunkiaer's terms (H = Hemicryptophytes—buds in surface layers of soil; Ch = Chamaephytes—buds close above ground; G = Geophytes—buds below soil; Th = Therophytes—annuals).

H	*Festuca ovina*	28	H	*Asperula cynanchica*	6
Ch	*Helianthemum chamaecistus*	24	H	*Ranunculus bulbosus*	6
H	*Poterium sanguisorba*	20	Ch	*Thymus serpyllum*	5
G	*Carex glauca*	19	Th	*Linum catharticum*	3
H	*Filipendula hexapetala*	18	H	*Campanula rotundifolia*	2
H	*Koeleria gracilis*	17	H	*Achillea millefolium*	1
H	*Plantago lanceolata*	16	Th	*Gentiana amarella*	1
H	*Avena pratensis*	12	H	*Hieracium pilosella*	1
H	*Bromus erectus*	12	H	*Lotus corniculatus*	1
H	*Briza media*	10	H	*Pimpinella saxifraga*	1
H	*Plantago media*	10	H	*Scabiosa columbaria*	1
H	*Hippocrepis comosa*	9	H	*Taraxacum officinale*	1
H	*Cnicus acaulis*	8			

In view of the very small area examined, it is extraordinary how closely this corresponds to Chalk grassland examined by Tansley and Adamson on the South Downs: of the fifteen most constant species given by these authors, fourteen are represented above. Many other highly characteristic species are to be found in other parts of the grassland on Royston Heath; among them are the following: *Leontodon hispidus, Avena flavescens, Galium verum, Primula veris, Carlina vulgaris, Polygala vulgaris, Daucus* *carota*, *Anthyllis vulneraria*, *Campanula glomerata*, *Euphrasia officinalis*, *Thesium humifusum*, and *Astragalus danicus*. Two species of special interest are the Pasque flower (*Anemone pulsatilla*) and the bee orchis (*Orchis apifera*).

In the above list, the preponderance of hemicryptophytes is particularly striking; it may perhaps reflect on the one hand grazing, on the other, summer drought to which the porous chalk soil is very liable. At Royston, there is much local differentiation of grassland types from short rabbit-grazed turf on the hill crests, with *Festuca ovina* dominant, to dense thick turf dominated by *Bromus erectus*, on the deeper soils of the slopes and valley bottoms. On disturbed soils, *Arrhenatherum avenaceum* becomes prominent.

The general uniformity of the Chalk grassland can be illustrated by comparing with the Royston list those for two separate one-metre quadrats, one on the old Roman road and the other on the Fleam Dyke. Frequencies of the species are given by the conventional symbols.

Northern end of the *Via Devana*	
Festuca ovina	co-d
Koeleria gracilis	,,
Avena pratensis	,,
Scabiosa columbaria	a
Thymus serpyllum	f to a
Lotus corniculatus	,,
Helianthemum chamaecistus	,,
Asperula cynanchica	,,
Poterium sanguisorba	,,
Galium verum	,,
Carex glauca	,,
Centaurea nigra	,,
Euphrasia officinalis	f
Daucus carota	o to f
Plantago media	,,
P. lanceolata	,,
Cirsium acaule	,,
Anthyllis vulneraria	,,
Linum alpinum var. *anglicum*	,,
Filipendula hexapetala	,,
Onobrychis sativa	,,
Phleum pratense	,,

Fleam Dyke[1]	
Festuca ovina	a
Poterium sanguisorba	co-d
Hieracium pilosella	va
Carex glauca	sd
(In descending order of frequency)	
Helianthemum chamaecistus	
Briza media	
Koeleria gracilis	
Avena pratensis	
Thymus serpyllum	
Hippocrepis comosa	
Lotus corniculatus	
Leontodon hispidus	
Asperula cynanchica	
Galium verum	
Cirsium acaule	
Plantago media	
P. lanceolata	
Scabiosa columbaria	
Pimpinella saxifraga	
Centaurea nigra	
Linum cartharticum	
Campanula rotundifolia	
Euphrasia officinalis	
Anthyllis vulneraria	
Carlina vulgaris	
Pinus silvestris (one seedling)	

[1] This list is taken from A. G. Tansley, *Types of British Vegetation* (1911), p. 178.

The moss flora of the Chalk grassland is equally characteristic; the following representative list is given by Dr P. W. Richards:

Camptothecium lutescens (abundant)	*Seligeria pauciflora*
Brachythecium purum (abundant)	*Phascum curvicolle*
Hypnum molluscum (abundant)	*Brachythecium globosum*
H. chrysophyllum (abundant)	*Fissidens decipiens*
Trichostomum flavo-virens	*Hypnum cupressiforme*
T. tortuosum	var. *tectorum*
Cylindrothecium concinnum	var. *elatum*
Ditrichum flexicaule	*Pottia lanceolata*
Weisia crispa	*P. recta*
W. microstoma	*Tortula pusilla*
Encalypta vulgaris	*Thuidium abietinum*

II. Woodland

There are many indications that, where grazing allows, scrub will invade the Chalk grassland, and that the incoming bushes and trees will often form dense thickets. Hawthorn, blackthorn, dogwood, and *Rhamnus catharticus*, are usually the commonest shrub species: the evergreen yew and juniper occur but sparsely.

There is little evidence about the natural woodland vegetation of the Chalk. Though several beech woods exist, they are either plantations or have been much altered by planting, and the beech regenerates feebly in them. In the upper peats by the fen margin, however (e.g. Wicken), quite high percentages of beech pollen are to be found, which suggests that natural beech woods were recently growing nearby.

The beech woods are small and the floor is often wind-swept, so that the undergrowth is sparse. It commonly includes in the shrub layer *Ligustrum vulgare*, *Rubus fruticosus*, *Ilex aquifolium*, *Sambucus nigra*, and sometimes *Taxus baccata*. In the herb layer, there are commonly *Poa nemoralis*, *Festuca rubra*, *Brachypodium silvaticum*, *Nepeta glechoma*, *Fragaria vesca*, *Myosotis silvatica*, *Listera ovata*: less common are *Cephalanthera grandiflora*, *Orchis maculata* and *Monotropa hypopithys*.

CHAPTER FIVE

THE ZOOLOGY OF CAMBRIDGESHIRE

Edited by A. D. Imms, F.R.S.

(With contributions by M. D. Brindley, W. S. Bristowe, J. E. Collin, H. St J. K. Donisthorpe, J. C. F. Fryer, A. D. Imms, G. J. Kerrich, A. G. Lowndes, W. H. Thorpe, H. Watson, and H. E. Whiting)

AMONG WRITINGS ON THE ZOOLOGY OF THE COUNTY, THE manuscript catalogue of insects, and related animals, which was compiled by the Rev. L. Jenyns (afterwards Blomefield), deserves first mention. Its author lived at Bottisham in the early part of the last century and his observations were made prior to 1849. His list makes it possible to ascertain, in a general way, what species have declined or become extinct during the last century or so. The catalogue is kept in the University Museum of Zoology. When the British Association visited Cambridge in 1904, there was produced the *Handbook to the Natural History of Cambridgeshire* (edited by J. E. Marr and A. E. Shipley). This has remained the only general account of the zoology of Cambridgeshire.

Between 1923 and 1932 there appeared *The Natural History of Wicken Fen*, edited by Prof. J. Stanley Gardiner. This work makes a notable advance on previous knowledge of the zoology of the County. In 1934, there came *The Birds of Cambridgeshire*, by D. Lack. Finally, the present year will see the publication of the first volume of the *Victoria County History of Cambridgeshire* which will contain the most up-to-date and detailed account of the local fauna.[1]

MAMMALIA

The mammals are rather poorly represented in the County. The absence of any large wooded areas is regarded as being one of the contributing causes, while the reclaimed Fenland seems to be unsuitable for supporting any considerable mammal population. Among the bats is included the scarcest mammal of the County, viz. the mouse-ear bat (*Myotis myotis*). A living specimen of this creature was recorded from Girton in 1888, and was doubtless a wanderer from the continent. The whiskered bat (*M. mystacinus*) and Natterer's bat (*M. nattereri*) are scarce, but apparently resident, species. Among other species, Daubenton's bat (*M. daubentoni*),

[1] I would like to acknowledge a general indebtedness to the Editor (Mr L. F. Salzman) for allowing us to use material prepared for the *Victoria County History of Cambridgeshire*.

the long-eared bat (*Plecotus auritus*), and the barbastelle (*Barbastella barbastellus*) also occur, but the last-named appears to be comparatively rare. The fox is general except in the Fenland, where it is a straggler. The badger has been noted occasionally in different parts of the County, and the otter occurs in the river near to, and above, Cambridge, but is rather infrequent. The stoat and weasel are plentiful, but the pine marten and true polecat are extinct. Some of the later records of the last-named species probably refer to polecat-ferrets and not to genuine wild specimens. The common shrew, the pigmy shrew, and the water shrew all occur, the first-named being the commonest. The dormouse appears to be very rare, and, of the voles, the water vole and the short-tailed vole are prevalent, while the bank vole is uncommon. The long-tailed fieldmouse appears to be local, and the harvest mouse has not often been recorded. The red squirrel occurs in various localities, while the American grey squirrel has only been occasionally reported.

AVES[1]

In general, a county is a most unsatisfactory unit for ecological and natural history studies. This is even more obvious when dealing with birds than it is with more sedentary animals. Indeed, the only reason for choosing counties as a basis for studies of bird distribution is that they provide accurately demarcated areas of convenient size. Accordingly, the object of this brief sketch is to call attention to the main bird habitats of the Cambridge district without deference to county boundaries.[2]

To the west of Cambridge lies a countryside of heavy clay soils (see Fig. 29), mostly under cultivation, with some small mixed deciduous woods and copses in which oak predominates. Here the birds are typical, in general, of the Midlands, although the absence of larger woods with old trees restricts the fauna considerably. Indeed, as D. Lack has pointed out, Cambridge itself is almost the only part of the district where old deciduous trees are numerous, and where tree-climbing and hole-nesting species are common.[3] Five species of tits, three woodpeckers, and the stockdove, are all associated with old trees and may be observed on the Backs. Here, too, the nuthatch is common, and the presence of the somewhat elusive tree creeper is shown[4] by the numerous roosting holes scratched out of the bark of almost every specimen of *Sequoia gigantia*.

[1] By W. H. Thorpe, M.A., Ph.D.

[2] A more detailed account is the excellent study by D. Lack, *The Birds of Cambridgeshire* (1934). I am indebted to this, and to the reports of the Cambridge Bird Club.

[3] D. Lack, *op. cit.* p. 13.

[4] W. H. Thorpe, "The Roosting Habits of the Tree Creeper", *British Birds*, xviii, 20 (1924).

This roosting habit is of interest in that there is no native European tree that has a bark sufficiently soft to allow of it, yet, since the introduction of the *Sequoia* in 1853, the habit has become established throughout the British Isles. Where undergrowth is found, bullfinch, goldfinch and hawfinch breed, and the nightingale, lesser redpoll, and spotted fly-catcher also occur; while, most notable of all, is the recently established nesting of the black redstart in the centre of the town.

The trees of Cambridge also provide nesting sites for the rook, which is so common that the town may be described as one large rookery. While at Madingley Hall, a few miles to the west, is a rook roost which accommodates 15,000 or more birds in winter: here come the birds which feed within a radius of six to eight miles or more.

There are also starling roosts in the district, some accommodating as many as 120,000 birds, but these shift very considerably and the *Annual Reports* of the Cambridge Bird Club should be consulted for details.

The natural vegetation of the chalk upland with its occasional beech woods is found at Royston Heath, Newmarket Heath, and in very restricted portions of the Gogmagog Hills.[1] The characteristic birds here are the woodpigeon, skylark, meadow pipit, and corn bunting. Stone curlews nest regularly in small numbers, and quails breed in some years—mere remnants of their former vast hordes. In winter, bramblings frequent the beech woods in considerable numbers. Elsewhere, the chalk country is under crops, and birds are sparse, though large flocks of lapwing, golden plover, redwings, and fieldfares are a feature of the winter landscape.

East of Newmarket Heath, lies that great area of sands, gravels and boulder clay, the Breck country, occupying 400 square miles of Norfolk and Suffolk, and bordering Cambridgeshire.[2] Its barren sandy heaths and pine woods are characterised by stockdove, woodlark, nightjar, wheatear, stone curlew, and crossbill. There is also that curious inland breeding "race" of ringed plover which perhaps represents a relic of the littoral fauna of the old Fen Estuary. The Forestry Commission is however rapidly altering the aspect of much of this country,[3] and this close planting has had considerable effect upon the distribution of certain species.[4]

The last and the most characteristic type of country in the Cambridge district is, of course, the Fenland. By far the greater part of this area is now under cultivation, and the corn bunting, sedge warbler, reed bunting, and, more rarely, the corn crake, are among the characteristic species. The tree sparrow and magpie are also abundant—in unexplained contrast to their comparative scarceness south of the Cambridge-Newmarket road.

[1] See p. 57 above. [2] See p. 208 below. [3] See p. 217 below.
[4] D. Lack, "Habitat Selection in Birds", *Journ. Animal Ecology*, ii, 239 (1933).

In winter, besides great flocks of lapwing and golden plover, the black-headed and common gulls are numerous, while pink-footed geese are not infrequently found, particularly near Wisbech. Although there are patches of open uncultivated fen country at Fulbourn, Chippenham, Reach, Quy, and Burwell, almost the only remnant of undrained fen is at Wicken.[1] But, particularly because of the lack of reed beds and open water, the avifauna of Wicken Fen is only a fraction of what it once was. Gone beyond recall are pelican, crane, and spoonbill, that once inhabited the fens. Gone too, as breeding species, are Savi's warbler, bearded tit, black-tailed godwit, ruff, black tern, and the bittern. But the last three or four of these are still visitors to the district, and there is a possibility that some of them might be induced to return if conditions were made suitable. However, the Montagu's harrier and the short-eared owl still breed at Wicken in most years, and the grasshopper warbler is perhaps the most abundant and the most characteristic small bird of the Fen, while, at other seasons, marsh and hen harrier, peregrine falcon, merlin and common buzzard are occasionally seen. The existing open water attracts mallard, shoveller, teal, garganey, tufted duck, wigeon and the pochard, the first four as breeding species. But there is no doubt that the greatest need of Wicken as a bird reserve is the digging of a large mere and the encouragement of reed beds.

Finally, no summary of the ornithology of the district, however brief, would be complete without mention of the Cambridge Sewage Farm, two miles north of the town. Regular watching, mainly by members of the Cambridge Bird Club, has revealed an astonishing variety of passage birds, particularly of waders. Of special interest are the records of yellowshank, turnstone, curlew, sandpiper, Temmincks' stint, grey phalarope, and dotterel. Indeed, more wading birds have been recorded at the Cambridge Sewage Farm than at any other inland locality in Britain, and the observations carried on there have done much to discredit the theory that birds on inland migration follow definite routes such as the courses of rivers. All the observations in this district go to show that waders, when migrating, habitually fly at a considerable height and move on a broad front across country.

REPTILIA

These include the common lizard (*Lacerta vivipara*), which seems to be local in distribution, but which is plentiful in Wicken Fen. The sand lizard (*L. agilis*) occurs about the Devil's Ditch near Newmarket, while the slow

[1] See pp. 45 and 50 above.

worm (*Anguis fragilis*) has been recorded by Prof. Stanley Gardiner from Wicken Fen. The grass snake (*Tropidonotus natrix*) occurs in suitable places, but it is very doubtful whether the viper (*Vipera berus*) can still be found in the County.

AMPHIBIA

Apart from the common frog and common toad, which are prevalent throughout the County, the natterjack (*Bufo calamita*) occurs chiefly at Gamlingay, where its spawn is to be found in the shallow water of some of the clay pits. The edible frog (*Rana esculenta*), though once common, is now seldom found. The crested or warty newt (*Molge cristata*) is common in ponds and ditches, while the common newt (*M. vulgaris*) is very general in its occurrence. The palmated or webbed newt (*M. palmata*) seems to be confined to Quy Fen; at least, there are no records from other parts of the County.

PISCES[1]

The sea lamprey (*Petromyzon marinus*) occurs in the River Nene and is sometimes caught above Earith. The river lamprey or lampern (*Lampetra fluviatilis*) is common in the Hundred Foot River, in the Ouse above Earith, in the Little Ouse, and in the Nene; and a number of lamperns was found in the Cam near Grantchester about the year 1927. The salmon (*Salmo salar*) is now only an occasional visitor. Trout (*S. trutta*) occur in the more rapid streams but are not very common. Pike, roach, dace, eels, minnow, rudd, tench, gudgeon, bleak, loach, perch, and miller's thumb, are all common. The grayling (*Thymallus thymallus*) is not indigenous but has been introduced into the River Lark. The chub (*Squalius cephalus*) is rather local and occurs near Cambridge in Byron's Pool. The silver bream (*Blicca bjoerkna*) and the bream (*Abramis brama*) occur commonly in the Fenland, while the Crucian carp (*Carassius carassius*) is apparently rare, and the common carp (*Caprinus carpio*), too, is not abundant. The spined loach (*Cobitis taenia*) occurs locally near Cambridge, and the burbot (*Lota lota*) is common in parts of the Fenland waters. The three-spined stickleback (*Gasterosteus aculeatus*) and the ten-spined stickleback (*Pungitius pungitius*) are both common: the latter occurs in fen ditches and lodes up to Lingay Fen above Cambridge. The flounder (*Platichthys flesus*) is frequently taken in the fenland rivers. Various marine fishes have been caught near Wisbech but, excepting the grey mullet, greater weever, and the dory, they have only been represented by single records

[1] From data supplied by H. E. Whiting, B.A.

MOLLUSCA[1]

The neighbourhood of Cambridge is very favourable for Mollusca. The Fenland and the quiet waters of the Cam and its tributaries form a suitable habitat for many freshwater species; thus, among the Gastropods, in addition to five species of *Lymnæa*, no fewer than eleven members of the Planorbidae have been found within about a mile of Cambridge. *Theodoxus fluviatilis* (Lin.) and various other operculate forms also live in the Cam near the college bridges, while from Wicken as many as ten species of the Pelecypod genus *Pisidium* have been recorded. *Vertigo moulinsiana* (Dupuy), a scarce land snail restricted to marshy places, may also be found in Wicken Fen, and *Laciniaria biplicata* (Mont.) lives close to the river not far from Cambridge itself, although it is found in very few other places in the British Isles.

Upon the chalk hills to the south of Cambridge, on the other hand, xerophilous species are common; *Helicella virgata* (da Costa), *H. gigaxii* (Pfr.), and *Monacha cantiana* (Mont.) being especially abundant, the two former showing much variation; while about 5 miles south of Cambridge is found one of the very few British habitats of the large *Helix pomatia* (Lin.). *Helicigona lapicida* (Risso) occurs at Fen Ditton, but it is rare in Cambridgeshire, whereas its ally *Arianta arbustorum* (Lin.) is common and has even been known to find its way into the roof of King's College Chapel.

Slugs are not exceptionally abundant in the neighbourhood of Cambridge, but about eleven species have been found, and *Agriolimax reticulatus* (Müll.) and *Arion hortensis* (Fér.) are both very common, the former varying greatly in colour. Moreover, all the eleven British species of the Zonitidae, a family of snails related to some of the slugs, have been recorded from the district.

In gardens around Cambridge, *Trichia striolata* (Pfr.), *Helix aspersa* Müll., and other forms are abundant, and in the University Botanic Gardens six or seven exotic species have become established in the hot-houses. Excluding these foreign introductions, about 110 species of land and freshwater Mollusca are known to live in Cambridgeshire, as well as two or three brackish-water forms that occur in the north of the County, e.g. *Hydrobia ventrosa* (Mont.).

Most of these 110 species are also present in the local Pleistocene and Holocene gravels, but some of them have not been found in these deposits, including certain species that are now among the commonest in the neighbourhood, such as *Trichia striolata* (Pfr.) and *Monacha cantiana*

[1] By Hugh Watson, M.A.

(Mont.). On the other hand, the gravels contain several species that have not yet been reported alive in Cambridgeshire. Some of these, such as *Ena montana* (Drap.) and *Helicodonta obvoluta* (Müll.), still live in other parts of England; others, such as *Clausilia pumila* (Pfr.) and *Corbicula fluminalis* (Müll.), are now found alive only on the Continent; and one or two, such as *Helicella crayfordensis* Jackson, seem to be wholly extinct. In fact, the extensive river-gravel system of Cambridgeshire throws valuable light on the gradual modification of its molluscan fauna from middle Pleistocene times to the present day. It is possible, however, that further search will show more living species than are at present known; for although Cambridgeshire is rich in Mollusca, it numbers very few collectors who are interested in these animals.

ARACHNIDA[1]

Records from the County comprise 245 spiders, 10 harvest spiders, and 7 pseudo-scorpions. The Acarina are not described. In collecting, the Fenland has deservedly received the greatest attention and Wicken Fen in particular. *Neon valentulus* Falc. (a small dark Salticid), *Maro sublestus* Falc. and *Centromerus incultus* Falc. (small black Linyphiids), are unknown elsewhere; *Zora armillata* Sim. has not been found elsewhere in Britain (this is a pale speckly Clubionid); *Maso gallica* Sim. has also been recorded only from Kent, *Entelecera omissa* Camb. doubtfully from Northumberland, and *Singa herii* Hahn. doubtfully from Berkshire.

May and June are the best months for collecting spiders at Wicken, and on a sunny day careful search will reveal several of the so-called rarities in abundance. Enclosed in silken cells in the fluffy heads of *Phragmites* will be found the handsome Salticid, *Marpessa pomatia* Walck. Running in the open, alongside the large velvety *Pirata piscatoria* Clerck and other Lycosids, will be seen the lighter coloured *Pardosa rubrofasciata* Ohl. Amongst clumps of hay-coloured grass will be found both the pale elongate *Tibelli* and another less common and also pale Thomisid, *Thanatus striatus* C.L.K. Most of the rarities must be sought for by grubbing at the roots of herbage or by turning over bundles of cut reeds: the black *Zelotes latetianus* L.K., the small rather pinkish *Clubiona neglecta* Camb., the large thick-set Lycosids *Trochosa spinipalpis* F. Camb. and *T. leopardus* Sund., the speckly Salticid *Sitticus caricis* Westr. and such small uncommon Theridiids and Linyphiids as *Crustulina sticta* Camb., *Theridion blackwallii* Camb., *T. instabile* Camb., *Taranucnus setosus* Camb., *Mengea warburtonii* Camb., *Gongylidiellum murcidum* Sim. and *Wideria melanocephala* Camb.

[1] By W. S. Bristowe, Sc.D.

Some of the College cellars provide a number of species. Rather damp cellars provide the largest fauna, but the long-legged and small bluish humpy-bodied Pholcid, *Physocyclus simoni* Berl., is exceptional in liking dry wine cellars such as those of King's and Trinity Hall. Since first adding this species to the British list in 1932, the present writer has discovered it in no less than nine counties, but, apparently, it has not been found elsewhere in Britain. Abroad, it is known in France only. The Pholcid, *Pholcus phalangioides* Fuess. is also present in Cambridge cellars, but its much larger size, different coloration and somewhat elongate body, easily distinguish it.

The only British Mygalomorph spider in the County, *Atypus affinis* Eich., has been recorded from Devil's Dyke, where its closed silken tube, like the finger of a glove, should be sought amongst vegetation on the sloping bank.

The recorded harvest spiders and pseudo-scorpions do not include any special rarities.

INSECTA

Accounts of five of the major groups in this class are given below. These will serve to give an idea of some of the more noteworthy species that are to be found in the County. Good reference collections of all the major, and most of the smaller, orders of insects are contained in the University Museum of Zoology.

HEMIPTERA (HETEROPTERA).[1] Cambridgeshire contains four main types of country, each of which possesses a distinctive Heteroptera fauna correlated with the associated flora and soil conditions. To the south lies the chalk; westwards are heavy clays; to the north is the drained alluvium of the fen basin; and in the east the boundary includes a small tract of the Breckland (see Fig. 56). The County list contains 256 species, out of 492 recorded for Britain. Drainage and agriculture have changed conditions, and in the Fenland at least the dominant species to-day are more typical of cultivated land than of marsh.

Although the fen basin was formerly an estuarine sea, no coast-loving species seem to have survived there, with the exception possibly of *Rhyparochromus praetextatus*, *Teretocoris antennatus* and *T. saundersi*, *Salda pallipes*, and a doubtful record of *Piesma quadrata*. On the other hand, the freshwater fauna may not have changed greatly; for although since the draining, standing water has diminished in extent, conditions in the habitat itself have probably remained fairly constant. Of the seventy-five species of water-bugs recorded for Britain, forty-seven have been found in Cambridgeshire. Most of them are widely distributed forms, charac-

[1] By Mrs M. D. Brindley.

teristic of the Northern Palaearctic region. An interesting species found at Wicken is *Glaenocorisa cavifrons*, which, apart from a larger darker form long known from the Scottish Highlands, has a restricted range in Britain. Four forms of *Notonecta* occur, while *Naucoris cimicoides* and *Ranatra linearis* are frequent in ponds. Cambridgeshire is not well provided with running water, but where it is found, as in the River Cam, *Velia currens* (Veliidae) and *Hygrotrechus najas* (Gerridae) are common. The five other Gerrids found in Cambridgeshire are inhabitants principally of standing water.

Turning to the land bugs, *Chartoscirta elegantula*, which in Britain is restricted to four counties only, is found at Wicken. Other species, recorded there and not elsewhere in the County, are *Hebrus ruficeps*, *Myrmedobia tenella*, *Pamera fracticollis*, *Teratocoris antennatus* and *T. saundersi*, *Adelphocoris ticinensis*, *Eurygaster maurus*, *Cyrtorrhinus geminus* and *C. pygmaeus*. *Chilacis typhae* occurs at Wicken and elsewhere where the reed-mace grows. *Doliconabis lineatus* is frequent on reeds, and *Oncotylus viridinervus* is found on *Centaurea* in fen pastures.

Characteristic species of the chalklands are *Calocoris roseomaculatus*, *Poeciloscytus unifasciatus*, and *Myrmus miriformis*. *Amblytylus affinis*, *Onychunemus decolor*, *Hoplomachus thunbergi*, and *Halticus apterus*, often occur in some numbers. *Eremocoris podagricus* has been recorded from near Royston, and two species of *Berytus* with *Metacanthus punctipes* are frequent, especially where the chalk bears hawthorn scrub. Where the fields are bounded by screens of *Pinus*, there appears an intrusive population of conifer-dwellers, such as *Gastrodes ferrugineus* and *Acompocoris pygmaeus*.

The claylands sometimes bear deciduous woodland.[1] As the Heteroptera here have been little studied, it is possible that additional species await discovery. Various Pentatomidae occur, such as *Eusarcoris melanocephalus*, *Palomena prasina* and *Gnathoconus albomarginatus*; while other characteristic species are *Macrotylus solitarius*, *Macrolophus nubilus*, and *Calocoris ochromelas*.

In the fruit-growing districts, the Capsids *Plesiocoris rugicollis* and *Lygus pabulinus* are pests of apple and currant. Other species of interest in the neighbourhood are *Reduvius personatus*, which is sometimes taken at dawn in the town; and the bird and bat parasites, *Cimex columbarius*, *Oeciacus hirundinis* and *Cimex pipistrelli*, which were first described by Jenyns from Cambridgeshire.[2]

LEPIDOPTERA.[3] For at least two centuries Cambridge and its neighbourhood have been celebrated for their butterflies and moths, and although some of the more interesting species have now become extinct,

[1] See p. 52 above. [2] See p. 60 above.
[3] By J. C. F. Fryer, O.B.E., M.A.

the fauna is still a remarkable one. Perhaps the best way to give the reader a brief introduction to it is to consider rather the different types of habitat exhibited by the country near Cambridge than to attempt any description of the different species.

First, Cambridge itself deserves mention, since its old walls are the haunt of a special race of a moth (*Bryophila muralis*), of which the typical form is largely confined to the southern and western coasts. The species has never established itself in the adjacent villages (even when introduced) but, in Cambridge town, it seems able to survive the changes of modern times. The moth appears in August and the larvae feed on the algal growth on old walls, like those of its common relative, *B. perla*.

Leaving Cambridge, the most important habitat is that of the Fenland. Most of this area is intensively cultivated, but fen species persist in the dykes and clay pits; instances are the local "Wainscot" moths, *Leucania obsoleta*, *Senta maritima* and *Nonagria arundinata*, found where the common reed is left uncut, and the marsh carpet (*Cidaria sagittata*), a rare and local species occurring in dykes (also fens) where its food plant—meadow rue—grows. A few areas remain in a more primitive condition, the fens of Wicken and Chippenham in Cambridgeshire being the most famous. Wicken, best known for the swallow-tail butterfly, is also the haunt of many interesting species, such as the reed leopard (*Macrogaster castaneae*) and the marsh moth (*Hydrilla palustris*), the latter a very rare insect with the habit of flying chiefly between midnight and dawn. The Dutch large copper butterfly, very closely resembling the extinct English large copper[1] is being re-established at Wicken.[2] Chippenham somewhat resembles Wicken in its Lepidoptera, but the swallow-tail is not found there, although some other species occur more abundantly—as, for instance, the Noctuid *Bankia argentula*, elsewhere in the British Isles almost confined to Killarney.

The higher land immediately bordering the Fenland, and also the higher parts of the Isle of Ely, support three characteristic Tortricid moths (*Phtheochroa schreibersiana*, *Pammene trauniana*, *Laspeyresia leguminana*) and, in addition, one of the scarcest of British "dagger" moths (*Acronycta strigosa*), which elsewhere has only been found near Tewkesbury, a remarkable distribution in view of the universal occurrence of hawthorn, its food plant.

Next to the Fenland, the most important area is that of the Chalk in the

[1] See p. 188 below.

[2] Woodwalton Fen, not far from the site of the former Whittlesea Mere, in Huntingdonshire, contains some of the fen species found at Wicken, and also another local Noctuid *Tapinostola extrema*, at one time thought to be extinct. This fen is best known for the successful re-establishment of the Dutch large copper butterfly a race very closely resembling the extinct English large copper.

south of the County. Here, most of the typical chalk Lepidoptera are found, e.g. the chalk hill blue, which occurs in an interesting race at Royston. The best localities for seeing the chalk Lepidoptera are the Devil's Dyke, the Fleam Dyke and the Roman Road.

Two other areas, each with a different fauna, are also accessible from Cambridge. Extending to the County borders in the south-east is the Breckland of Norfolk and Suffolk, which has a very characteristic fauna. Such species as the Noctuids *Dianthoecia irregularis*, *Agrophila trabealis* and the Geometrid *Lithostege griseata* are found nowhere else in the British Isles, while some species otherwise largely confined to the seashore occur there also.

At the opposite side of the County, just across the border into Huntingdonshire, another distinct fauna is found, that characteristic of oak woods growing on clay, the most characteristic species being the black hairstreak, which in Britain has a very restricted distribution. Monks and Warboys Woods are typical of this type of country and are those best known to lepidopterists.

COLEOPTERA.[1] The following account of beetles deals only with the rarer species. Many species thought, in 1904, to occur only in Cambridgeshire have since been taken elsewhere.

Aleochara fumata Gr. (Fowler—*A. brevipennis* Gr. var. *curta* Sahlb.): taken by the late G. C. Champion at Soham; since been taken by the writer in the New Forest and Windsor Forest. *Rhantus adspersus* J.: taken in profusion by Charles Darwin, but is now apparently extinct in Cambridgeshire. *Trichopteryx championis* Matt., *Ptilium caesum* Er. and *P. incognitum* Matt.: all from Wicken Fen, where they have not been taken again, nor have they been recorded from any other locality. *Cryptophagus schmidti* Strm.: two specimens taken by G. C. Champion in Wicken Fen, and one by the late E. W. Janson at Whittlesea, remained unique, until it was rediscovered by the late Miss F. J. Kirk and the writer in Burwell Fen. *Cryptocephalus primarius* Har.: a single specimen was taken by the late Dr Power on the Gogmagog Hills; not since taken, until discovered by R. O. Richards, and also taken by J. Collins, near Oxford. *Tychius polylineatus* Ger.: introduced by Crotch on a specimen taken by himself at Cambridge, about 1863; was retaken in fair numbers by the late Hereward Dollman on the downs at Ditchling, Sussex.

The following species have not been found in Cambridgeshire for many years: *Pterostichus aterrimus* Pk.: formerly common in the Fens; some years ago now Sir T. Hudson Beare took a single specimen in Norfolk, but Mr Bullock has taken it in numbers at Killarney in recent years.

[1] By H. St J. K. Donisthorpe, F.R.E.S.

Graphoderes cinereus L.: has not been taken in the Cambridgeshire fens for very many years; F. Balfour-Browne took a fair number in Norfolk, over twenty years ago.

Of the species Fowler considers to have disappeared before the draining, one may mention: *Trechus rivularis* Gyll.: thought to be extinct. The late A. J. Chitty and the writer took it sparingly in cut sedge bundles at Wicken Fen in 1900. Since then, however, it has been taken again in some numbers by several collectors. *Dytiscus dimidiatus* Berg.: considered to have become exceedingly rare; in 1899 and 1900 the writer took it not uncommonly in Wicken Fen. *Oberea oculata* L.: also considered to have disappeared; was plentiful in 1898 and 1900, and was also taken on other occasions by Beare, Bouskell and the writer. I believe, however, it has got more scarce again. *Lixus paraplecticus* L., also supposed to have disappeared, was found by Mr F. Bouskell and the writer in fair numbers, in 1894, and appeared to be spreading. This also I believe is getting scarce again.

Space allows only brief notes on a few species from the different sections of Coleoptera.

In the Geodephaga or ground beetles, *Ophonus obscurus* F., which occurs at the foot of the Devil's Dyke near Swaffham, appears to be almost confined to Cambridgeshire, though the writer has taken it at Abbotsbury in Dorsetshire. *Chlaenius holosericeus* (*tristis* Schal.) was formerly recorded at Fen Ditton in 1827, and by Charles Darwin near Cambridge. Dr Power took it in Burwell Fen, but it has not since been taken in Cambridgeshire. The beautiful *Panageus crux-major* L. occurs sparingly under horse-cut sedge; it used to be more plentiful formerly.

The water beetles are well represented and we have already dealt with the most interesting species. The most noteworthy of the Hydrophilidae is the large *Hydrophilus piceus* L., which used to be common under water lilies in the Wicken Poor's Fen; it is scarcer now. The very rare *Spercheus emarginatus* was taken by Prof. Babington in Burwell Fen; it has not occurred in Cambridgeshire since.

It is hard to choose which species to mention out of the very large number of Staphylinidae recorded. *Microglossa marginalis* Gr., once recorded as British by Crotch (a single example near Cambridge), has since been taken in birds' nests in many other counties. The very rare *Schistoglossa viduata* Er. has recently been retaken in Cambridgeshire by E. C. Bedwell in Wicken Fen. Passing on to the Clavicornia, *Silpha tristis* Ill. is found on paths and at roots of grass at Wicken Fen, and the much rarer *S. nigrita* Creutz. was taken by Dr Power on the Gogmagog Hills.

Copris lunaris L., in the Lamellicornia, was recorded by the Rev. L. Jenyns as plentiful in 1828 in a field near Melbourn; but it has not been taken in Cambridgeshire since. Of the Serricornia, the last specimen recorded for the very rare *Ludius ferrugineus* L. was taken on a poplar by the Cam about 100 years ago. It has since been taken and bred in Windsor Forest by the late Miss Kirk and the writer in some numbers in recent years. *Platycis minutus* J. was discovered by the late G. H. Verrall in Chippenham Fen in 1898, and has been taken there by the writer and other collectors.

Of the longicorns, *Oberea oculata* L. has already been dealt with. The large *Saperda carcharias* L. used to be abundant both in Wicken village and the Fen some thirty years ago. I understand it is much scarcer now. *Agapanthia lineaticollis* Donov. may still be swept off thistles in the fens.

In the Chrysomelidae, *Chrysomela graminis* L., which used to be abundant on water mint in various parts of the fens, is much scarcer now I am told, and restricted to small local patches. A number of the Donaciae are found, and the rare *D. dentata* Hoff. used to be common on various water plants. *Adimonia oelandica* Boh., a very rare species, was found by the late Mr Blatch in number in Wicken Fen in 1878; it has not occurred since.

Among the Heteromera, the most interesting species are *Cteniopus sulphureus* L., a coast species, not uncommon by sweeping in Wicken Fen. *Lytta vesicatoria* L., the "blister beetle", was originally recorded from the Gogmagog Hills, where it was found again by the writer in 1901. It is sometimes abundant at Newmarket, etc., on privet hedges. *Anthicus bifasciatus* Rossi (new to Britain) was discovered by the Fryers in manure heaps round Chatteris; subsequently taken by Williams in a manure heap at Wicken; and by the late Miss Kirk and the writer in Burwell Fen; all in numbers. It has since been found in Oxfordshire (J. J. Walker), etc. The Rhynchophora, or weevils, are abundant. *Ceuthorrhynchus angulosus* Boh., a local and rare species, was taken by J. C. F. and J. H. Fryer on *Stachys* and *Galeopsis* at Chatteris and Somersham (they also took the rare Halticid *Dibolia cynoglossi* Koch. on *Galeopsis* at Somersham). The curious *Lixus paraplecticus* L., which occurs on *Sium latifolium* in Wicken Fen, has already been mentioned. It is covered with a yellow dust, which is renewed in life. The local *Dorytomus salacinus* Gyll. can be beaten off sallow bushes in Wicken Fen, etc.

DIPTERA.[1] Our knowledge of the distribution of species of Diptera is still very incomplete. Intensive collecting, even in a county such as Cambridgeshire, could not fail to produce a number of species previously unrecorded (or even undescribed). We know still less of the changes that

[1] By J. E. Collin, F.R.E.S.

may have taken place in the fauna but it is interesting to note that Jenyns[1] recorded the occurrence of three of the larger species of Diptera, the Tabanids, *Tabanus bovinus* L. at Ely and Bottisham and *Atylotus rusticus* F. at Cambridge, and the large Tachinid *Peleteria nigricornis* Mg. on the Devil's Dyke. The identity of the last two can be proved by an examination of Jenyns' specimens, but none of these three has been since taken in the County. An interesting case of the reverse condition is that of the handsome Trypetid *Anomaea permunda* Harr. (*antica* Wlk.). Formerly regarded as a rarity it is now abundant, at least locally, and is freely bred from hawthorn berries gathered near Cambridge. Recently it was found in such numbers on the windows of a house on the outskirts of that town as to be considered a "pest".

The physical features of a county are always of primary importance in connection with insect life. The Fenland in the north of Cambridgeshire harbours many species not found in the drier strip of the Chalk to the south. This, and the clay strip of south-eastern Cambridge, the river valleys, and the fringe of Breckland on the east, all provide characteristic species (see Figs. 29 and 56).

Of particular interest is Wicken Fen, where intensive collecting might well produce species unknown elsewhere in the whole country. It was here that the author found the Chloropid *Lipara similis* Schin. which attacks the growing point of the reed (*Phragmites communis*) without doing much apparent damage, while its close relative *Lipara lucens* Mg. causes a large gall-like swelling. Here also was found in 1935 and 1936 the tiny midge *Pterobosca paludis* Mcfie. sucking the juices from the wing-veins of dragonflies. An unexpected capture in 1936 was that of the rare Syrphid *Myiolepta luteola* Gmel.; but some hollow tree on the outskirts of the fen must have been harbouring this species for many years, in the same way that the pollard willow trees at Upware probably accounted for the capture years previously of *Xylomyia marginata* Mg. The occurrence of the rare *Odontomyia angulata* Pnz.—a marshland species—is not surprising.

Chippenham Fen, not very far from Wicken, is almost surrounded by woods and plantations and therefore possesses a somewhat different fauna. This is the home of the uncommon Syrphids *Chilosia nebulosa* Verr., and *Sphegina kimakowiczi* Strbl., and here a specimen of the giant Pipunculid *Nephrocerus flavicornis* Ztt., and the interesting Conopid wasp parasite *Brachyglossum* (or *Leopoldius*) *signatum* W. have been taken. Here are also to be found a few rare Trypetids such as *Spilographa abrotani* Mg., *Rhacochlaena toxoneura* Lw., and *Oxyphora corniculata* Fln., and other Acalyptrates such as *Ochthiphila coronata* Lw., *spectabilis* Lw., and *elegans* Pnz., and

[1] See p. 60 above.

Chymomyza costata Ztt. Chippenham Fen is also at present the only locality (in addition to Spain) where the very tiny Dolichopodid described by Strobl as *Micromorphus albosetosus* is known to occur.

On the chalkland of the south, the Devil's Dyke and Fleam Dyke are excellent localities for the downland species associated with chalk. Here, rare Tachinids may be obtained such as *Lydella angelicae* Mg., *Demoticus plebeius* Fln., *Zophomyia temula* Scop., *Neaera albicollis* Mg., and *Ocyptera interrupta* Mg., as well as many species of *Sarcophaga.* It was here that the new Trypetid *Trypeta* (*Ceriocera*) *microcera*, recently described by Dr Hering of Berlin, was found by Mr G. C. Varley living in the stems of *Centaurea scabiosa*, while many other interesting Acalyptrates occur.

The clayland woods (see Fig. 29) begin at Woodditton, and their associated insects are naturally different from those of the rest of the County. Among the Syrphids, *Chilosia maculata* Fln. is common on the wild onion; *Platychirus tarsalis* Schum. is common on the flowers of *Geum rivale*, while the rare *Chilosia pubera* Ztt. and *fasciata* Egg. have also been taken. The peculiar plant *Paris quadrifolia* is the host plant of the Cordylurid *Parallelomma paridis* Her. Interesting Tachinids such as *Camplyochaeta praecox* Mg., *Actia nigrohalterata* Vill., and *Blepharomyia amplicornis* Ztt. have occurred; while the rare *Xysta cana* Mg. has been taken at Kirtling not far away. Among the Acalyptrates, the rare *Acartophthalmus bicolor* Old. was once found in Woodditton Wood sitting on dead twigs at the bottom of a dense thicket.

An account of the Diptera of Cambridgeshire would not be complete without mention of some of the captures of the late Francis Jenkinson of Cambridge. Particularly interesting was the occurrence of two very little known Tachinids (*Stenoparia monstrosicornis* Schin., and *Helocera delecta* Mg.) in his garden, but he also found at Cambridge the large Pipunculid already mentioned—*Nephrocerus flavicornis* Ztt.—and the Drosophilid *Acletoxenus formosus* Lw., together with many other good species too numerous to mention including (in 1901) the rare Tachinid *Stomatorrhina lunata* F., which is probably only an occasional visitor to this country. Finally, Jenkinson and other Cambridge entomologists have proved that many of the rare species associated with rotting wood are to be found in and about the very old trees of the College Gardens and along the Backs. These include such species as the Syrphids *Mallota cimbiciformis* Fln., and *Pocota apiformis* Schrnk., the four species of the Dolichopid genus *Systenus*, as well as many Anthomyids and Acalyptrates which frequent sappy exudations.

HYMENOPTERA.[1] Although our knowledge of the Hymenoptera of Cambridgeshire is in advance of that of most counties, it is considerably

[1] By G. J. Kerrich, M.A.

behind our knowledge of, say, the Coleoptera; and an attempt to assess the hymenopterous fauna of the County must, in the main, be regarded as preliminary.

The sawfly fauna[1] should be very rich, considering the abundance and variety of willows, grasses, and horse-tails, the most characteristic food plants of these insects; and sawflies certainly are numerous in individual species, particularly in the fen country. Mr Benson collected them energetically during his student days, but they have since received very little attention; and, probably, the known total of 172 species could be nearly doubled. Three Pamphiliidae, five Cephidae, and five Cimbicidae are known: a Cimbicid larva sometimes rewards a search on willows. *Xiphidria prolongata* Geoffr. has several times been taken; and three Siricidae are known, as are both their parasites, *Rhyssa persuasoria* L. (Ichneumonidae) and *Ibalia leucospoides* Hochenw. (Cynipoidea). Of the Tenthredinidae, the occurrence of the rare *Ametastegia albipes* Thoms. is especially interesting.

For Aculeata, the County is much less favourable. On account of the absence of sandy ground, except along two parts of the County boundary, the numerous sand-living species are absentees or strays. The bees are the group least affected; Adrenidae, in particular, are well represented, and many of them visit willow flowers in the spring. Bumble-bees are conspicuous both in and around the fens and in College gardens. An interesting fen bee is the little *Hylaeus pectoralis* Först., which nests in old reed galls of the fly *Lipara lucens* Mg. Its parasite is *Gasteruption rugulosum* Ab. The best distributed ant is *Acanthomyops flavus* Fabr. The hornet, *Vespa crabro* L., nests in old willows and old cottages. *Cleptes semiauratus* L., a sawfly parasite, is fairly common; and the Chrysididae are well represented. Several of these latter belong to the interesting fauna which frequents old posts, including species nesting therein, and their parasites. There are many such posts along the approaches to Wicken Fen, and these have frequently been studied. The commonest species is *Trypoxylon figulus* L.; *Sapyga clavicornis* L., and the rare *Cuphopterus confusus* Schulz, recently discovered there by the late H. P. Jones, may also be mentioned. The species total of Aculeata for the County, excluding the Gamlingay district, is 236.

Of the gall-wasps, only those living on *Centaurea* and *Rosa* have been seriously studied. *Isocolus scabiosae* Giraud and *fitchi* Kieff., *Rhodites spinosissima* Giraud and *mayri* Schlechtd. have not been found; but the other known British species occur in the County, as do the two *Periclistus*

[1] I wish to thank Mr R. B. Benson for giving me access to the manuscript of his section on sawflies for the account in the *Victoria County History of Cambridgeshire*.

spp., inquilines in *Rhodites* galls. *Diastrophus rubi* Bouché and *Xestophanes potentillae* Vill. are known; but the oak fauna is almost untouched. The commonest Figitidae are known, but not much can be said of the other parasitic forms.

Of Ichneumonidae, 452 species are known, a number of which were first British records. Many species are attracted to the flowers of Umbelliferae, which are conspicuous along roadsides and in meadows. *Aritranis carnifex* Grav., *Epiurus melanopygus* Grav., and *Diblastomorpha bicornis* Boie., are characteristic fen species; also *Hemiteles balteatus* Thoms., known in Britain only from Wicken Fen and the Norfolk Broads. Tufts of *Deschampsia caespitosa*, in fens and poor wasteland, provide winter quarters for the females of numerous species. 180 Braconidae have been identified, the majority by the late G. T. Lyle. Only 28 Serphoidea are known; and the list of 60 Chalcidoidea is almost entirely composed of species bred in the course of general biological work.

OTHER ORDERS. Among the smaller orders a beginning has been made by Dr C. H. N. Jackson with the *Collembola*. From among a total of over 150 British species of this order, some 55 have been found in the County, where they have been mostly collected in Wicken Fen and near Cambridge. Among the *Orthoptera*, several of the rarer species have not been found for many years. The most notable recent record is that of the great green grasshopper (*Tettigonia viridissima*), which has been found at Madingley by G. C. Varley. The *Ephemeroptera* have been but little collected, and records of only 13 species are apparently known. Out of a total of 44 British species of *Odonata*, 27 kinds have been found in Cambridgeshire; most of the recent records are due to the late W. J. Lucas and J. Cowley. About half the recorded British species of *Psocoptera* (booklice) have been found in Cambridgeshire by R. M. Gambles and others. Although a few rare species occur in Wicken Fen, there are none that are peculiar to the County. Knowledge of the *Hemiptera-Homoptera* is still scanty, and much work needs to be done on this suborder before an adequate idea of its representatives can be ascertained. In the *Neuroptera*, some 34 species out of a total of 57 British forms have been recorded. The snake flies *Raphidia xanthostigma* and *R. maculicollis* occur in woods, while *Sisyra fuscata* may be found around Cambridge and at Wicken Fen, and probably elsewhere, along with the freshwater sponges with which it is associated. The most notable member of the order is the very rare *Psectra diptera*, an example of which was taken in Wicken Fen in 1934 by H. Donisthorpe. Of the *Mecoptera*, all three British species of *Panorpa* occur, *P. cognata* being recorded from Fleam Dyke. The *Trichoptera* or caddis flies are well represented, especially in Wicken Fen. The most interesting records are perhaps those of the very local *Limnophilus decipiens* and of *Agraylea pallidula*, which were found in the Fen by M. E. Mosely in 1926: the last-named had only been taken once previously in Britain. Some 63 species representing 11 families have so far been found, but there is much scope for futher work on this order.

MYRIAPODA

According to E. B. Worthington, 28 species have been found in Cambridgeshire, and those requiring special mention include *Glomeris marginata* and *Polyxenus lagurus*. The last-named has been found in damp timbers around Cambridgeshire and Wicken. The difficulties attending their identification and the paucity of reference collections probably account for the scanty attention given to the "myriapods" in Britain.

CRUSTACEA[1]

Considering that Cambridgeshire is so essentially an inland county, its crustacean fauna is truly remarkable. This is represented by no less than 76 genera and 166 species. Quite recently, the fauna of Wicken Fen has been investigated fairly thoroughly, but previously to that a great deal of work had been done by Brady and Robertson as early as 1870. Wicken of course represents a part of the original Fenland and possesses its own interesting fauna. There is also the remains of an interesting salt marsh with brackish water fauna to be found at Wisbech.

Among the higher Crustacea are records of *Carcinus maenas* (Pennant) and *Palaeomonetes varians* (Leach). *Chirocephalus diaphanus* Baird is also recorded from Bottisham Park, while there are several records of *Niphargus* or the blind well-shrimp, though it is quite certain that several of Gilbert White's records of the spring keeper refer to *Niphargus* and not to *Gammarus*. The higher Crustacea, with the addition of *Chirocephalus* and *Argulus*, are represented by 18 genera and 24 species.

The Cladocera are represented by 21 genera and 40 species. Most of them come from Wicken Fen, and a few of these are rare or of exceptional interest. *Macrothrix hirsuticornis* Brady and Norman, recorded by W. A. Cunnington, is a rare species, and so are *Acroperus angustatus* Sars and *Alona tenuicaudis* Sars, both recorded from Wicken Fen by P. M. Jenkin. *Anchistropus emarginatus* Sars, recorded by A. G. Lowndes from Wicken Fen, was at one time considered the rarest species of Cladocera in the British Isles, but it is really fairly common. *Polyphemus pediculus* L. is very abundant on Wicken Fen, but it does not occur in many of the midland counties.

[1] By A. G. Lowndes, M.A.

The Copepods are represented by 5 genera and 37 species, but it is safe to say that with the possible exception of Wicken Fen the group has hardly been touched. It is certain that a careful investigation of Whittlesea would greatly add to the number of species. Of the recorded species, that of *Cyclops gigas* Claus is the most important, since it is the only authentic record in the British Isles.

The Ostracods are or were till quite recently a sadly neglected group of Crustacea and yet they should be of considerable interest. In Cambridgeshire, the ostracod fauna is remarkable, no less than 32 genera and 65 species being recorded. *Siphlocandona similis* Baird is a rare species, but occurs fairly abundantly on Wicken Fen. The genus is not recorded outside the British Isles. *Prionocypris olivacea* (Brady and Norman) is a rare species, but it was found in large quantities quite recently at Ashwell (in Hertfordshire just outside the County) by P. F. Holmes. The genus *Pseudocandona* is also recorded from Wicken. This is an important record, for there has probably been more confusion over the species *P. pubescens* (Koch) than over any other species of freshwater ostracod.

The sperms of ostracods are the largest among the whole animal kingdom, and moreover they are highly mobile. In many species, and even in many genera, males are unknown and yet in those cases where males do occur the male genital organs are of a highly complicated type. There is on record one species, *Herpetocypris reptans* Baird, which was known to breed entirely by parthenogenesis for eleven years. The males of this genus are unknown while the females still retain spermatheca with ducts of relatively enormous length, presumably for the reception of these giant sperms, and yet it is pretty certain that the sperms ceased to exist long before the Tertiary period. Holmes has recorded a second species of *Pseudocandona* from Lake Windermere which promises to throw a considerable amount of light on this obscure subject.

There is yet another record which should be of great interest. *Hemicythere villosa* Sars is recorded from the River Cam, where it is quite abundant on occasions. It can be traced right out to sea and is recorded by Sars from some of the deepest fiords of Norway. The same species is also recorded in the fossilised state from the Pre-tertiary period.

HIRUDINEA

W. Ambrose Harding records nine out of the eleven species of British freshwater leeches in the County. The medicinal leech (*Hirudo medicinalis*) has apparently disappeared from Cambridgeshire many years ago. Among the more local species, *Theromyzon tesselata* and *Hemiclepsis marginata* sometimes occur in numbers: the first-mentioned is stated to live upon water-fowl and the latter is at least partially a fish parasite. Very little definite information exists, however, with regard to their hosts.

TURBELLARIA

Five species of flatworms occur in ponds and ditches, and two species are to be found in running water. These latter, viz. *Planaria alpina* and *Polycelis cornuta*, occur in springs where the temperature is low and varies little throughout the year. They are regarded as relics of a former glacial fauna which once populated the County.

PORIFERA

The two freshwater sponges, *Ephydatia fluviatilis* and *Spongilla lacustris*, are prevalent. According to G. P. Bidder, the common species, *E. fluviatilis*, is to be found on wooden piles, lock gates, or under floating wood, in the waters around Wicken. *Spongilla* prefers deeper and moving waters, and may be found growing up from the bottom of the River Cam along the Backs at Cambridge.

CHAPTER SIX

THE ARCHAEOLOGY OF CAMBRIDGESHIRE

Edited by J. G. D. Clark, M.A., PH.D.

(With contributions by J. G. D. Clark, T. C. Lethbridge, and C. W. Phillips)

CAMBRIDGESHIRE IS FAMOUS AMONG ARCHAEOLOGISTS FOR the distribution studies of Sir Cyril Fox. His book, *The Archaeology of the Cambridge Region*, has had a widespread influence, and it covers the extensive material housed in the University Museum of Archaeology and Ethnology, up to the time of its publication in 1923.

Since 1923, the most important work on the upland has been the dyke and cemetery excavations carried out by the Cambridge Antiquarian Society, which have served to place Cambridgeshire in the forefront of Anglo-Saxon studies. In the Fenland, the excavations sponsored by the Fenland Research Committee, with the assistance of the Percy Sladen Memorial Fund, have thrown a flood of light on the relation of successive phases of human settlement to the geographical evolution of the Fenland basin. A fuller survey of the prehistoric archaeology of the County, complete with references, will be found in the forthcoming *Victoria County History of Cambridgeshire.*[1]

The close connection between human settlement and land movement is brought out by Figs. 18–21. The most striking feature of these is the density of settlement in the southern fens during the Bronze Age, and the sparseness of settlement during the Early Iron Age. This change is certainly to be connected with the post-glacial subsidence of the area. In Romano-British times, the distribution of settlement was similar in broad outline to what it had been during the Early Iron Age, with the important exception that the silt fens in the north of the County and in south-eastern Lincolnshire were then intensively cultivated (see Fig. 47). This may have been due to a minor phase of re-elevation, but it may have been facilitated by the superior technical ability of the Romans. Finally, in Anglo-Saxon times the silt fens ceased to be cultivated. This change was due partly, perhaps, to the breakdown of drainage or defensive works, but also to a further slight subsidence. The emptiness of the peat fens at the close of this period is emphasised by the map of Domesday villages (Fig. 22).

[1] I am indebted to the Editor (Mr L. F. Salzman) for permission to use this material in the preparation of this chapter.

THE PALAEOLITHIC AGE

The earliest certain traces of man in Cambridgeshire consist of flint implements incorporated in deposits dating from the late Prof. J. E. Marr's "Period of Aggradation", when gravel accumulated on the flood-plains of rivers in the southern part of the County while the northern portions were submerged beneath the sea.[1] The implements include hand axes of Acheulian type as well as Clactonian and Levalloisian flake tools. The only site in the County at which Lower Palaeolithic implements have been obtained from a well-studied geological section is the famous Travellers' Rest Pit[2] at Cambridge itself; unfortunately the pit has recently gone out of use and the section is no longer visible. Within the borough of Cambridge, also, the Lower Barnwell Village beds have produced implements at Chesterton and Barnwell. Large numbers of unabraded flake and core implements have been obtained at different times from the gravel ridge at Upper Hare Park, Swaffham Bulbeck. A few stray implements have come from the Granta Valley near Hildersham and Linton, while others from Girton, Oakington, and Willingham, mark the course of an extinct river that once flowed from the neighbourhood of Trumpington towards Earith. The gravel spread in the Kennet-Kentford area is known to have yielded many palaeoliths (many of them from over the Suffolk border), but precise information is lacking. The same applies to many finds from the fen islands (e.g. from "Shippea Hill" near Ely). A few flake implements of somewhat doubtful affinities have been obtained from the March gravels, which at this time were situated on the coastline.

No Upper Palaeolithic sites have so far been located in the County, but a few stray flints may point to their existence in the neighbourhood—notably an angle burin from Wicken which exhibits a remarkable dark green-and-white mottled patina. According to Marr, these Upper Palaeolithic flints should be contemporary with a "Period of Erosion", when the sea coast lay far out beyond the Dogger Bank, and when the rivers of Cambridgeshire were eroding their banks.

THE MESOLITHIC AGE

During Mesolithic times Cambridgeshire, in common with southern Britain as a whole, began to undergo a progressive, though not uninter-

[1] Although closely bordering areas of intensive Palaeolithic research, Cambridgeshire does not occupy a prominent place in this field. The best documented finds can be seen in the Sedgwick Museum of Geology, where the admirable index catalogue compiled by the late Prof. J. E. Marr can be consulted. See also the papers by J. E. Marr in *Quart. Journ. Geol. Soc.* lxxv, 210 (1920), and *ibid.* lxxxii, 101 (1926).

[2] See p. 16 above.

rupted, subsidence, which was not fully accomplished until the Early Iron Age. The initial stage in this process was marked by the submergence of the North Sea "moorlog", from which the prong of a Maglemosian fish spear has been obtained, some 25 miles from the Norfolk coast. Part of a very similar specimen was found many years ago in the Royston district, probably from a low-lying site in the Cam Valley. Microlithic industries of Tardenoisian aspect have been found at Fen Ditton, at Chippenham, and on several sandy hillocks in the Ely fens. Excavations on the flanks of one of these hillocks at Peacock's Farm, near Shippea Hill station, revealed evolved Tardenoisian flints stratified in the lowermost peat bed underlying the fen clay and at a depth of some 17 ft. *below* mean sea-level (Newlyn). These flints were of an industry previously well known from the sand dunes between Wangford and Lakenheath, Suffolk. It is thus established that Cambridgeshire, by the close of Mesolithic times, was still at least 30 ft. higher in relation to the sea than it is to-day. Pollen analysis shows that the Late Tardenoisian industry immediately antedates the change-over from pine to alder dominated woods, which marks the Boreal-Atlantic transition in this area.[1]

THE NEOLITHIC AGE

If stray finds of flint implements be excepted (and it is no longer possible in this country to assign any single type exclusively to this phase), there is very little material evidence for a Neolithic settlement of Cambridgeshire. The pottery obtained from the Peacock's Farm excavations, overlying the Late Tardenoisian level, shows, however, that the area was affected by the Neolithic "A" (Windmill Hill) culture, while the level at which it was found (*minus* 15 ft. O.D.) indicates that the subsidence was still at this period far from complete. It is likely that the Neolithic "A" culture spread to the Essex coast and the fen basin by direct overseas movements; but the long barrow at Therfield Heath, Royston, on the line of the Icknield Way, suggests that influences did move up the chalk belt from Wessex, although the absence (with one possible exception in Norfolk) of long barrows from the rest of East Anglia seems to indicate that such influences were unimportant.

No pots decorated in the "A2" style have yet been found in the County, but the recent discovery of a complete bowl in Mildenhall Fen, only a short distance over the Suffolk border, suggests that such finds are not unlikely in the future.

Nor, despite its proximity to the type site (Peterborough), can Cambridgeshire yet show any certain traces of the Neolithic "B" culture.

[1] See p. 18 above.

THE BRONZE AGE

Although but little is yet known of the economy and dwelling sites of the Bronze Age inhabitants of Cambridgeshire, sufficient stray finds (mainly metal objects, but also a few pots) have been made to give some idea of the areas settled at this time (see Fig. 18). The densest zone of settlement was the fen margin between Cambridge and Isleham. Fen islands, such as March, Manea, Chatteris, Littleport, Ely, and Stuntney, have also yielded many finds; so have flat fens like Burnt Fen, Wilburton Fen, Grunty Fen, and the Chatteris-Mepal fens. On the upland, in the south of the County, Bronze Age finds are more or less limited to the chalk belt, the neighbouring areas of Boulder Clay, Gault, and Kimeridge Clay being virtually empty.

One explanation for the relative density of settlement in the Fenland during the Bronze Age is to be found in the natural conditions prevailing at this time. It is known, from excavations at Shippea Hill, that during the Early Bronze Age the fen basin was still several feet (at least 15 ft.) higher in relation to the sea than it is to-day. This was a height quite sufficient to affect profoundly the possibilities for settlement in such a low-lying area by prehistoric man. Conversely, as subsidence set in again, settlement tended more and more to move out of the fen basin. Already by the Late Bronze Age, half the hoards, and more than a third of the loose finds, come from the higher land in the southern part of the County: and by the Early Iron Age the evacuation was almost complete. The gradualness in the drift of population seems to discount the explanation that it was due to sudden economic change, and, in particular, to the introduction of a more intensive type of agriculture, which in this area can hardly have occurred before the Early Iron Age.

According to Sir Cyril Fox, the following stages can be recognised in the local Bronze Age:

Transitional	2000– 1700 B.C.
Early Bronze Age	1700– 1400 B.C.
Middle Bronze Age	1400– 1000 B.C.
Late Bronze Age	1000–5–400 B.C.

Transitional and Early Bronze Age. The earliest metal forms are rare in Cambridgeshire, comprising only four flat axes without expansion of the cutting-edge, and one round-heeled flat riveted dagger. Objects of the full Early Bronze Age, on the other hand, are plentiful, and include 17 flat axes with expanded cutting-edges, 23 flanged axes, two spearheads of class I, one of class II, and one halberd. In addition, there is the recently discovered grooved dagger, found with a perforated stone axe-hammer of

Snowshill type accompanying a contracted skeleton under a round barrow at Chippenham—the only metal object of this period in the County with any associations. This find is an outlier from the recently distinguished Early Bronze Age culture of Wessex.

The scarcity of the earliest metal objects is doubtless due to the over-running of the County by the Beaker people, who first reached here in a more or less "neolithic" stage of culture. Of the two main groups distinguished in south-eastern Britain, Cambridgeshire was affected mainly by the "A" beakers, the "B" group being represented only by a solitary example from Isleham Fen. The "A" beakers are found distributed round the entire margin of the Fenland, and evidently the Wash formed a main avenue of entry for these people. In Cambridgeshire, they settled upon the larger fen islands (March, Doddington, Ely), and on the low-lying Burnt, Burwell, Isleham, Lode, and Quy Fens. They also pushed up the valleys of the Snail, the Cam, and the Granta; and finds from Therfield Heath and Hitchin suggest that some of them pressed down the Icknield Way to Wessex. The Beaker pottery from the County is outstandingly rich, and special mention should be made of the three handled beakers; two of them are of the rare straight-sided type.

The open settlement sites, found on sandy hillocks at Shippea Hill (Plantation and Peacock's Farms) and Isleham, have produced quantities of sherds from beakers with rusticated surface; and, in addition, they have yielded cord-impressed sherds of "food-vessel" affinities with internally bevelled rims. The latter are highly significant, for they show that the "native" element was not submerged by the Beaker invaders. It was among a mixed population that the metal types of the full Early Bronze Age circulated. The flint work from the fen sites is of a high standard; shallow pressure flaking is seen to great advantage on the scrapers, the barbed and tanged arrowheads, and the plano-convex knives.

The County is rich in contracted inhumation burials accompanied by beakers, the graves in every case being flat. Generally, the graves occur singly, but an undoubted cemetery was destroyed over a period of years in a sand-pit off Springhead Lane, Ely, where the discoveries included at least 14 human jaws, an "A" beaker found together with a perforated stone axe-hammer near the head of a skeleton, and a "C" beaker in company with another skeleton. Among the objects associated with isolated beaker burials may be mentioned "a bull's horn", found with the Wilburton Fen beaker in 1847; while a grave group, recently discovered at Little Downham, yielded an "A" beaker, a flint dagger,[1] a flint knife, a V-perforated button, and a pulley ring of shale.

[1] Seventeen similar ones have been found loose in the County.

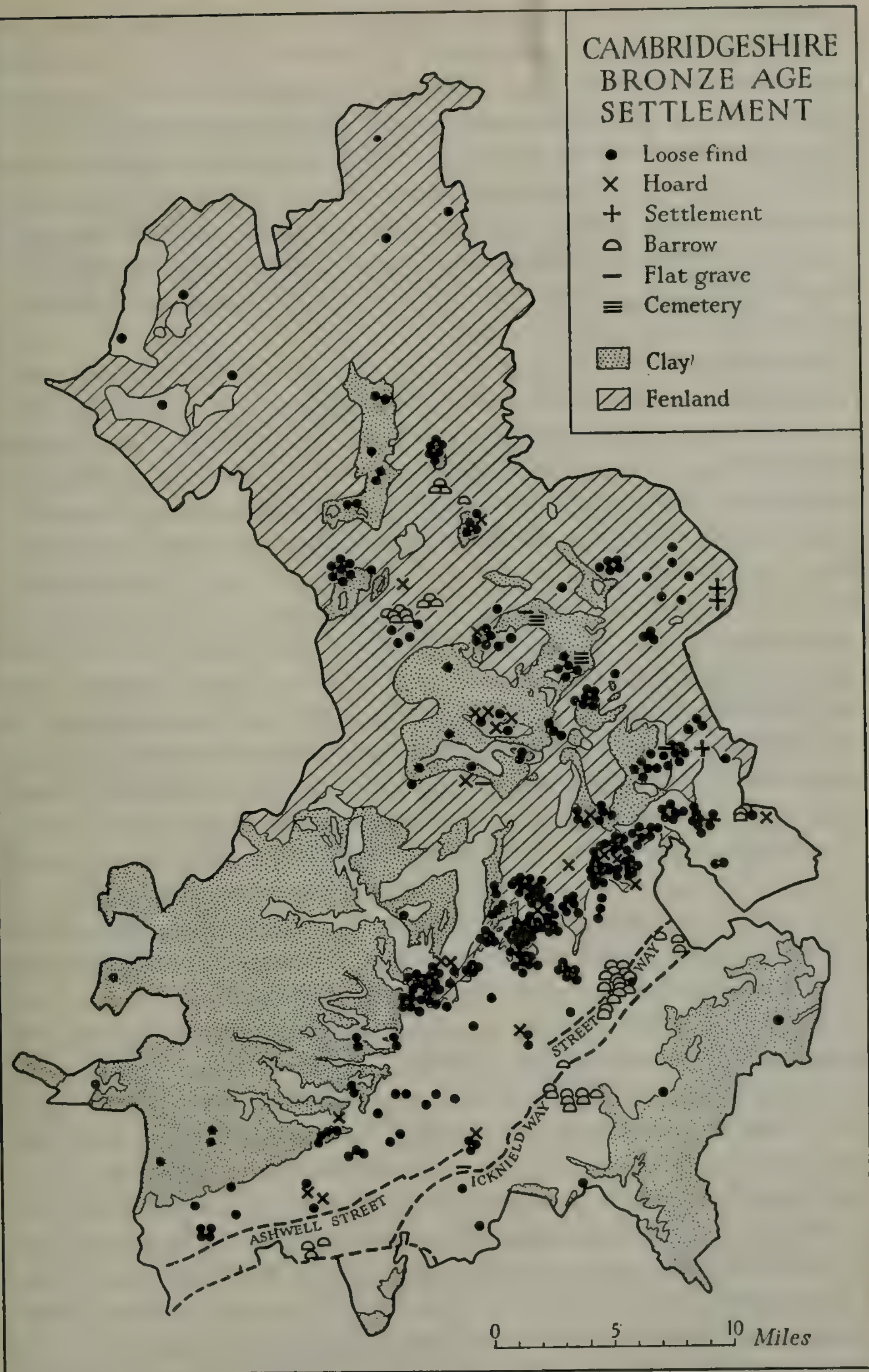

CAMBRIDGESHIRE
BRONZE AGE
SETTLEMENT
Loose find
Hoard
Settlement
Barrow
Flat grave
Cemetery
Clay
Fenland
ICKNIELD WAY
STREET WAY
ASHWELL STREET
0
5
10
Miles

Fig. 18.

Middle Bronze Age. The Middle Bronze Age in Cambridgeshire was a period of prosperity undisturbed by invasion. Bronze implements came more widely into use; no fewer than 122 palstaves, two daggers and dirks, 20 rapiers, and 27 looped spearheads, have been recorded from the County. Irish gold also found its way into the area in some profusion; and there have been two famous finds from Grunty Fen, Wilburton, one in 1844 comprising a splendid multiple-ribbon twisted gold torc with solid terminals and three looped palstaves, and another in 1850, which included a similar torc, a part of a bronze rapier, and a gold bracelet with attached ring money.

The pottery in use locally at this time was the overhanging-rim ware, known from a single open settlement in Isleham Fen, and from many burials. Cremation was the dominant rite, the ashes being contained either in an urn or in some kind of bag. It is to this period that many of the Bronze Age round barrows in the County belong; although some had already been erected in the Early Bronze Age, the cremations being inserted secondarily. The barrows on the chalk belt are strung out along the line of the Icknield Way in five main groups; at Chippenham, on Newmarket Heath, at Upper Hare Park, near the junction of the Fleam Dyke and the Icknield Way, and on the downs east of Royston in the parish of Melbourn. The group of fen barrows, found mainly within the triangle Mepal-Manea-Chatteris, is significant from its occurrence at heights barely above mean sea-level; this emphasises the geographical conditions prevailing in the earlier stages of the Bronze Age in this region.

Late Bronze Age. The Late Bronze Age saw a further substantial increase in the use of metal. Finds are nearly three times as numerous as in the preceding period and they embrace a much wider range of types. A high proportion of the bronzes (some 374 out of a total of 495) comes from 19 hoards. Some of these, like the pair of shields from Coveney Fen, are probably "votive"; others, e.g. the leaf-shaped swords from Chippenham, mark a local metallurgical industry; but most of them belong to the classes known as merchants' hoards (e.g. the Wilburton hoard of 163 pieces—mostly spearheads), and founders' hoards (e.g. the hoard at Green End Road, Cambridge, containing many broken objects and over 17 lb. of metal cakes). Together, these reflect the extensive trade responsible for the introduction of a flood of exotic types, mainly of Central European origin, to Cambridgeshire. It is evident, from the fact that all the leaf-shaped swords belong to the "V" type, that the "U" sword complex did not affect Cambridgeshire in its earliest stage. Further, although marginal to an area strongly affected by the "Carp's tongue" sword complex, Cambridgeshire was hardly influenced; none of the characteristic swords has been found in the County, and only one fragmentary winged axe.

Although exotic influences resulted in a revolution of the metallurgical industry at this time, it is likely that the change was mainly brought about by trade rather than by ethnic invasion, since the County is strictly marginal to the area of the so-called Deverel-Rimbury pottery. Finds of pottery of this class consist only (1) of a few sherds from a circular trench (2–3 ft. deep with a diameter of 68 ft.) at Swaffham Bulbeck, (2) the lower part of a finger-printed barrel urn from Chesterton, and (3) the upper part of a small pot with slashed rim and applied bosses from the Little Thetford–Fordham causeway. Evidence that the Middle Bronze Age overhanging-rim urn pottery in this part of the country continued into the succeeding period is supplied by the material from a settlement site in Mildenhall Fen, only just over the Suffolk boundary, where a fusion between the two wares can be detected. It is thus probable that some of the "Middle Bronze Age" burials from the County really belong to this period.

THE EARLY IRON AGE

The material available for the study of the Early Iron Age in Cambridgeshire is scanty, and only an insignificant proportion has been obtained from scientific excavation. This is largely due to the scarcity of "hill-forts", or other prominent sites, that might have invited excavation. There are no certainly established Early Iron Age defended sites in the County, apart from (1) Wandlebury, a circular triple-banked site with a diameter of about 1000 ft. crowning the crest of the Gogmagog Hills, and (2) the War Ditches, a smaller single-ramparted site on a spur of the same hills. The slight excavations carried out at the War Ditches, prior to the partial destruction of the site, prove that the ditch was quarried by people of Early Iron Age "A" culture, although the surviving material is meagre. The key to the interrelations of the Early Iron Age cultures of Cambridgeshire must be sought in further digging in the surviving portions of the War Ditches and in Wandlebury, a site that encloses a private house, and which has yet to be excavated.

The normal settlement was open and undefended, generally without surface indication, but sometimes delimited by a low bank as at Bellus Hill, Abington Pigotts. It is perhaps for this reason that little systematic work has been done, and that the discovery of settlement sites of this period has invariably been accidental. From the meagre information available it would appear (Fig. 19) that settlement was concentrated in the valleys of the Cam above Cambridge, the sites commonly being placed in pairs either side of a ford, e.g. Grantchester and Trumpington, Barrington and Foxton. The clay areas remain completely blank apart from stray finds of Belgic coins, many of which doubtless continued to circulate at a later date, and, in any case, can hardly be regarded as indicative of settlement. The evacuation of the Fenland, already begun in the later stages of the Bronze Age, was virtually complete by the Early Iron Age, with the exception of certain of the larger islands. Coin finds in the north of the County may well relate to the Romano-British settlement of that region. The only finds from the fens between Cambridge and Isleham consist of a stray brooch and a discarded chariot-wheel boss of Belgic type. Yet, in the Bronze Age, this area was the most populous district in Cambridgeshire.

The County was affected mainly by two successive spreads: the so-called "A" culture, and the "C" (or Belgic) culture. If the number of brooches and pins of early La Tène type is any criterion, it would appear that the "A" culture spread into the County not later than the latter half of the fifth century B.C. The distribution of the finger-impressed pottery, in Cambridgeshire and neighbouring counties, certainly suggests the Wash as the main entrance, although certain elements, such as the "plugged-in" handle, may well have come here from Wessex by way of the Icknield Way and kindred routes.

The "B" culture is represented by the famous Newnham Croft burial, at Cambridge, accompanied by outstandingly rich grave goods; but settlement material is entirely lacking.

Although the "C" culture was essentially intrusive, penetrating from the south about the middle of the first century B.C., it seems unlikely that there was any complete break in the continuity of the Early Iron Age settlement of the County. Many sites, such as Abington Pigotts and Hauxton, have yielded pottery from both cultures. Among the numerous cremation cemeteries of the County, only one—that at Guilden Morden—has been scientifically excavated, and comparatively few graves in this were of pre-Roman age. An iron fire-dog, with ox-head terminals, from Lord's Bridge may have come from the vault of some important individual, but little is known of the circumstances of the find.

There is ample evidence for trade at this time. Pottery from the kilns of Arezzo in Italy was imported through Gaul, while several finds of amphorae of Mediterranean type probably indicate trade in wine or oil. Such imports were doubtless paid for in part by the export of slaves; a fine slave chain with six collars from Lord's Bridge may be a reminder.

The native coins, in which Cambridgeshire is rich, show that most of the County fell within the territory of the Catuvellauni, whose princes[1]

[1] Tasciovanus (20/15 B.C.–A.D. 10) and Cunobelinus (A.D. 10–40/3).

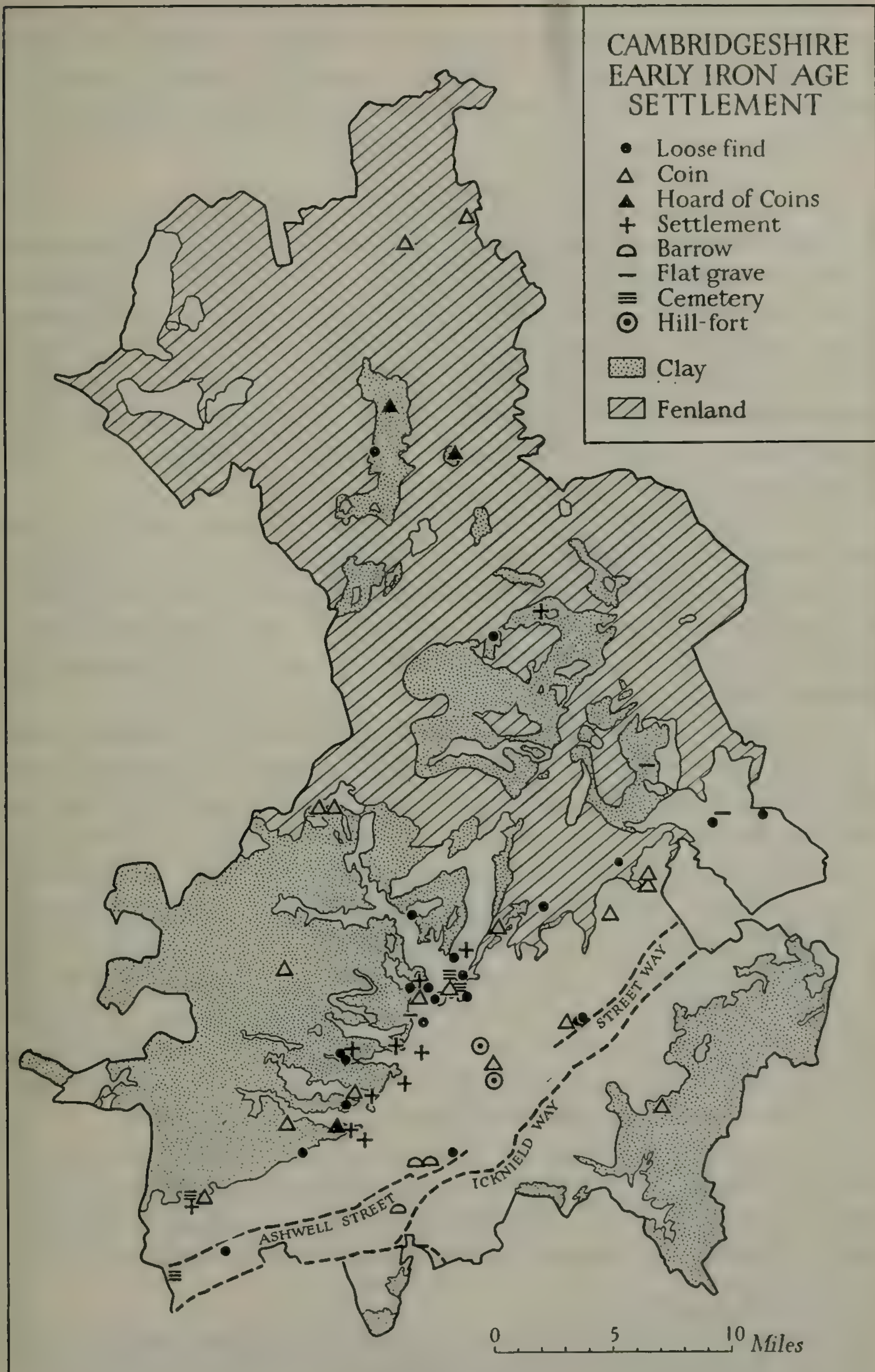

CAMBRIDGESHIRE
EARLY IRON AGE
SETTLEMENT
Loose find
Coin
Hoard of Coins
Settlement
Barrow
Flat grave
Cemetery
Hill-fort
Clay
Fenland
STREET WAY
ICKNIELD WAY
ASHWELL STREET
0
5
10 Miles

Fig. 19.

minted their coinage at Verulam and Colchester during the sixty years prior to the Roman Conquest. But the northern part of the County, and a fringe to the east of the Devil's Ditch, formed part of the tribal area of the Iceni, hoards of whose coins are known from March and Wimblington.

ROMANO-BRITISH TIMES[1]

In Roman, as in prehistoric times, the human settlement of Cambridgeshire was dominated by its physical features and its superficial geology. With the exception of the silt-land farms, the distribution of Romano-British population was not fundamentally different from what it had been in the Early Iron Age, and it is clear, despite the existence of some finds on the Boulder Clay in the south-west of the County, that no serious attack was made on the considerable areas of scrub which must have covered much of the district (see Fig. 20).

One of the dominating features of life in all the Roman provinces was the presence of a developed road system and, in greater or lesser measure, of organised town life. The dominant feature of the Roman road system in this district is the Ermine Street, entering the County at Royston, passing out of it towards Huntingdon at Papworth Everard, and taking its name from the Cambridgeshire hundred of Armingford.

Secondary roads converged on Cambridge. From the south-east, came the *Via Devana* from Colchester,[2] which is probably the earliest Roman road in the region. Entering Cambridge from the south-west, was the Akeman Street which branched from Ermine Street, north of the Cam crossing, to continue its course north-east to Ely, and possibly to Littleport. There was also a local road from Braughing, through Great Chesterford, to join the Icknield Way at or near Worstead Lodge on the line of the so-called *Via Devana*. In the west of the County there was also the secondary road from Sandy to Godmanchester which now forms the County boundary for a short way. Last comes the Icknield Way, which must have continued in use in Roman times, though there is no evidence that it was metalled or otherwise regulated by Roman standards.

During earlier times, the clay areas of the upland had been almost without population, which seems to have been concentrated partly along the chalk belt and partly in the valley of the Cam. It might be supposed that the Roman road system, cutting through these clay areas both in the

[1] By C. W. Phillips, M.A.

[2] According to Fox, this originally may have missed Cambridge to join the Ermine Street at or near Caxton, but was later re-aligned to pass through the town and lead north-west to Godmanchester. This view, however, seems less likely now than in 1923.

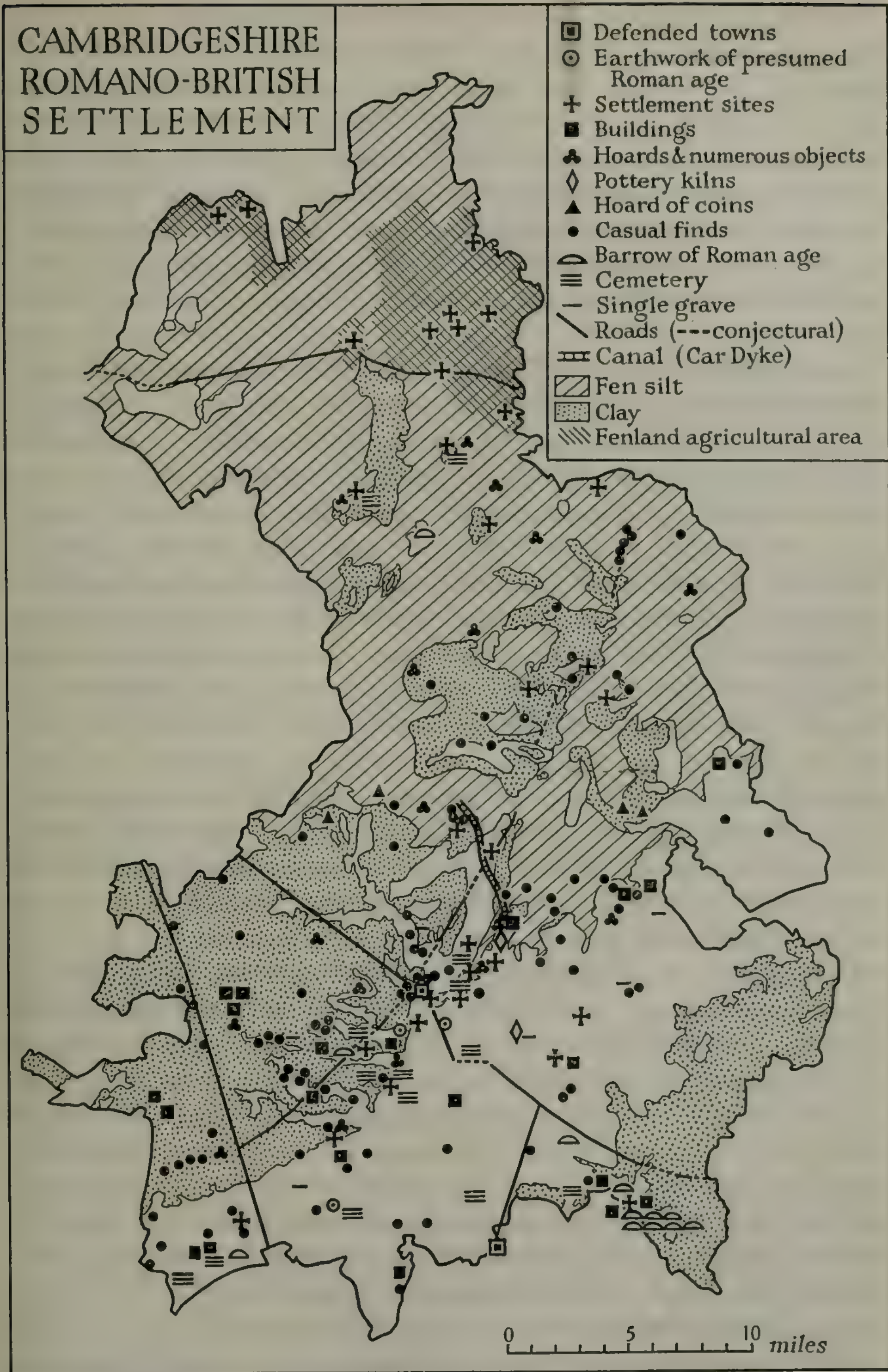


Fig. 20.
The Car Dyke joined the Old West River (not marked here) at Lockspit Hall. See Fig. 7.

south-east and south-west, would have induced settlement, but there is little evidence of this. Penetration of settlement into the uplands was still by way of river valleys; those of the Linton Granta, the Newport Cam, and the Bourn Brook, show this very clearly, while the courses of the roads are almost without settlement. In thrusting their way into the clay lands by the river valleys the Romano-Britons were carrying on, in larger numbers and with better equipment, a movement that had begun in the Early Iron Age, but they do not appear to have made any serious effort to occupy the wooded country as a whole. This task was reserved for the Anglo-Saxons.

The general style of rural life seems to have been humble. No country house of any importance has yet been found anywhere in the County. There are no indications of any industrial activity except for a pottery for coarse wares at Horningsea which enjoyed a fairly wide local market. Few individual finds of much importance have been made in the County, but the Fenland has yielded a number of good pieces of Roman pewter, and there is also the remarkable group of cult objects associated with the worship of the Emperor Commodus as Romanus Hercules found in Willingham Fen. The County, too, has some notable examples of the high conical type of barrow of Roman age. The Bartlow group, though badly damaged, remains the finest of its kind in Britain.

In the Fenland, an extensive Romano-British occupation has been recently demonstrated, more particularly on the silt areas and on certain islands. This settlement was agricultural, and the region of maximum farming activity seems to have been to the north upon the siltlands of south-eastern Lincolnshire.[1] Before this discovery, the frequency of stray Romano-British finds in the fens had been a puzzling fact.

The exploitation of the silt areas began at once after the Roman conquest, and a large population of relatively well-to-do peasant cultivators spread over a region which, it has been suggested, was administered as a domain of the Roman people, though this fact can only be inferred from the general conditions, and does not rest on any confirmatory discoveries. In the less favourable parts of the Fenland, there was a fair sprinkling of folk living in small groups. Many of their sites had close relation to watercourses, but both their house sites, and the adjacent small fields, were carefully protected against tidal floods, for it must be understood that, at this time, tides came far up the wide fen estuaries. Towards the close of the third century, conditions seem to have deteriorated. Whereas it may normally have been unnecessary to organise any drainage works, a slight subsidence of the whole fenland basin may have made the last one and a half centuries

[1] See p. 20 above.

a time of increasing difficulty for the fenland cultivators. Alternatively, it is possible that the disaster may have been due to a combination of tide and wind causing a general breach in the natural silt defences which the sea built against itself around the southern margins of the Wash. In any case, matters had reached such a condition by the fifth century, that the general abandonment of the region was due to take place whether the Anglo-Saxons had come or not. It is significant that the latter made no attempt to settle anywhere in the fenland basin, and that they confined themselves to the country round the edge. In view of their farming habits it is unlikely that they would have failed to occupy a region that had been intensively and successfully cultivated, if it still remained in any physical condition favourable to their enterprise. The Saxons, for the first time, subdued and occupied the scrub-clad uplands of the County, but they had only been able to make a sparse settlement in favourable parts of the Fenland by the time of the Domesday Survey, half a millennium after their first settlement.

There are no large urban sites in Cambridgeshire. Roman Cambridge was a subrectangular area about 26 acres in extent defended by a bank and ditch of late date. It was a road junction of local importance, but no architectural remains of any kind have ever been found in its area. We are compelled to envisage little more than a village built of wood, clay, and thatch.

Conditions have not been favourable for finding out much about Roman Cambridge because at various times a great thickness of top soil has been removed from one of the most hopeful areas, but, as a result of the finds made recently during the building of the new Shire Hall, it can now be said that there was some occupation of the site in the middle of the first century A.D. and that the former existence of a military camp belonging to the period of the Claudian conquest is probable. No trace of a wall has ever been found round Roman Cambridge, though Bowtell, in the early nineteenth century, reported that some traces were then visible in his judgment close to the Huntingdon Road's exit from the enceinte.[1]

The only other Roman town in the district was just over the Essex border, at Great Chesterford. This was a more important centre with a strong wall, much of which was still visible in Stukeley's time, though all above ground has now vanished. The numerous and important finds made here at different times suggest that it was an active local centre of the smaller kind.

[1] T. Bowtell, MSS. in Downing College Library, Cambridge.

THE ANGLO-SAXON PERIOD[1]

The Anglo-Saxon Age in eastern England can be divided into three periods:

(1) The period of the Pagan Cemeteries which may be thought to include the fifth, sixth and part of the seventh centuries. This was the age of early settlement, and may perhaps be compared with the seventeenth and eighteenth centuries in North America.

(2) The Early Christian Period, which of course overlaps the Pagan Period to some extent. It included part of the seventh, the eighth and part of the ninth centuries.

(3) The Viking Age, which closed with the final extinction of Anglo-Danish culture towards the end of the eleventh century.

These periods are by no means watertight compartments, and, compared with the Early Iron Age, the Anglo-Saxon period is very imperfectly understood. It is seldom realised that the Anglo-Saxon period lasted for nearly seven hundred years, and that, except for the Pagan Cemeteries, it is extremely difficult to locate sites whose excavation would throw any light on conditions of those times.

In the Cambridge area, recent years have seen a great advance in the study of the period. There have been excavations spread over several years upon the big linear earthworks that are so outstandingly a feature of the County. The following earthworks are shown in Fig. 21, starting with the most north-easterly: Devil's Dyke, Fleam Dyke (in two parts), Brent or Pampisford Ditch, Bran or Heydon Ditch, and the Mile Ditches. Two cemeteries have been discovered which belonged to the period of overlap between the pagan and Christian periods. Small villages of both pagan and Christian periods have been found and investigated either within or not far beyond the boundaries of the County. Finally, several small excavations have provided a very hopeful start with the study of the pottery belonging to the last three centuries of the period. But, with the exception of linear earthworks, the student of Anglo-Saxon England is very much handicapped by having to wait till a chance find may give him a clue upon which to work. There are no villages surrounded by great earth-ramparts that are so helpful in the study of the Early Iron Age in some parts of Britain. The great majority of Saxon villages are beneath those of the present day, and are therefore irretrievably lost to Archaeology. Manor-sites may perhaps offer a slightly better field for study, but there again the chance of finding one not occupied by later buildings is remote.

[1] By T. C. Lethbridge, M.A.

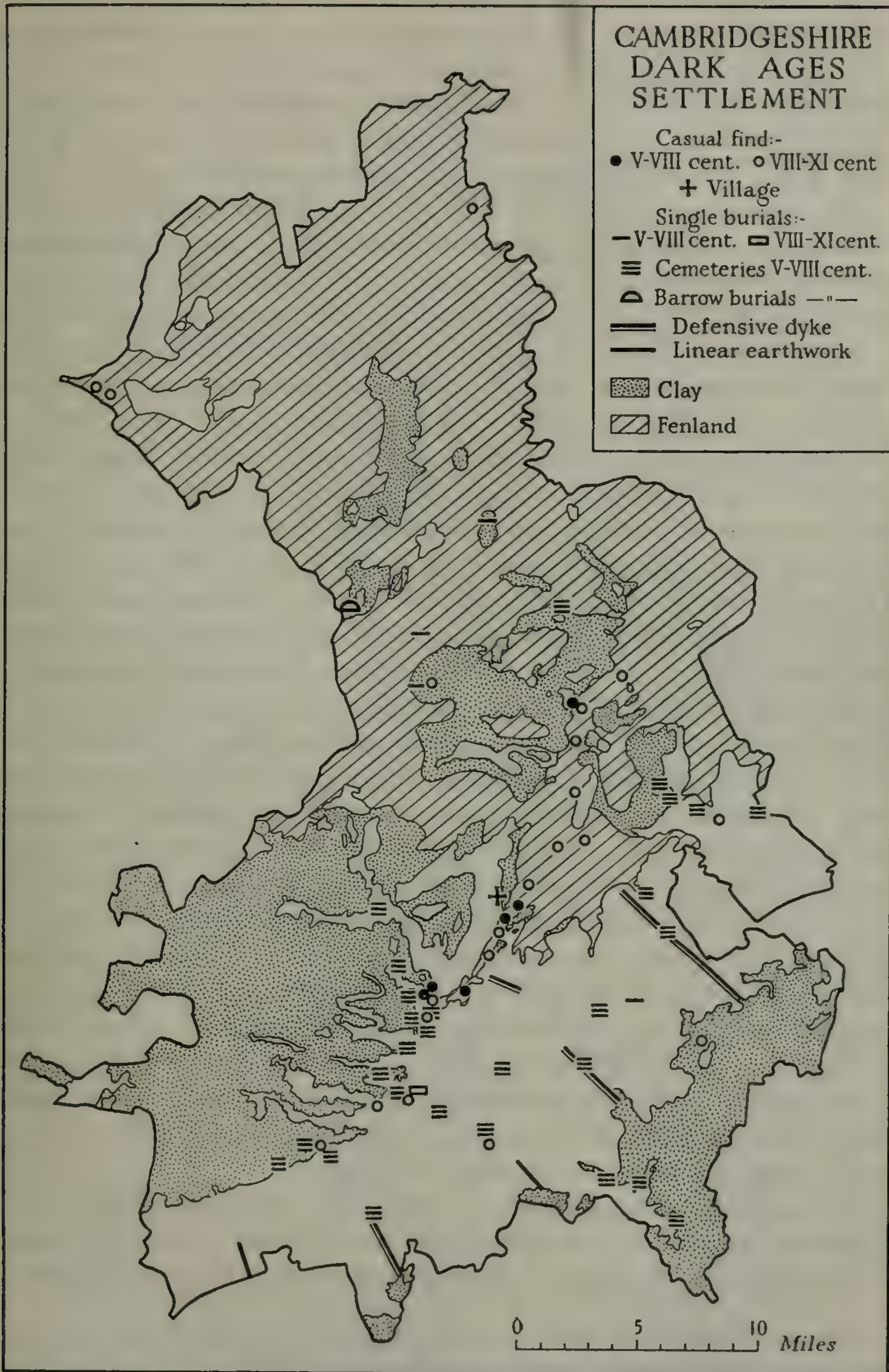

CAMBRIDGESHIRE
DARK AGES
SETTLEMENT
Casual find:-
V-VIII cent.
VIII-XI cent
Village
Single burials:-
V-VIII cent.
VIII-XI cent.
Cemeteries V-VIII cent.
Barrow burials —"—
Defensive dyke
Linear earthwork
Clay
Fenland
0
5
10
Miles

Fig. 21.

(1) *The Pagan Period.* The evidence for this period in Cambridgeshire is abundant. More than two dozen burial sites are known. Most of these have produced a considerable number of graves (sometimes running into hundreds), and most of the burials were accompanied by grave-goods. There is therefore a very large collection of objects from these finds on view in the University Museum of Archaeology and Ethnology.

The men were usually buried with their weapons and often with food for the next world; while the women were dressed in woollen clothes with numerous ornaments such as brooches on the breast, beads in long festoons round the neck, girdle-hangers at the waist, and clasps at the wrists. In some cemeteries (e.g. at Little Wilbraham) numerous cremation burials have been found, but, on the whole, cremation seems to be the exception rather than the rule in Cambridgeshire. Fig. 21 shows that the cemeteries are confined to the fen margins and to the river valleys. No cemeteries have, as yet, been found on the Boulder Clay covered uplands. This raises several points which are difficult to explain, for although the clay areas were apparently devoid of pagan Saxon settlement, yet the Domesday Survey shows them to be as densely occupied as the river valleys (see Fig. 22). It is not clear whether this means that the uplands were unpopulated in pagan times, or whether, perhaps, survivors of the Romano-British population lived on them. Another phenomenon, as yet unexplained, is the scarcity of settlement along the River Ouse. Cemeteries are so numerous in other Cambridgeshire valleys, and in those of the adjacent parts of Norfolk and Suffolk, that one would have expected a similar concentration along the Ouse. There are some burial sites along the river, but they contain very few burials until Kempston is reached near Bedford.

Only one village of the Pagan Age has as yet been explored in Cambridgeshire. This is situated on the east bank of Car Dyke at Waterbeach, and its rubbish overlay the silting of this Roman canal. The huts were of the same semi-pit-dwelling type that has been noticed elsewhere. A village of the Viking Age at St Neots just outside the County had huts of similar form. It has not been definitely established whether these pit-like hovels were really the living rooms of the period, or whether they were undercrofts to upper stories as indicated in the Bayeux Tapestry.

Of the great linear earthworks of the County, the Devil's Dyke is one of the most spectacular monuments of its kind in Britain,[1] while the Fleam Dyke is little less remarkable. Not much can be seen now of the Bran

[1] The Devil's Dyke is $7\frac{1}{2}$ miles long with a rampart 15 ft. high. It runs across the open chalk country from the Fenland to the one-time wooded clayland, and has been laid out in three straight sections, the north-westerly one of which was apparently aligned on a Roman canal or lode which ran from Reach to Upware.

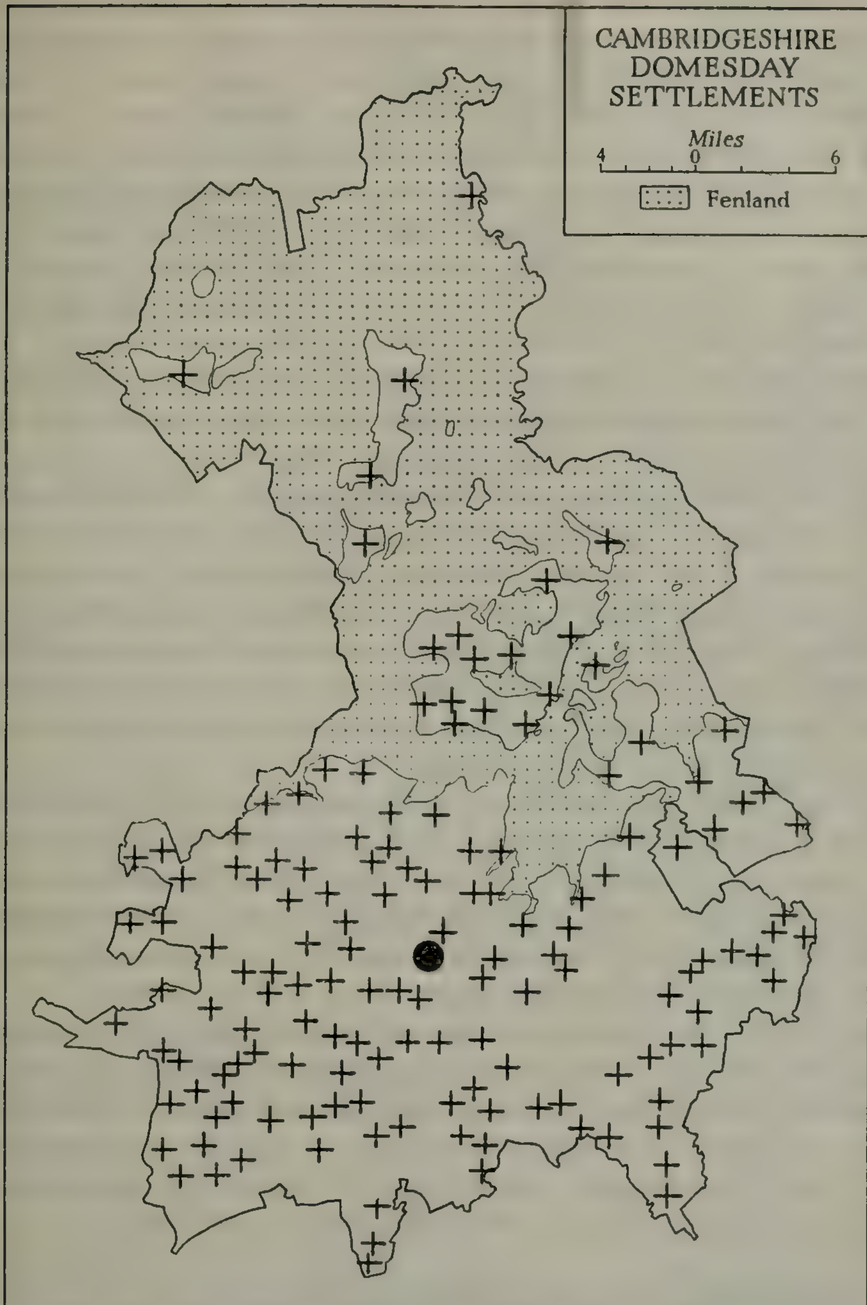


Fig. 22.

From H. C. Darby, "The Domesday Geography of Cambridgeshire", *Proc. Camb. Antiq. Soc.* xxxvi, 39 (1936). The town of Cambridge is indicated by a black circle. Compare the distribution of villages on the upland with the empty clay areas of Fig. 21.

Ditch, while Brent Ditch has no bank. All four appear to represent attempts to hinder communication along the open chalk downland during the Pagan Age. Burials of warriors with their weapons have been found at Devil's Dyke and Fleam Dyke, beside the Worsted Street. Possibly there were also burials in the Brent Ditch. At the Bran Ditch, groups of decapitated skeletons have been found at two localities. They had associated objects of this period. Excavations[1] have shown that these linear earthworks were, in all probability, constructed during the early wars of the Heptarchy, when Penda of Mercia (*c.* 655) overthrew the rulers of East Anglia.

Other indications of warfare in early settlement times may be deduced by finds of early swords, spears, shields, and human bones in the Cam at Clayhithe, opposite the end of the Car Dyke.

(2) *The Early Christian Period.* Generally speaking, this period has left but the scantiest of material remains in the County. A few chance finds may be seen in the University Museum. But in recent years extensive cemeteries at Burwell and Shudy Camps have been explored. These appear to belong to the period of overlap, and may perhaps have continued into the eighth century. It is probable that another cemetery exists at Foxton, while a burial at Allington Hill may perhaps be that of an important man killed at the Dykes in the seventh century.

(3) *The Viking Age.* Finds of small objects, weapons, and human skeletons, at Hauxton Mill probably indicate trouble there about the year 870. Pottery of the Viking Age is now being recognised from many localities, especially in the town of Cambridge itself, and, to a less extent, down the Ouse Valley, while small crosses and grave slabs characteristic of the district have been widely noted.

But the chief finds relating to this period are the rich series of weapons found in the rivers. For the most part, these may belong to the final campaign when William I overcame the last resistance of Hereward the Wake and his followers in the Isle of Ely. By this time, the Domesday book (see Fig. 22) presents a complete picture of settlement in the County, and sums up the economic activity of the Anglo-Saxon period.

[1] Objects of the later part of the Roman period were found beneath the Fleam Dyke and Devil's Dyke, while skeletons of the Anglo-Saxon period and late Roman pottery were found under the vallum of the Bran Ditch.

CHAPTER SEVEN

THE PLACE-NAMES OF CAMBRIDGESHIRE[1]

By P. H. Reaney, LITT.D., PH.D.

CAMBRIDGE, GRANTCHESTER, AND ELY WERE RIGHTLY derived some thirty years ago by the great Cambridge philologist and pioneer of English place-name studies, the late Prof. Skeat.[2] A few scholars in other fields hesitated to accept his etymology of Cambridge but recent advances in the study serve only to confirm it. The earliest reference to the town is Bede's *Grantaceastir* (*c.* 730), "the Roman fort on the Granta". This would normally become "Grantchester", but the reference is undoubtedly to Cambridge and not to the modern Grantchester which appears in early sources as *Granteseta*, "the settlers on the Granta". As early as 745, in Felix's *Life of St Guthlac*, had come the change in the second element which has given rise to the present-day name of Cambridge (*Grontabricc*). The site of a Norman castle and a centre of Norman administration, the town was subject to strong Norman influence which had its effect on the name, until, through such forms as *Cantebruge* (*c.* 1125), *Cauntebrig'* (1230) and *Caumbrigg* (1353), the ancestor of the modern spelling was reached in *Cambrigge* (1436).

Grantchester is an interesting example of phonetic change and popular etymology resulting, ultimately, in the form that Cambridge should have had. *Grantsete* became *Gransete* and *Grancete*, pronunciations which suggested an analogy with such names as Leicester and Worcester. The name was accordingly spelled *Granceste*, *Grancestre*, *Grancetcr*, and finally *Granchester*, a spelling which has not yet been noted earlier than the seventeenth century.

The river on which Cambridge stands is known in various parts of its course as Granta, Cam, and Rhee. Granta, the real name, is unique and pre-English, meaning, probably, "fen river" or "muddy river". When Cambridge came to be known as *Cantebrigge*, this was interpreted as "the bridge over the *Cante*", an artificial back-formation found from 1340 onwards. Similarly, the modern *Cam* is a later back-formation from the

[1] This essay is based on a preliminary survey of material so far collected for a volume on "The Place-names of Cambridgeshire" to be published by the English Place-name Society. The discovery of further material may necessitate some modification of detail.

[2] W. W. Skeat, *The Place-Names of Cambridgeshire* (Camb. Antiq. Soc. 1901).

spelling *Cambrigge*, and this was sometimes Latinised as *Camus*. *Rhee* is from Old English *ēa* "river"; *æt þǣre ēa* "by the river" became Middle English *at ther ee*, which was wrongly divided as *at the ree*. In 1285, William *atte Ree* lived by the Granta at Grantchester.

Ely occurs first, in Bede's *Ecclesiastical History*, as *Elge* "eel-district". The second element is the archaic *ge*, corresponding to the German *gau*, found also in the names Surrey, Eastry, Lyminge, and Sturry in Kent, and Vange in Essex. Here, too, popular etymology was early at work and, already in the Anglo-Saxon version of Bede, the name appears as *Elig* "eel-island". Domesday Book records the yield of innumerable eels from the fisheries of Ely and renders of eels were common elsewhere in the island. In Sutton, too, was a place called *Cappelode*, a name identical with the Lincolnshire Whaplode, "eelpout stream".

These names are of especial interest because of their age. Cambridge and Grantchester contain the Celtic name of the river Granta. Grantchester, too, was a folk-name—"the settlers on the Granta", the second element being that found in the names of such large districts as Dorset and Somerset. Ely was the name of the whole island, called by Bede a *regio* and in the Anglo-Saxon version *þēodlond*. These names are of high antiquity and may well date from the Anglian settlement which archaeologists agree in placing in the latter half of the fifth century.

From the earliest periods, the estuary of the Wash has been a magnet for successive hordes of invaders from beyond the sea, and its river valleys have afforded an easy way inland. The Angles found the Fenland largely unattractive for settlement. They pushed on, and clear evidence of their former presence has been found in a number of cemeteries in the Cam Valley, where the finds point clearly to a settlement by the end of the fifth century.[1] Numerous place-names of recognised early type might therefore be expected here. But of the oldest type, that ending in *-ingas*, there is only one, Kirtling, as against some twenty-four in the neighbouring county of Essex, where, too, names pointing to heathen worship are common. Of these, in Cambridgeshire there is not one. The probable explanation of this curious contradiction—the absence of numbers of place-names of high antiquity combined with clear archaeological proof of very early settlement—is that the struggles for supremacy between East and Middle Anglian and the Danish invasions resulted in such confusion, devastation, and depopulation that memory of the names of all but the most important places was utterly lost.

Names of early type do exist. Badlingham, Cottenham, Dullingham,

[1] R. G. Collingwood and J. N. L. Ayres, *Roman Britain and the English Settlements* (1936), pp. 386–7.

and Willingham, "homes of the followers of Bæddel, Cotta, Dulla, and Wifel", are of a very early type. Haslingfield, "the open country of the dwellers by the hazel-wood", is an early name and the site of a fifth-century cemetery. Armingford Hundred and Arrington, "the ford and the farm of the people of Earna", to whom Ermine Street also owes its name, are early formations, and the occurrence of their name in three distinct places suggests that the *Earningas* were of some importance.

The element *ham* is, on the whole, earlier than *tun*, but some -*ton*-names are older than some in -*ham*. Sawston, earlier *Salsingetune*, "the farm of the followers of Salsa" (a personal-name otherwise unknown in England), and Hinxton, deriving from *Hengestingatun*, "the farm of the followers of Hengest" (a name known to have been borne by one of the earliest of the invaders of Kent), are undoubtedly of greater age than such names as Fordham, Coldham, and Downham; whilst from their very meaning the three examples of Newnham and the two of Newton must be comparatively late. So, too, are place-names which provide evidence for women as landholders: Wilbraham (the site of a cremation cemetery) and Wilburton, "the *ham* and *tun* of Wilburg" and Babraham, "the *ham* of Beaduburg".

Although Cambridgeshire was part of the Danelaw, it was never so thoroughly Scandinavianised as Norfolk and Lincolnshire. Caxton and Croxton contain the Scandinavian personal-names *Kakkr* and *Krókr* respectively.[1] Conington is a Scandinavianising of an English *Kington*, and Carlton of *Charlton*, "the farm of the ceorls". "Toft" is a Scandinavian word meaning "the site of a house and its buildings", or "homestead". It survives as the name of a parish, and formerly occurred as a field-name in eight other parishes. But, in general, the parish-names of the County give no such clear evidence of Danish influence as do those of Lincolnshire.

Minor names, however, suggest that Danish influence was not negligible, and that, in places, it was strong. Denny, "the island of the Danes", implies that Danes were not numerous in the neighbourhood. Such words as *holm* "small island or dry land in a fen" (a common field-name), and *bigging* "building" (e.g. Biggin Abbey in Fen Ditton), while Scandinavian in origin, are no criterion of Scandinavian settlement. But Clipsall in Soham, together with three field-names, contain the Scandinavian personal-name *Klippr*, and Hoback Farm is a hybrid, "the beck or stream in the hollow". On the borders of Huntingdonshire there are two examples of *lundr* "a grove", and several of *krókr* "a bend". But more interesting is the occurrence of the Scandinavian *kirk* and its interchange

[1] They (and Toft below) may well point to a late settlement by Scandinavians in this wooded area. For the woodland see p. 52 above.

with the English *church*. In Tydd St Giles, both Kirkgate and Church Lane occur, and in Thriplow to-day there is a Church Street where in the thirteenth century a neighbouring field was *Kirkefeld*. So, too, in Newton-in-the-Isle, the modern Church Croft is paralleled by the seventeenth-century *Kyrkelandefield*, and other lost field-names give further examples of Scandinavian influence. A similar interchange occurs in the second element of Landwade. The modern spelling represents the English *wæd* "ford" but early forms often have the Scandinavian *vað*.

In the north of the County, on the Lincolnshire border, there are such names as Gate End Bridge (Parson Drove), Eaugate Field, Fengate Field, Kirkgate, and Newgate (all in Tydd St Giles). These contain the Scandinavian *gata* "road", common in such street-names as Waingate in Sheffield, Briggate in Leeds, etc. It is often impossible to distinguish this from the common word *gate*. Kirkgate, for example, might well mean "church-gate", but Eaugate and Fengate can hardly mean "gate leading to the river or the fen", nor does the common *gate* make much sense in Gate End Bridge. Clear proof, however, is forthcoming from *Kyrkestrete* (1393) in Leverington described in 1486 as "regiam viam vocatam *le Kirkgate*", and from a "highway called *Crossegate*" in 1438 at Tydd St Giles.

In the Isle of Ely there is no parish-name of Scandinavian origin. The evidence suggests the naming of minor places in a more or less settled time, and the substitution of English terms by similar, corresponding Scandinavian ones, some of which were common to various districts where the settlement was not strong.

French influence is much less in evidence. Marmont, the name of a priory in the fens in Upwell, was transferred from France, *mirum montem* "the famous hill". Not far away, in Elm, is Beauford, "the fair ford", whilst in Willingham an old earthwork, Belsars Hill, is identical in origin with the Durham Bellasis and the Belsize of Hertfordshire and Northamptonshire; these are derived from *bel assis* "the fair seat", and the name thus loses its historical associations with the fictitious Belasius, the knight who, in the campaign against Hereward, acted as the Conqueror's Commander-in-Chief.[1]

Apart from these names, French influence is confined to the commemoration of the Norman families of *Everard* de Beche in Papworth Everard; *Agnes* de Papworth in the erroneously canonised Papworth St Agnes; the Colvilles in Weston Colville; the Bolebecs in Swaffham Bulbeck; and in such manorial names as De Fréville Farm in Great Shelford, D'ovesdale Manor Farm in Litlington, Lacy's Farm in Duxford, and Lacies Farm in Grantchester (from the Lacis, earls of Lincoln).

[1] E. Conybeare, *Highways and Byways in Cambridge and Ely* (1910), pp. 283, 292.

Southern Cambridgeshire consists of three well-defined areas, two belts of clay on east and west, with an intervening stretch of chalk (see Fig. 29). The claylands were presumably wooded in early times, but the chalk was always open country, along which ran the Icknield Way (see Fig. 19). By Domesday times, wood had disappeared neither from these clay areas of the upland nor from those of the fen islands (see Fig. 17). Earlier evidence is not wanting. The most common woodland terms found in English place-names are *-ley* "a wood" or, later, "a clearing"; *hay* "enclosure", and often "enclosed wood"; *stubbing* "clearing"; and *stocking* "land cleared of stocks". The clay areas of southern Cambridgeshire each contain parishes with names ending in *-ley*.[1] In these, as well as in neighbouring parishes, other names have survived, as well as a number of field-names containing all four of the above elements. In the intervening belt of chalk, there is a solitary lost *ley* in Little Wilbraham, another in Fulbourn, and "The Leys" in Burwell—a marked contrast with Ashley-cum-Silverley, on the eastern clay, with its four additional examples of *ley*, two of *hay*, and one of *stubbing*.

Croydon Wilds and Hatley Wilds, on the western clayland, are names of particular interest in connection with Cambridgeshire woodland. The first occurs in 1285 as *in Waldis de Craudenn'* and, as late as 1760, as *Croydon Wold*; the second is found in 1277 as *in Weldis subtus boscum de Hayley*, i.e. Hayley Wood in Little Gransden. Both are on high ground and both names contain *weald*, *wald*, used in Old English of forest-land, especially of high forest-land. Other evidence of the wooded nature of the ground is to be found in the names Hatley and Hayley, and in *Longehay* and *Dreyhirst*, in Little Gransden. Farther north, there was a *Woldeslande* and a *Grenewold* in Elsworth, while Dry Drayton was formerly called *Walddraiton*. Between Croydon and Elsworth there are references to *Berstuneswealde* and *Kakestuneswealde* in Caxton and *in Waldis de Brune* (i.e. Bourn). There can thus be little doubt that this district was once known simply as *Weald* or *Wold*.

The clay islands of the fen were also well wooded. The first element in Chatteris and Chettisham is probably the British *cet* "wood". Near Chatteris is Langwood Fen, whilst medieval woodland is frequently mentioned at Chettisham. On these clay islands there are examples not only of the terms already discussed, but also of *hyrst* and *holt*, two other names for a wood. The last two elements are often difficult to recognise without early forms. *Holt* occurs in Singlesole at Thorney; Throckenholt

[1] In the east are Ashley-cum-Silverley, Brinkley, Cheveley and Westley Waterless. In the west are Childerley, Eltisley, Graveley, East Hatley, Hatley St George and Madingley.

in Parson Drove; and Apes Hall in Littleport; *hyrst* in Shrewsness Green in Upwell; and Boleness in Wisbech St Peter. The occurrence of *hyrst*, *leah*, and *holt* on the peat and silt is noteworthy.

From the Isle of Ely, too, comes an example of *wold* covering a wide area in which other names indicative of the former existence of woodland are to be found. In early medieval documents, numerous references are found to a place *Walde* or *Wolde* in Witchford. According to Bentham, the name *Wold* survived in his day as that of certain arable and pasture lands in Witchford,[1] but it was undoubtedly once used of an extensive district. In the west of the Isle, at Sutton, was a *hythe* or landing-place in the wood known as *Waldhethe*, and in the same parish there were two hills called *Waldun* and *Waldelowe*, while a Woolden Lane still survives in Haddenham. From Witchford, this forest-land stretched into Ely where both *le Wold* and *Woldeffeld* are mentioned, whilst in Downham, too, there is *Brodwold*. A road or track called *Waldehethewey* ran across this *wold* which must have included most of the high clayland of the Isle of Ely; part of this, at least, in Wilburton and Witcham, was known as *Bruneswold* at the end of the thirteenth century.

Much more that is of topographical and historical interest may be gathered from the place-names of the Isle of Ely. Seadike Bank and Sea Field in Leverington recall the memory of the sea-wall that once protected the coast of Cambridgeshire from Tydd St Giles to Wisbech, and that gave name in Norfolk to Walsoken, West Walton, and Walpole. Coveney, "the island in the bay", is a reminder that the West Fen was once marsh and water; the coast of the ancient bay can easily be traced from the contours. Not far away is Wardy Hill, "the island from which watch was kept", a name that may be of some historical significance for it is on the line of the long, narrow island running north to March, "the boundary", probably that between East and Middle Anglia. Among the other numerous islands are Shippea and Quy where sheep and cows were pastured, Henny and Cranney, frequented by wild fowl and herons, and Manea near where the parishes of the neighbourhood pastured their cattle in common.[2]

Around the island ran innumerable watercourses, the Old and the South Eau, unetymological Gallicisings of the Old English *ēa* "river", already noted in the Rhee (or Cam) and surviving also in Welney and Wissey. Bradney House in Benwick, and Bradney Farm in March, are both near

[1] J. Bentham, *The History and Antiquities of the Conventual and Cathedral Church of Ely* (2nd ed. 1812), p. 75.

[2] Manea means "island or low-lying land held in common or where commoning took place".

the old course of the Nene, here once known as *brādan ēa* "the wide river". The extensive area known as Byall Fen was formerly called *Byee* from a river which formerly flowed across the fen from Chatteris to Downham.[1] Very common, too, are the terms *lode* and *gote* (meaning some kind of stream), *ditch*, *delph*, used of an artificial watercourse, and *lake*, "a sluggish stream".

Of the numerous fenland *meres*, Whittlesey Mere and Soham Mere were probably the largest. But the names of others survive, and many more are lost. With the draining of the fens, these meres became marshland and now often appear as *moors*. Redmoor was "the reed-mere", Gosmoor was frequented by geese, and Foulmire was the home of wild fowl. Fisheries, too, were common, and were called *weirs* as at Upware. There are many references also to landing-places, or *hythes*, not always easy to recognise in their modern form, e.g. Horseway, Willey, Aldreth, Swavesey, and Witcham Hive. The old industries of the fens, digging for peat (always called turf by the fen folk), the cutting of sedge for thatch, and the growth of fodder for cattle, are commemorated in the common names Turf Fen, Sedge Fen, Fodder Fen, and Mow Fen. The Joist Fen at Waterbeach was one in which cattle were *agisted*, i.e. allowed to feed for a fixed rate per head.

Many fenland names owe their origin to the exigencies of draining or commemorate the reclaimers of the fens. Adventurers' Land and The Undertakers represent part of the land assigned to the Earl of Bedford (after whom the Bedford Level is named) and his associates in return for their *undertaking*, and *adventuring* upon, their immense task.[2] "The Lots" preserve a common term "the lot or dole" used of the allotment of land in the fens, while Lockspit Hall owes its name to the lockspits or small trenches used to divide these lots. Cradge Bank was so called because it was backed with clay to prevent water from trickling through. Stampfen Drove, Gravel Dike, and The Stacks, preserve local names for "letts or impediments hindring the fall of the waters". Other terms of interest are found, particularly in names now lost, but with these there is not space to deal. Sufficient evidence has, however, been given in this brief and incomplete survey to show that the place-names of Cambridgeshire and the Isle of Ely are full of interest and that the nomenclature of each locality has its peculiar characteristics.

[1] This is marked on Saxton's Map of Cambridgeshire (in W. Camden's *Britannia*, 1607) as *The fyrth dyck*, so called, no doubt, from Doddington *Frith*.

[2] For "Undertaker" and "Adventurer" see p. 181 below.

CHAPTER EIGHT

THE VILLAGES OF CAMBRIDGESHIRE

By John Jones

THE VILLAGES OF CAMBRIDGESHIRE NUMBER ABOUT ONE hundred and sixty.[1] There are, in addition, some half-dozen urban areas, but many of these only contain overgrown villages. Nearly all of them are mentioned in the Domesday Book. It is certainly true to say that the village geography of the County has been stable through the centuries. Fig. 22, showing the distribution of Domesday settlements, represents quite well the pattern, if not all the detail, of the present-day village distribution as shown in Fig. 23. The main difference between the two maps is the addition of a number of villages upon the silt area of the northern Fenland.[2]

LOCATION OF VILLAGE SITES

Even a cursory glance at Fig. 23 shows that these villages are not evenly distributed. Three stretches of country appear villageless: a district in the north of the County; an east-west strip south of the Ely cluster of villages; and a north-east—south-west strip in the south-eastern quadrant of the County. On the other hand, the area with the most dense distribution of villages occupies the south-western quadrant.

Four physical features are reflected in this variation: the Fenland and its islands; the upland with its contrast between clay and chalk; the river valleys; and the narrow strip of country between fen and upland which may be conveniently designated the "fen-line". From the point of view of location, therefore, the villages fall into four groups:

(1) Fen villages.
(2) Upland villages.
(3) Valley villages.
(4) Fen-line villages.

Naturally, these groups are not mutually exclusive, for some sites claim admission to more than one group. Take Cambridge town itself, for example. It is on the fen-line, but it is also in the valley of the Cam, and

[1] For a fuller account, see J. Jones, *A Human Geography of Cambridgeshire* (1924).

[2] For a discussion of these differences, see H. C. Darby, "The Domesday Geography of Cambridgeshire", *Proc. Camb. Antiq. Soc.* xxxvi, 35 (1936).

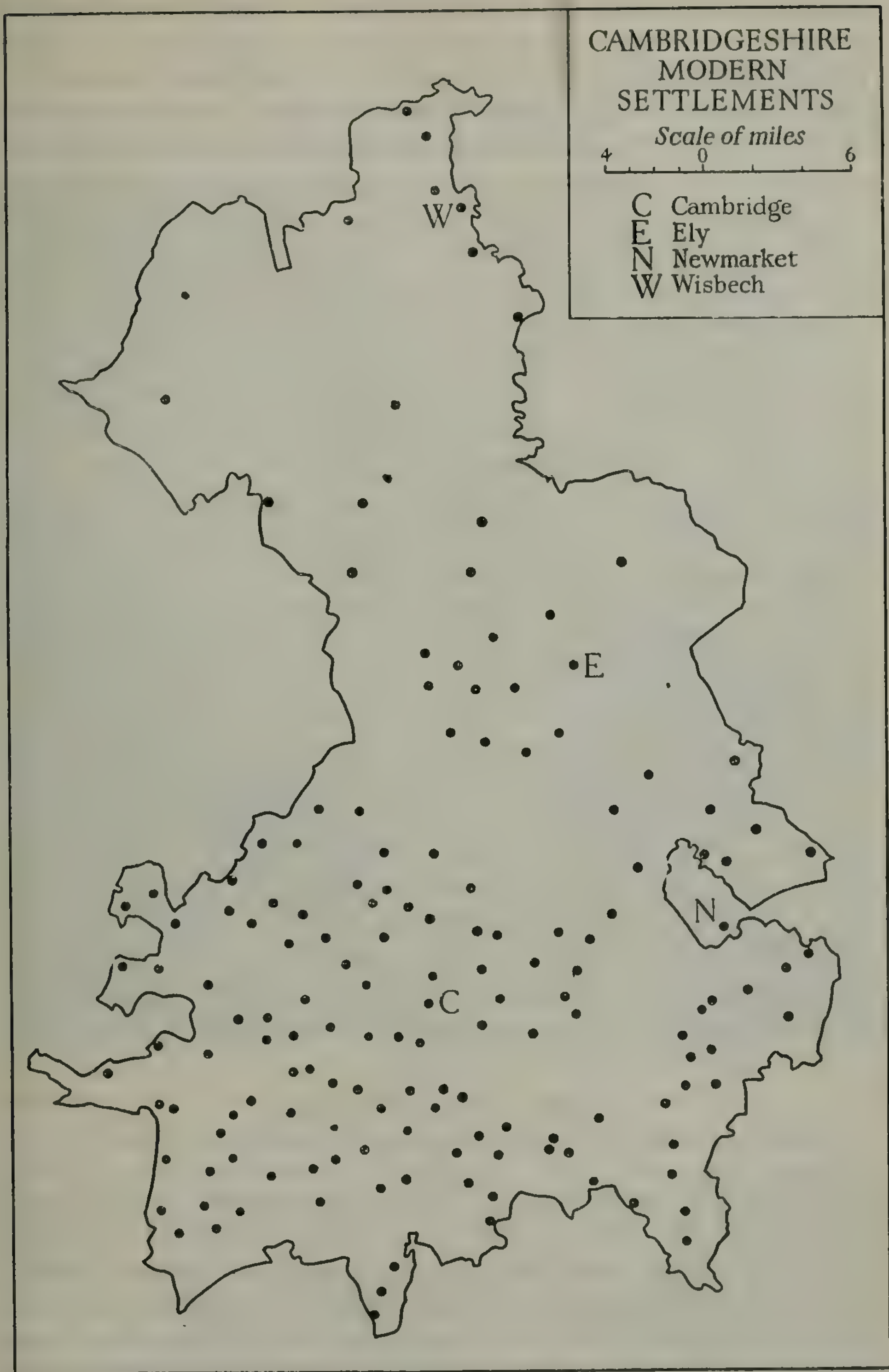

CAMBRIDGESHIRE
MODERN
SETTLEMENTS
Scale of miles
4
0
6
C Cambridge
E Ely
N Newmarket
W Wisbech
W
E
C
N

Fig. 23.

owing to the latter fact it has outstripped in size the other fen-line settlements.

South-eastern Cambridgeshire. Fig. 24 gives details of the country between Cambridge town and the eastern County boundary. It covers an area of 170 square miles, and all the village sites and watercourses are inserted.

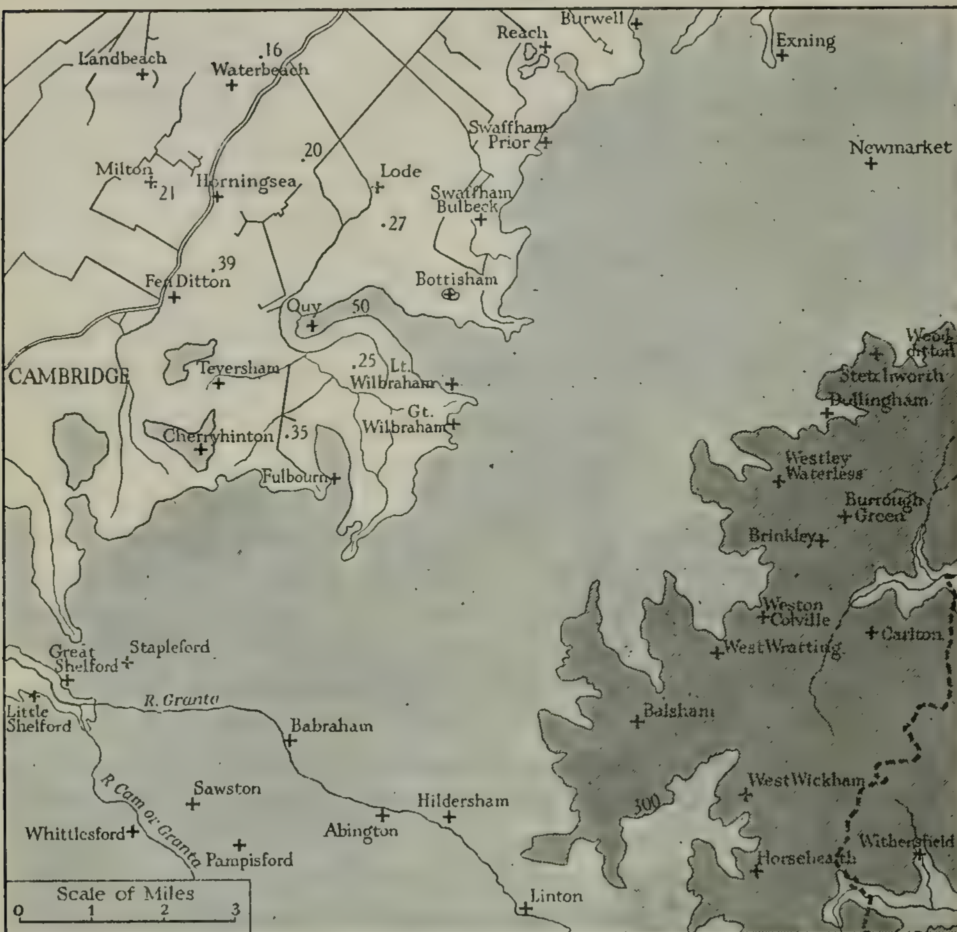


Fig. 24.

Villages to the east of Cambridge.

The figures in the area below the 50 ft. contour are spot heights above O.D.

The relief is indicated by the 50 ft. and 300 ft. contours. At the time of the settlement, this part of the County most probably included three zones of vegetation. To the north-west, there was a marsh of reeds and rushes, interspersed with patches of better drained land. The country between the two contours was a dry area, with large open spaces of grass, dotted here and there with hawthorn bush, and probably with some beech woods. The 300 ft. contour line agrees very closely with the edge of the Boulder

Clay which overlies the Chalk on the top of this upland area (see Fig. 29), and which, most probably, was covered with deciduous woodland (see Fig. 17).

The outstanding feature of this district to-day is the considerable area, running north-east—south-west, that is villageless and also streamless until the River Bourn is reached in the south. This belt is the wide strike exposure of the porous chalk country. In its completely dry character lies the explanation of its unpeopled state. To the south, near the Bourn and Granta rivers, come the valley villages. Some of them have "ford" terminations; in each case, the precise site was determined by rising ground safe from flood. In the east, very near to the 300 ft. contour and between woodland and open country, are the upland villages. Until recently, some of these villages have used for domestic purposes the surface rain-water that drains into hollows on the impervious clay; wells have to be sunk so deep before reaching the bottom of the chalk, that they are expensive items. Along the edge of the clayland, then, most of the upland villages are situated. The advantages of a site on the edge of the wood, rather than within it, can be readily appreciated. Finally, the 50 ft. contour marks, roughly, the junction between chalk and fen. Along this line, springs gush out from beneath the chalk, and so, following the fenland edge, is a string of eleven villages from Burwell in the north to Cherry-hinton in the west. To the north-north-west of these fen-line sites, recti-linear watercourses indicate the drained fen. There, on the drier spots, are other marginal villages; a few spot heights have been inserted.

South-western Cambridgeshire. Fig. 25 gives details of villages and streams in the country to the west of Cambridge town, and it covers the same acre-age as Fig. 24. The Cam Valley occupies the south and east of the map. In the middle west is a small plateau over 100 ft. above sea-level; this contains two ridges (above 200 ft.) that run east-west to form the boundaries of the Bourn Brook Valley. The surface of the plateau is almost entirely composed of clay of various kinds, mainly Boulder Clay and Gault, but also Kimeridge and Oxford Clays (see Fig. 29); presumably it was wooded in early times.[1] Generally speaking, the plateau forms the watershed between the tribu-taries of the Ouse and those of the Cam; many of the streams marked on the map are only a foot or two in breadth, but, even so, their presence shows this to be a district quite different from that on the eastern side of the County.

The top of this plateau is almost villageless. The villages are set around the edges. The elements important in their distribution seem to be as follows:

(1) The valley sites of the Bourn Brook and of the Cam explain

[1] For Domesday Woodland, see Fig. 17 above; for earlier evidence, see p. 103.

themselves; the Hatley's and the Gransden's to the west belong also to this category.

(2) With a west-to-east dip, the lower limit of the Chalk (i.e. the spring-line) is at a higher level than in the east of the County. To some extent, it coincides with the 100 ft. contour, but it does rise to 200 ft.

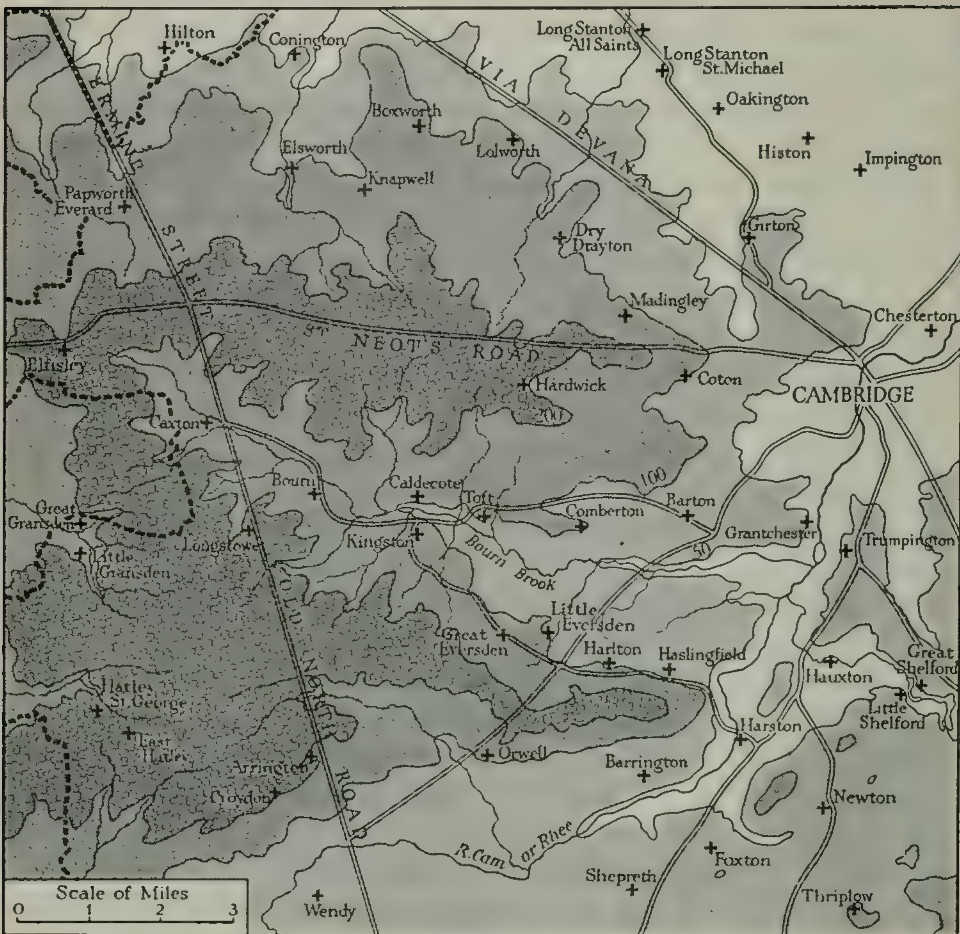


Fig. 25.
Villages to the west of Cambridge.

Following interrupted outcrops of the Chalk, along the eastern and southern edges of the plateau, are the villages of Madingley, Coton, Barton, the Eversden's, Harlton, Haslingfield, Barrington, Orwell, Arrington, and Croydon.

(3) In the north, the village sites still follow the 100 ft. contour, which roughly marks the junction between the Boulder Clay and a varied series of Gault, Lower Greensand, Kimeridge Clay, and Ampthill Clay. It is interesting to note that these upland villages lie off the *Via Devana*.

(4) To the north, and outside the map, come the fen-line villages—Cottenham, Rampton, Willingham, Over, Swavesey. These lie much lower than the corresponding villages in the east of the County, where the chalk escarpment borders the peat and brings the 50-ft. contour near to it.

Northern Cambridgeshire. In the undrained Fenland the islands were the critical sites determining settlements. An eighth-century monk, Felix, records that:

> There is in Britain a fen of immense size, which begins from the river Granta [*Grante*] not far from the city, which is named Grantchester [*Granteceaster*]. There are immense marshes, now a black pool of water, now foul running streams, and also many islands, and reeds, and hillocks and thickets, and with manifold windings wide and long it continues to the north sea....[1]

By the eleventh century, however, as the Domesday map shows (Fig. 22), the Fenland was not without villages. But settlement was prohibited upon the peatlands because the soil provided no stable foundations on which to build. Judged by the analogy of later times, even those portions that escaped winter flooding were subject to an annual heaving motion as the swollen peat absorbed more and more water. Consequently, not one Domesday village was located in all the peat area, with the sole exception of Benwick, and there only because a local gravel substratum approached within a few inches of the surface.[2] The open unoccupied area to the north of the County, shown on Fig. 23, is an expanse of peat. So is the east-west strip of country south of the Ely cluster of villages; here, in pre-drainage days, the Old West River carried part of the Ouse around the island of Ely and hence to the Wash.[3] The villages were all upon the islands. But the silt area, to the north, was composed of a substance more solid than fen peat, and offered better opportunities for continuous settlement. It is true that, in Domesday times, Wisbech alone stood here, but on the modern map, the silt area bears a number of additional villages.

PARISH BOUNDARIES

Not only the sites of villages, but also the size, shape, and boundaries of parishes, are related to the geographical circumstances of a country. Fig. 26 shows the modern parish boundaries in Cambridgeshire. The parishes vary considerably in size. There are eleven parishes each containing less than

[1] Felix, *Life of St Guthlac* (Anglo-Saxon version), edited by C. W. Goodwin (1848), p. 21. Grantchester here refers to Cambridge itself.
[2] S. B. J. Skertchly, *The Geology of the Fenland* (1877), p. 4.
[3] See footnote 1, p. 183 below.

1000 acres—all in the southern part of the County; while ten parishes cover more than 10,000 acres each—all on the Fenland. This latter fact arises from the scarcity of sites in the fens. Thus, just below the northernmost part of the County, Whittlesea and March together stretch 15 miles across a part of the County whose total width is only 17 miles. Whittlesea and March stand 26 ft. and 20 ft. respectively above sea-level, and in this district there is now no other spot more than 10 ft. above sea-level.

The parish boundaries are partly natural and partly artificial. The Cam itself is a good example of the former. It is used as a boundary for almost its entire length from its source to where it joins the Old West River (i.e. the Ouse) in the Fenland. Its tributary, the Bourn Brook, coming from the south-western plateau, also forms the boundary between many parishes, despite the fact that it is of no great width; on the other hand, the wider River Bourn, coming from the south-east, flows right through the middle of several parishes. In the Fenland, many parish boundaries are so straight because they follow artificial watercourses; and in some cases, apparently, adjustments were made when the drains were cut.

In a county of low relief, there cannot be many boundaries fixed by a crest-line, but there is one well marked in the south-west, and lettered A–B on Fig. 26. It runs along the ridge separating Bourn Brook from the upper Cam Valley. This is also the line of an old trackway, the Mare Way, leading from Ermine Street to the Cam Valley at Harston. The line C–D is not exactly along the crest of the ridge to the north of Bourn Brook, but it is very nearly so. In any case, it runs along the Cambridge-St Neots Road, and although this is not usually claimed to be an ancient way, still it does seem to date from the time when boundaries were being established. The *Via Devana* also formed parish boundaries during a great portion of its length (line E–F). It is part of the Roman road from Colchester to the Midlands. From where it enters the County at E, for a distance of 11 miles, it separates parishes; and it continues this function to the north-west of Cambridge along the line G–H, which here forms the main road from Cambridge to Huntingdon. Ermine Street, also, helped to define boundaries (line K–L). Probably the oldest track in the County is the Icknield Way,[1] which is represented to-day by a section of the London-Newmarket road (line M–N). The parish boundaries, it is true, do not strictly follow the road, but that does not indicate that the way was not in existence when the boundaries were established.[2] Another straight boundary that attracts attention on Fig. 26 is the line P–Q. This is not a road, but the Devil's Dyke, that is a parish boundary for 10 miles.

[1] See p. 84 above.
[2] See Fig. 19.

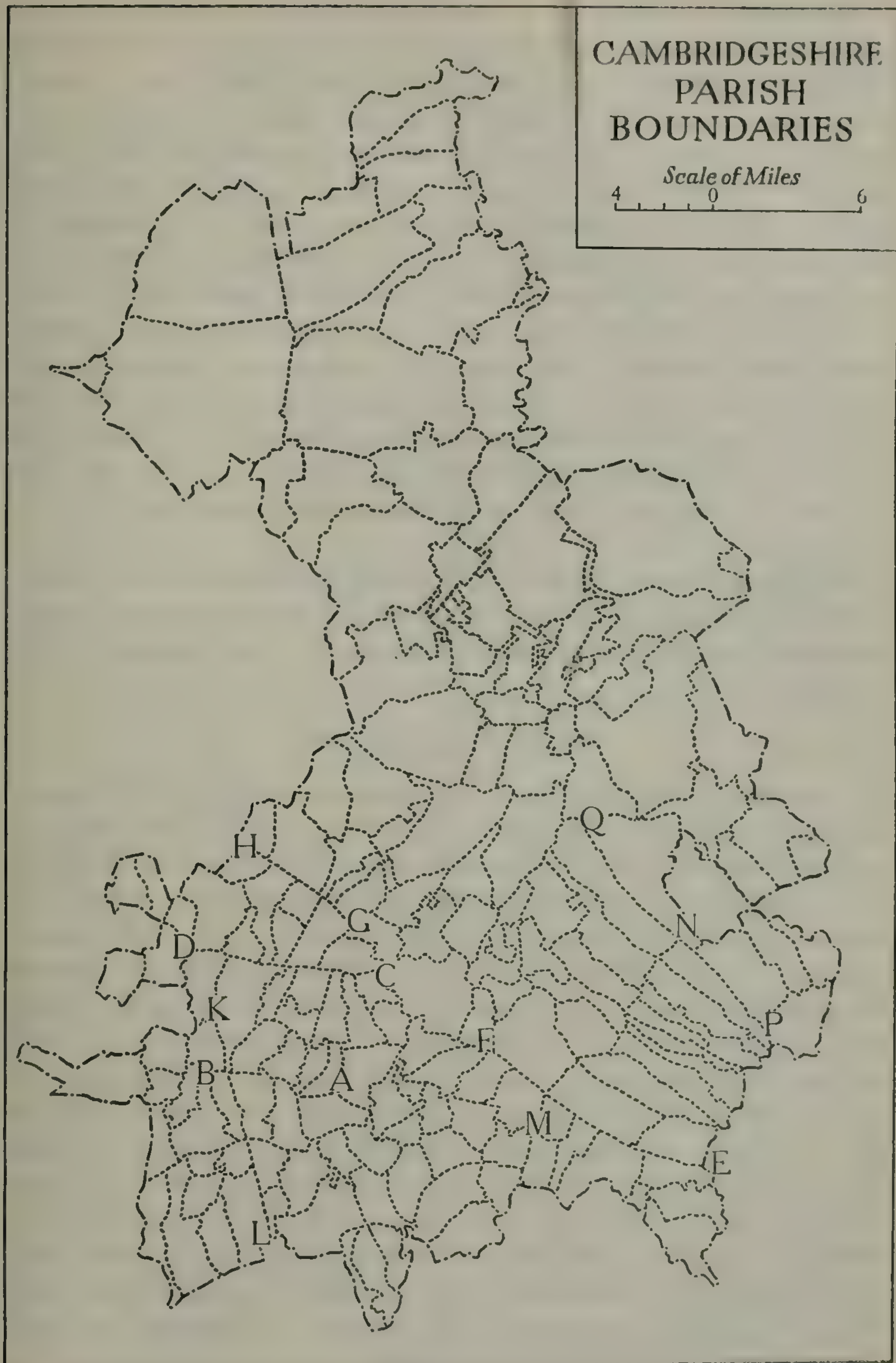


Fig. 26.

The letters A–Q refer to the sites named on p. 112.

The shapes of parishes are so varied that they defy classification. The chances of topography and of time cannot be reduced to generalisation. But the configuration of parishes in the south-east of the County certainly stands out as something peculiar. The upland villages of Fig. 24 are seen, on Fig. 26, to form a group of long narrow parishes lying side by side. They are oriented down the slope and not along it. This arrangement gives to each parish a variety in soil and vegetation. If the parishes had been, say, square, any single one might have contained nothing but clay (and woodland), while its neighbour could have consisted entirely of open chalk down. Beyond the boundary M–N, there is a second tier of parishes, lower down the slope. This side-by-side arrangement secures for them, also, two types of terrain—an area of chalk and an area of fen. South of the line E–F the parishes are oriented at right angles to the *Via Devana*; this gives to each a stretch of chalk upland as well as a share in the valley alluvium. A somewhat similar arrangement is found in the parishes of the Bourn Brook Valley.

All these indications of order in the parish map are but stray hints and glimpses; from them, however, we can see reason at work when the early settlers laid down the foundations of the present village geography of the County.

PARISH CHURCHES

The prominent centre in every village was the parish church, and it is interesting to see how the churches, like their parishes, reflect the geographical circumstances of their environment. The four maps of Fig. 27 show the main types of material used in their construction; naturally this classification is based only upon the chief materials used. Very often, a clunch or flint church has imported stone pillars and arches. For it should be remembered that Cambridgeshire is not very rich in good building stone. Even so, facilities of transport have not destroyed the local background. Flint churches are located mainly along the southern boundary of the County, where the flint was obtained from the Upper Chalk (see Fig. 4). Clunch, out of the Lower Chalk, is easily weathered, but, in the absence of better stone, it has been used to help build a number of churches. Rubble, probably consisting of stones gathered from the Boulder Clay and mixed with mud, has been used in thirty churches, but these often have stone dressings. The remaining churches are built of imported stone; most of the fen churches come within this category, for transport was comparatively easy along the fen waterways.

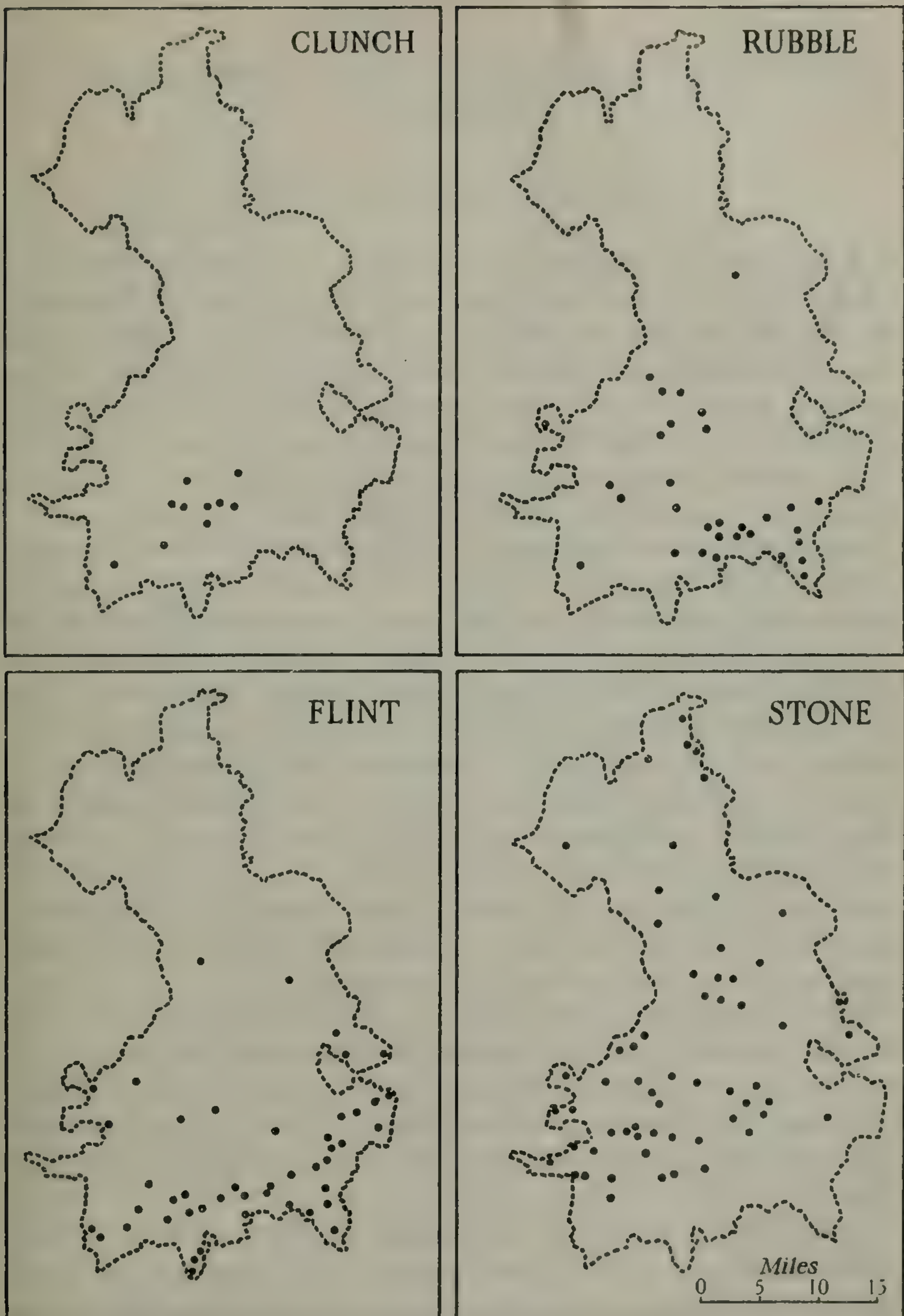


Fig. 27.

Cambridgeshire: Building materials used in churches.

CHAPTER NINE

CAMBRIDGESHIRE IN THE NINETEENTH CENTURY

By H. C. Darby, M.A., PH.D.

A PICTURE OF CAMBRIDGESHIRE ABOUT THE YEAR 1800 CAN be gained from two reports made to the Board of Agriculture. Both have the same title—the *General View of the Agriculture of the County of Cambridge*. The first was written by Charles Vancouver, and was published in 1794. The second was written by W. Gooch, and, although its Preface is dated 1807, the book itself was not published until 1813. Taken together, these two surveys supplement one another to provide an outline of the main features of the geography of the County in 1800.

Both reports are accompanied by what is substantially the same map of land utilisation (Fig. 28). Comparison with the geological map of the County (Fig. 29) provides an explanation. The outstanding contrast was between the northern Fenland and the southern upland. Conditions in the upland area reflected directly the geological division. Between the "close heavy, compact Clay" in east and west, the belt of chalk country stood out, running south-west—north-east. Where the chalk outcrop was almost waterless, there was "heath"; while the "valley through which the river Cam flows to Walton, is chiefly laid out into dairy farms, and hence it has its name, i.e. the Dairies".[1] In the Fenland, to the north, the islands stood out. Of the peat fens around, some were "under cultivation", others were "drowned or waste". Finally, the silt area of the extreme north maintained its reputation as "rich pastures".

One of the main objects in the making of these reports was an enquiry into measures necessary for improvement. In Cambridgeshire, there were two agricultural controversies that reflected the geographical circumstances of the time. One was associated with the need for an improved drainage; the other with the need for an increased enclosure of the common open-fields.

Vancouver estimated[2] the total acreage of the County as 443,300, divided as shown in the following table. The correct area of Cambridgeshire is 553,555 acres; but, even so, the proportion between the

[1] C. Vancouver, p. 87. Walton, on the north bank of the river, and lying to the south-east of Orwell, is marked on John Cary's map of 1818.
[2] *Ibid.* p. 193.

different districts, and their relative values as given by Vancouver, are probably accurate enough to be indicative.

Description of land	Number of acres	Rental or value per acre
		£ s. d.
Enclosed Arable	15,000	18 0
Open Field Arable	132,000	10 0
Improved Pasture	52,000	1 0 4
Inferior Pasture	19,800	10 9
Wood Land	1,000	15 0
Improved Fen	50,000	15 0
Waste and Unimproved Fen	150,000	4 0
Half Yearly Meadow Land	2,000	12 6
Highland Common	7,500	10 0
Fen or Moor Common	8,000	3 0
Heath Sheep Walk	6,000	2 6

DRAINING THE FENLAND

The condition of the Fenland towards the end of the eighteenth century was far from satisfactory,[1] and the phrases that Vancouver used to describe the fen parishes were monotonously similar. The fens of Fordham were "in a very bad state"; those of Bottisham were "in a deplorable situation, and subject to frequent inundations"; those of Burwell, too, were "constantly inundated". So were those of Ely and Upwell and Outwell. At Elm, cultivation was "very uncertain"; at Littleport, it was "extremely precarious". And so the tale of woe continued throughout the whole of the Isle of Ely. Only in a few parishes were the fens "tolerably well drained"; and even then often at "very considerable expense". The tragedy was all the greater because some parts of the fen under improved cultivation yielded "a produce far beyond the richest high lands in the county".[2]

These deplorable conditions were attributed by the men of the time to the "want of a better outfall through the haven of Lynn".[3] Within 3 miles of Lynn, the Ouse made a great bend following a course of about 6 miles or so. The channel of this river was of varying width and was full of shifting sandbanks. In some places it was as much as a mile wide, comprising a number of uncertain streams; and, during floods, the flow of the river was much impeded.[4] Many people believed that the only solution

[1] See p. 187 below.
[2] C. Vancouver, pp. 202–3. This estimate was based upon conditions in 50,000 acres of fen around "Chatteris, Elm, Leverington Parson Drove, Wisbich, St Mary's, and Thorney". Compare with the general information given in Vancouver's table above.
[3] *Ibid.* p. 139.
[4] See p. 191 below.

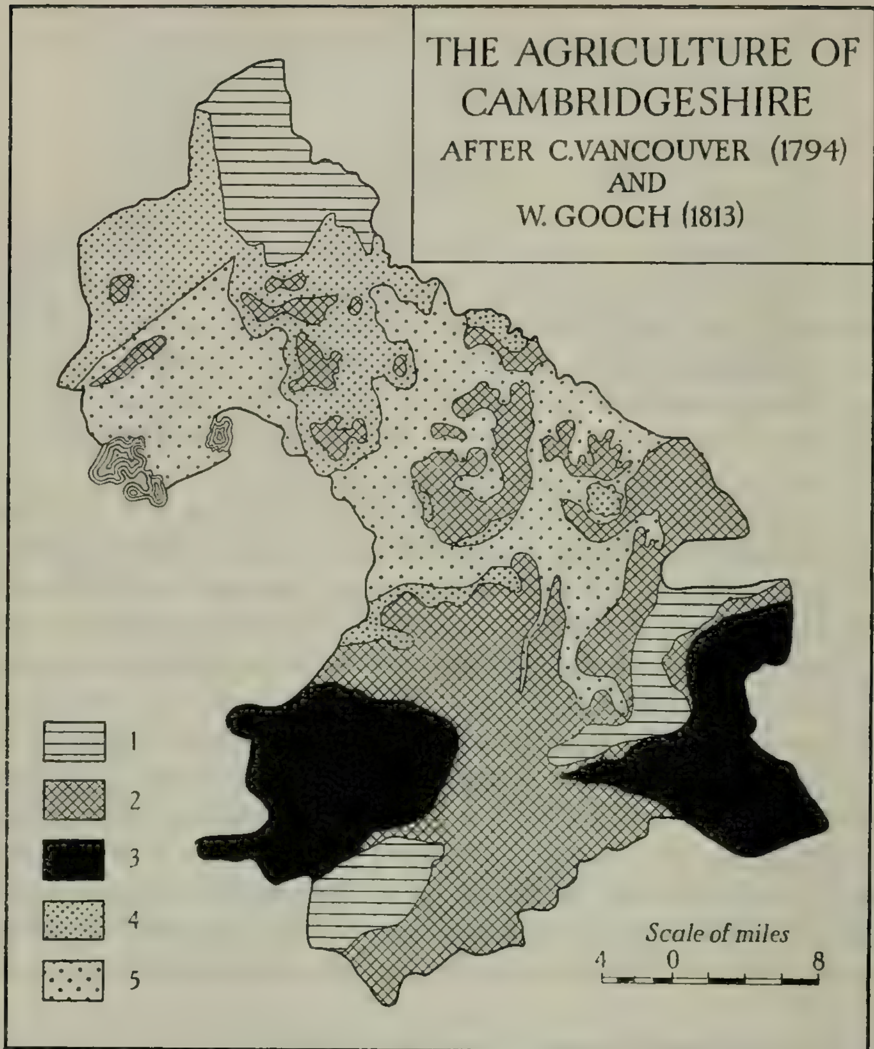


Fig. 28.

Land Utilisation in Cambridgeshire about A.D. 1800.

1. Part of Newmarket Heath, the Valley called the Dairies; and rich pastures produced from the Sea around Wisbeach.
2. Chalky, Gravelly, Loam and tender Clay.
3. Close heavy, compact Clay upon a Gault.
4. Fen under Cultivation and in the High Land Sand.
5. Drowned or waste Fen but all very capable of being reclaimed.

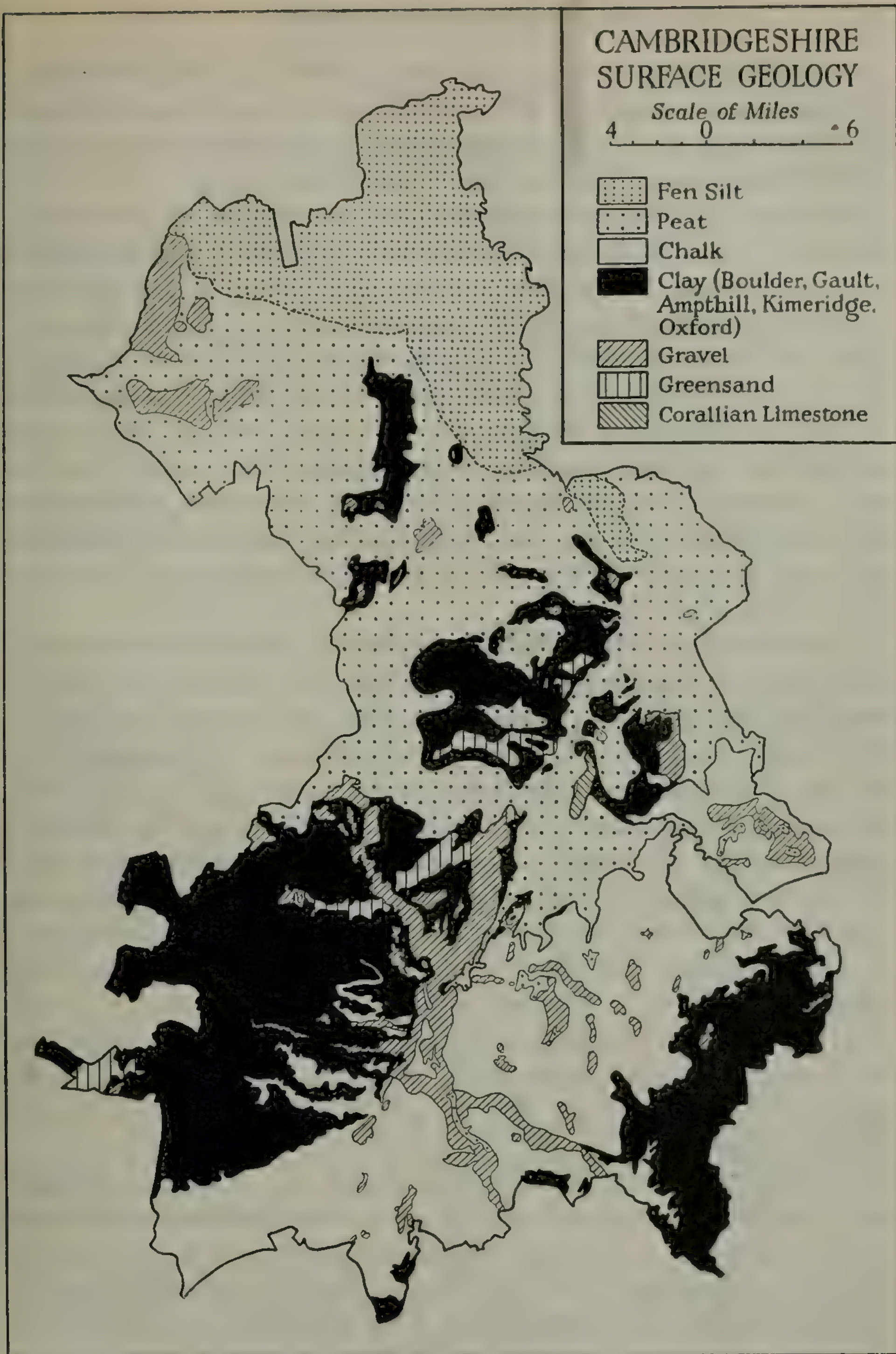


Fig. 29.

This map is based upon that of the Geological Survey. The boundary between the fen silt and peat is taken from the map accompanying S. B. J. Skertchly's *The Geology of the Fenland* (1877). This boundary is "very obscure, for the peat thins out insensibly..." (p. 129).

was to eliminate the great bend by a new straight cut made large enough to take quickly out to sea the whole body of the Ouse water. The Eau Brink Act, authorising the cut, was obtained in 1795. Disputes and delays followed, and the work was not completed until 1821.

It was therefore in an atmosphere of controversy that the two reports of 1794 and 1813 were compiled. Indeed, one of Vancouver's main objects was to see "how far the proposed measure, of diverting the course of the river Ouze, from its present channel between Eau-brink, and the Haven of Lynn, would embrace all the objects so fondly anticipated by the promoter of that measure".[1] Gooch, too, dealt with the desirability of an Eau Brink Cut. As he pointed out, the opinions were varied enough. But, at any rate, "all were agreed that something must be done or the country will be lost". During the succeeding years of the century, the straight line of the Eau Brink Cut was continued seawards by the Marsh Cut of 1852, and, later, by training walls built to carry the Ouse waters out to sea amid the shifting banks of the estuary.[2]

Complementary to the outfall question was the problem of the internal drains. Internal drainage during the eighteenth century had been accomplished by windmills that pumped water from the lowering peat surface into the high-riding river channels.[3] But, in the report of 1813, there is a hint of changes to come. The mills, depending on wind, were "often useless when most wanted",[4] and the proprietors consequently sustained "material injury". To remedy this, steam engines had been recommended, and, declared Gooch, "I found many persons in the county entertaining an opinion that they would answer". It was argued that the advantages to be obtained from the introduction of steam to the fen country were "almost incalculable". But there was delay and hesitation. Not until 1820 was the first steam-driven mill set up at Bottisham Fen. Succeeding years[5] but verified the prophecy of Gooch that "until a power can be commanded at will, for the drainage of the fen-country, it can never attain its full prosperity".

The improvements in draining that marked the nineteenth century were paralleled by improvements in agricultural practice. Owing to continued shrinkage and wastage, the surface layer of peat in the southern Fenland was becoming so thin, that, in some districts, the underlying clay was within easy reach of the plough.[6] Thus the virtues of clay were discovered,[7] and so potent did they prove to be that, where the clay was too

[1] C. Vancouver, p. 8.
[2] See pp. 191 and 201 below.
[3] See p. 187 below.
[4] See W. Gooch, pp. 239 *et seq.* for the quotations that follow in this paragraph.
[5] See pp. 188–9 below.
[6] See pp. 131 and 186 below.
[7] J. M. Heathcote, *Reminiscences of Fen and Mere* (1876), p. 90.

deep down for the plough, the practice of digging for it became frequent. In 1811, there was being advocated the use of the "most excellent clay marl" that underlay "the greatest part if not the whole of the Bedford Level".[1] By 1830, the practice was "so very modern" that Samuel Wells found "some difficulty in giving an accurate account of its singular process".[2] But the practice had come to stay; so much so that, in 1852, J. A. Clarke could write:

> Within the last 30 years the system of digging and throwing up this clay where it is too deep for the plough has been introduced into universal operation. The new husbandry quickly extended itself: farmers may be cautious of new improvements, but this was too obvious for dispute, too near at hand for refusal.[3]

Wherever clay could be found tolerably near the surface, "claying the land became the acknowledged mode of cultivation in the Fenland".[4] The peat lands thus became "the most productive of soils yielding the most luxuriant crops of wheat, oats, coleseed and turnips".[5]

ENCLOSING THE COMMON-FIELDS

Of the 147,000 acres of arable land in Cambridgeshire in 1794, 132,000 acres lay in open-fields, and followed the traditional open-field husbandry of the English plain. Of course it is possible that some of the 52,000 acres of "Improved Pasture", recorded in Vancouver's estimate, included land laid down to grass on enclosure. But even with this allowance, there can be no doubt about the unenclosed character of the Cambridge countryside. Corroboration is provided by the fact that of the ninety-eight parishes described in detail by Vancouver, eighty-three were still open; only fifteen had been enclosed. And Vancouver considered that no improvement was possible until the intermixed strips "dispersed in the common open fields" had been brought together into compact holdings. Enclosure appeared "to be indispensably necessary" and urgent.

> "I have made it my particular care", he wrote, "to mix and converse with the yeomanry of the county, and in their sedate and sober moments, to possess myself fully of their experience, and local knowledge; and finally to ascertain the general sentiment as to this important innovation upon the establishment of ages."[6]

In some places, people were doubtful; thus at Teversham the idea of enclosing was "not all relished".[7] In other places, "the most thinking

[1] R. Parkinson, *The Agriculture of the County of Huntingdon* (1811), p. 299.
[2] S. Wells, *History of the...Bedford Level*, i, 442 (1830).
[3] J. A. Clarke, *Fen Sketches* (1852), pp. 244–5.
[4] J. M. Heathcote, *op. cit.* p. 90. See p. 152 below.
[5] J. A. Clarke, "On the Great Level of the Fens", *Jour. Roy. Agric. Soc.* viii, 92 (1848).
[6] C. Vancouver, p. 195. [7] *Ibid.* p. 47.

farmers"[1] were very much in favour of "the laying of the intermixed property together in the open fields".[2] And Vancouver was emphatic in demonstrating the improvement in crop yields that resulted from enclosure.

Want of enclosure was also felt in the Highland Common which "in severalty" would have been doubled in value; while the Half Yearly Meadow Land, "dispersed through the hollows of the open fields", would even more than double in value "by proper draining and being put into severalty".[3]

The report of 1813 by W. Gooch showed that "most of the arable husbandry of this county" was still foreign "to present practice in the best cultivated countries".[4] Many people still believed that the older methods were the best, and "this bigotry" was widely spread. But something had certainly been done to redeem the County "from the imputation it has so long lain under, of being the worst cultivated in England".[5] By 1807, the open-field arable was "much lessened", and a great part of "the waste and unimproved fen, half-yearly meadow, highland common, fen or moor common, sheep-walk heath", had become enclosed arable and pasture. In the case of open-field conversion, the total rental had more than doubled: on other lands it had trebled at least.[6]

But more still remained to be done. In 1822, when William Cobbett travelled along the Old North Road from Royston to Huntingdon, much of the country was still treeless and hedgeless, full of "those very ugly things, common-fields", and looking "bleak and comfortless" to the eye.[7] Still later, in 1830, between Cambridge and St Ives, Cobbett again saw "open unfenced fields".[8] But Cambridgeshire was coming into line with the rest of the English plain. By 1847, all its open common-fields, "with the exception of five or six parishes",[9] had been enclosed.

THE CURVE OF PROSPERITY

The fluctuations of agricultural fortune in Cambridgeshire during the nineteenth century reflected, very largely, variations in the prosperity of

[1] C. Vancouver, p. 53. [2] *Ibid.* p. 147. [3] *Ibid.* p. 204.
[4] W. Gooch, p. viii. [5] *Ibid.* p. 56. [6] *Ibid.* p. 2.
[7] W. Cobbett, *Rural Rides* (Everyman's edition), i, 80–2: "Immediately upon quitting Royston, you come along, for a considerable distance, with enclosed fields on the left and open common-fields on the right....The fields on the left seem to have been enclosed by act of parliament; and they certainly are the most beautiful tract of *fields* that I ever saw. Their extent may be from ten to thirty acres each. Divided by quick-set hedges, exceedingly well planted and raised" (p. 80).
[8] *Ibid.* ii, 236.
[9] S. Jonas, "On the Farming of Cambridgeshire", *Jour. Roy. Agric. Soc.* (1847), p. 38. G. Slater gives nine parishes enclosed after 1847 (*The English Peasantry and the Enclosure of the Common Fields* (1907), p. 273). But for at least ten, see E. M. Hampson, "Cambridge County Records", *Proc. Camb. Antiq. Soc.* xxxi, 143 (1931).

the country as a whole. Generally speaking, these variations can be summed up by saying that the period 1815 to 1837 was marked by depression; that of 1838 to 1874 was marked by improvement and prosperity; while after 1874 the century was again characterised by adversity and difficulty. It is in the light of this general curve that the evidence for Cambridgeshire must be examined.

The County shared with the rest of England in the disastrous effects of the Napoleonic wars. The year 1815 brought peace and beggary. Between 1814 and 1816, agriculture passed suddenly from prosperity to extreme depression. As Richard Preston asked, "Was Great Britain ever before in so reduced and impoverished a condition?"[1] As for Cambridgeshire, Lord Brougham, speaking in the House of Commons on 9 April 1816, said:

> The petition from Cambridgeshire presented at an early part of this evening, has laid before you a fact to which all the former expositions of distress afforded no parallel, that in one parish, every proprietor and tenant being ruined with a single exception, the whole poor-rates of the parish thus wholly inhabited by paupers, are now paid by an individual whose fortune, once ample, is thus entirely swept away.[2]

In the same year, it was said that "a detestable spirit of conspiracy" was manifesting itself "in the counties of Norfolk, Suffolk, Huntingdon and Cambridge, directed against houses, barns and rick-yards, which were devoted to the flames".[3] This was generally ascribed to a "want of agricultural employment, joined to the love of plunder".[3] In some localities, the general unrest broke out into riots, and the number of labourers, committed to the county gaol under the Vagrancy Law for "refusing to work for the customary wages", rapidly increased from the twenties onwards.[4]

In December 1829, came a petition from the farmers of Ely to Parliament. It could but repeat what was well known already. The labourers, "no longer able to maintain themselves by the sweat of their brows", were driven "to the scanty pittance derived from the parish funds".[5] Frequently, their distress sought a violent outlet. There was an outbreak of

[1] Richard Preston, "Review of the Present Ruined Condition of the Agricultural and Landed Interests", *Pamphleteer*, vii, 150 (1816).

[2] *Speeches of Henry, Lord Brougham*, i, 504 (1838). Lord Ernle notes that, in 1815, nineteen farms in the Isle of Ely were without tenants; and that the number of arrests and executions for debt in the Isle increased from 57 in 1812–13 to 263 in 1814–15 (*English Farming Past and Present* (1932), pp. 322–3).

[3] *Annual Register* (1816), p. iv.

[4] See E. M. Hampson, *The Treatment of Poverty in Cambridgeshire*, 1597–1834 (1934), p. 196.

[5] See *ibid.* p. 215.

rick-burning in Cambridgeshire, as in England generally. The commissioners appointed to investigate the causes of these disturbances found "distress and want of employment" all through the County.[1]

Of course, all years were not equally bad, and amidst many variations of statement and opinion, it is difficult to assess the degree of distress at any particular moment.[2] But the weight of Mr Thurnall's evidence, in 1836, leaves no doubt about the general picture.[3] Land in a neglected state was "every day increasing in quantity" owing to the "low price of agricultural produce".[4] On the other hand, he thought that, as yet, no land had been "thrown out of cultivation". Still, the condition of the tenantry was "verging on insolvency".[5] Rick-burning was frequent, and several of his best and honest labourers were threatening to rob on the highway before they would "go to the union work house". They were "ripe for everything in the world", ready to be stirred into "a state of revolution".

The accession of Queen Victoria in 1837 coincided with the beginnings of improvement. The formation of the Royal Agricultural Society in the following year was at once a symptom of revival and an aid to prosperity. Despite ups and downs, the decade that followed was marked by an advancement that reflected itself in one of England's most agricultural of counties. In his survey of 1847, Samuel Jonas declared that "few counties, if any, have improved more in cultivation than Cambridgeshire has lately done".[6]

All the open common-fields have been enclosed (with the exception of five or six parishes), and instead of a system of cropping so exhausting to the land as a fallow and two white-straw crops in succession, with other men's flocks of sheep eating up your food and preventing improvement, we now see the land farmer on the four course system—the best that can be adopted, unless on very fine land.

[1] *Parliamentary Papers* (1834), xxxiv, Appendix B, Pt. v, to the Report on Poor Laws, pp. 49–72.

[2] Thus a calculation of the amount of unemployment in Cambridgeshire in 1830–1831 stated that "the total number of unemployed labourers in 156 parishes [out of a total of 164] in Cambridgeshire was 811; not one sixteenth of the total number of labourers, very little more than five men being so reckoned to a parish, and one man to a population of 169". And, again, "we cannot suppose any to remain unemployed during the three months which hay and corn harvest last". *Parliamentary Papers* (1834), xxxvii, Appendix C to the Report on Poor Laws, pp. 72–3.

[3] *Rep. Select Committee on Agricultural Distress* (1836), viii, Pt. 1, pp. 115 *et seq.*

[4] He attributed this to the contraction of the currency; "that is the main cause; Irish produce is another cause; want of protection against foreign corn is another; but I should say that the contraction of the currency is the main cause". (*Ibid.* p. 121.)

[5] For this, and the remaining quotations in the paragraph, see *Rep. S.C. Agricultural Distress* (1837), v, 129.

[6] For the quotations that follow, see S. Jonas, "On the Farming of Cambridgeshire", *Jour. Roy. Agric. Soc.* (1847), p. 35.

Large flocks of sheep were fattened with corn and cake for the London markets; indeed, Mr Jonas Webb of Babraham was "one of the first and most justly celebrated breeders of Southdown sheep in existence". Large numbers of cattle were also to be seen. "Comparing the present system with the former," wrote Jonas, "it is astonishing to mark the increased wealth our present improved system brings to the state; not only thus largely increasing the national wealth, but also giving full employment for our labourers."

Jonas divided Cambridgeshire into four districts:

(1) "The southern and central part of the county, extending from Ickleton to the north side of Newmarket, is light land, consisting of chalk, sands, tender loams, and gravels." On these "thin-skinned, poor, light, hungry lands", where turnips formed part of the rotation, the application of bones and guano had done much; at Duxford and Whittlesford were "two very extensive and most excellent bone-mills".

(2) The eastern side of the county, adjoining parts of the counties of Essex and Suffolk, up to Cheveley, near Newmarket, was heavy clayland of various qualities, "all well hollow-drained, and generally speaking well farmed".

(3) Thirdly, came "the Fen district, an accumulation of vegetable deposit resting on the fen-clay". The improvements in this district due to draining and claying the land were "truly wonderful. Drainage condenses the land, and claying consolidates it".[1]

(4) Lastly, "the western side of the county, adjoining Bedfordshire, Hertfordshire, and Huntingdonshire, consists of a tough tenacious clay of little value on the hills,[2] but the flats are good, strong, deep, staple lands". This area was not as well managed as the eastern clayland, "particularly as relates to draining".

But although, generally speaking, "improvements and high farming" were bringing prosperity to the County, Jonas had to confess that "there yet remained some districts that were badly cultivated". He seems to have had the western clays particularly in mind.

The picture that James Caird gave of the County in 1850–51 is quite another impression:

> In any district of England in which we have yet been, we have not heard the farmers speak in a tone of greater discouragement than here. Their wheat crop, last year, was of inferior quality, the price unusually low, and to add to this, their live stock and crop are continually exposed to the match of the prowling incendiary.[3]

[1] For the importance of claying the fen peat, see p. 121 above.
[2] See p. 27 above.
[3] J. Caird, *English Agriculture in* 1850–51 (1852), pp. 477 *et seq.*

Fires were "of almost nightly occurrence". "A few bad fellows in a district are believed to do all the mischief, and bring discredit on the whole rural population." But this does not imply that the rural population had no grievance; although employment was available, wages were low, "7s. to 8s. a week being the current rate".[1] Taken together, these two pictures, by Jonas and Caird respectively, of technical improvement and social discontent, may give some idea of conditions in Cambridgeshire during the middle of the century.

However conflicting the evidence, nothing can gainsay the fact that many parts of rural Cambridgeshire had a special boom of their own, after about 1850, through the setting up of the "coprolite" industry for the manufacture of manures. By origin, the word "coprolite" signifies petrified dung, presumably of enormous reptiles, but the term came to include phosphatised casts of vertebrate remains in general. Coprolites were to be found in the Cambridge Greensand[2] that marked the base of the Chalk, and that ran north-east—south-west through Soham, Burwell, Swaffham, Horningsea, Cambridge, Grantchester, Barrington, and so westward into Bedfordshire.[3] The deposits were described by O. Fisher in 1873:

> The Cambridgeshire phosphatic nodules, as is well known, are extracted by washing from a stratum (seldom much exceeding a foot in thickness) lying at the base of the lower chalk, and resting immediately, without any passage-bed, upon the Gault. There is, however, a gradual passage upwards from the nodule-bed into the lower chalk or clunch. The average yield is about 300 tons per acre; and the nodules are worth about 50 shillings a ton. The diggers usually pay about £140 per acre for the privilege of digging, and return the land at the end of two years properly levelled and re-soiled. They follow the nodules to a depth of about 20 feet; but it scarcely pays to extract them to that depth.[4]

Generally speaking, the years between 1850 and about 1870 were prosperous ones for much of the County. A footnote in the Census Returns attributes an increase of population at Orwell, between 1861 and 1871, to the "demand for labour in the coprolite diggings". The same cause, too, was responsible for growth at Barton, Great Eversden, Harston, Haslingfield and Trumpington; and at Wicken the increase was likewise "attributed to the extensive coprolite digging having attracted numbers of labour". By the end of the century, however, the coprolite beds had

[1] J. Caird, *op. cit.* p. 468.

[2] See p. 13 above.

[3] The Lower Greensand phosphatic deposits were also being worked about the years 1866–68, chiefly near Wicken. W. Keeping, *Fossils of the Neocomian Deposits of Upware and Brickhill* (1883), pp. 1–2. See p. 11 above.

[4] O. Fisher, "On the Phosphatic Nodules of the Cretaceous Rocks of Cambridgeshire", *Quart. Jour. Geol. Soc.* xxix, 52 (1873). A detailed account of coprolite digging, based upon direct observation at Burwell, is given by C. Lucas, *The Fenman's World* (1930), p. 25.

become practically exhausted,[1] and were only temporarily revived during the Great War of 1914–18.

Despite the coprolite prosperity, Cambridgeshire shared in the general ebb that marked English agriculture from the seventies onwards. An idea of the nature of farming in the County is provided by the following figures,[2] derived from the Agricultural Returns of 1874:

	Cambridge	England
Percentage of corn crops to cultivated land	53·4	31·4
Number of cattle per 100 acres	9·9	17·0
Number of sheep per 100 acres	67·4	80·0

The type of farming indicated by these figures made the district very susceptible to the depression that started[3] between 1875 and 1879. Mr Druce, who visited the County in 1880, attributed the depression, first and foremost,

> to a succession of four or five years' deficient harvests [due to wet seasons], accompanied with extremely low prices, occasioned by the excessive importations from America. Among the contributory causes were increased rates of wages and the difficulty of obtaining juvenile labour due to the Education Acts.[4]

In those districts of the County where attention had been directed to the production of meat, the depression was not so much felt; but the tenor of Mr Druce's report leaves no doubt about the general situation. In the Fenland, "one-half the farmers were absolutely insolvent, and the other half greatly reduced in circumstances". On the upland, "there were considerable quantities of land unlet and which could not be let".

Nor did the story end there. In another report, made in the following year, Mr Druce found "that the depression has very much increased";[5] and he added that the Agricultural Returns for 1881 "afford confirmatory evidence of the continuance and depth of the depression in this county". The numbers of stock were continuing to decrease, notwithstanding an earlier decrease since 1875. Mr Druce graded the intensity of the depression among the counties that comprised his district as: (1) Huntingdon, (2) Essex, (3) Cambridge.

[1] E. Conybeare, *A History of Cambridgeshire* (1897), p. 269. See also T. M. Hughes and M. C. Hughes, *Cambridgeshire* (1909), p. 112.

[2] F. Clifford, *The Agricultural Lock-Out of* 1874 (1875), pp. 339–40.

[3] *Royal Commission on Agriculture: Report on Cambridgeshire, Report by Mr Wilson Fox* (1895), p. 25.

[4] *Royal Commission on Agriculture, Report by Mr Druce* (1881), p. 365.

[5] *Royal Commission on Agriculture, Report by Mr Druce* (1882), pp. 14–20.

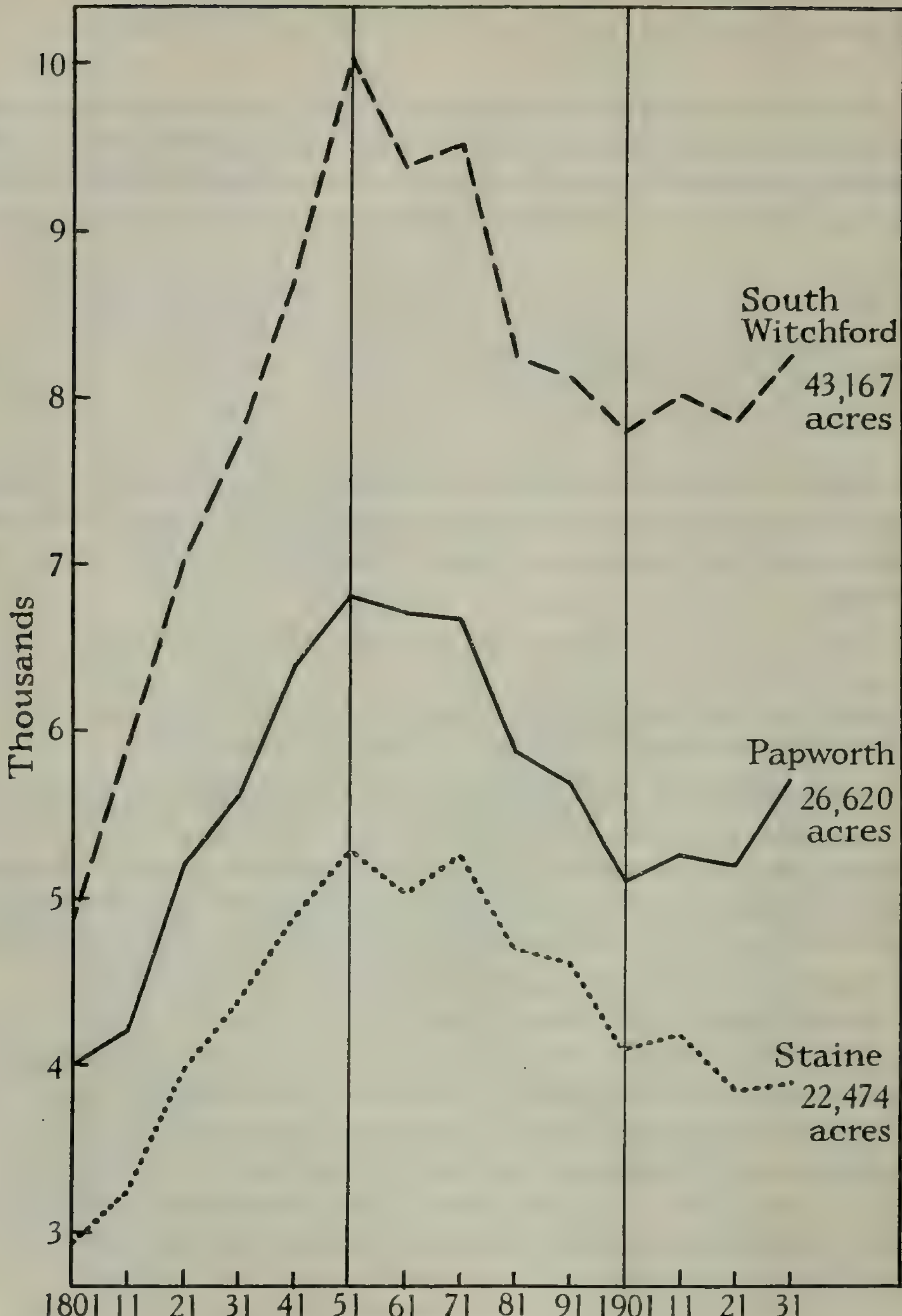


Fig. 30.

Population changes in three rural hundreds of Cambridgeshire, 1801–1931. I am indebted to the Editor of the *Victoria County Histories* (Mr L. F. Salzman) for access to the Population Tables (by G. S. Minchin) in the forthcoming *Cambridgeshire*, vol. ii.

This second half of the nineteenth century was also marked by another feature in the rural circumstances of the County. During the earlier half of the century, population had continued to grow despite distress and unemployment. But before the middle of the century, this situation was changing. A hint of things to come is provided by that footnote, in the 1841 Census Returns, which states that, from Willingham, "upwards of 100 persons have emigrated to the United States since 1831". At Wimpole, a decrease was "attributed to several large families having left the Parish, and others having emigrated since 1831". The 1851 Returns noted that the decrease at Croxton was also due in part to emigration, as was that at Wimpole and West Wratting. The 1861 Returns have very many of these references. One footnote tells its own story:

> General decrease of population throughout the district of Caxton and especially in the parish of Caldecote is mainly attributed to emigration and migration owing to lowness of wages, etc.

Similar causes helped to account for a decrease in thirty other villages in the County. The reason stated was sometimes "emigration"; sometimes "migration of labourers to towns", or to "manufacturing districts", or to "London and the north of England", or to "Manchester and its vicinity", or, again, to "the metropolis and other large towns". It is true that the Census footnotes also record some increase due to "the erection of new cottages on a recent enclosure", as at Gamlingay and Hardwicke; or due to a temporary influx of labour employed upon railway construction at Great Shelford and Harston, or employed upon a new cut at Clenchwarton.[1] At Sawston and Whittlesford, the increase was "due to the paper mill and parchment factory at Sawston". Then, too, there were the attractions of the coprolite diggings[2]; there were also some miscellaneous explanations.

After 1871, the explanatory footnotes cease to appear in the Census Returns, but the figures themselves tell their own story. Fig. 30 sums up the evidence for three rural hundreds in the County, and shows quite clearly how the countryside was emptying itself. The difference between this diagram and that of Fig. 31 is explained by the growth of Cambridge,[3] Wisbech, Ely, March, and Whittlesey, and also by local circumstances (e.g. jam-making at Histon). By the end of the century, the urban and semi-urban centres had grown; the rural settlements had become smaller.

A full picture of rural conditions towards the end of the century is given in the Cambridgeshire section of the *Report of the Royal Commission on Agriculture* (1895). This drew a great distinction between north and south Cambridgeshire:

[1] The Eau Brink Cut; see pp. 120, 191. [2] See p. 126 above. [3] See Fig. 41.

In the north there are a number of districts where the fen land has depreciated but little, and some where it has not depreciated at all, while in the south there are large tracts where the deteriorated state of the land is painfully apparent to all, being practically worthless to owner and occupier alike, and scarcely able to be designated as cultivated. Between Cambridge and Huntingdon the state of the land is as bad as in the worst districts in Suffolk, and in some other localities it is little if any better.[1]

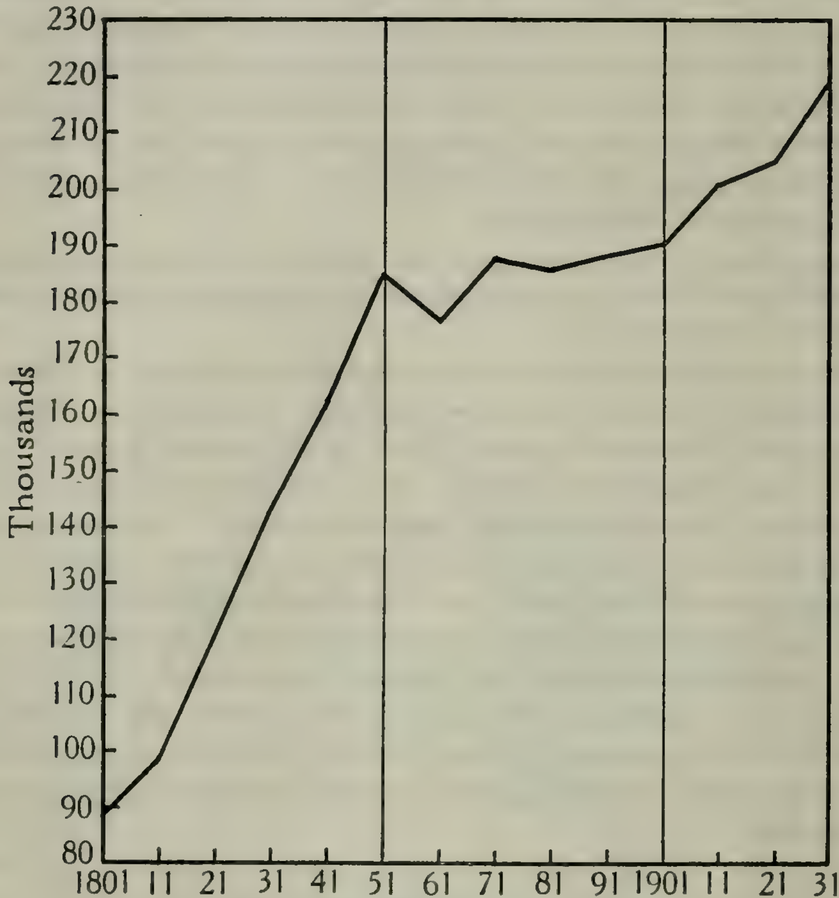


Fig. 31.

Population changes in Cambridgeshire (including the Isle of Ely), 1801–1931. I am indebted to the Editor of the *Victoria County Histories* (Mr L. F. Salzman) for access to the Population Tables (by G. S. Minchin) in the forthcoming *Cambridgeshire*, vol. ii.

On the upland areas of the County, it was generally acknowledged that there was "a great deal of rough land very nearly out of cultivation". Captain Hurrell of Madingley, "in a nine mile run with the hounds",[2]

[1] W. Fox, *Royal Commission on Agriculture: Report on Cambridgeshire* (1895), pp. 25–6. "Between Cambridge and Huntingdon" was heavy clayland (see Fig. 29).
See also R. Bruce, "Typical Farms of East Anglia", *Jour. Roy. Agric. Soc.* (1894), p. 497, for details of farms at Barton, Bourn, Linton, Little Eversden, Littleport, Trumpington, and Whittlesford.

[2] See W. Fox, *op. cit.* pp. 26–7 for the quotations that follow in this and the next paragraph.

rode over only nine arable fields; "most of it had been seeded down". On "the boulder clay formation to the west of Cambridge, a considerable area" had been left uncultivated.[1] There were also "much fewer stock and sheep being kept in the county". Mr Dymock, who farmed 600 acres at Waterbeach, said that the condition of the land had been going back for twelve years. "It began in the bad season of 1879, when the heavy land got into a very bad state. Then bad prices came, and hence so much money could not be spent on it." Mr W. J. Clark, of Thriplow, could "point to farms that 10 years ago were patterns for cleanliness and good farming that are now in a deplorable state". Arrears and reductions of rent were "undoubtedly large in number".[2] Mr Martin Slater, of Weston Colville, thought that the land had "very greatly gone back in condition during the last 25 years in his district". Of the land outside the Fens, "the turnip and barley land near Newmarket" (i.e. light land) was said to have suffered least.

The evidence from the Fenland was less doleful. It was true that some localities had deteriorated, "partly from the effect of the seasons and partly from want of capital". Since the depression, fewer cattle and sheep had been kept. At Chatteris, it was stated that "the high lands and gravel lands have certainly gone back". That all was not desolation, however, can be seen from the following statement made at a meeting of farmers at Wisbech in 1894:

> Generally speaking, the strong land has deteriorated. The wet seasons had a great deal to do with it, as well as loss of capital. Last year [1893] did a lot to help the strong land. Men will not put money into strong land farming. The acreage of wheat crop has decreased by 25 per cent in this district. The fen land has gone back very little in condition; but it is not clayed so much, partly from want of capital, but partly because it is becoming stronger on account of the peat disappearing owing to the drainage.[3] The marsh land has not gone back a bit between Wisbech and Long Sutton; there has been the means of enabling the people to escape from the depression. They are able to grow the best class of potatoes, vegetables, and fruit. The men in the marsh have been hit to some extent by prices, but are better off than other people occupying land.

Thus was a new element called in to redress the balance of the older economy. The first orchard had been planted in the Wisbech area as early as the fifties; now, in the eighties and nineties, many farmers found themselves forced to adopt a fresh form of husbandry, and so turned to market gardening and fruit farming.[4] The new crops had also been spreading on the upland.[5] The Chivers' enterprise around Histon dates from the middle

[1] See p. 56 above. [2] W. Fox, *op. cit.* p. 34. [3] See p. 120 above.
[4] See C. Wright and J. F. Ward, *A Survey of the Soils and Fruit of the Wisbech Area* (1929), pp. 25–7.
[5] J. F. Ward, *West Cambridgeshire Fruit-Growing Area* (1933), pp. 29–33.

of the century;[1] while, at Rampton and Cottenham, a considerable amount of fruit was being grown by the villagers. In 1873, there were about 1000 acres of fruit within 10 miles of Histon; by 1894, this acreage had increased to 3000. The other fruit-farming area on the upland was around Meldreth and Melbourn, where, during the fifties, a substantial acreage of fruit had been planted.

It is little wonder, then, that the *Report* of 1895 could state that the profits made from fruit growing and market gardening "have in the last few years been more satisfactory than those from ordinary farming".[2] From the depression of the nineteenth century, the twentieth was to inherit at any rate some beginnings of prosperity.

NOTE ON RAILWAY CONSTRUCTION

"The principal rivers are the Cam or Granta, and the Ouse: the latter river is navigable from Cambridge to Lynn, in Norfolk, to which port large quantities of the grain produce of this county hitherto have been sent by this navigation; but it will soon be a question whether the corn-produce will not in future travel to London by the railroad".[3] Thus wrote S. Jonas in 1847, two years after the opening of the London-Cambridge-Norwich main line. The other railway lines quickly followed.

The lines passing through the County mostly formed part of the Great Eastern system. But three other railway companies also ran over lines of the G.E.R. Co. to Cambridge: the Great Northern from Hitchin via Shepreth; the Midland from Kettering via Huntingdon; and the London and North-Western from Bedford and Bletchley via Hills Rd. Junct. Cambridge; while, in the north of the County, the Peterborough, Wisbech, Sutton Railway was part of the Midland and Great Northern Joint Committee's line. The various lines (see Fig. 32) were opened[4] at the following dates:

1. The G.E.R. main line from London to Norwich, entering the County at Chesterford, and leaving it after passing through Cambridge and Ely.	30 July 1845.
2. Ely to March and Peterborough.	9 December 1846.
3. March to Wisbech.	3 May 1847.

[1] H. Rider Haggard, *Rural England* (1902), ii, 51. In 1873, the manufacture of jam was started at Histon. See p. 156 below.

[2] W. Fox, *op. cit.* p. 6.

[3] S. Jonas, "On the Farming of Cambridgeshire", *Jour. Roy. Agric. Soc.* (1847), p. 38.

[4] For this information I am much indebted to Mr J. H. Wardley of King's Cross Station. Mr E. D. Robinson of Cambridge has also given me help in this connection: an older list is in E. Conybeare's *A History of Cambridgeshire* (1897), p. 279.

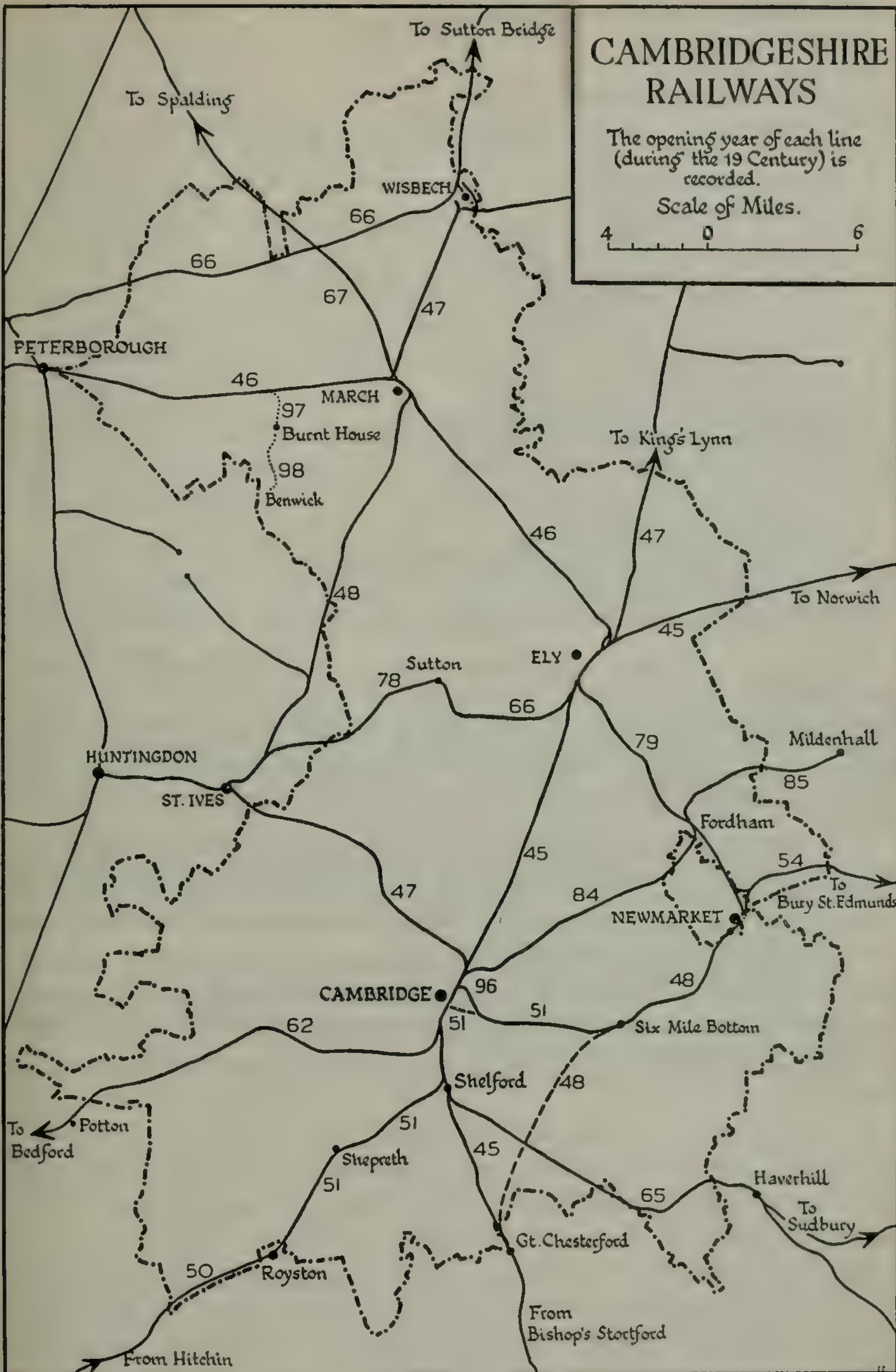


Fig. 32.

I am indebted to Mr J. H. Wardley of King's Cross Station for the information on this map.

4.	Cambridge to St Ives and Huntingdon.[1]	17 August 1847.
5.	Ely to Lynn.	26 October 1847.
6.	March to St Ives.	1 February 1848.
7.	(*a*) Chesterford to Newmarket.[2]	4 April 1848.
	(*b*) Newmarket to Bury St Edmunds.	1 April 1854.
8.	(*a*) Hitchin to Royston.	2 October 1850.
	(*b*) Royston to Shepreth.	3 August 1851.
9.	Shepreth to Shelford.[3]	25 April 1851.
10.	Cambridge to Six Mile Bottom.[4]	9 October 1851.
11.	Bedford to Cambridge (L.N.W.R.), entering the County north of Potton.	1 August 1862.
12.	Shelford to Haverhill (Suffolk).	1 June 1865.
13.	March to Spalding.[5]	1 April 1867.
14.	(*a*) Ely, Haddenham and Sutton.	6 April 1866.
	(*b*) Sutton to Needingworth (Hunts).	10 May 1878.
15.	Peterborough, Wisbech, and Sutton.	1 August 1866.
16.	Ely to Newmarket.	1 September 1879.
17.	(*a*) Cambridge (Barnwell) to Fordham.	2 June 1884.
	(*b*) Fordham to Mildenhall.	1 April 1885.
18.	(*a*) Goods line from Three Horse Shoes Junction to Burnt House Siding.	1 September 1897.
	(*b*) Burnt House Siding to Benwick.	2 August 1898.

[1] By agreement of 26 June 1864, the Midland trains ran from Kettering to Cambridge over this line.

[2] The section of this line from Chesterford to Six Mile Bottom (about 12 miles in length) was closed on 9 October 1851, upon the opening of the line from Six Mile Bottom to Cambridge. It was abandoned by the Eastern Counties Railway Act of 1858. The deserted cuttings and embankments are still striking features of the landscape.

[3] The G.N.R. were compelled by their Act to permit the G.E.R. to meet them at Shepreth, and did not get running powers over the line to Cambridge until 1866. Before the Shelford and Shepreth line was made available, the G.N.R. used to run coaches from Shepreth to Cambridge by road, in connection with their trains, timed to do the distance (9 miles) in 40 minutes.

[4] To join the unabandoned section of the Chesterford-Newmarket line. There was an extension from Newmarket to Bury St Edmunds on 1 April 1854. The junction at Cambridge Station was taken out when the present diversion line over Coldham's Common was opened on 17 May 1896.

[5] Originally a G.N.R. line, but owned jointly with the G.E.R. up to the Railways Act of 1921.

CHAPTER TEN

THE AGRICULTURE OF CAMBRIDGESHIRE

By R. McG. Carslaw and J. A. McMillan

(A) THE PERIOD 1900-1936

By R. McG. Carslaw, M.A., PH.D.

THE FOUR PHASES EVIDENT IN THE FARMING OF THE COUNTRY since the beginning of the century have been well marked in Cambridgeshire:

(i) The pre-war years up to 1914, when, on the whole, profits and wages were gradually rising. During this time, adjustments in cropping, in livestock policies, and in methods of production, were being methodically, if slowly, evolved.

(ii) The abnormal war, and immediately post-war, years of 1914–20, characterised by scarcity prices, by high profits, and by the improvisation of methods to meet a shortage of labour and raw materials.

(iii) The post-war depression of 1921–31, with heavy capital losses, with statutory minimum-wage legislation, and with much searching for new methods and types of farming, e.g. the development of sugar-beet growing, poultry, motor tractors, etc.

(iv) The years 1932–36, marked by the combined effect of (*a*) Governmental action, e.g. subsidies, tariffs, and quotas; (*b*) Marketing Boards (milk, pigs, potatoes, etc.); (*c*) cheap feeding stuffs; and (*d*) improved technical efficiency. This period, too, has been characterised by better profits and by rising wages.

Between 1900 and 1936, there was a decrease of over 20,000 acres (about 4 per cent) in the area under crops and grass (from 490,306 acres in 1900, to 467,980 acres in 1936).[1] Rather more than one-half this decline can be attributed to the deterioration of cultivated land, particularly since 1920, into "rough grazings"; but as much as 10,000 acres was lost to agriculture as a result of the encroachment of buildings, roads, etc. In spite of this decline in cultivated area, there is reason to believe that the

[1] The figures in sections A and B of this chapter are derived largely from Ministry of Agriculture Statistics.

aggregate agricultural production has not diminished. The principal "cash crops" for farmers in the County are wheat, barley, sugar beet, and potatoes. As the following table shows, the combined area of these crops has increased considerably:

	1900	1910	1920	1930	1936
	acres	acres	acres	acres	acres
Wheat	95439	100432	96091	81552	104790
Barley	52484	53821	56071	47703	32995
Potatoes	22790	26865	36416	32152	41324
Sugar beet	—	—	—*	43970	41458
Total	170713	181118	188578	205377	220567

* Less than 50 acres were grown in 1919.

The increased acreage devoted to "cash crops" has been secured by reducing the area under crops grown primarily for fodder—turnips, swedes, mangolds, oats, and rotational grasses. As the table below shows, there has been a steady decline from 1900 to 1936 in these principal fodder crops:

	1900	1910	1920	1930	1936
	acres	acres	acres	acres	acres
Turnips and Swedes	15755	14114	9461	5868	2830
Mangolds	17858	16934	15184	9336	6952
Oats	49619	47220	46303	41456	33540
Clover and rotational grasses	57166	44305	40813	38770	28746
Total	140398	122573	111761	95430	72068

This very startling reduction in the acreage of fodder crops has been accompanied by a large reduction in the number of sheep and a marked decrease in the cattle population:

Live stock	1900	1910	1920	1930	1936
	number	number	number	number	number
Cows and heifers in milk and in calf	17835	17206	16610	16434	18704
Other cattle	39277	41873	30459	29235	26631
Sheep	208272	168778	73543	71926	62548
Pigs	46180	51959	52021	72653	118051
Poultry	?*	564794 †	508534 ‡	791637	926716
Horses for agriculture	21808	23609	19987	18552	15461

* Not known. † 1913. ‡ 1921.

On the other hand there have been increases in pigs and poultry, types of live stock for which special fodder crops are seldom grown, and which are primarily dependent on concentrated feeding stuffs. The crude figures in the annual 4th June statistics show, between 1900 and 1936, decreases of 12,000 in cattle, and 146,000 in sheep, and increases of 72,000[1] in pigs, and possibly 400,000 in poultry. Though it is very difficult to reduce the different categories of live stock to a common denominator, it seems probable, on balance, that the monetary value of the livestock output may even have increased during the period.

The explanation of this apparent anomaly is naturally complex. Undoubtedly, farmers have become increasingly dependent upon purchased feeding stuffs for their live stock, particularly after 1930. The expansion in livestock commitments took place chiefly in pigs and poultry—two categories primarily dependent on concentrated feeding stuffs. The decline in livestock numbers has been in sheep and beef cattle, which in arable districts commonly consume large quantities of home-grown bulky foods. The number of sheep has fallen by two-thirds, and "other cattle" by one-third, as compared with a decrease of 50 per cent in the acreage of fodder crops. Undoubtedly the development of sugar beet has contributed, particularly in the case of sheep, to the maintenance of the fodder supply, for the "tops" have replaced large acreages of "sheep keep" (e.g. turnips, kale, etc.) formerly grown to be close-folded. Further, the reduction in the number of working horses (from 23,600 in 1910 to 15,500 in 1936) must have liberated a considerable area, perhaps as much as 20,000 acres, formerly required for growing horse feed. This latter economy has, of course, been at least partly off-set by increased expenditure on machinery, oil, paraffin, etc.

Judged by money values, the crop output of the County in 1936 appears to have been substantially greater than at the beginning of the century, while the livestock output was at least not smaller. Further, the area under fruit, on holdings of one acre or more, increased from some 6000 acres in 1900 to 15,000 acres in 1936. This apparent expansion in total agricultural output was secured despite a decline in the number of workers employed, and a decrease in the number of horses used for agriculture. Statistics of employment are not available for years earlier than 1921, when the number of workers (including casuals) stood at 24,610. It seems probable that in 1900 the number was larger, but by 1930 it had fallen to 23,068; and in 1936 it stood at 21,644. Thus between 1921 and 1936 there was a decline of nearly 3000 workers (12 per cent). Output per worker

[1] With two gestation periods in the year, this figure should be approximately doubled to determine the rise in the annual pig output.

must therefore have increased very markedly during the period, partly as a result of increased mechanisation (particularly tractors), partly as a result of the alterations in the types of commodities produced, and partly also, owing to greater skill in the supervision of labour.

These changes have undoubtedly been most pronounced since the war, particularly after 1930, when legislation prevented agricultural wages from falling proportionately with the drop in commodity prices. Faced with the problem of wage rates fixed at roughly double their pre-war level, farmers were forced to devise means of increasing the output per worker.[1] Broadly speaking, the years 1920–36 probably constitute a period of unprecedented rate of change both in the internal and external organisation of farming in the County.

(B) GENERAL SURVEY

By J. A. McMillan, B.SC.

Organiser of Agricultural Education, Cambridgeshire County Council

Cambridgeshire and the Isle of Ely are now separate administrative units. When it will be convenient to refer specifically to one or the other, the Administrative County of Cambridge will be termed "the County", as opposed to "the Isle of Ely". Taken together, they have an area of 553,555 acres. Of this, in the year 1937, some 466,600 acres were "under crops and grass" and 12,671 acres were "rough grazings". There are $3\frac{1}{2}$ acres of arable land to every acre of grassland, a concentration only exceeded in England by the Holland Division of Lincolnshire, where the proportion is four to one. A markedly rural character is reflected also in the population figures. The total population in 1931 was 217,702, a density of 260 per square mile, which compares with a density of 690 per square mile for England and Wales as a whole. Of the total employed persons over fourteen years of age, 28 per cent were engaged in agricultural occupations, compared with 6 per cent for England and Wales.

MAIN CROPS

"Holdings of 1 acre and upwards" returned in 1937 numbered 7257; and three-quarters of these fall in the group class from one to fifty acres; while there are many holdings of less than one acre, which are not included

[1] R. McG. Carslaw, "The Changing Organisation of Arable Farms", *Econ. Jour.* xlvii, 483 (1937). On a group of 150 farms, the physical output per worker increased by some 27 per cent between 1931 and 1936.

in the official returns. Though in some districts large farms stretch as far as the eye can see, Cambridgeshire as a whole may be regarded as a county of small farms, small-holdings, and market or cottage gardens. Of the ordinary farm crops, excluding rotation and permanent grass, the most important on an acreage basis are wheat, barley, oats, potatoes, and sugar beet, which together covered 245,000 acres in 1937, more than two-thirds of the total arable acreage. Fig. 33 shows the acreage of each of these crops in 1913 and from 1919 to 1937.

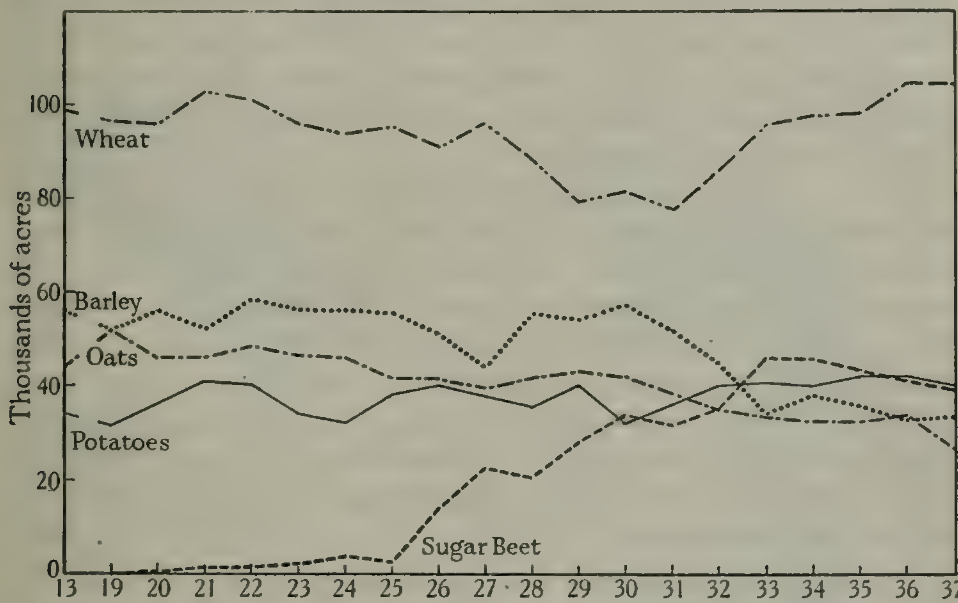


Fig. 33.

Acreages of Main Crops, 1913–37 (Ministry of Agriculture Statistics).

The *Wheat* acreage has been maintained fairly well in recent times, except during a period of low prices just before 1932. The effect of the Wheat Act of that year was to check the decline, and later to increase the acreage to a little above the pre-war level. The wide variations in soil type lead to an equally wide variation in the choice of seed. Some of the more common varieties are Little Joss, Squarehead's Master, Victor, Wilhelmina, Yeoman, and Rivett's. In the County, a distinctive feature is the large proportion of wheat which is grown after a one year's sainfoin or clover leyer. If the latter has not been heavily folded with sheep, it is customary to apply a dressing of farmyard manure prior to ploughing for the wheat crop. In the Fenland wheat usually follows a fallow crop and receives no special manuring in the autumn.

The *Barley* acreage has been declining gradually in recent years. In the County, the acreage has been fairly constant between 30,000 and 37,000 acres. In the Isle, however, where there are greater difficulties in growing a good malting sample, the area under barley is now only one-quarter of the pre-war figure. On the lighter and better barley soils it is indeed rare to find a field sown with any other variety but Spratt Archer. On the heavier soils and in the Fens, some choose Plumage Archer.

The *Oat* acreage is distributed fairly evenly between the County and the Isle. In the former, the greater part of the oats is autumn-sown, and Grey Winter is a popular choice. Though inclined to "lodge" at times, this variety proves a reliable cropper, and is liked by those who buy for the racing stables at Newmarket. Marvellous and Resistance, often sown in the very early spring, are also widely grown. Spring oats, when sown in March, crop reasonably well as a rule, and are grown on a limited area. Victory is the variety most in favour.

Potatoes. Rather more than nine-tenths of the 40,000 acres of potatoes are grown in the Isle (see Fig. 34), where this is one of the principal crops contributing to farm income. The tendency in recent years has been to concentrate potatoes on those soils proved to be best suited to the crop, and to manure more intensively than formerly. Now, too, only a few proved varieties are grown at all widely; recent reports of the Potato Marketing Board indicate that well over 30,000 acres are planted with Majestic and King Edward VII. The cultivation of early varieties is limited to some 3000 acres on the lighter and more silty soils, Eclipse being the most commonly grown.

The *Sugar-beet* acreage has increased very considerably since the thirty-nine acres that were grown in 1919. Some two-thirds of the total acreage is now grown on the richer fen soils (see Fig. 35), where yields considerably above the average for the country are obtained in most seasons. It is rather remarkable how this new crop has taken its place in the farm rotation without any considerable upheaval in farming practice. It has replaced fodder crops, rotation grasses, mustard for seed, oats, and barley, the latter particularly in the Isle. Its introduction has resulted in busier times in early summer and late autumn. On those farms where it is grown on any extensive scale, it is necessary to employ casual labour to assist the regular farm staff.

Leyers are to be found almost exclusively in the County, where they are a definite feature of the light and heavy land rotations. On chalk soils, a one year's ley of sainfoin or broad red clover is extremely common, the crops being either folded and seeded, hayed and seeded, or merely folded. Grass and clover mixtures are not now so popular, partly on account of the smaller demand and lower prices for this type of hay in recent years, and partly because the crops which follow appear to yield less well after mixtures. On both light and heavy soils, the leyers are ploughed up after one year, except on certain of the latter soils where there is a growing appreciation of the value of the system of alternate husbandry. The failure of the leyer on the lighter soils may well prejudice the yields of the other crops in the rotation and in consequence great importance is attached to its proper establishment and careful management.

Fig. 34.

Fig. 35.

Redrawn from M. Messer, *An Agricultural Atlas of England and Wales* (1932).

Mustard for seed is also an important crop. It may take the place of a cereal or fallow crop, and in a catchy season the land upon which it is to be sown may not be determined until late spring. The heavier yields are obtained on the fen soils, but excellent results are also recorded on the lighter and heavier soils, which carry some 50 per cent of the 8000 odd acres grown each year. A further considerable acreage is sown each year with mustard for folding or for ploughing in.

Beans, long regarded as a standard crop of a heavy land rotation, are not now entitled to a place of prominence. The 6000 acres grown in 1937 represented only one-quarter of the area under this crop in 1913. There is little doubt that the change has arisen from a desire to grow crops giving a higher gross return per acre than can normally be obtained from the bean crop.

Mangolds and the various types of Brassicae for sheep feed are grown to a less extent each year. The combined acreage under these crops in 1937 was only 12,000 acres, the lowest return of post-war years. Though the larger decline in 1937 may in part be due to adverse weather conditions, there is little doubt that the establishment of the sugar-beet crop and the reduction in the number of arable flocks of sheep and of winter-fed bullocks have played the major part in the gradual reduction of the acreage under these crops.

Market-Garden Crops occupy a relatively small acreage, but make no mean contribution to the gross income per acre in those areas specially selected for their cultivation. With the exception of crops such as asparagus, and of very limited areas (e.g. on the Gamlingay Greensand), market-garden crops are taken generally in the ordinary farm rotation. The two principal crops of this nature, celery and carrots, are grown chiefly in the Isle. Celery thrives well in the cool, deep and moist black fen soils and its cultivation now covers over 3000 acres—three times the area in 1913 and more than one-third of the total celery acreage in England and Wales as a whole. Carrots, chiefly of the stump-rooted type, are grown extensively in certain well-defined areas, e.g. around Chatteris. Other crops of some importance are peas and beans for pulling, grown chiefly in the Isle, Brussels sprouts (grown mainly on some of the stiffer soils in the County), cabbages, cauliflower, broccoli, and asparagus, though the latter has not been grown so widely of late years.

It will have become evident that the rotations to accommodate the large number of crops already mentioned must show some considerable variation. In general they vary from three to six courses. On the one hand, a three-course system of two fallow crops and a corn crop is common on the best fen soils. At the other extreme, there is the light land three- or six-course rotation of the mechanised farmer, who hopes to grow corn on two-thirds of his arable acreage each year. Then there is the standard four-course rotation of the heavy-land farmer, where 50 per cent of the land is cropped with corn; and the common rotation of the chalk farm, viz. fallow crop, corn, corn, seeds, corn, though in this case there are modifications in the arrangement of the crops and some still prefer the Norfolk four-course rotation.

Fruit growing and flower culture are concentrated in certain well-defined areas, which were mainly under grass, or part of an ordinary mixed farm rotation, until some sixty years ago.[1] With certain notable exceptions, fruit

[1] See pp. 131 and 153.

and flower growing are in the hands of small-holders, quite a number of whom cultivate less than one acre, and as the available statistics do not include these small units, it is difficult to arrive at a reasonable estimate of the total acreage and production of these crops. One interesting feature of these comparatively new developments is the growth in the number of carriers and commission agents, who collect, arrange transport for, and sometimes bulk, the marketable produce.

Strawberries are the most important soft fruit in the area. The greater proportion of the 4000 odd acres is grown in the Isle around Wisbech. In the County the bulk of this fruit comes from small growers in the Cottenham, Willingham, and Histon districts. Varieties in favour are Royal Sovereign, Sir Joseph Paxton, Oberschelien, and Brenda Gautrey. Gooseberries still take second place in point of acreage, even though there has been a considerable falling off in the past few years. Black and red currants and raspberries were also widely grown at one time, but their cultivation is now confined to a few of the smallest holdings.

A feature of the last decade has been the development of bulb growing in the Wisbech district and of the culture of flowers, e.g. pyrethrums, scabious and outdoor chrysanthemums, in the Cottenham, Willingham, and Fordham areas. Here and there, too, nurseries have been established for the raising of fruit trees and shrubs.

A number of growers has recently erected glasshouses for the production of tomatoes, bulbs, forced mint, and indoor chrysanthemums, and though production has not yet reached large figures, development in this line is taking place steadily year by year.

Recently the Land Settlement Association has acquired two estates in Cambridgeshire and these have been equipped for the production of certain fruits and vegetables both indoors and under glass.

Over 10,000 acres are planted with top fruit, plums predominating in the County and apples in the Isle. On the whole, the climate is not all that might be desired, especially for apples, because the high winds and late frosts occasionally cause serious reductions in the crop yields. River's Early, Czar, Victoria, and Monarch are the more common varieties of plums; and of apples, most of which at present are of the cooking varieties, Bramley Seedling is a favourite, though Emneth Early was more popular in the old days. There is now a slow but steady change from culinary to dessert apples. An important area of fruit (particularly greengages) is to be found in the parishes of Melbourn and Meldreth, and other scattered areas of top fruit occur on the chalk soils in the south of the County, and on the gravels, peats, and skirt-soils near Burwell, Exning, and Fordham, apart from the larger areas around Cottenham, Histon, Rampton, Willingham and Wisbech.

Possibly in no other direction has research yielded such striking results or suggested such revolutionary changes as in fruit production. There is a definite tendency to grub up old orchards, planted before this new knowledge was available, and to replant with newer varieties on improved stocks and under conditions more conducive to the control of the many pests which do so much to limit the production of high-grade fruit. Many of the older orchards were mixed plantations of plums and apples, an unsuitable combination under modern methods of management.

LIVE STOCK

The total number of *Horses* shows a steady decline from some 33,000 in 1913 to 20,000 in 1937 (Fig. 36), an experience which Cambridgeshire shares with the other arable counties. Though this decline has been more marked in the County than in the Isle, the replacement of the horse by the internal

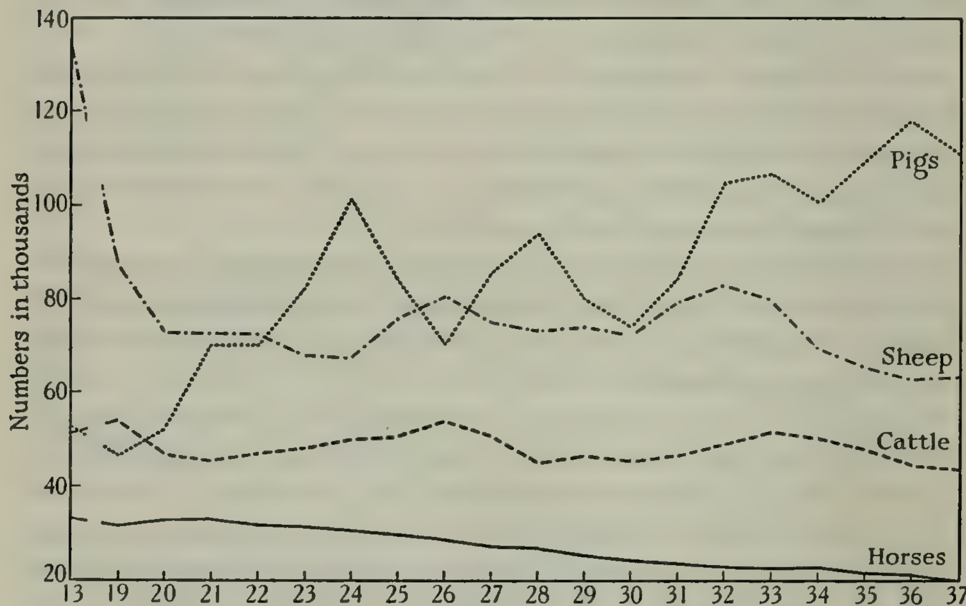


Fig. 36.

Main Live Stock, 1913–37 (Ministry of Agriculture Statistics).

combustion engine has been taking place quite as quickly in the latter part of Cambridgeshire. The horse still proves an essential supplement to the farm tractor, and now works even more constantly and effectively than formerly. In fact, the introduction of newer types of light-draught implements and of pneumatic tyres for farm carts and, in certain cases, the construction of concrete farm roads have had much the same effect as if there had been evolved by breeding and selection an animal of higher horse power.

No less apparent, than the decline in numbers, is the steady improvement in the type of horse to be found on the general farm. Pedigree breeders of Shires, Suffolks, and Percherons, the three most common breeds, find a good demand from local farmers as well as from those in other industries where a good heavy draught horse still proves invaluable. Quite a few, too, of even the smaller farmers, who have adequate facilities for rearing, breed one or more foals each year and assist in maintaining the high reputation gained during the last century by Cambridgeshire breeders of heavy horses.

The breeding and training of race horses is a feature on a considerable stretch of land around Newmarket both on the Cambridge and the Suffolk sides of the border. The greater part of this area, apart from Newmarket heath, is laid out in neat grass paddocks surrounded by shelter belts of trees, which give a distinctive appearance to a wide tract of land that otherwise would be featureless and rather bleak. Good paddock management, a matter requiring considerable skill and experience, is aided by the grazing of cattle in the summer and by the production of manure from yard-fed stock in the winter. Though localised, this industry is one of considerable importance to those who farm in the surrounding districts from the point of view of the demand for certain products of the farm and its requirements for labour.

The *Cattle* of Cambridgeshire are predominantly of the Shorthorn type, but dairy herds of Red Polls, British Friesians, and Jerseys, are to be found here and there, while Aberdeen Angus crosses occupy a number of the fattening yards and boxes. The total number of cattle has changed very little since 1913, though in recent years there has been some decline. Marked changes, however, have occurred in the cattle distribution and in the relative importance of the two main products—milk and beef. The number of cows in milk has increased slightly in the County and remained fairly constant in the Isle, but there is evidence that the quantity of milk coming on to the liquid market has increased very considerably, due mainly to three changes in practice, viz. the better management of the dairy herds; the almost complete suspension of farm butter-making; and a restriction of calf rearing and of the amount of milk fed to calves. To-day, Cambridgeshire dairy herds supply the local requirements for liquid milk, and they yield, in addition, an exportable surplus, much of which is consigned through local depots to London. These changes have naturally had their effect on the general farming system in certain areas, and there are many instances of the conversion of open yards, feeding-boxes and outhouses to cowsheds and milk and sterilising rooms.

Beef production in the same period has declined, partly through the utilisation of bullock-feeding yards for pork and bacon production and partly through the change over to milk production. There has been a steady decline in the number of older cattle fed for beef, especially in the Isle. The majority of the animals is now marketed at or under two and a half years of age and only a relatively small proportion at lighter weights or when fully mature.

The *Sheep* numbered, in 1913, some 108,000 in the County and 26,500 in the Isle. In 1919 the respective numbers were 74,500 and 14,000, and in 1937, 53,000 and 7500. These figures include a large number of lambs nearly fat and take no account of those stores which are purchased, fattened and sold during the winter season. Though there is little doubt that now more stores are purchased than formerly, the figures may be taken to indicate a general decline during the post-war period. This is an experience not uncommon to counties where formerly a high proportion of the sheep was maintained on arable land, and it represents a change in practice that may have some effect on the maintenance of fertility of the lighter soils.

On the chalk soils there are many parishes, which contained five or six large breeding flocks of folded sheep ten or twenty years ago, but which now can boast of only one or two flocks of much-reduced size. Important factors underlying this change undoubtedly have been the relatively high cost of labour and the desire, often the dictate of necessity, to grow a large acreage of direct cash crops. The introduction of sugar beet, a cash crop with a useful feeding residue in the form of tops, certainly has tended to check the decline in the arable sheep numbers during winter, for though ewe flocks may have been dispersed, it is not an uncommon practice to fold off the tops with store sheep.

The decline in the numbers of breeding sheep, however, cannot wholly be attributed to the reduction of the arable flocks. At least four factors have tended to a reduction of grass sheep, viz. (*a*) the more widely held opinion that, within certain limits, the lighter the sheep stock the better the health of the flock, (*b*) the increasing need for an adequate drainage of much of the heavier grassland, which must take precedence over its improvement by manuring and stocking, (*c*) the reduction in the number of grass orchards formerly grazed by sheep, (*d*) the replacement of sheep by pigs in many of the orchards which remain in grass.

Most of the arable flocks are of the Suffolk breed. In a number, kept pure, the breeding of rams for sale is an important item of the gross receipts from the flock; in others, the ewes are crossed with rams of another Down breed, usually the Hampshire. The Half Bred predominates on the grass farms. Generally the ewes of this breed are crossed with a Suffolk for the production of fat or store lamb. Scattered flocks of Hampshires are also maintained on arable land; South Downs run partly on grass and partly on arable land; while cross-bred ewes of various types are kept on a few of the grass farms.

The number of *Pigs* shows wide variations from year to year. Since 1932 the numbers have been considerably in excess of 100,000, which is double the figures of 1913 and 1919. No one factor is wholly responsible for this increase, but the greater part undoubtedly is due to the fact that it has been more profitable to stock the yards with pigs than with fattening cattle.

The stimulus to production given by the Pig Marketing Scheme has had effects on the changes in method of pig management. To-day, pigs are kept under very varied conditions, e.g. some are kept tethered out of doors all the year round, while others spend most of the year in a modern type of Danish piggery. Many barns, horse stables, and cattle yards have been converted for the use of pigs, and in all such alterations an important consideration has been to secure a layout which would allow of the maintenance of the largest number of stock per unit of labour.

Most of the store or fat stock coming on the market from Cambridgeshire farms are pure Large Whites or of the Large White-Large Black cross. There are numbers of pedigree breeders who favour these two breeds and who find a ready demand for the animals which they offer at their annual sales. There are also a few herds of the Essex and Middle White breeds.

Poultry Keeping has become an increasingly important branch of agriculture in Cambridgeshire as in other counties. It is mainly an activity of small-holders and general farmers, who derive only a part of their income from this source; but the number of specialist poultry farmers has increased considerably in recent years. Most of these keep flocks of from one to three or four thousand birds, and are interested both in egg production and table poultry. There are few larger units or special hatcheries.

CONCLUSION

The post-war years have seen more far-reaching changes in Cambridgeshire agriculture than in any other short period of its history.[1] It is too early, yet, to see clearly the effects of these changes on the fertility of the soil or upon the livestock industry. There is little doubt that the scarcity and high cost of labour have led farmers to think more and more on the lines of mechanisation and to consider restricting the production of certain commodities which make heavy demands on labour. A short time ago it was not uncommon to find a number of farmers who were inclined to the

[1] See p. 135 above.

belief that soil fertility could be maintained, in successful arable farming, without the use of animal manure. To-day, this view receives little support. Though the labour problem is still as acute as ever, the tendency definitely is towards systems of balanced farming. Although some farmers may continue to deprecate the disappearance of the arable sheep flock and of the yard filled with fattening bullocks, it should be remembered that the larger numbers of folded store sheep and fattening pigs are proving a compensating factor in the maintenance of soil fertility.

Cambridgeshire as a whole may be regarded as a fertile county, but it includes considerable stretches yielding only a strictly limited amount of produce, chiefly because of badly drained soils and the lack of hard-bottomed roads. Field drainage, indeed, may be classed as one of the major problems of local agriculture. Blockages in small streams, overgrown ditches, bad outfalls (and, in consequence, blocked field drains) take a heavy toll on the crops over large tracts of land, particularly on the heavier soils. Most of the land in this condition has already been tile-drained within the last seventy or eighty years. A number of these systems may not be efficient, but there are many instances where the old drains function really well when given the opportunity. It is unfortunate that the greatest need for attention arises in those very districts where, through a variety of circumstances, the landlord or the tenant is not in a position to undertake the necessary work. The use of the mole drainer is helping to some extent, but this is not solving the sometimes greater problem of removing the water from the drained area.

There are also wide stretches of land (including some of the most fertile), and many farms, set two or three miles from a hard road and served only by a muddy track. Farmers so situated find it necessary to limit their production very largely to those commodities that can be carted off the farm during the drier months of the year. It is not unreasonable to suggest that over large areas the value of the crops might be increased by quite 50 per cent if field drainage were adequate and if roads permitted easy access all the year round.

(C) REGIONAL TYPES OF FARMING

By R. McG. Carslaw, M.A., PH.D.

Within the County boundaries there are marked contrasts both in the organisation and in the productivity of the farming. These contrasts are due primarily to differences in the nature of the surface soils, of which there is a remarkable variety.[1] Four major districts, comprising three-quarters of the land area of the County are to be discerned: (1) the chalks in the south and south-east, (2) the clays in the south-west, (3) and (4) the peats and silts in the middle and north (see Fig. 29). A brief comparative description of farm organisation in these districts, based on surveys during the 1931–36 period, will provide some indication of the principal types of farming in the County at this time.

(1) *The Chalk Soils.* In this area, farms and fields are large, many of the latter extending to more than 100 acres. The working capital here required[2] for stock, crops, and equipment (excluding value of land and buildings) approximates £12 per acre; gross income amounts to £10–£11 per acre; and employment is at the rate of three workers per 100 acres. Of the gross income approximately half is derived from crops. Rents average roughly 21*s.* per acre. Little more than 10 per cent of the farmed land is under permanent grass, and a common rotation for the arable land is (1) sugar beet, (2) barley, (3) barley, (4) seeds, (5) wheat. Barley is the principal cash crop, and excellent malting qualities are grown. Wheat and sugar beet are both important sources of income, while clovers and sainfoin are the principal short ley crops.

Sheep are the type of live stock traditionally associated with this district, and, formerly, large flocks of the heavier breeds (e.g. Suffolk) were kept for manuring and consolidating the arable fields. But the high labour costs entailed by close-folding, the decline in sheep prices, and the relatively more favourable returns offered by growing sugar beet in place of "sheep keep", have contributed towards reducing this practice. Indeed, on some farms sheep as an aid to soil fertility have been entirely superseded by artificial fertilisers and green manuring with rape or mustard. A relatively large number of pigs is kept to consume tail barley, and to convert straw into dung. Some cattle are yard fed during the winter months, going out fresh or fat in the spring, but in recent years low prices have kept many "yards" empty. Dairying is confined almost entirely to farms situated near villages, where an opportunity for retailing occurs.

Mechanisation in crop cultivation has here proceeded comparatively

[1] See Chapter ii.

[2] Cambridge University Farm Economics Branch, *Report* 24 (1937).

rapidly in recent years, partly, no doubt, owing to the presence of large fields, and partly also because of the extensive areas of cereals (three-fifths of the arable area) which are grown. Tractors and modern large-scale tractor equipment are now comparatively common.

(2) *The Heavy Clay Soils.* This is the least productive and most depressed agricultural district in the County. The soil is heavy and intractable; much land is in need of re-drainage; road facilities are in many places inadequate; derelict and semi-derelict fields are not uncommon.[1] In one parish, in 1932, it was found that out of nine holdings over 20 acres in size, five were uncultivated, one was vacated during the year as a result of bankruptcy, and two changed hands within the year owing to the financial difficulties of the occupiers. The Wheat Subsidy Act of 1932 gave a new lease of life to farmers in the district.

A survey carried out during 1933 showed that roughly 60 per cent of the farmed land is arable.[2] Working capital in stock, crops, and equipment averages some £7 to £9 per acre, gross incomes £5 to £7, rents 15*s*., and employment little more than two workers per 100 acres. The soil is so heavy that opportunities for diversified cropping are limited, and high-value crops such as sugar beet and potatoes can seldom be grown. The barley produced is generally of poor quality. There is a comparatively high proportion of bare fallow, and a considerable amount of cross-cropping. A not uncommon rotation is (1) wheat, (2) seeds with a bastard fallow, (3) wheat, (4) bare fallow. Beans are the chief fallow crop, and a limited area of field peas is also grown. Red clover is the main "short leyer" crop, and second cuts are frequently taken for seed. Trefoil and sainfoin are also found amongst the leyers. Wheat is the principal cash crop, but where soil conditions permit small areas of fruit, potatoes, and market-garden crops are grown.

Fewer live stock, particularly smaller numbers of pigs and sheep, are produced here than on the chalk soils. The grazing season is relatively short, perhaps owing to poor drainage and management, and the land "poaches" badly in winter. In recent years, some movement has been made towards developing a system of cropping involving long leyers of 3–5 years in place of the usual 1-year leyer. This lengthening of the rotation into, say, 4 years plough followed by 4 years grass, appears to hold opportunities for improving the organisation of farms in the district. But there is an acute shortage of working capital amongst the farmers, and improvements or adjustments necessitating capital outlay, such as fencing and provision of water, can only be slowly adopted.

[1] See pp. 56 and 131 above.
[2] Cambridge University Farm Economics Branch, *Report* 22 (1935).

(3) *The Black Peat Soils.* The farms here tend to be rather smaller in acreage than those on the neighbouring uplands, but the organisation of production is much more intensive. Capitalisation, employment, and output per 100 acres are high. A survey[1] made in 1936 showed that an ingoing tenant requires some £25 working capital per acre of farmed land (arable plus pasture), that employment is at the rate of about five workers per 100 acres, and that gross sales amount to £20 per acre per annum. Rents, including the tenant's share of drainage rates, average 50*s.* per acre, while the labour bill approximates 90*s.* per acre. These various measures are roughly double the comparable data for upland farms in the south of the County. Road facilities are in many cases poor, surfaces being unmetalled and virtually impassable in winter, but the numerous waterways provide an alternative means of transport.

The usual rotation is (1) wheat, (2) potatoes, (3) sugar beet. A few acres of oats for horse feed are grown, while other crops commonly found are celery, carrots, and mustard seed. There is very little temporary grass, and permanent pasture consists mainly of off-lying "wash" grazings, frequently flooded during winter. The outstanding cropping characteristics are the large proportion of the farmed area which is cultivated, the large proportion of the cultivated area which is devoted to cash crops, and the concentration on crops giving a high money output per acre, e.g. potatoes (see Fig. 34), sugar beet (see Fig. 35), and celery. Crop yields per acre are high, probably averaging one-third above that on upland farms; yields of 15 tons of sugar beet, 12 tons of potatoes, and 50 bushels of wheat, per acre are not uncommon. These good crop yields are no doubt mainly due to the inherent fertility of the soil, but liberal applications of artificial fertilisers and good management contribute to the result. Most of the dung and fertilisers is applied to the sugar beet, potatoes, and celery. Potatoes, for example, are commonly dressed with 15–20 loads per acre of farmyard manure, plus 6–10 cwt. of fertilisers, while sugar beet may get 4–6 cwt. of fertilisers.

Of the gross sales from the black peat farms surveyed in 1936, crops accounted for three-quarters and live stock for one-quarter. Both the absolute and relative importance of crops on the peats is thus very much greater than on the chalks and clays in the south of the County. Sugar beet and potatoes are the two major items of revenue, and together they amount to more than half the total receipts, with wheat coming third in importance. In cash values, pigs are the most important livestock enterprise, with cattle second.

[1] R. McG. Carslaw, "Farm Organisation on the Black Fens of the Isle of Ely", *Jour. Roy. Agric. Soc.* xcviii, 35 (1937).

A common practice in the cattle management is to buy stores in the early spring, yard feed them for a few weeks before sending them to summer grass on a "wash" pasture, and to bring them back to the yards in October for fattening off during the winter. Where no cattle are fattened pigs are usually kept to tread down the straw. Sheep are conspicuous by their absence, and very little milk or poultry production is undertaken. Although horse-breeding is associated with this district, sales of horses amounted to only a little over 1 per cent of gross incomes on the farms covered by the survey.

Considering the types of crops grown, the farmers are remarkably independent of imported casual labour, as the wives and families of the regular employees commonly assist with seasonal operations. Beet thinning, potato and celery planting, and the beet and potato harvests are generally let out at piece rates, and individual families of workers may earn substantial sums at certain times.

Fenland farming depends, of course, on a complex system of artificial drainage. On many farms, however, the drainage appears to be satisfactory, the most usual complaint being that the water level is kept too low during the summer months. Surface water-logging seems to be a more serious difficulty to the farmers than any defect in the main drainage system. Particularly on land where, owing to "wastage" of the peat,[1] the underlying clay is now close to the surface, pools of water form after heavy rain. The crop will quickly deteriorate in these patches unless the surface water is removed, and this is generally done by ploughing or hand-digging water furrows to the nearest ditch.

The practice of "claying" the peat soils[2] is less frequently practised than in the past owing to the high labour costs involved. In some cases wastage, however, has proceeded so far that the clay is now being ploughed up and mixed with the peat during the ordinary field cultivations.

(4) *The Silt Soils*. These extraordinarily fertile alluvial deposits vary from a light to a heavy consistency according to the percentage of clay. The economic organisation of farms in the district is in many ways similar to that on the peats, but production is even more intensive; and capitalisation, output, and employment are generally higher. Rentals range from £3 to £5 per acre. Compared with the black peats, less sugar beet is grown, and potatoes (chiefly Majestics and King Edward's) are a relatively more important crop; further, the quality of the potatoes, and therefore

[1] See p. 186 below.

[2] See p. 120 above. It is generally carried out by digging narrow ditches across the field down to the underlying clay, throwing the clay out and spreading it evenly over the field, and then filling up the excavations to make all reasonably level. In 1936 the operation cost from £10 to £15 per acre.

the price per ton received by growers, is better. The rotation approximates to (1) potatoes, (2) sugar beet, mustard for seed, and various root seeds, (3) wheat, but is widened by inserting oats, peas, or clover where desirable. Considerable areas of root seed crops (turnips, swedes, mangolds and sugar beet) are also grown. Fruit is important, particularly in the vicinity of Wisbech.[1] In 1936, over 3000 acres of strawberries, 3000 acres of apples, 1400 acres of gooseberries, and 1400 acres of plums were grown in the Isle of Ely, principally on these silt soils. Although some 500 acres of bulbs were grown in 1936 this industry, together with market-garden and glasshouse production, is less fully developed than in the Holland Division of Lincolnshire which lies immediately to the north of Cambridgeshire. Indeed, the silts which lie within Cambridgeshire are only a small part of the large compact silt area surrounding the Wash, and which includes the whole of the Holland Division.

(5) *Other Districts.* The four main areas already described cover approximately three-quarters of Cambridgeshire. The remainder of the County includes a number of small areas of varying soil types.[2] In particular, no account of the agricultural regions of the County would be complete without reference to the fruit-growing area immediately to the north of the town of Cambridge. This includes the parishes of Milton, Waterbeach, Landbeach, Impington, Histon, Cottenham, Rampton, Long Stanton, Willingham and Over.[3] Since the middle of the nineteenth century, a strong concentration of fruit growing (especially plums, apples and strawberries) has been developed here.[4] In more recent years, fruit has been supplemented by the introduction (often by underplanting the top fruit) of market-garden produce (asparagus, cauliflowers, broccoli, dwarf beans, and peas), and of cutting flowers (pyrethrums, scabious, iris, gladioli, asters, marguerites, gypsophila, etc.). Small-holdings of 20 acres or less, producing these intensive crops, are numerous in the district, while there is a large number of "part-time" holdings, of an acre or so, in the occupation of agricultural labourers and other wage-earners. Poultry and pigs are the most usual types of live stock, and are kept largely to produce manure and to utilise by-products.

[1] See C. Wright and J. F. Ward, *A Survey of the Soils and Fruit of the Wisbech Area* (1929). See p. 143 above.
[2] See Chapter ii, and Fig. 29.
[3] See J. F. Ward, *West Cambridgeshire Fruit-Growing Area* (1933). See p. 143 above.
[4] See p. 131 above.

CHAPTER ELEVEN

THE INDUSTRIES OF CAMBRIDGESHIRE

By F. M. Page, M.A., PH.D.[1]

DANIEL DEFOE, WRITING IN 1724–26, SUMMED UP HIS impression of Cambridgeshire by saying that "this county has no manufacture at all".[2] Although this is less true now than then, it is still not surprising that industry should occupy a subsidiary place in one of the most agricultural of counties. Chronologically, the industries of the County fall into three groups. In the first place, there are the extinct industries: thus during the seventeenth and eighteenth centuries the production of saffron was flourishing in the south-east of the County, there were two saltpetre factories at Cambridge and Barnwell, and bell-founding was carried on in Cambridge. During the nineteenth century came the digging of coprolites.[3] At this time, too, there was the activity of the various industrial schools; spinning establishments existed at Fowlmere, Soham, and Histon, and in this last village, stockings were also made; while at Wisbech and Linton hemp was made into rope. Perhaps the most interesting of these extinct industries was the manufacture of woad at Parson's Drove, some 6 miles from Wisbech. Working ceased in 1914, but the mill still survives. This mill together with two others near Boston are the last representatives in Europe of an industry that dates from pre-Christian times.

Secondly, come the industries with a long continuous history through the Middle Ages to the present day. The best examples, perhaps, are basket-making, printing and book-binding, quarrying for stone and clay. Finally, there are the industries of recent growth, conspicuous elements in shaping the modern economy of the County. In this class come the manufacture of sugar, the canning and preserving of fruit and vegetables, and the construction of scientific instruments and apparatus.

The account that follows does not profess to cover every commercial undertaking in the County. It can deal only with the most important and the most characteristic.

[1] For help in the preparation of this chapter, I am indebted to Mr F. J. Corbett, the Secretary of the Cambridge Chamber of Commerce, to Mr R. S. Whipple, past President of the Chamber, to Mr John Saltmarsh of King's College, and to the numerous firms who have given me information. I am indebted to the Editor (Mr L. F. Salzman) for allowing me to use material prepared for the *Victoria County History of Cambridgeshire*.

[2] D. Defoe, *Tour through England and Wales* (1724–26), Letter I.

[3] See p. 126 above.

Agricultural Industries. The basket-making and wicker industry of the County is very long-standing. One of the first things that struck Camden, in 1587, was the "willows in great abundance, either growing wild or set on the banks of rivers to prevent overflowing. It is of these that baskets are made."[1] This fen occupation survived the draining, and, to-day, the chief centres are Ely, Soham, Chatteris, Over, and Somersham (Hunts). Here are made wicker-chairs, bottle-containers, and every form of wicker-work. A different kind of basket is made at the Wisbech saw-mills. A good local market was provided by the surrounding fruit district. The Wisbech and District Fruit Growers Association bought up all the baskets, before the output increased sufficiently to supply a national market. The most important firm was Messrs Dewsbury Bros. Now, the British Basket and Besto Co. Ltd. carries on the industry, and the timber-working firms themselves have established departments for it.

Timber-working at Wisbech was established during the nineteenth century. The first cargo of foreign timber arrived at the port in 1824, brought by an English barque. The next hundred years saw considerable expansion. The leading firm in the development is Messrs English Bros, Ltd., and its raw material is mainly Norwegian. There are branches at Sutton Bridge, Boston, and Peterborough, as well as at Cambridge itself; and the firm has been a pioneer in the use of creosote oil for making wood weatherproof. The oil is forced into the timber by steam pressure in air-tight cylinders, and, impregnated in this way, it withstands the effects of damp without being coated either with tar or paint. Telegraph poles, gateposts, and railway sleepers, submitted to this treatment, have remained unrotted after fifty years' exposure.

Characteristic occupations that have lingered into recent times are the preparation of reed and sedge for thatching, the digging of turf for fuel, the making of hurdles from willows; Reach and Burwell were the main centres near Cambridge; but now these activities have almost disappeared. Another product of the soil provided material for the straw-plaiting industry. This once flourished in the south-west of the County at Little Gransden and Littlington, but now the only firms are at Cambridge, Ely, and Little Shelford. A more famous industry born of the soil has been brewing. Cambridgeshire barley was at hand, and it seems as if malt, beer, and ale were among the commodities for which the County was most famous throughout the middle ages into modern times.

Sugar manufacture is a more recent, but a more important industry. The encouragement of the sugar-beet industry by government aid in post-war years had important consequences for the County. The acreage under

[1] W. Camden, *Britannia* (1637 edition), p. 491.

sugar beet rapidly grew,[1] and, now, the Isle of Ely and the County of Cambridge together account for 66 per cent of the total acreage under beet in Britain. In 1924, a factory at Ely was built in the centre of this agricultural activity. In the beet "campaign" of 1933, Ely out of eighteen competitors came second (with Peterborough) in "rated beet capacity",[2] its figure being 240,000 tons; the factory came third in "through-put of beet", producing 272,264 tons.

In addition to the production of sugar, there are useful by-product industries. The beet tops are used for manure or for cattle feed; molasses produced during refining are sold for distillation or for fodder; beet pulp (fibre after extraction of the juice) also forms good cattle food, equivalent to eight times its weight in mangolds; finally, the lime sludge is used for manures.

Preserving and Canning. Fruit growing had long been famous in Cambridgeshire, but the danger of over-production was great. Without some method of preserving on a large scale, fruit that could not find a local market had to be left to rot as it stood. Mr Stephen Chivers and his sons, about the year 1873, decided to experiment with the surplus fruit of the small farm at Histon that had been held by their family since the beginning of the century. The first boiling took place in a barn that can still be seen. A Cambridge grocer, greatly daring, volunteered to dispose of the jam, and was apparently much surprised to find that it sold. Accordingly, in 1875, a small factory was built conveniently near the railway, in case the venture might justify distribution to wider markets. Improvements in equipment were steadily made. The Galloway boiler was introduced about 1885; and the introduction of electric light enabled fruit to be made into jam as soon as it was picked. At this time 150 workmen were employed. To-day, the Orchard Factory has between 2000 and 3000 employees; its estates cover 8000 acres; its market is world-wide. It is estimated that 100 tons of jam can be produced daily. To this initial manufacture, other commodities have been added—jelly tablets, custard and blanc-mange powders, mincemeat, and marmalade. Thus an even pressure of employment is kept up throughout the year, in and out of the English fruit season.

Messrs Chivers & Sons were also among the pioneers of the canning industry in this country. The first bottle of preserved fruit was produced in 1890, and the first "tin" of fruit in 1893. By 1931, a new factory was opened at Huntingdon to take over the canning of vegetables, the fruit being still treated at Histon. It is interesting to note that all containers, jam-pot covers, and boxes, etc. are made on the spot.

[1] See Fig. 33.
[2] *Report on the U.K. Sugar Industry* (Blue Book, 1935), Table xvi, p. 30.

Recently, a branch of Messrs S. W. Smedley & Co. has been established at Wisbech, the northern fruit and vegetable centre of the County. The main attraction was the strawberries of the Wisbech district, and the plums and greengages growing around Ely.

Agricultural Implements. In this agricultural setting, it is not unnatural to find a number of firms manufacturing agricultural implements. As early as 1884, the Falcon Works (John Baker Ltd.) at Wisbech were notable for the invention and manufacture of corn- and seed-dressing machines, at a time when mechanism was only slowly being introduced into agriculture. Firms like Messrs Kidd of Willingham and Messrs Lack & Sons Ltd. of Cottenham have also a long history as agricultural engineers; while the chaff cutters of John Maynard of Whittlesford have reached many parts of the world. Prominent, also, are Messrs Edwards & Sons of Wisbech and Messrs Macintosh & Sons Ltd. at Cambridge. The only iron foundry now in the district is that of John Hart at Cottenham.

Quarrying. Three geological formations are of importance in Cambridgeshire industry. The Chalk contributes flints, chalk for limeburning and for cement, and the soft building stone known as clunch. Surviving bursar's accounts show that when the Cambridge colleges were being built, considerable amounts of stone were obtained from Haslingfield, Barrington, Cherry Hinton, Reach, and Burwell. The stone for the Great Gate at Trinity came from Burwell and Cherry Hinton; that of the Gate of Honour at Caius came from Reach. Clunch was also much used in interior decoration; examples may be seen in the fan-tracery of the Lady Chapel at Ely Cathedral, or in St John's College Chapel. Quarries are still to be found along the line of this outcrop (Burwell Rock, or Totternhoe Stone) in the Lower Chalk. Many of the old quarries at Isleham and elsewhere are now disused, but clunch is still dug for road-making.

The Lower Greensand furnishes an easily dressed stone, known as Carstone, which has been used for houses and churches, but there is no great quantity, and it has seldom been carried for long distances.

Brick and Cement Works. The clays of Cambridgeshire have given rise to pottery and brickworks. The potteries have disappeared but the brickworks are very active. The bricks are of two kinds: the Gault produces a yellowish grey brick, very common in Cambridge itself; while the Jurassic Clays yield red bricks. At Cambridge, Ely, and Whittlesea, there are a number of well-established brickworks. Clay mixed with chalk also provides material for cement works at Shepreth, Meldreth, and Barrington, and for the British Portland Cement works at Coldham's Lane, Cambridge, with a weekly output of about 2000 tons. Then, in addition, there are several concrete manufacturers such as the Cambridge Concrete Co. of

Milton, with its own pit of gravel and sand, specialising in roofing tiles, blocks and bricks; the Cambridge Artificial Stone Co., dealing mainly with architectural specialities; the Atlas Stone Co., producing paving slabs and kerbing; and Messrs Tidnams Ltd. of Wisbech, concerned with a variety of concrete products.

Printing. Amidst much that is obscure and controversial, two facts stand out clearly in the early history of Cambridge printing: first, that John Siberch, a friend of Erasmus, started printing in 1521; and, secondly, that the University received clear authority to "print all manner of books" under the charter granted by Henry VIII in 1534. During the sixteenth and seventeenth centuries the primary policy of the University was to protect their printers' privileges rather than to develop the business of book distribution, and it was the common practice for Cambridge books to be sold through London booksellers. At the end of the seventeenth century, the University Press was organised as a University department. Large-scale reorganisation was undertaken by Richard Bentley, who secured the appointment of the first Press Syndicate; from 1698 to the present day, the Press has been governed by a body of resident graduates known as the Syndics of the Press. During the eighteenth century, the Syndics felt their way towards publishing as well as printing. Their chief stock-in-trade at this time consisted of Bibles and Prayer Books,[1] but some notable books, such as Newton's *Principia* and Browne's *Christian Morals*, were also published in the early part of the century. Stereotyping was introduced about 1734 and an improved method early in the next century. The earliest printers carried on their work in various parts of the town, and the first University printing house was on the site of the present lodge of St Catharine's College. In 1804 a new building was erected on the south side of Silver Street, and in the course of time the Press has gradually absorbed the whole of the site between Silver Street and Mill Lane, the most prominent feature being the Pitt Press, erected in memory of Pitt in 1833, and recently reconstructed. A publishing department was inaugurated in London in 1873, and the University Press now employs about 320 men in the printing house at Cambridge and about 120 in Bentley House, the headquarters of its London publishing. Its catalogue contains the titles of about 5000 books and journals which are distributed to booksellers throughout the world from Bentley House. All these are issued with the *imprimatur* of the Syndics of the Press.

[1] In common with the King's Printers and the Oxford University Press, the Syndics retain the privilege of printing the Authorised Version and the Book of Common Prayer—a privilege exercised by virtue of the charter granted by Henry VIII in 1534, and confirmed by Charles I in 1628.

Apart from the history of the University Press, there is very little authentic record of printing in Cambridge[1] until near the middle of the eighteenth century when a weekly newspaper, *The Cambridge Journal & Weekly Flying Post*, was published in September 1744. Eighteen years later, in 1762, *The Cambridge Chronicle* was first issued, and about four years afterwards *The Journal* was incorporated. This paper had no rival until 1839, when *The Cambridge Advertiser* (which subsequently became *The Independent Press*) first saw the light. These local newspapers were mainly responsible for general commercial printing, although in the early years of the reign of Queen Victoria, one or two small printers established themselves in the town, but their activities never assumed large proportions. *The Cambridge Express* also came into being; but with the advent of *The Cambridge Weekly News* in 1887, the three other local newspapers were absorbed, *The Chronicle* being the last to be incorporated a few years ago. The printing department of the latter now survives as "St Tibb's Press". A few of the old private firms remain without having shown much expansion, with the exception of Messrs W. Heffer & Sons Ltd., who started by taking over the small jobbing section of *The Independent Press*, and who now have one of the most up-to-date works in the Eastern Counties.

Instrument-Making. When Sir Michael Foster was appointed to the University Chair of Physiology in 1883, he found a startling lack of medical equipment of British and modern design; most instruments needed to keep pace with medical discovery had to be imported from German firms. Consequently he started to design and manufacture instruments on a small scale with the aid of two former pupils, Dr Dew Smith and Mr Francis Balfour. Soon, the co-operation of Sir Horace Darwin was obtained, and this was the beginning of the Cambridge Instrument Company Ltd. It was not until 1895, however, after the retirement of the senior partner, that the business registered itself as a company under the chairmanship of Darwin. Among the important inventions of those early days were the bifilar pendulum form of seismograph, and the rocking microtome for the rapid preparation of specimens for the microscope; then again there was the thread-recorder for marking the path of a moving pointer.

After the changes in reorganisation, the business was removed from St Tibb's Row to Chesterton Road, where it has remained, adding block to block, until the present day. Darwin took a leading part in the study of aviation. In 1912, he was appointed a member of the Advisory

[1] There are of course printers at work outside the town of Cambridge itself. Thus "the earliest newspaper bearing a Wisbech title—the *Lynn and Wisbech Packet*—came into existence on January 7th, 1800". For the subsequent newspaper history of the town, see F. J. Gardiner, *History of Wisbech and Neighbourhood* (1898), pp. 65–74.

Committee on Aeronautics, and the Company began to produce height-finders and instruments to locate the presence of aircraft. When war broke out, experimental effort was redoubled. A special thermometer was produced for testing the temperature of water in an aeroplane radiator—a very important invention when flying at great heights became normal. Other aircraft instruments were also produced.

Another Cambridge firm specialising in the manufacture of scientific apparatus was the "Granta" Works founded by W. G. Pye about 1897. In addition to supplying equipment to laboratories for teaching purposes, many pieces of apparatus were manufactured for some specific experiment, and the demand grew. Graduates from Cambridge, in equipping laboratories elsewhere, looked for apparatus similar to that which they had used in their training. Particular attention had been paid to electrical instruments, and in order to provide work for men returning from active service in 1919 and 1920, apparatus was developed for teaching the principles of wireless telegraphy. Broadcasting commenced, and, soon, the teaching panels were in great demand for listening-in. With their circuit lines engraved in white on ebonite panels, they took principal place in many drawing rooms. Later developments, and especially the advent of the portable receiver, resulted in very great extension of wireless production, and in 1929 it was decided to separate the two activities. The Radio department was disposed of to the Pye Radio Co. Ltd. The original business of scientific instrument-making was carried on in modern premises in Newmarket Road; later, an Aeronautical Instruments Section was added, retaining the style as W. G. Pye & Co. Ltd.

A more recent instrument-making firm is Messrs Unicam Ltd. This was started in 1933 at St Andrews Hill, but has recently moved to enlarged premises at Arbury Road. Finally, Clifton Instruments Ltd., founded at Bristol in 1929, was transferred to Cambridge in 1938. This is concerned with physiological instruments.

Paper-Making, etc. The Sawston Paper Mill is one of the oldest paper mills in the country, and the only one now in existence in East Anglia. The mill is known to have been making paper in 1664, and possibly the manufacture had been carried on from a much earlier date.

The name of Fourdrinier is found in association with the Mill as early as 1780, and it is certain that one of the earliest paper-making machines in the country was installed at Sawston. In 1836 the Mill passed into the Towgood family, who made paper continuously from that date to 1917, and who built up a reputation for a very high grade of paper. In 1917, the Mill was incorporated as a Limited Company, under the name of Edward Towgood & Sons Ltd., and passed into the possession of the well-known London Stationers, Spicer Bros. Ltd., now Spicers Ltd. Spicers Ltd. have developed on this estate a flourishing group of factories, where Woodpulp Containers, Envelopes, Waxed Wrappings, D'oyleys, Account Books, and other products of the stationers' craft are made.

Forming part of the factory extension at the Sawston Mills is the activity of Dufay-Chromex Ltd. Over the last ten or twelve years, experimental work in connection with the manufacture of non-inflammable colour film, under the Dufaycolor process, has been developed, and the manufacture of this well-known colour film is proceeding.

There are also other activities at Sawston. The manufacture of chamois ("shammy") leather has been carried on for over a hundred years.[1] Like that of paper, it was no doubt started here because of the good supply of water and the easy means of transport. The refuse from the skins goes to make soap, glue, dubbin, or manure for fruit trees. Glove-making is also carried on, making Sawston a unique example of an industrial village in Cambridgeshire.

Miscellaneous Industries have sprung up spasmodically in Cambridgeshire with no particular reason for their location. Wisbech provides example of an extensive tent-manufacture, which was of great importance in providing equipment for the Boer War, as well as for flower-shows, fairs, garden-parties, and camps. In this town there is also a label factory (Messrs Burall) which was one of the first firms to produce the clip-on type of label as opposed to the more usual eylet-and-string model.

At Littleport, Messrs Hope Bros. have recently set up a factory for shirt-making giving employment to over 300 people; at Whittlesford there is vinegar-brewing; while at Whittlesford and Pampisford there are artificial manure works.

In Cambridge itself there are some famous firms manufacturing brushes —the Cambridge Brush Company, the Kleen-e-ze Company, and the Premier Company. The Cambridge Tapestry Company is important for the special study that has been made of the repair of ancient fabrics and upholstery, by means of which many medieval treasures have been saved for posterity. Finally there must be mentioned the firms of box-manufacturers, turners, and furnishers, the Cambridge Metal Stamping Company, and the Cambridge Gas Company. But this does not exhaust the list.

[1] For a description of the process, see T. M. Hughes and C. Hughes, *Cambridgeshire* (1909), p. 103.

CHAPTER TWELVE

THE GROWTH OF CAMBRIDGE

By J. B. Mitchell, M.A.

TWO ESSENTIAL ELEMENTS NEED STRESS IN THE SITUATION of Cambridge: its position on the Cam at the junction of Fenland and Upland and its relation to the open chalk country and gravel terraces controlling the land routes. The Cam, navigable from Lynn to Cambridge, was a main artery of communication through the Fenland: sea-going vessels were still discharging their goods at Cambridge quays and hithes in 1295, while river traffic remained of great importance until the competition of the railways ruined the watermen in the nineteenth century. Cambridge also owes much to its land routes. A slight transverse fold in the south-easterly dipping chalk throws a finger from the chalk escarpment north-west across the valley of the Cam,[1] the severed tip of this finger forming the chalk outlier of Castle Hill. This ridge capped and extended by gravel-spreads provided a ford across the river, and constituted a south-east—north-west land route from East Anglia to the Midlands crossing the north-east—south-west river route at Cambridge. The main Roman road of the area, the *Via Devana*, exactly followed this ridge: the modern roads south of the river approach along the gravel terraces of the valley but continue north-west along the ridge to-day (Fig. 37).[2]

The site itself, where chalk and gravel approach the river, afforded the essentials of solid banks for bridging and dry ground for building, and was partially protected by the sweep of the river and its marshes. The distribution of the gravels, which largely determine the minor elevations (compare Figs. 37 and 38),[3] assume a great importance on a site as low and liable to flood as Cambridge. The gravels within the meander consist essentially of the Higher Terrace gravels of Trumpington to the west and of Barnwell to the east rising to 50 ft., separated by a spread of Lower and Intermediate Terrace gravels lying approximately at 30 ft. O.D. Along the centre of the

[1] I.e. the Gogmagog Hills which are shown up clearly in Fig. 8.

[2] On Fig. 37, the 100 ft., 50 ft. and 20 ft. contours have been traced from the Ordnance Survey 6 in. sheets: form lines, at 10 ft. intervals, have been interpolated from the O.S. 25 in. plans (1925 edition). The unequal intervals of the layer colouring have been deliberately chosen for comparison with the built-up areas.

[3] Fig. 38 is based on the 1 in. Geological Survey sheets (Drift edition) of the area. For access to the 6 in. map of the southern part of the area, and to a map of the Geology of Cambridge, by A. J. Jukes-Brown, I am indebted to the Director of the Geological Survey.

Fig. 37.

The Cambridge Area: Relief.

A = The Castle. B = St Bene't's Church. N = Newnham. G = Grantchester. T = Trumpington. C = Cherryhinton. Ch = Chesterton. For sources see footnote 2, p. 162.

Intermediate Terrace gravels runs a depression occupied by Hobson's Brook in the south and followed in part by the King's Ditch in the north. To the east, these gravels are separated from the Higher Terrace gravels of Barnwell by an outcrop of Chalk and Gault which coincides markedly with the eastern belt of open land formed by the University Sports Ground (Fenner's), Parker's Piece, Christ's Pieces and Butt's Green. North and west of the river are gravel-spreads equally important to the settlement of the area; the Higher Terrace gravels at Grantchester are comparable to those of Trumpington; Intermediate and Lower Terrace gravels stretch from thence to the valley of the Bin Brook and are separated by an outcrop of gault from the high-lying Observatory gravels of Castle Hill. East of Castle Hill lies a wide spread of Intermediate and Lower Terrace gravels reaching to Chesterton and beyond. The Cam River is bordered by alluvium, once marsh, but now largely drained and raised to form a belt of open land to the west and north of the town, comprising Sheep's Green, Coe Fen, the Backs, Jesus Green, Midsummer Common, Chesterton Fen and Stourbridge Common.

The gravels not only afforded well-drained building sites, but, paradoxically, gave the early town an ample, if not always sanitary, water supply. The underground seepage of water towards the Cam, held up by the impervious Gault, was tapped by shallow wells in the gravels, and provided until the seventeenth century a water supply considered adequate for all needs. In modern times,[1] two sources of water supply have replaced the easily contaminated surface wells; artesian water from the Lower Greensand and, more important, water from the Chalk.

THE MEDIEVAL PERIOD[2]

Evidence of a pre-Roman settlement at Cambridge is lacking, but Bronze Age finds, beakers, and burials, clustered near the ford, point to its use at this date; and in Roman times a fortified settlement mounted guard on Castle Hill over the important crossing at its foot. In Anglo-Saxon times, a flourishing settlement, or more probably settlements, developed in association with the river bend: (1) to the south of the river the Saxon

[1] (*a*) "This year [1610] the Town and University completed a new river from a place called Nine Wells in the Parish of Great Shelford to the Town of Cambridge." C. H. Cooper, *Annals of Cambridge*, iii, 36 (1845). "This year [1614] Henry King and Nathaniel Craddock with the King's sanction, and at the joint charge of the University and the Town, undertook to convey water by pipes from the new river to the Market Place, and there to erect a conduit of stone." *Ibid.* iii, 62.

(*b*) The Cambridge Town and University Waterworks Co. was formed in 1853.

[2] The theories regarding the early history of the town are discussed in an admirable paper by H. M. Cam, "The Origin of the Borough of Cambridge", *Proc. Camb. Antiq. Soc.* xxxv, 33 (1935).

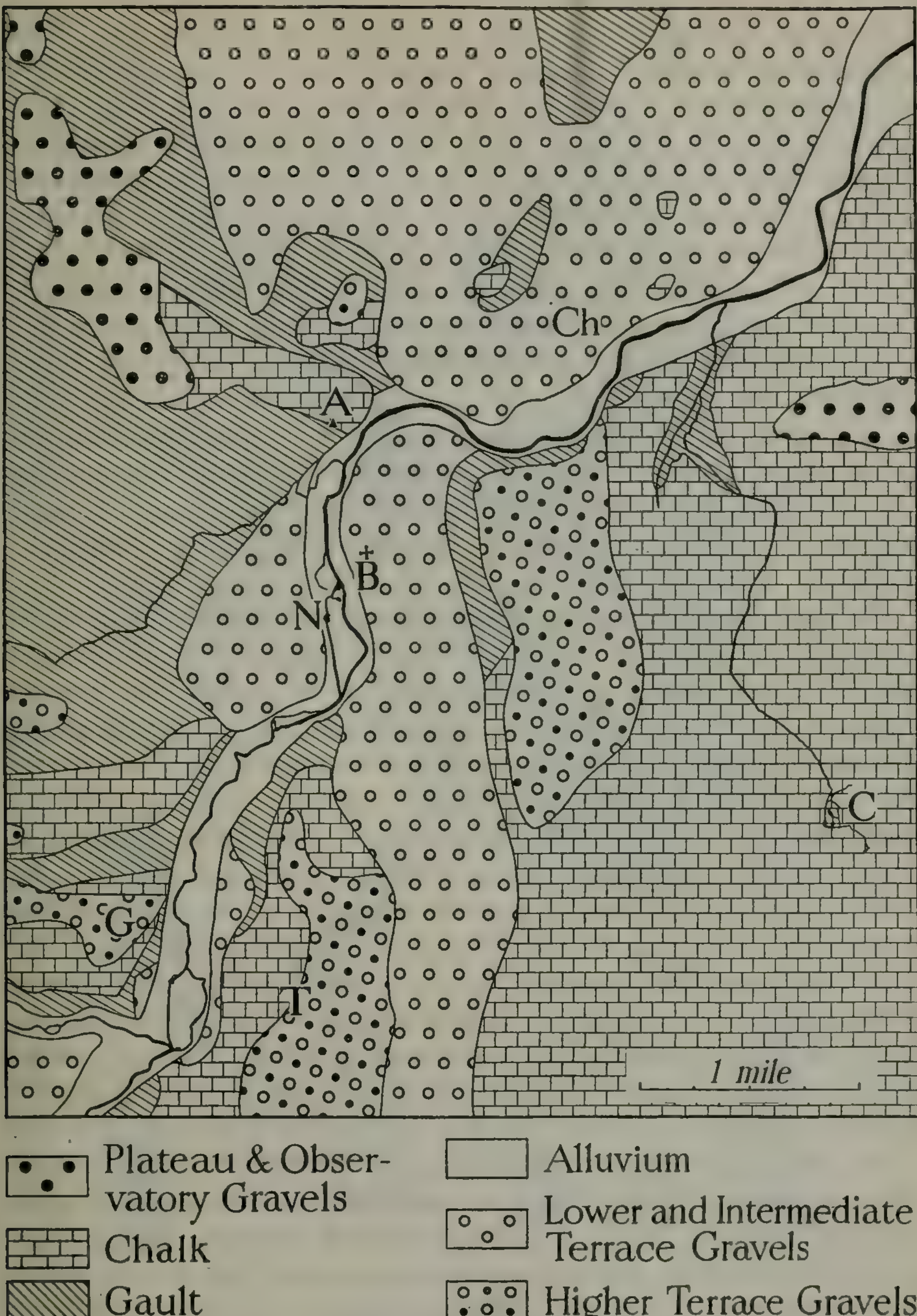


Fig. 38.

The Cambridge Area: Drift Geology.

A = The Castle. B = St Bene't's Church. N = Newnham. G = Grantchester. T = Trumpington. C = Cherryhinton. Ch = Chesterton. For sources see footnote 3, p. 162.

tower of the Church of St Benedict bears witness to the early date of the occupation of the gravels of Market Hill and Peas Hill, between the Cam and the King's Ditch depression; (2) to the north of the river, Castle Hill had also been early built upon; Domesday Book records that there were fifty-four tenements in Castle Ward. There also appear to have been two smaller settlements; (3) to the west on the rising ground of the river terrace at Newnham, the Mill at Newnham is mentioned in the Domesday Survey; and (4) to the east on the northern edge of the gravels at Barnwell. The main ford of the river at the foot of Castle Hill, where gravel and chalk afford firm banks, was early bridged; the Great Bridge of the documents was situated here. The Small Bridges, near the Mill Pool, connecting the settlement within the meander with that of Newnham, are also of early date. The medieval town was formed by the expansion of the two centres at Castle Hill and at Market Hill; but it was not until modern times that the settlements at Newnham and Barnwell were completely absorbed.

The medieval town so formed may have been bounded eastwards by the King's Ditch (see Fig. 39), cut, most probably, primarily for the defence of the crossing, not for the safety of the settlements. By the thirteenth century, the town had grown beyond these limits: the parish of St Mary the Less to the south and that of St Andrew the Great to the east both lay almost entirely outside the town as defined by the King's Ditch, and both had a considerable population. The further extension of medieval Cambridge was, however, confined on the one hand by the alluvial marshes of the river, and, on the other, by the inviolability of the town fields. Already by the end of the thirteenth century, the edge of the gravels was being raised and drained to provide extra building sites without sacrificing valuable agricultural and meadow land: the chapel and infirmary of the Hospital of St John (later the site of St John's College) and the nunnery of St Radegund (later the site of Jesus College) encroached on the alluvial land of the western and northern slopes of the Intermediate gravels.

The University, already powerful at this period, did not, however, possess elaborate buildings. The great period of University and Collegiate building belongs to the fourteenth and fifteenth centuries. By this date, most of the desirable gravel sites within the King's Ditch boundary had been occupied, and thus the Collegiate buildings fall into two groups: those upon good gravel sites on the outskirts of the medieval town, and those upon "made" ground along the western edge of the river terrace where the gravel descends below the alluvium.[1]

[1] T. McKenny Hughes, "The Superficial Deposits of Cambridge and their effect on the distribution of the Colleges", *Proc. Camb. Antiq. Soc.* xi, 293 (1907).

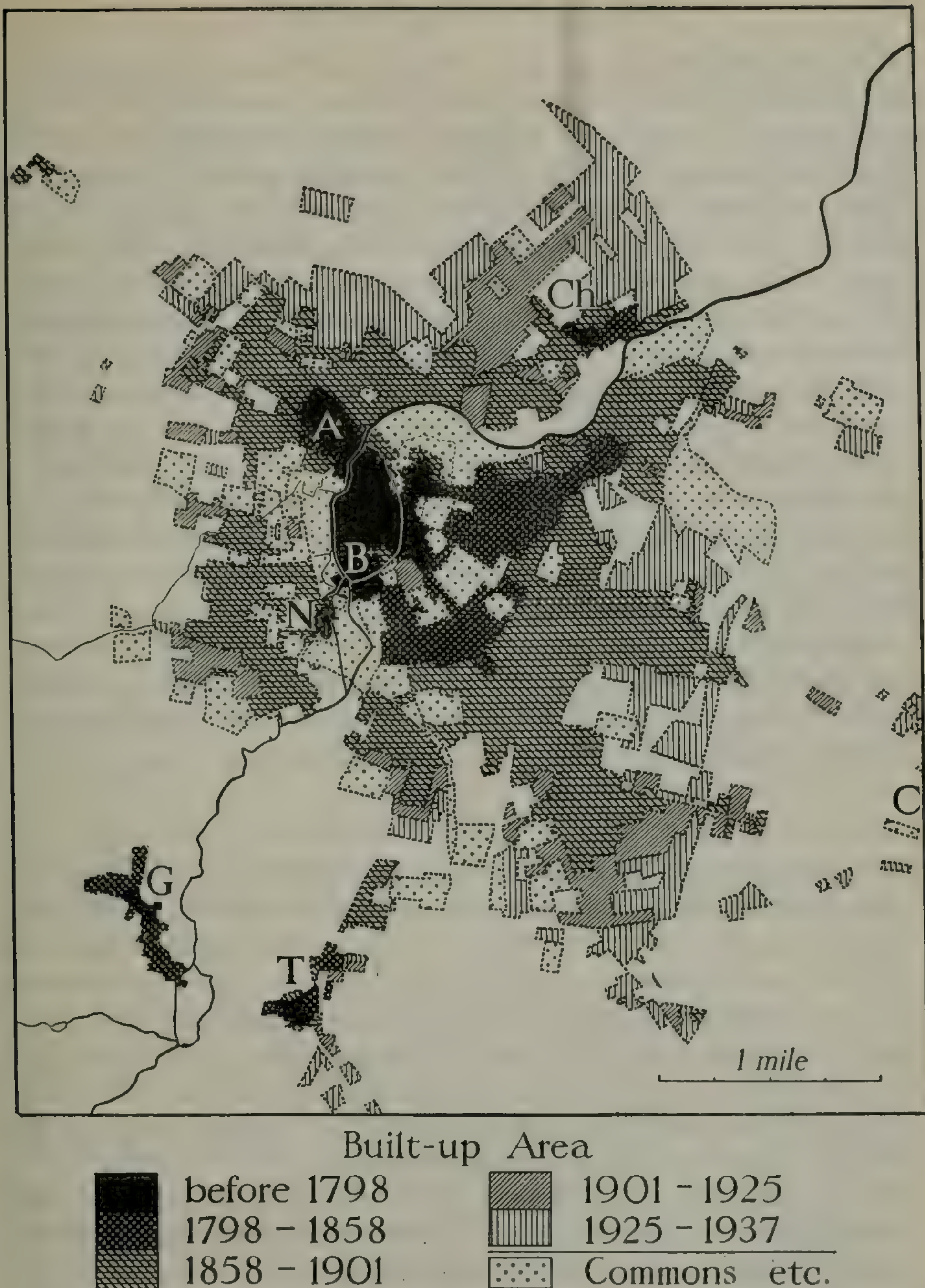


Fig. 39.

The Growth of Cambridge.

A = The Castle. B = St Bene't's Church. N = Newnham. G = Grantchester. T = Trumpington. C = Cherryhinton. Ch = Chesterton. The white line curving from below B to the river opposite A represents the course of the King's Ditch. Commons, etc. includes Commons, the Backs, Playing Fields, Recreation Grounds and Cemeteries. The area shown in solid black was covered with houses by 1574. For sources see footnote 2, p. 168.

The first group of Colleges chose the easier solution; Peterhouse (1284) and Pembroke (1347) were built upon gravels on the southern edge of the town, while the monastic buildings of the Friar Preachers (1240), later utilised by Emmanuel College, indicates the extension of building to the gravels on the east of the King's Ditch depression. Many of the earlier buildings of the second group (Michael House, 1324, Clare Hall, 1326, Gonville Hall, 1348, Trinity Hall, 1350) are clustered around slightly higher ground indicated by the modern name of Senate House Hill, upon which was built the first University buildings, the Grammar School, the Law School, and the Arts School. Lower sites to the north and south were soon utilised, King's Hall (Trinity College) was built in 1337, King's Chapel in 1446, Queens' College in 1448, and St Catharine's College in 1473. As the river Colleges have grown and extended their buildings in modern times, the alluvial river marshes have been drained and raised, and the Cam has been canalised, resulting in the stretch of College gardens, playing fields and commons which constitute the Backs to-day. St John's College, in the nineteenth century, placed new buildings west of the river on a purely alluvial site; but Clare College, in the twentieth century, preferred to separate its new buildings from the old and placed them on the rising ground of the gravel terrace west of the river.

THE PERIOD 1500–1800

The built-up area of the medieval town can be deduced only indirectly from archaeological and literary sources, but its extent from the later sixteenth century onwards is clearly revealed in the excellent series of plans and maps of varying dates which have survived.[1] The earliest extant of these detailed plans, those of Lyne (1574) and Hammond (1592), have been taken as the basis of the map showing the growth of Cambridge (Fig. 39).[2] These early plans raise complex architectural questions which are not important here: they provide, at any rate, a reliable picture of the extent of the town in the later sixteenth century.

In the south, a few houses flanked the two main roads into the town, Trumpington Street and St Andrew's Street, separated by marshy ground of a depression in the gravels occupied then by St Thomas' Leys and by Swinecroft. This area was known later as the Downing site and was not

[1] J. W. Clark and A. Gray, *Old Plans of Cambridge, 1574–1798* (1921).

[2] Fig. 39 has been constructed from the plans of Richard Lyne, 1574, John Hammond, 1592; the surveys of David Loggan, 1688, William Cunstance, 1798, George Baker, 1830, Richard Rowe, 1858; and the 6 in. editions of the Ordnance Survey of 1885, 1901, 1925. For the extension of the built-up area between 1925 and December 1937, I am indebted to the Cambridge Borough Engineer and Surveyor for permission to use plans in his possession.

built over until the nineteenth century. On the east, Parker's Piece, Christ's Pieces and Butt's Green marked the edge of the built-up area and are shown as cornlands. No buildings of note extended beyond Jesus College; and north of the river the gravel-spread towards Chesterton was entirely open; but houses had crept down the west side of Castle Hill to the edge of the alluvium of the Bin Brook. The Backs are shown as completely rural on the maps; grazing animals on the alluvium suggest meadow, conventional grain fields on the flanking terrace suggest arable land. The river was bridged at Silver Street, but, beyond the bridges, the road was replaced by field paths leading to the small settlement around the Mill at Newnham.

Cambridge at the end of the sixteenth century, then, covered much the same area as the medieval town; the further changes in area, as shown on the plans of Loggan (1688) and Cunstance (1798), are so slight as to be perforce omitted from the Growth of Cambridge map (Fig. 39). These show a few more buildings on the outskirts of the town; but the rural environs as sketched for the sixteenth century remain essentially the same. This almost complete halt in territorial growth during the seventeenth and eighteenth centuries is most striking.

During these centuries, however, there is evidence of a large increase in population within the existing built-up area. The Poll Tax Returns for 1377 record 1902 persons more than fourteen years of age for the Cambridge Borough.[1] Cooper quotes estimates and counts of population out of the Colleges[2] during the early modern period which may be summarised thus:

1587	1728	1749	1794	1801
4990	6422	6131	8942	9276

These figures suggest a considerable increase during the later Middle Ages, followed by a period of slow growth during the seventeenth century changing to relatively rapid growth in the second half of the eighteenth century. The details of the figures for the eighteenth century show that, although there was a general increase of population density, the changes in the central parishes, where the density of population was highest, were not great. A marked increase, however, characterised the parishes with land on the outskirts; the figures for the parish of St Giles show the first

[1] E. Powell, *The East Anglia Rising in 1381* (1896), p. 121.
[2] C. H. Cooper, *op. cit.* ii, 435 (1842); iv, 203, 274, 451, 470 (1852). The number of hearths in the town in 1662 was recorded as 4031. *Ibid.* iii, 501.

sign of growth to the north of the river, later to assume such astonishing proportions.

The growth of the University during this period can best be seen from the Matriculation figures (Fig. 40).[1] These figures fluctuate considerably, but between 1600 and 1675 average 307 per annum; numbers decline during the last quarter of the century, and remain below 200 per annum (average 161) throughout the eighteenth century. This suggests a resident

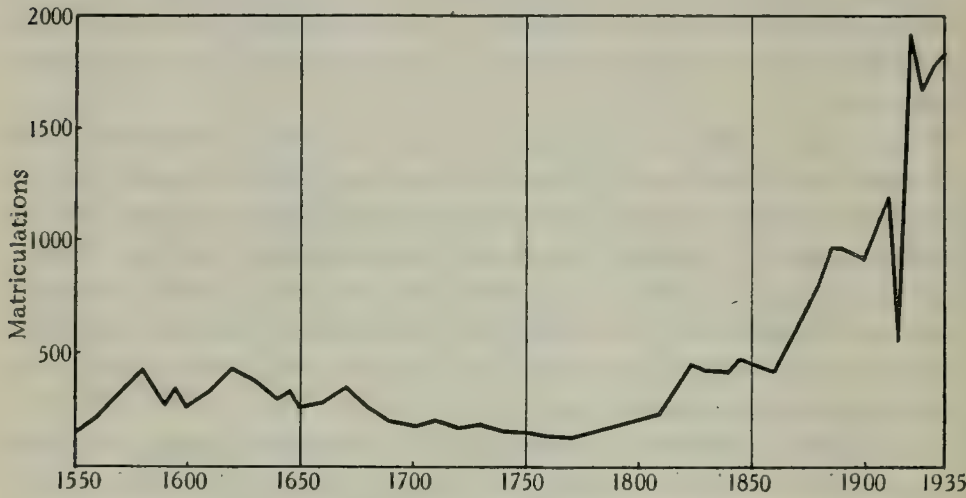


Fig. 40.

Matriculations in the University of Cambridge, 1550–1935.

I am indebted to Dr J. A. Venn for permission to reproduce this graph.

University population of about 1220 in the seventeenth century[2] and 650 in the eighteenth. The curve of Matriculation shows an upward trend in the second half of the eighteenth century and the Census Returns for 1801 record 811 resident members of the University.

The increase of population within the Borough between 1500–1800, without a corresponding increase in the built-up area, indicates therefore a steadily increasing density of population,[3] and the lack of territorial expansion reflects strongly the building restriction caused by the marshes and by the open-fields around the town. The enclosure of the open-fields between 1801 and 1807 was followed at once by a great increase in the built-up area, which represented, in part, the relief from cramped and

[1] C. H. Cooper, *op. cit.* iii, 553, quotes an estimate made by John Ivory in 1672 which gives 2522 as the number resident in the Colleges, including Fellows, Scholars and Servants.

[2] J. A. Venn, "Matriculations at Oxford and Cambridge, 1544–1906", *The Oxford and Cambridge Review*, No. 3, p. 48 (1908).

[3] F. W. Maitland, *Township and Borough* (1898), pp. 101–5.

overcrowded conditions in the old town. The population of the Borough was also increasing rapidly in the nineteenth century (Fig. 41).

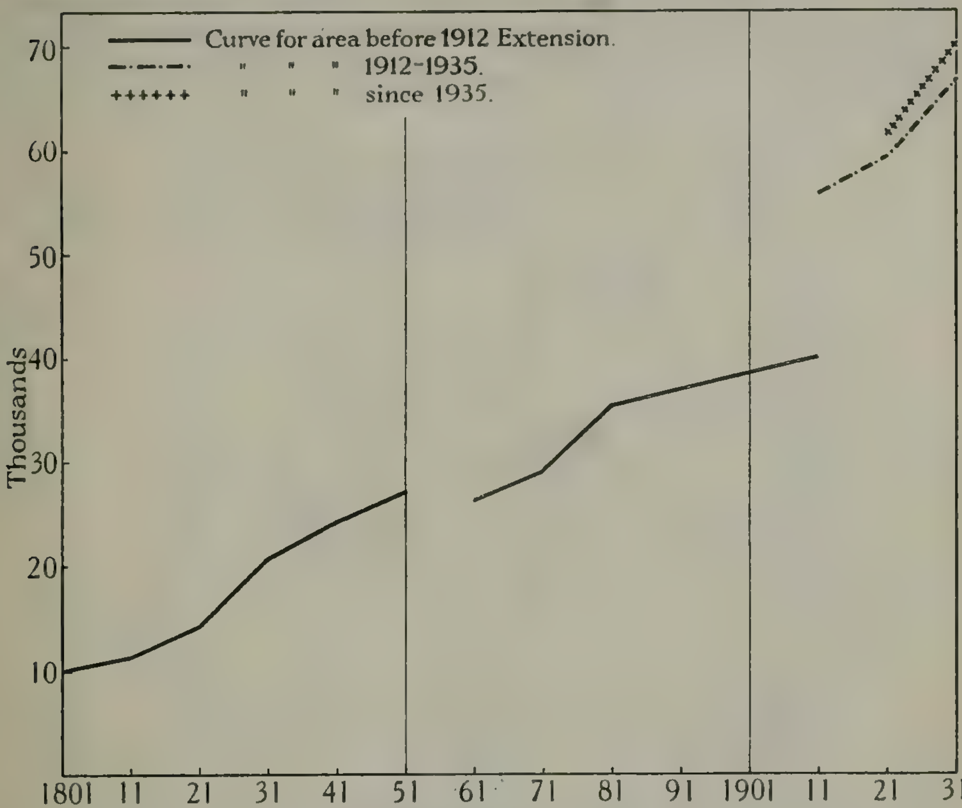


Fig. 41.

The Population of Cambridge, 1801–1931.

The break in the curve between 1851 and 1861 is due to the fact that between 1811–51 the University was in Residence on Census Night, and from 1861–1911 in Vacation. In 1921, Census Night was 19/20 June at the beginning of the Vacation; in 1931 the Census was taken in Full Term. The population in 1921 and 1931, within the area of the Borough as extended in 1935, is shown in addition to that of the area of the Borough at the time of the Census. The official estimate of the population of the Borough in 1936 was 76,760; and in 1938, including the "overspill" beyond the Borough boundary, the population is about 90,000. I am indebted to Dr J. A. Venn for these later figures.

THE NINETEENTH CENTURY

Fig. 39 shows the area built over between the publication of the plan of William Cunstance in 1798 and that of Richard Rowe in 1858. North of the river there was a small extension to the south-west of Castle Hill, but the striking growth of the town was southward and eastward. A large area on the Intermediate Terrace gravels, on either side of the central depression of the Downing site, was built upon during this period. Beyond the clay strip, largely occupied by the open commons, streets of small houses appear on the western portion of the Higher Terrace. The movement of population to the periphery, and the relief of congestion in the central parishes, are clearly shown by the Census Returns for the nineteenth century (Fig. 42). The 1851 Returns attribute the decrease in Great St Mary's

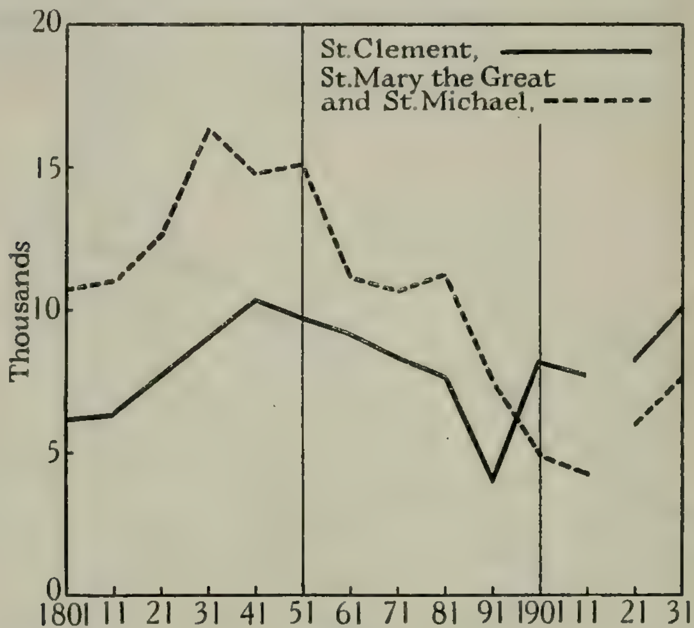


Fig. 42.

Population curves of two central parishes, showing decline in numbers due to movement into the suburbs. (N.B. The break in the curve between 1911 and 1921 indicates a change in the parish boundaries.)

parish to "the recent destruction of houses on Market Hill by fire and not rebuilt"; while, in the parish of the Holy Sepulchre, "the decrease of population is caused by the demolition of a number of old and unsafe tenements". The peripheral growth is seen in the population curve for the parish of St Andrew the Less (Fig. 43), where, says the Census Returns, "the marked increase is due to the erection of public buildings and the enlargement of the Colleges and therefore an increase in the number of labourers and mechanics. Several streets of small houses have been built."

The growth of the town in size and numbers in the first half of the nineteenth century was probably due largely to the progress of medicine and sanitation. Cambridge had suffered in the past repeatedly and severely from pestilence. The increase in University numbers and the building activity of the Colleges added to the prosperity of the town, and there also appears to have been an increase in its importance as a market for the rural areas. The Hay Market in 1820 and the Cattle Market in 1842 were removed from the centre of the town to more spacious sites on Pound Hill (the western slope of Castle Hill). The old Corn Exchange on St Andrew's Hill was also opened in 1842. The outlying villages, Newnham, Grantchester, Trumpington, Cherryhinton, Chesterton, also show in-

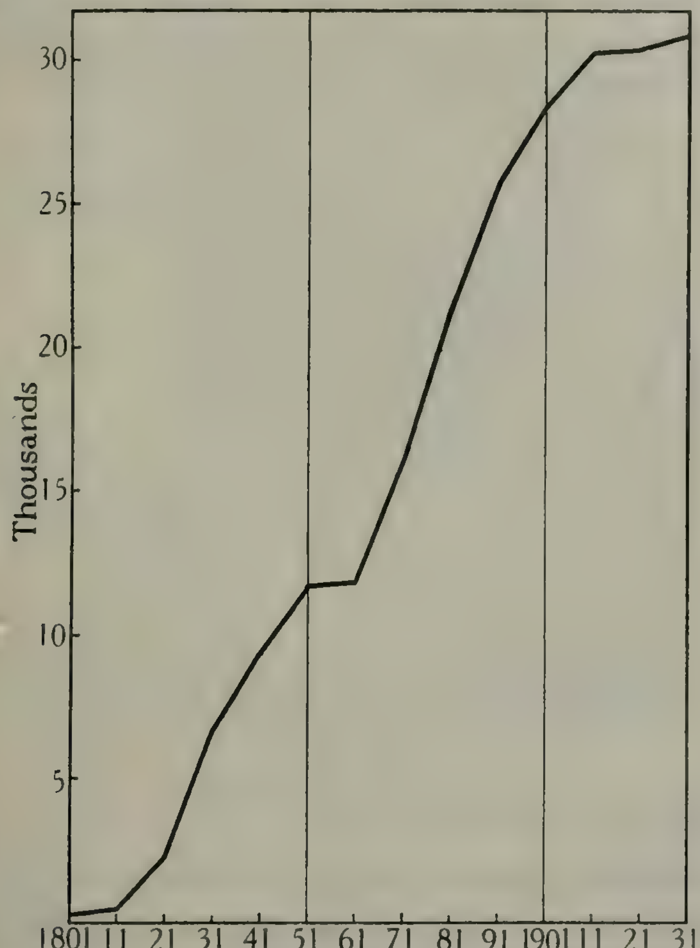


Fig. 43.

Population curve for the parish of St Andrew the Less, including after 1845 the parishes of St Paul (1845), St Matthew (1870), St Barnabas (1888), and St Philip (1903).

crease of population (Fig. 44) and area at this period; in the case of Cherryhinton, the 1821 Census Report specifically attributes the increase to enclosures.[1]

The expansion of Cambridge in the second half of the nineteenth century was even greater (Fig. 39). The town continued to grow rapidly

[1] As was the case with other villages of the County. See p. 129 above.

eastwards upon the gravel forming higher ground between Hobson's Brook and Coldham's Brook. The railway, built along the summit of the ridge, was opened in 1845.[1] The railway fostered development in this area; as early as 1851 the competition of railway transport was causing a shift of population. The riverside parish of St Clement showed a decline in numbers, and the 1851 Census Report declared "many families have left as the Eastern Counties Railway is absorbing the trade of the Cam". Industrial enterprises were attracted to sites near the railway; and the strip of Gault, appearing beneath the gravels in the area between Coldham's Brook

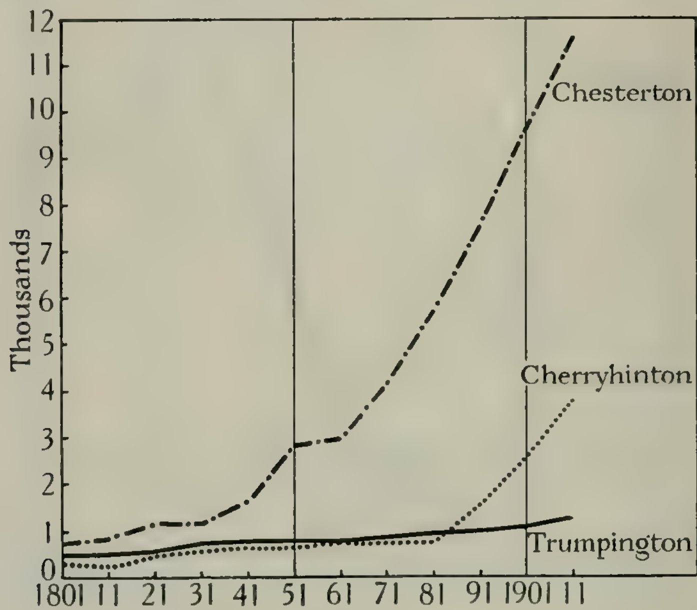


Fig. 44.

Population curves for the outer parishes largely added to the Borough by the Extension Acts of 1911 and 1934 (see Fig. 46).

and the Cam, was utilised for brick and tile works. The parish of St Andrew the Less grew rapidly (Fig. 43), and was subdivided repeatedly during the nineteenth century. Much of this area became a district of mean streets and small, crowded houses, forming in the twentieth century one of the most densely populated areas in Cambridge (Fig. 45).[2]

The other area with a density of more than thirty persons per acre at the beginning of the twentieth century lies to the north of the river on the Intermediate and Lower Terrace gravels which raise the ground above the

[1] See pp. 132–4 above for the opening dates of the various railway lines.

[2] Fig. 45 has been constructed from the Census figures, using the wards before the 1935 changes as the unit areas. These units were chosen in preference to the existing wards because they are smaller, and figures for three decades, 1911, 1921 and 1931, are available.

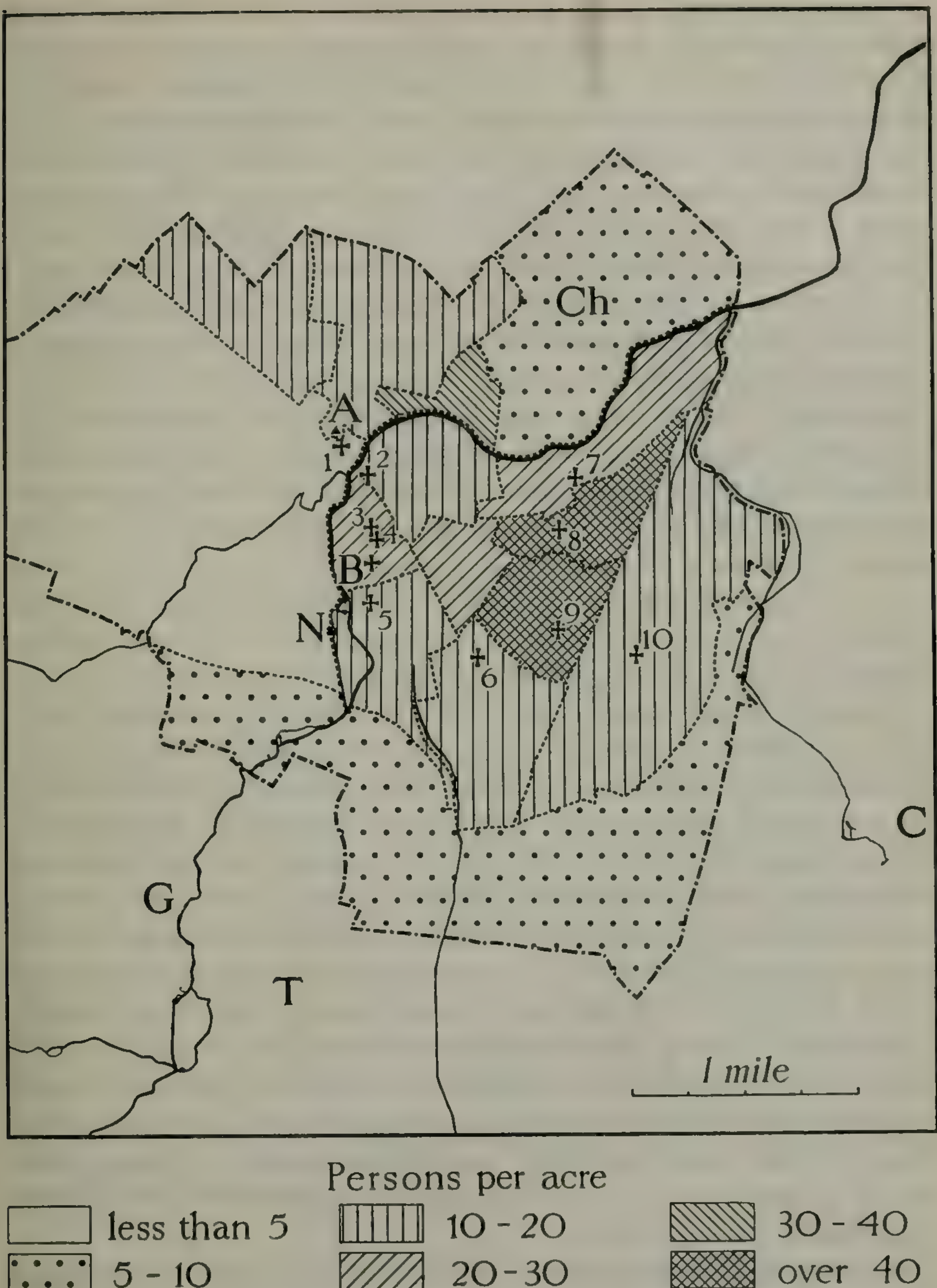


Fig. 45.

Cambridge: Density of Population, 1911–1931.

A = The Castle. B = St Bene't's Church. N = Newnham. G = Grantchester. T = Trumpington. C = Cherryhinton. Ch = Chesterton.

The parish churches marked + are: 1. St Giles'. 2. St Clement's. 3. St Michael's. 4. Great St Mary's. 5. Little St Mary's. 6. St Paul's (New Town). 7. St Andrew's the Less (Barnwell). 8. St Matthew's. 9. St Barnabas' (Petersfield). 10. St Philip's (Romsey Town).

20 ft. contour and so above the flood level. This district was also largely built up, during the second half of the nineteenth century, to provide accommodation for the growing working-class population: indeed, by the end of the century it had stretched out and joined the expanding village of Chesterton. The remarkable growth in the Chesterton district was already foreshadowed in 1851. The Census Returns attribute a decrease in the non-collegiate population of St Giles' to "the removal of many families to the neighbouring suburb of Chesterton"; and the return for Chesterton noted that "upwards of 200 houses have been erected in the last ten years principally inhabited by persons attracted by low rents and light taxation to reside there, though engaged in business in the town of Cambridge". Finally, there was also a considerable expansion of the residential area during this period. Building proceeded apace to the south, along the higher ground on either side of Hobson's Brook, to the west, along the Newnham gravel terrace, and to the north, along the Castle Hill ridge.

Three factors account, in the main, for the growth of the town at this period: (1) the development of railway communication, (2) the development of industries, and (3) the marked growth of the University.

The negotiations between the Town and University authorities took a long and arduous course. A suitable site for a station was very much discussed: that of the Eastern Counties Railway, the first opened, occupied the present site, but it was felt "it was so exceedingly bad that altogether the advantages of the railway were almost superceded by the disadvantage of the station".[1] The Midland and Eastern Company "proposed to remedy that evil" and "to run their line through Coe Fen and bring their station to the very heart of the town". Numerous sites were considered, Sheep's Green, Butt's Green, Midsummer Common among them, but eventually "the difficulties likely to stand in the way of obtaining a site easily accessible and convenient to all the railways likely to branch off from the town and at the same time not interfering with the beautiful walks around the town or with College grounds" proved insuperable. The project for a central station, which might have radically altered the town plan of Cambridge, was abandoned.

It was urged that the railway would "afford unquestionable advantages to a large district hitherto shut out from the benefit of railway communication";[2] and it was argued that "the river would feed the railway and the railway feed the river".[3] The opening of the railway did bring Cambridge

[1] Report of a Railway Meeting, *Cambridge Chronicle*, 22 Nov. 1845. The remaining quotations in this paragraph are also taken from this report.

[2] *Cambridge Chronicle*, 11 Feb. 1843.

[3] *Ibid.* 3 Oct. 1834.

into closer touch with the London market but, in spite of assurances to the contrary, it had a devastating effect on the trade of the Cam; the long lines of barges carrying coal, wood, and stone soon disappeared.

Concurrently with the development of communications came the development of industries. Brick and tile works at Cherryhinton and Coldham's Lane, cement works at Romsey Town, are conveniently placed near the railway. Flour-milling, sausage-making, brewing and malting occupied increasing numbers, and Chivers' jam factory, opened at Histon in 1873, also drew workers from Cambridge. Printing, an old-established industry in the town, occupied 286 men in 1901 and in 1881 the Cambridge Instrument Company was founded. Building and construction work provided employment for a large number of industrial workers.

The two main industries, building and printing, together with retail trade, are in fact closely connected with University development; and the marked expansion of the University, in the second half of the nineteenth century, was the most important single factor in the growth of the town at this period. The numbers of undergraduates rose steadily:

1861	1871	1881	1891	1901	1911	1921	1931
1529	2097	2688	3029	2958	3781	4748	5204

With this increase went a corresponding increase of teaching and administrative officers. After 1871 the abolition of religious tests by the University and Colleges was, at any rate, one among many causes that lay behind the increase. College buildings became inadequate to house the growing numbers, and the demand for lodgings grew. Then, again, in 1882 came the abolition of the rule that Fellowships must be surrendered on marriage; and the same Statutes decreed that Fellowships were to be conditional on active work in the Colleges or the University, thus necessitating residence in Cambridge. These changes could not but affect the growth of the town; married Fellows needed house accommodation as well as rooms in College and this was doubtless a factor in the development of the residential suburbs.

THE TWENTIETH CENTURY

The twentieth-century extensions of area are shown on Fig. 39 for two periods. The first quarter of the century is differentiated from the development of the last ten years in order to emphasise present tendencies. The period 1901–25 was characterised by rapid development along the main roads; the last ten years by an attempt to control ribbon development and to fill in the empty areas between these roads.

In 1906 it was said that "our town is mostly built";[1] even in 1925 it could be stated "Cambridge is still to an appreciable extent a rural township".[2] Present development is rapidly changing these conditions. Twice at short intervals, in 1912 and 1935, the Borough boundaries (Fig. 46) have been considerably extended—boundaries hitherto unchanged throughout the centuries. The additions to the Borough show the position and amount of the extension:

By Extension Order Act of 1911.

Parish	Area	Population
Chesterton (whole)	1,173 acres	11,330 persons
Cherryhinton (part)	388	2,749
Trumpington (part)	497	527
Grantchester (part)	166	1,179

By Extension Order Act of 1934.

Parish	Area	Population
Cherryhinton (whole)	1,671 acres	1,254 persons
Trumpington (part)	1,439	1,179
Gt. Shelford (part)	188	74
Fen Ditton (part)	441	437
Impington (part)	570	341
Milton (part)	294	95

The most rapid growth is now in the south, and along the gravels to the north of the river. The tendency to move out from the medieval nucleus has largely ceased and the recent extension of College buildings will result, if anything, in a rise of population density in this area during term time.

But the general movement to the periphery continues. The residential areas to the south and west are still growing. There is also a marked movement from the eastern and northern slums of the nineteenth century to the adjoining areas. The wards of South Chesterton, Petersfield and St Matthew, which had a density of more than 40 persons per acre in 1911, showed a decrease to 37·3, 38·8, and 37·8 persons per acre respectively in 1931. During the same period the density increased in the rest of Chesterton and in Romsey Town (Fig. 45). A new factor is at work here, the twentieth century demands better standards in housing conditions and looks askance at the crowded buildings of the previous one. This factor is additional to the continued development of the University, and to the continued increase in industries, largely of a skilled character: the Cambridge

[1] E. M. Jebb, *Cambridge. A Brief Study of Social Questions* (1906), p. 25.
[2] A. Gray, *The Town of Cambridge* (1925), p. 167.

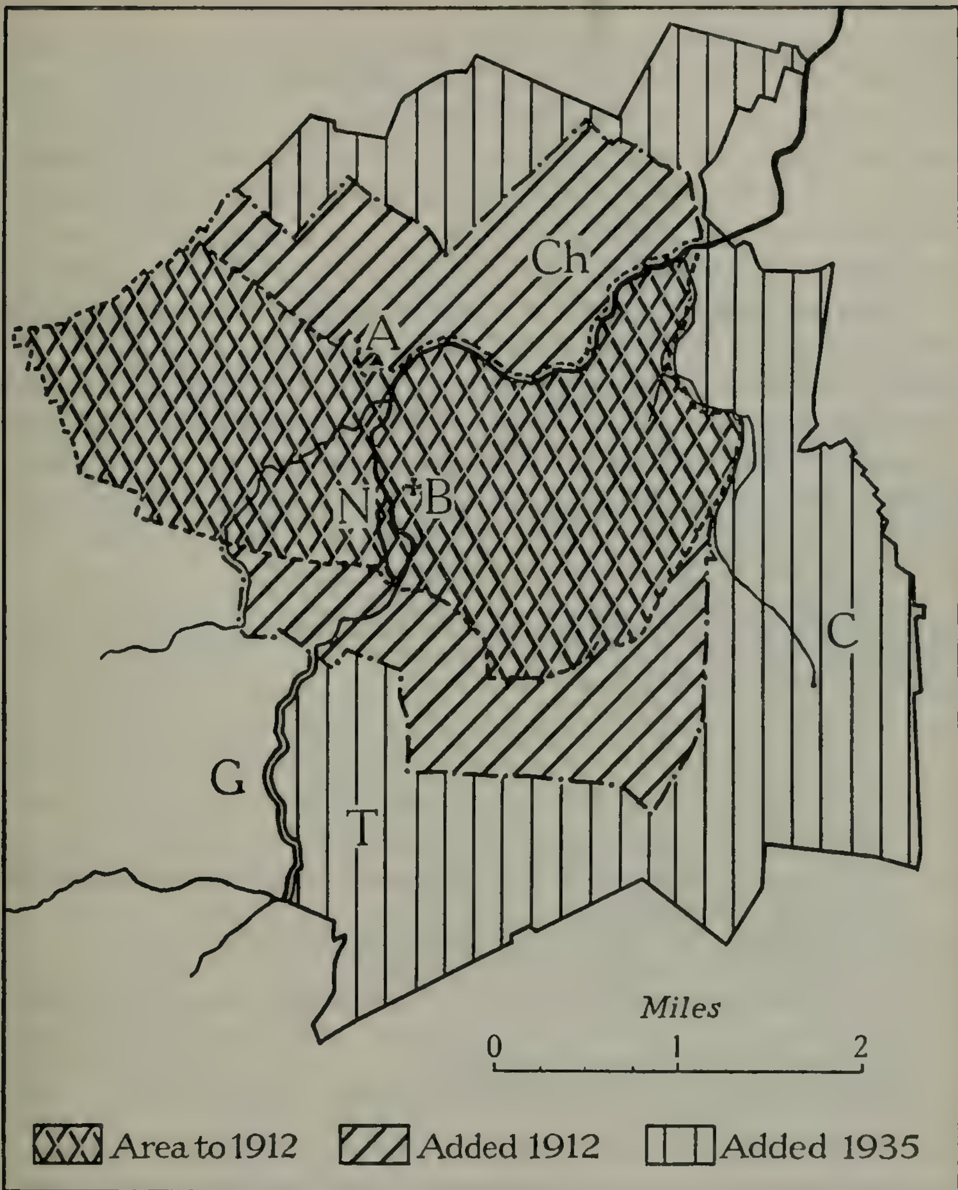


Fig. 46.

Cambridge: Extensions of the Borough.

The Borough boundaries are taken from the town plans of Cambridge published by Mr W. P. Spalding. The Extension Order Acts are dated 1911 and 1934; they came into effect in 1912 and 1935.

A=The Castle. B=St Bene't's Church. N=Newnham. G=Grantchester. T=Trumpington. C=Cherryhinton. Ch=Chesterton.

Instrument Company now employs 700 hands and the Pye Radio Works is a new and important industry.[1]

The *laissez-faire* development of the nineteenth century has been taken in hand. The Town Planning Department is in full working order, and the Borough land has been tentatively allotted to various purposes. Building schemes of twelve houses to the acre are a feature of the east and north-east, to which area, with its good railway sites, it is hoped to confine new and expanding industrial enterprises. In the west it is planned to curtail the density of building to, at most, four houses to the acre.

There was a proposal of the Town Council in 1841 to enclose portions of the Commons for building sites and market gardens.[2] Fortunately for the beauty of Cambridge and the preservation of the individuality of the old town, this was turned down by a meeting of the townsmen "characterised by extreme noise and tumult".[3] Medieval Cambridge is thus largely separated from the expanding Cambridge of to-day by a ring of open land formed by the Commons and the Backs.

[1] See pp. 159–60 above.
[2] Report read to a meeting of the Town Council, April 1841. Quoted by C. H. Cooper, *op. cit.* iv, 633.
[3] C. H. Cooper, *op. cit.* iv, 634.

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(1) C. H. Cooper, *Annals of Cambridge*, 5 vols. (1842–53).
(2) R. Willis and J. W. Clark, *The Architectural History of the University of Cambridge*, 4 vols. (1886).
(3) J. B. Mullinger, *The University of Cambridge*, 3 vols. (1873–1911).
(4) F. W. Maitland, *Township and Borough* (1898).
(5) F. W. Maitland and M. Bateson, *The Charters of the Borough of Cambridge* (1901).
(6) A. Gray, *The Town of Cambridge* (1925).
(7) A. Gray, *Cambridge University. An Episodical History* (1926).

CHAPTER THIRTEEN

THE DRAINING OF THE FENS
A.D. 1600–1850

By H. C. Darby, M.A., PH.D.

DURING THE MIDDLE AGES THE DRAINING OF THE 1300 square miles of the Fenland had remained largely a matter for local concern. When necessity arose, owing to the ravages of the sea or to the overflowing of the watercourses, the Crown granted a commission to remedy the evil. A succession of Commissions of Sewers combined with local custom to maintain the medieval economy. The upkeep of any single channel involved many interlocking interests, and the dissolution of the monasteries in 1539 served only to increase the confusion of divided responsibilities. But, as Samuel Hartlib wrote, "in Queen Elizabeth's dayes, Ingenuites, Curiosities and Good Husbandry began to take place". The time was becoming ripe for a "greate designe" in the Fenland. During the later years of the sixteenth century, various schemes and experiments prepared the way. At last, in 1600, there was passed "An Act for the recovering of many hundred thousand Acres of Marshes...". Of the many stretches of marsh in the kingdom, that of the great Fenland itself provided the most spectacular transformation.

Many schemes were afoot during the early years of the seventeenth century, and there was great opposition from those with vested interests in the fen commons and in the fenland streams. There was also much debate about ways and means. Nothing effective was done; general dissatisfaction was felt everywhere. The net result was that some fenmen approached Francis, 4th Earl of Bedford, the owner of 20,000 acres near Thorney and Whittlesea, who contracted within six years to make "good summer land"[1] all that expanse of peat in the southern Fenland, later known as the Bedford Level. An agreement was drawn up in 1630. In the following year, thirteen Co-Adventurers[2] associated themselves with the earl; and in 1634 they were granted a charter of incorporation. Their hope was to turn this expanse of "great waters and a few reeds" into "pleasant pastures of cattle and kyne"; and they secured the services of the Dutch engineer Vermuyden, who had been at work upon the reclamation

[1] I.e. Land free from floods in summer. This is the story told by C. Vermuyden in *A Discourse touching the drayning the great Fennes* (1642).

[2] So called because they "adventured" their capital. See p. 105 above.

of the Axholme marshes. Under his direction, cuts, drains, and sluices were made. Chief among these was the Old Bedford River extending from Earith to Salter's Lode, 70 ft. wide and 21 miles in length.[1]

In 1637, at a Session of Sewers in St Ives, the Level was judged to have been drained according to the true intent of the agreement of 1630. But complaints and petitions showered upon the Privy Council, and royal feeling turned against the Corporation. The inner history of this change in royal favour is obscure; at any rate, in the following year, the award was set aside. The Level still remained subject to inundation in winter, and so it was maintained that the contract of 1630 had not been fulfilled. The king himself, now, planned to drain the Fens "in such manner as to make them winter grounds", and he retained the services of Vermuyden. Soon, however, the fen difficulties were overshadowed by greater troubles. The country was at war within itself.

During the Civil War the draining was in abeyance, but the project had not been forgotten. After many committees and sub-committees, an "Act for the draining the Great Level of the Fens" was passed in May 1649; and the 5th Earl of Bedford and his associates were "declared to be the undertakers of the said work". In his *Discourse* of 1642, Vermuyden had divided the Great Level into three areas:

"1. The one from Glean to Morton's Leame.
2. From Morton's Leame to Bedford River.
3. From Bedford River southwards, being the remainder of the level."

These became the North,[2] the Middle, and the South Levels respectively (see Fig. 47). Despite continued hostility, activity was restarted under Vermuyden. The earlier works were restored; banks were made; sluices built; and channels scoured. In particular, the New Bedford River was cut running parallel to the Old Bedford River.[3] Between the two Bedford Rivers, a strip of land[4] was left open to form a reservoir for surplus water in time of flood (see Fig. 48). The old course of the Ouse was sluiced at Earith (the Hermitage Sluice) and at Denver, so that it became merely the drain for the fens in the Isle of Ely. The New Bedford

[1] Before this, the Old West River carried part of the Ouse in a circular course around the Isle of Ely and so to Denver, and thence to the sea at Lynn. Now, this water reached Denver directly through the Old Bedford River.

[2] Later, however, the North Level did not extend beyond the Welland.

[3] The New Bedford River was alternatively known as the Hundred Foot River, and, for the greater part of its course, ran half a mile to the east of the older cut.

[4] This became known as "The Wash", "The Washes", or "The Washlands". High "barrier" banks on the outer sides of the two cuts kept the water within definite limits. It could be run off at convenience; in the early part of the nineteenth century Welmore Lake Sluice was built to facilitate the run-off into the New Bedford River above Denver Sluice.

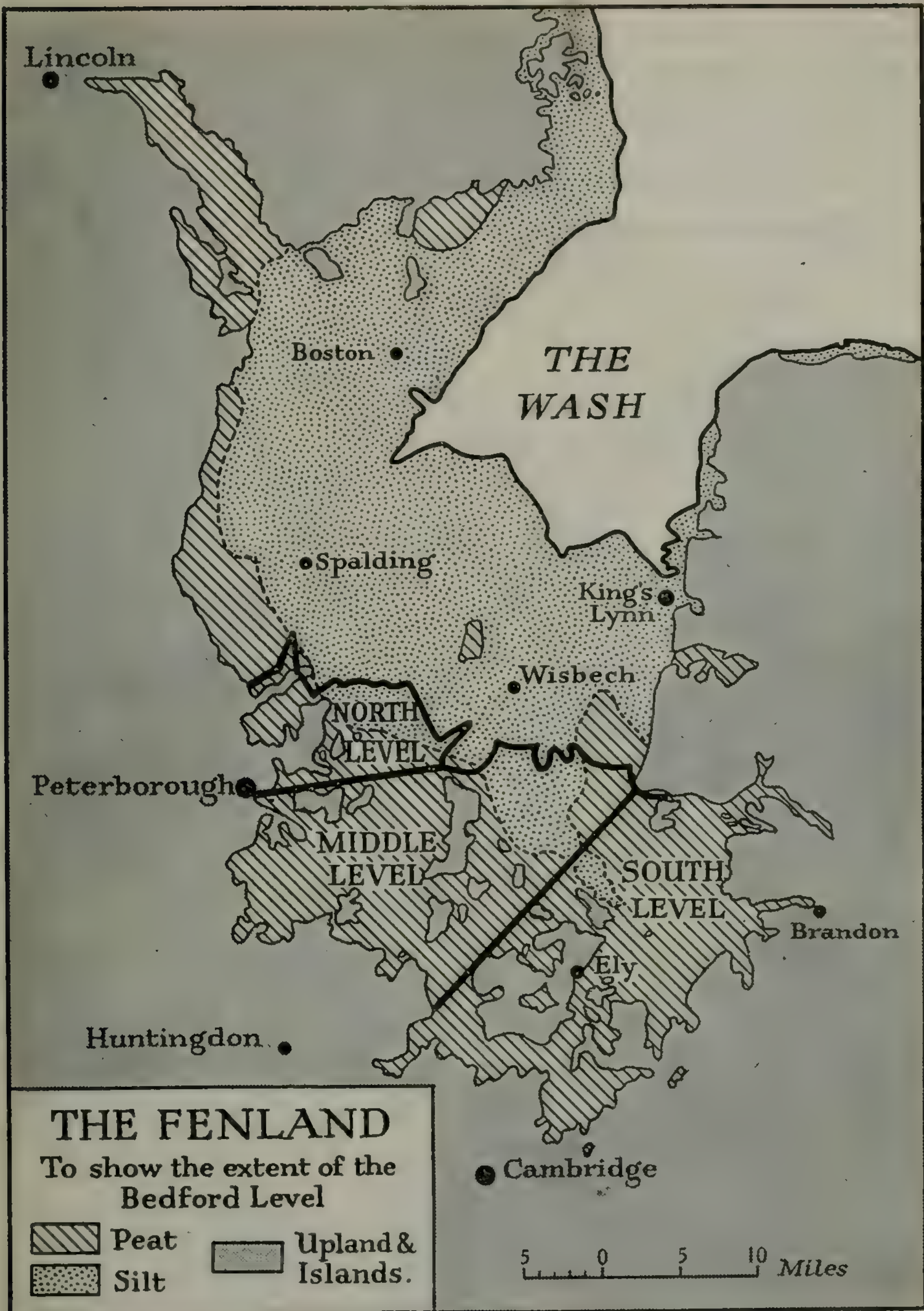

Fig. 47.

S. B. J. Skertchly, in *The Geology of the Fenland* (1877), p. 129, noted that the precise boundaries of the peat and silt were "very obscure, for the peat thins out insensibly along its borders." The limits of the Bedford Levels are taken from Samuel Wells' map of 1829 on a scale of $1\frac{1}{2}$ miles = 1 inch.

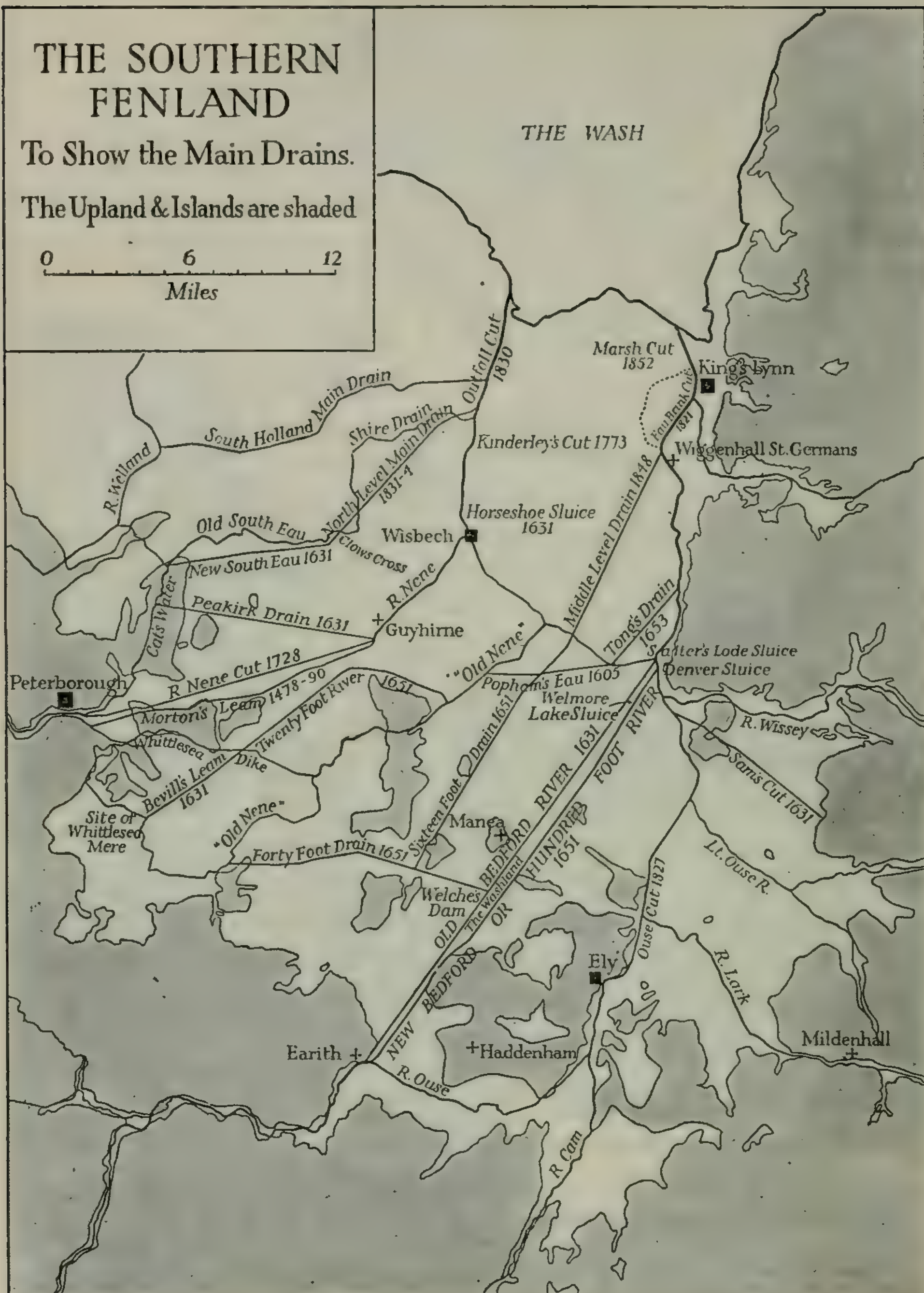

Fig. 48.

The approximate dates of the drains are given. In some cases there was an appreciable interval between the start of a project and its completion.

River became the main channel of the Ouse. The Seven Holes Sluice at Earith[1] kept the waters of the Ouse from flowing into the Old Bedford River (see Fig. 49). The final warrant of adjudication came in March 1652. Successive changes in the administration of the realm witnessed the completion of the machinery for preserving the works of the drainers, until, finally, there was passed the General Drainage Act of 1663.

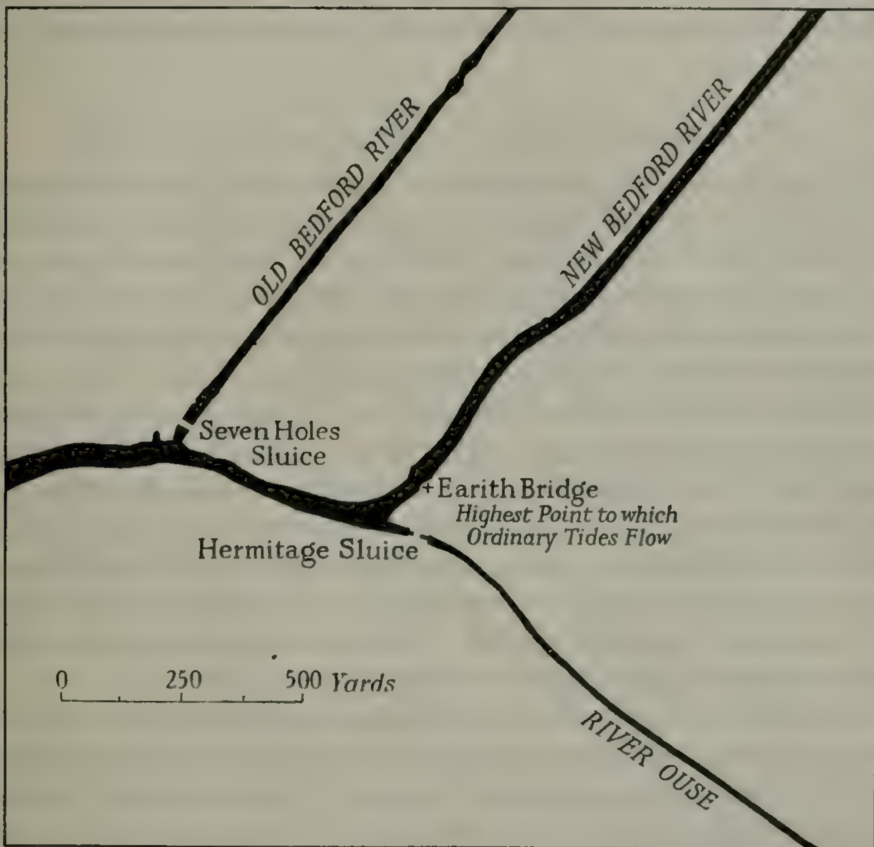

Fig. 49.

Seven Holes Sluice and Hermitage Sluice.

The highest point to which high spring tides flow is at Brownshill Staunch, some $2\frac{1}{4}$ miles above Earith Bridge.

At first, great success followed upon the works of the drainers. Cultivation was introduced on land that, as far as record went, had never before known a plough. As Thomas Fuller wrote in 1655, "the best argument to prove that a thing may be done is actually to do it". But time was not to fulfil these hopes. Despite many praises, it soon became evident that all

[1] Thus it protects the Washlands. But if the water coming down the upland Ouse is considerable it is opened—in summer rarely, in winter more often.

was not well in the Bedford Level. Some of the complaints that followed the final adjudication in 1652 were only to be expected—disputes about the allotment of the reclaimed land, and about the management of the new drains. These were problems of routine administration; they could be settled by negotiation and compromise. But there were other difficulties of a much more fundamental character, difficulties that brought the very success of the drainers near to disaster. Right up to the present day these difficulties have remained important in all discussions about draining. They are of two kinds.

THE LOWERING OF THE LEVEL OF THE FENS

The first group of difficulties resulted from the drying up of the peat fen. As the peat was drained it rapidly became lower in level. This lowering was due in part to the shrinkage of the peat, and in part to the wasting away of the peat surface owing to bacterial action and owing to cultivation. As a result, the surface of the peat soon became lower than the level of the channels into which it drained. The channel beds were lined with silt, and so escaped as rapid a lowering. This difference in height can be seen to-day along many of the fen rivers; they are at a higher level than the land through which they flow. The small drains right in the heart of the peat area suffered most. In time, they came to flow at a lower level than the main cuts into which they tried to discharge their waters! And the more these evils were combated by more effective draining, the more rapidly the peat surface continued to sink. Thus it was that the works of one generation became inadequate for the needs of the next.

An idea of the amount of this lowering can be seen to-day from Holme post in Huntingdonshire just outside the boundary of Cambridgeshire, along what was the south-western margin of Whittlesea Mere. In 1851, the top of this iron column was even with the surface of the ground. By 1870, nearly 8 ft. was exposed. To-day, it stands about 11 ft. high. This case is fairly extreme. The amount of shrinkage in any particular locality depends upon the original thickness of the peat as well as upon the intensity of the drainage operations. Even a shrinkage of half an inch per annum is important. It may be viewed with equanimity from one year to another, but the result over a period of years becomes critical.

Further, not all the Fenland is peat. This only made matters worse. The coastal areas are composed of silt (see Fig. 47), less liable to shrinkage and wastage than peat. Before the draining, the silt zone was over 5 ft. *lower* than the peat area that lay inland. To-day, the silt area is about 10 ft. *higher* than the peat. As a result of this differential shrinkage, the beds of the outfall channels became almost as high as the peat fen behind.[1]

[1] See p. 191 below.

The consequences of these fundamental difficulties were apparent even before the seventeenth century was over. Soon, disaster was abroad everywhere. What had seemed a promising enterprise in 1652 had become a tragedy by 1700. There was but one way to save the situation—the substitution of an artificial for a natural drainage. Water was pumped out from small dyke to drain, from drain to river, and so to sea.

An early mode of fen drainage was the horse mill, but the only satisfactory source of power at hand was the wind. The introduction of windmills for pumping purposes was, in fact, the critical factor that saved most of the Fens from being re-inundated. As the seventeenth century passed into the eighteenth, windmill drainage became more and more frequent. The whole of the Fenland came to consist almost entirely of small sub-districts, each pumping its water into one of the larger drains that traversed the region. For example, a pamphlet of 1748, written by Thomas Neale, stated that there were no less than 250 windmills in the Middle Level. "In Whittlesey parish alone, I was told by some of the principal inhabitants there are more than fifty mills, and there are, I believe, as many in Donnington (*sic*) with its members. I myself, riding very late from Ramsey to Holme, about six miles across the Fens, counted forty in my view."

But the windmill was far from being the perfect engine. It was at the mercy of gale and frost and calm. It was never very powerful, and soon it ceased to provide a satisfactory solution to the problem of clearing water from the drains. For as the surface level continued to subside, the windmill became increasingly ineffective. Inundations grew frequent. It is easier to put down statistics relating to these "drownings" than to imagine the bankruptcy and distress when crops not merely failed but completely disappeared beneath the rising waters. By the end of the eighteenth century, according to Arthur Young,[1] there were many fens "all waste and water", where twenty years previously there had been "buildings, farmers and cultivation". Some places had been particularly unfortunate: "three years ago five quarters of corn an acre; now sedge and rushes, frogs and bitterns". It was with dismay that he viewed the scene spread before him in the summer of 1805:

> It was a melancholy examination I took of the country between Whittlesea and March, the middle of July, in all which tract of ten miles, usually under great crops of cole, oats and wheat, there was nothing to be seen but desolation, with here and there a crop of oats or barley, sown so late that they can come to nothing.

He predicted the ruin of the whole flat district.

> The fens are now in a moment of balancing their fate; should a great flood come within two or three years, for want of an improved outfall, the whole country, fertile as it naturally is, will be abandoned.

[1] A. Young, *Annals of Agriculture*, xliii, 539 et seq. (1805).

Other evidence bears out the impression of desolation.[1] As one traveller of 1833 could write: "We are now in the very perfection of the fen-country, being several feet below the level of the great running streams, upon land subject to frequent inundation."

In addition to land that had deteriorated, some patches of original fen remained; in the west, for example, were the large reed-bordered lakes of Whittlesea Mere and Ramsey Mere. The great copper butterfly was not yet extinct; nor were all the species of fen birds; nor yet was the ague against which the fenmen took their opium pills. Indeed, many people were still "fearful of entering the fens of Cambridgeshire lest the Marsh Miasma should shorten their lives". That was in 1827. By 1858, the complaint had become "infrequent". The improvement was generally ascribed to better drainage.

Not only malaria, but many other distinctive features of the Fens disappeared before the changes of the nineteenth century. The time came when "sportsmen from the University" were no longer able to indulge a passion for shooting in the fens of Teversham, Quy, Bottisham, and Swaffham. And, in 1854, Henry Gunning was "happy to say that these incentives to idleness no longer exist. Thousands and tens of thousands of acres of land, which at the time I speak of produced to the owners only turf and sedge, are now bearing most luxuriant crops of corn."

The important factor that was giving the fen country of the nineteenth century this more stable economy was the advent of the steam-engine. The possibility of steam-driven pumps for draining had been discussed before 1800, but the idea was slow in gaining support. At length, John Rennie induced the proprietors of Bottisham Fen to erect a small engine to help their windmills.[2] There, in 1820, the first Watt engine was applied to work a scoop-wheel. Despite predictions of failure, other steam-engines followed and soon justified their introduction. An inscription on a pumping station along the New Bedford River is dated 1830, and reads:

> These fens have oftimes been by water drowned,
>   Science a remedy in water found,
> The power of steam she said shall be employed,
>   And the Destroyer by Itself destroyed.

It was a premature claim, but by 1838 Joseph Glynn, one of the pioneers of steam pumping, certainly had "the pleasure to see abundant crops of wheat take the place of the sedge and the bulrush". The "swamp of marsh, exhaling malaria, disease and death" had been converted into

[1] See p. 117 above. [2] See p. 120 above.

"fruitful cornfields and verdant pastures". By the middle of the century, according to one estimate, the number of steam-driven pumps between Cambridge and Lincoln was about 64; the number of windmills had declined from about 700 to about 220.

Further improvement was at hand. Among the great sights that "astonished the visitors" to the Great Exhibition of 1851 was Appold's centrifugal pump; and its application to fen problems was immediately realised. One of the new Appold pumps was erected to drain Whittlesea Mere. And so, witnessed by "large crowds of people", there disappeared the last remaining large stretch of water in the Fenland—a stretch of water that had enjoyed considerable reputation as the scene of regattas in summer and of skating in winter. The wind which, "in the autumn of 1851 was curling over the blue water of the lake, in the autumn of 1853 was blowing in the same place over fields of yellow corn".[1]

The success of the steam-engine in the Fenland did not mean that all difficulties were over. The lowering of the peat surface necessitated a constant building-up of the river banks. In the absence of easily accessible clay, many banks had been made of peat or light earth, and, during floods, they were subjected to considerable hydrostatic pressure. Breaches were frequent. The paradox was that an effective draining only increased the lowering of the peat surface. What this meant in terms of pumping can be seen from a solitary example. Methwold Fen, until 1883, had drained naturally into the Ouse through a dyke, Sam's Cut.[2] In that year, owing to the lowering of the fen, artificial drainage became necessary, and a pump was erected. During the years that followed the fen continued to sink so rapidly that a second pump had to be installed in 1913. Then, it was estimated that the surface had sunk "5 to 6 feet within the last 50 years". This one example illustrates conditions generally. It was the steam-engine that turned the desolation of 1800 into some prospect of prosperity.

OUTFALL PROBLEMS

The second group of changes that marked the nineteenth century was associated with the outfalls of the fen rivers into the Wash. In a normal river, the current of water is strong enough to force its way out to sea. But the fenland rivers were far from normal. The downward force of the fresh waters in the gently graded streams was no match, especially in summer, for the strong tidal flow twice each day. With swift flood tides and weak ebb tides (see Figs. 50 and 51), deposition was inevitable, and the

[1] W. Wells, "The Drainage of Whittlesea Mere", *Jour. Roy. Agric. Soc.* (1860), pp. 140–1.

[2] See p. 196 below.

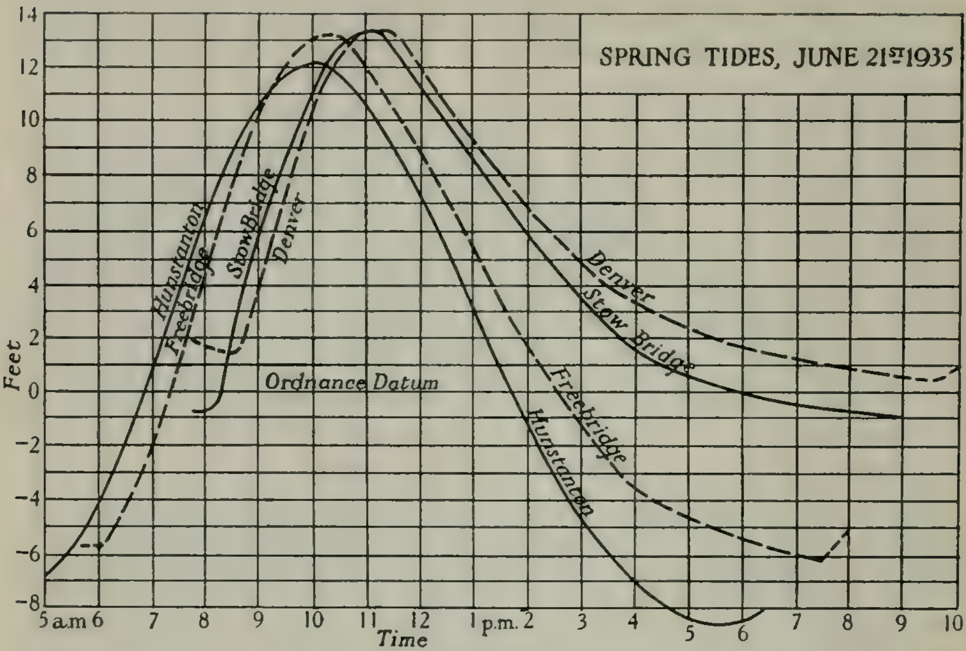

Fig. 50.

Tidal Curves for the Great Ouse Outfall.

Reproduced by the courtesy of the Chief Engineer of the River Great Ouse Catchment Board.

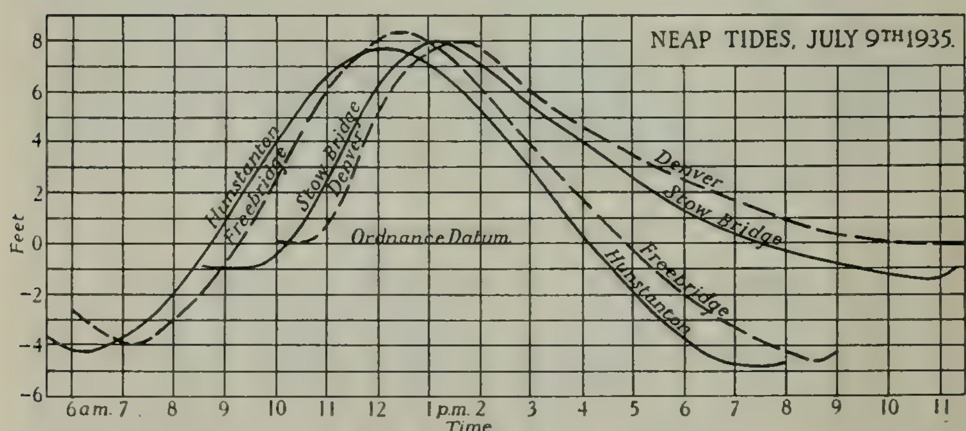

Fig. 51.

Tidal Curves for the Great Ouse Outfall.

Reproduced by the courtesy of the Chief Engineer of the River Great Ouse Catchment Board.

beds of the estuaries were continually being raised. This fact, combined with the lowering of the peat surface in the interior of the Fenland (see Fig. 47), made the outfall channels almost as high as the peatlands behind.[1] The fresh waters found great and greater difficulty in reaching the sea, and the maintenance of a clear channel seawards became of paramount importance. As Colonel Dodson wrote in 1664, just after Vermuyden's draining: "if we cannot be masters there, all other endeavours signifie nothing". The great controversies of the eighteenth century were outfall controversies, concerned with topics like the disposition of sluices, the mechanics of silting, the formation of sandbanks, and the nature of tidal scour. Thomas Badeslade's pamphlet of 1729 was but one of very many. As he said:

> all parties acknowledge the misfortune, for they all suffer; but all do not agree in the cause of this general calamity, nor in the method that must be put in practice to relieve them; but all agree and declare, that if something be not done, this country will be rendered uninhabitable.

By 1800, the Ouse reached the sea through a channel of varying width, filled with shifting sandbanks; in the Nene, "ships of large burden could no longer reach" Wisbech; the Welland estuary, too, was full of shoals; that of the Witham was in no better plight. During the nineteenth century, various remedies were tried. New cuts were made, straightening and improving the lower tidal courses of the rivers. The estuary most affecting the Cambridgeshire fens was that of the River Ouse. Above King's Lynn, the Ouse (carrying so much of the water of the Bedford Level) made an extensive bend of about 6 miles to St Germans (see Fig. 48). The channel was nearly a mile wide in places, and comprised a number of uncertain streams. During floods, the flow of the river was much impeded, and it was clear that no improvement could result until the obstructions had been cleared. The remedy was to cut off the great bend of the Ouse; to make the new channel large enough to contain the whole body of the river; and, incidentally, to increase the velocity of the current by shortening the line of the stream.[2] An Act was obtained in 1795 enabling the cut to be made, but the work was not started until 1817. Finally, in 1821, seventy years after it had been first proposed, the Eau Brink Cut was opened. It had an immediately beneficial effect upon the Middle and South Levels—until further accumulations of silt made new improvements urgent. In 1852, further straightening was secured by means of the Marsh Cut towards the sea. Some years before this (in 1846), the Norfolk Estuary Company had been established. Intended to recover land from the Wash, the primary object of this body became the maintenance of an extension seawards of the Eau Brink Cut. Training walls were built to induce the

[1] See p. 186 above.
[2] See p. 117 above.

River Ouse to keep a defined channel among the shifting shoals that, accumulating in the estuary, interfered with the effective discharge of

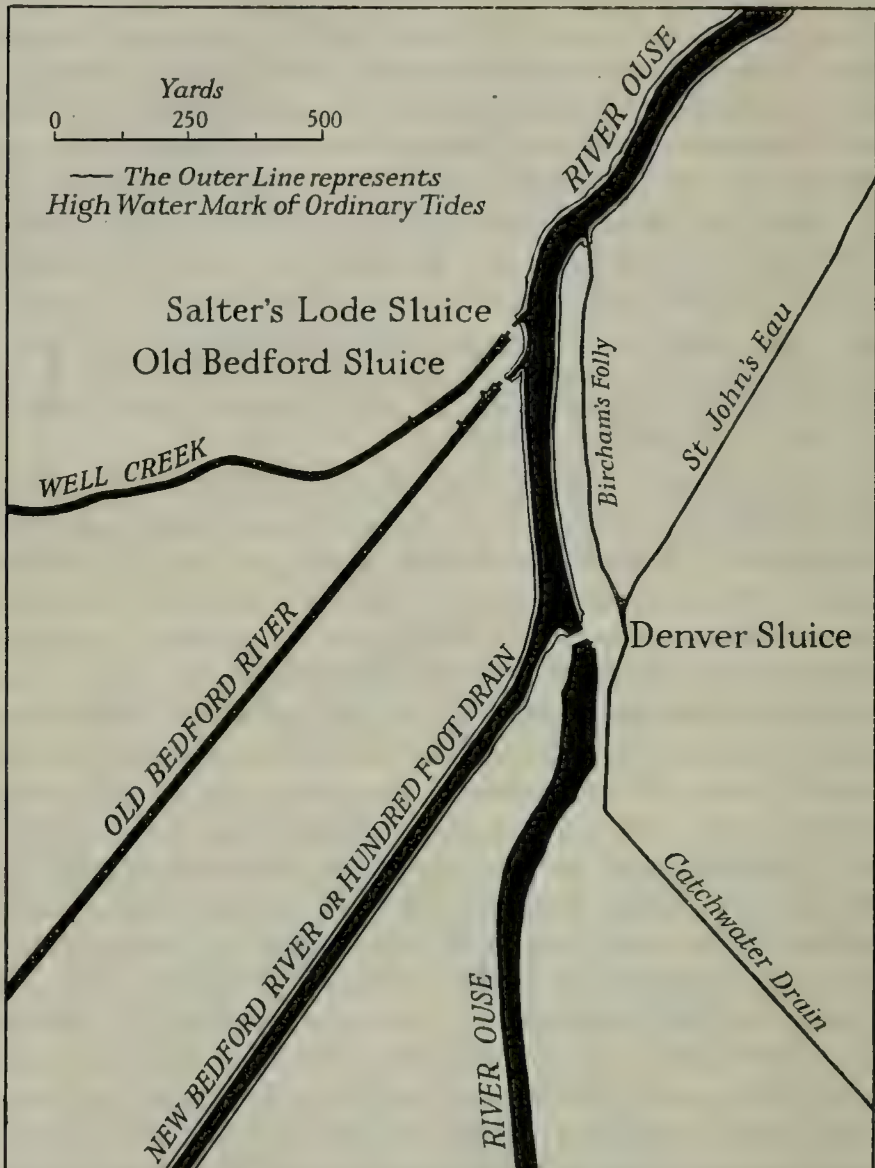

Fig. 52.
The Sluices near Denver.

water from the two Levels.[1] But the nineteenth century brought no solution to this problem.

Important features of the outfalls were the sluices necessary to prevent the tidal waters from passing up the rivers. On the Witham, were

[1] See p. 201 below.

the Grand Sluice and the Black Sluice; on the Nene, the North Level Sluice; on the Ouse, the St Germans Sluice and Denver Sluice.[1] Upon Denver Sluice depended the safety of the South Level. Its importance will be apparent from E. G. Crocker's summary in 1913: "The level of the top of the banks is from 12 to 13 feet above O.D. whilst an ordinary spring tide rises to 14 feet above O.D. at the sluice, the highest recorded tide being 17·51 feet above O.D., so that should anything occur to prevent these gates closing in a spring tide, practically the whole of the Fens of the South Level would be flooded." There was a further complication. Immediately below Denver Sluice, the waters of the Hundred Foot River (carrying the upland Ouse from Earith) fell directly into the estuary (see Fig. 52). Consequently, when there was a great volume of upland water passing down the Hundred Foot River, the level of water in the Ouse outfall (on the seaward side of the sluices) never fell low enough for the sluicegates to be opened to provide an adequate run-off for the waters of the South Level. These waters could only accumulate within the straining banks of their dykes and drains. A crisis was, therefore, always liable to be produced by the combination of (1) adverse wind conditions, (2) a high spring tide and (3) heavy land floods.

Bibliographical Note

The older works of most general interest are:

(1) S. Wells, *The History of the Bedford Level*, 2 vols. (1830).
(2) S. B. J. Skertchly, *The Geology of the Fenland* (1877).
(3) S. H. Miller and S. B. J. Skertchly, *The Fenland Past and Present* (1878).
(4) S. Smiles, *Lives of the Engineers* (1st ed. 1861), has much interesting material.
(5) Sir William Dugdale's *History of Imbanking and Drayning* (1662). This is, of course, the classic account of the seventeenth-century draining.

The following more recent accounts give detailed sources for the facts recorded in this chapter:

(6) H. C. Darby, "Windmill Drainage in the Bedford Level", Official Circular No. 125, Brit. Waterworks Assoc. (1935); also *The Engineer*, clx, 75 (1935).
(7) H. C. Darby, "The Draining of the Fens A.D. 1600–1800" in *An Historical Geography of England before A.D. 1880* (1936).
(8) H. C. Darby and P. M. Ramsden, "The Middle Level of the Fens and its Reclamation", *Victoria County History of Huntingdon*, iii, 249 (1936).

[1] In addition to the sluices at the outfalls of the Old Bedford River and Well Creek (see Fig. 52).

CHAPTER FOURTEEN

MODERN DRAINAGE PROBLEMS: 1850–1938

By Oscar Borer, B.E. (N.Z.), M.INST.C.E., A.M.I.MECH.E.

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DESPITE THE ADVENT OF THE STEAM-ENGINE IN THE nineteenth century, it was reported that windmills were still used in parts of Norfolk in 1913; indeed, at Soham Mere, in Cambridgeshire, a windmill still supplements the steam plant. But these are exceptions. In general, the last hundred years have witnessed many changes in fen pumping. After the introduction of the centrifugal pump in 1851, the scoop-wheel was gradually discarded, but not before its diameter had in many cases increased to 36 ft., and even to 50 ft., to accommodate the lowering surface of the land.[1] Its efficiency, however, was always low, being in the region of about 30 per cent. Still, scoop-wheels were old friends, and with all their splashing they had handled large quantities of water against low heads.

There have also been other changes in the steam-driven plants. Gradually, more modern types replaced the beam and the old low-pressure steam-engine.[2] Thus at Prickwillow (near Ely) a new pumping engine was installed in 1897 to replace a side-lever condensing engine of 60 nominal horse-power. This had been erected in 1833; it had used steam at a pressure of 6 lb. per sq. in.; and it had driven a scoop-wheel 33 ft. 6 in. in diameter. The capacities of the old and new machinery are of interest:

Type of plant	Date	Steam pressure (lb. per sq. in.)	Revolutions of engine (per minute)	Lift in ft. in.	Water lifted (tons per minute)
Side-lever condensing engine driving scoop-wheel	1853	6	25.5	9 10¾	68·9
Vertical compound condensing marine engine direct-driving horizontal centrifugal case-pump	1897	76	132	13 4	153·2

[1] See p. 186 above.

[2] For a description of some of the older types of engines, see R. W. Allen, "Modern Pumping Machinery for Drainage of the Fens", *Proc. Inst. Mech. Engin.* (1913), p. 787.

The increase in lift, owing to the lowering surface, stands out. By 1913, this had become 15 ft. 4 in.—or an increase of nearly 6 ft. over a period of 80 years. Some of the older engines still exist to-day, standing near the modern pumps. There is one at Upware; another (installed about 1840) stands alone in the Glassmore district of the Middle Level.

Crude oil or Diesel engines were introduced for pumping purposes about the year 1913. The weight per unit horse-power is much less than for steam—a great advantage in an area of soft earth where foundations are expensive. There are, however, other advantages of special moment. Most of the pumping stations are, of necessity, near river banks and hence away from hard roads. The cartage of coal, particularly if extra supplies are required during winter, is expensive. But the great loss with steam comes in starting-up and stand-by costs. Pumping for drainage is mainly seasonal, and even then spasmodic. With heavy rainfalls, the pumps must be under way by the time the water has percolated into the drains. When this water has been discharged from drain to river, the engine must ease down, or even stop, for some hours until the drains fill up again. With a steam-driven plant, steam must first be raised in the boilers in anticipation of pumping; then, when the pump has shut down, the fires must be either banked or drawn until the plant is required again. This involves unnecessary fuel consumption and may at times require the services of an extra driver. On the other hand, the use of oil-driven plants does mean that, in the event of hostilities, provision will have to be made for a supply of fuel-oil. It is possible, therefore, that the steam-engine could be kept, with advantage, as a stand-by, and this is the practice adopted by some of the best managed Internal Boards.

The pumps installed since 1919 show an increase in horse-power and capacity, not entirely accounted for by the necessary increase in lift. The average lift in 1913 was between 7 and 15 ft. Higher lifts are more generally met with to-day, rising to 21 ft. for the plants in the Swaffham, Bottisham, Littleport, and Downham districts, and even up to 26 ft. in the North Side district, Wisbech. This increased lift is only partly accounted for by the lowering surface; it is also due to the modern practice of deepening and enlarging the drains so that the water can be kept lower. This provides greater storage capacity, and at the same time gives better drainage. Some of these leading drains are of considerable size, so that the pumps need not run so frequently.

It is difficult to generalise about the size of pumps used. About 1926, the size rose to 42 in. pumps with a capacity of 150 tons per minute; requiring about 250 horse-power; but more recently the size of pumps installed has fallen to 24 in. with a capacity of 70 tons per minute. This is a reasonable size; the smaller unit is more economical for normal use, while the possibility of duplicating the plant allows for additional safety.

An outline of pumping installation in the Littleport and Downham district, a well-managed Board, gives some idea of the constant struggle to maintain internal drainage. The district has a taxable area of 26,000 acres, but the total area drained is of the order of 35,000 acres. It includes approximately 26 miles of drains. The district was among the first to adopt steam-driven pumps. A 30 h.p. engine and scoop-wheel was originally erected on the Ten-Mile River bank; this was increased to 80 h.p. in 1843. A similar engine had been installed on the Hundred Foot River bank in 1829, seven miles away. Both these were condensing beam-engines, the steam pressure being 15 lb. per sq. in., and the scoop-wheels about 41 ft. in diameter. Owing to the lowering surface, these scoop-wheels were increased in size to 50 ft. in 1882, and they weighed 75 tons each. In 1912, at the Ten Mile station, the Commissioners installed two double-acting open compound condensing engines of 200 h.p. directly coupled to 48 in. Allen pumps, each with a capacity of 150 tons per minute. While in 1914, at the Hundred Foot River station, a 400 h.p. steam-engine was installed to drive a 50 in. Gwynne pump handling 212 tons per minute against a total head of 21 ft. This was supplemented in 1925 by a Mirrlees Diesel engine with a 36 in. Gwynne pump lifting 110 tons per minute. Finally, in 1937, the Ten Mile set was further improved; 340 h.p. Allen engines replaced the older unit, but the existing pump casing was maintained.

Irregular surface lowering may cause an entire change in the direction of the drainage. That has been the fate of the Methwold and Feltwell Board. To aid the original drainage through Sam's Cut, a pump was installed in 1883 where the cut joined the Ten Mile River at Hunt's Sluice.[1] In 1913, it was felt advisable to erect an additional pump. This was the first crude oil engine in the Fenland, and was installed by the Campbell Gas Engine Co. By 1928, however, this ancient system had to be abandoned, and the drainage was taken across country to the River Wissey. A Mirrlees engine and an Axial flow pump (another newcomer) was installed, and this has now been supplemented in 1938 by two Allen engines and pumps, developing, between them, a total of 260 h.p. and capable of pumping 170 tons per minute. This improved drainage has necessitated a deepening and widening of the drains, which now flow in an opposite direction to the original layout. Certain drains and culverts have had to be abandoned because they can no longer function owing to the lowering of the peat surface.

[1] See p. 189 above.

ADMINISTRATION

The outstanding difficulty of the past has been the lack of a single controlling authority and the absence of co-operation amongst existing authorities. The problem of finance has always been a very great stumbling-block to concerted action. In 1850, there were three principal bodies controlling the southern Fenland: (i) The Bedford Level Corporation controlling the North Level; the Hundred Foot and Old Bedford Rivers; and the Middle Level. (ii) The South Level Commissioners controlling the South Level as then defined. (iii) The Eau Brink Commissioners controlling the remaining portions of the Tidal River (below Denver), but with rights vested in the several *ad hoc* authorities.

In 1858, the North Level separated and now falls principally within the purview of the Nene Catchment Board. In 1862, the Middle Level separated from the Bedford Level Corporation. Bills were introduced into Parliament in 1877, 1878, 1879, and 1881, concerned with the idea of setting up Conservancy Boards, but difficulties of rating were among the main reasons that prevented their establishment.

The functions of the Eau Brink Commissioners were, in general, divided between the Ouse Banks Commissioners, the Lower Ouse Drainage Board, the Ouse Outfall Board, the Denver Sluice Commissioners, and the South Level Commissioners. In addition, there existed the Norfolk Estuary Company, which built and controlled the Marsh Cut and training walls; and also there was the King's Lynn Conservancy Board, mainly concerned with the port of King's Lynn and its navigation. In 1920, the functions of the Bedford Level, South Level, Denver Sluice Commissioners, Ouse Outfall Board, Lower Ouse Drainage Board, and the Ouse Banks Commissioners, were transferred to one Authority termed the Ouse Drainage Board, which had powers of direct rating over the area controlled.

Under the Land Drainage Act of 1930, the River Great Ouse Catchment Board was instituted to take over the functions of the Ouse Drainage Board. It also had control over the remaining portions of the River Ouse Catchment Area (excluding the Norfolk Estuary Company and the King's Lynn Conservancy Board), but without the powers of direct rating. The area covered by the Board is over two million acres, having a rateable value of over £3,350,000, and running into twelve administrative counties. Its income is derived partly from precepts laid on the Internal Boards as far as may be considered fair, and partly from precepts laid on the County Councils, with a statutory limit of 2*d*. in the £. It has been found necessary to levy this full rate, as the total income from the two sources is not sufficient to meet the onerous duties which fall on the Board.

The area covered by the Internal Drainage Districts is approximately 276,063 acres, with an annual value, for drainage rates, of about £635,000. These Internal Drainage Boards, which number 90, are responsible for local drainage.

The total mileage of main river for which the Board is responsible amounts to about 500 miles, of which 306 are in the Upland Area, and 190 in the Fenland itself. Work in the Upland Area rivers is similar to that carried out by other Catchment Boards, but with the strict necessity of bearing in mind the fact that these rivers discharge into the Fenland. In upper Cambridgeshire, the Board is responsible for the Rivers Cam and Rhee, which are now kept in as satisfactory a state as the funds will permit. The fenland areas are roughly at Ordnance Datum level, and the upland streams are carried through this area as embanked rivers above the level of the land on either side. The Internal Drainage Boards have, therefore, to pump the water up into the river above their own ground, and the pumps must be capable of lifting to a height sufficient to reach the flood levels within the banks.

THE MIDDLE LEVEL

The Middle Level lies between the Old Bedford River and the River Nene (see Fig. 47). It contains 165,000 acres of ground, of which about 120,000 acres are actual fen with 150 miles of main waterways. Most of its fifty Internal Districts discharge by pumping into the Middle Level Drainage System, for which the Middle Level Commissioners are responsible. But the area of the Sutton, Mepal, Manea, and Welney Internal Boards has separate pumping stations discharging into a Counterwash drain that runs parallel with the Old Bedford, separated from it by a low bank. This drain has a gravity outfall just below Denver Sluice.

Some portions of the Middle Level fall to 4 ft. *below* Ordnance Datum; and it must be remembered that the high-tide levels outside rise to something of the order of 13 or 17 and even to 18 ft. above O.D. Prior to 1848, the Level discharged by Tong's Drain into the Ouse below Denver; but, on the advice of Messrs Burgess and Walker, a twelve-mile cut was made through Norfolk Marshland, and the Middle Level waters discharged into the Ouse through a new sluice at St Germans, eight miles below Denver Sluice. The cost of this scheme amounted to £450,000.

The reduced low-tide levels that resulted from these measures proved satisfactory for many years. In 1862, however, the sluices at St Germans "blew up"; the river banks broke down, and tides flowed up the cut to flood some 6000 acres. Sir John Hawkshaw devised a dam with a series of

16 syphons, each 3 ft. in diameter. This arrangement lasted until 1880, when a new sluice was erected to take the place of the syphons; the total cost of this disaster, and the improvement of 1880, amounted to £250,000.

By 1912, low water in the Ouse was not as low as it had been in 1880, due to a general deterioration of the river and its outfall into the Wash. In the meantime, the old scoop-wheels had been replaced by modern pumps so that the water had to be got away more quickly. The floods of 1916, 1923, and 1926 confirmed the Middle Level Board in its opinion that the position in time of heavy flood was becoming more dangerous. In 1923, Major R. G. Clark, as Engineer to the Commissioners, recommended the installation of improved sluices; but by 1928 it was decided to install a pumping station at St Germans, and this, after due negotiation, was completed in 1934. The new sluice has only two sluice gates each 35 ft. in width, thus securing 50 per cent greater width for discharge than in 1880. These sluices are assisted by three pumping units, and provision has been made for the installation of a fourth. The pumping plant was erected by the Premier Gas Engine Co.; each unit consists of a horizontal eight-cylinder Diesel engine developing 1000 h.p. driving a Gwynne centrifugal pump 8 ft. 6 in. in diameter, capable of discharging up to 1000 tons per minute at low heads or 840 tons per minute against a static head of 10 ft.

THE SOUTH LEVEL

The problems of the Middle Level, intricate as they are, are very much simpler than those of the South Level. Those of the Middle Level are concerned mainly with the rainfall that falls on its own area, while the South Level has to arrange for the drainage of nearly one-half of the catchment basin of the Ouse.

Before the institution of the Ouse Drainage Board, the maintenance of banks was the responsibility of the Internal District Commissioners. During high floods, the better drained and richer areas were fairly well protected, because they had been able to maintain their banks in a satisfactory condition; but Internal Districts, whose financial position was not so strong, were liable to breaches in the banks. Some districts were flooded at frequent intervals; Hockwold Fen, for example, was drowned in 1912, 1915, and again in 1916. In 1919, three breaches occurred in the River Cam, and in 1928 there was a serious breach in the right bank of the River Wissey, when some 2000 acres were flooded. It was about this date that the Ouse Drainage Board received a grant of £276,000 from the Ministry of Agriculture to enable it to carry out extensive dredging and embanking throughout the South Level.

By 1934, this sum had been expended, and it became necessary to prepare a supplementary scheme of £103,000 to carry on the work. Early work under both these schemes was mainly confined to dredging. The dredged material was not of much use for embankment work, and for this purpose the Board, following the practice of the Internal Boards, obtained its clay from the Roswell Pits near Ely. Photographs of the pits in 1913 show work being done by hand, but gradually operations have been mechanised and fully organised. It has also been found economic to open up subsidiary pits. As a general rule, the banks are heightened and breasted or faced with clay to prevent them being washed away under wave action caused by high winds on the flood waters.

In floods prior to that of 1937, one bank at least had always broken. But during the floods of March 1937 no breach of any consequence took place, so that water levels in the streams rose higher than hitherto. The danger is that these high-water levels create a head sufficient to force water *under* the banks. It is therefore felt advisable to strengthen the banks, and future work will carry the clay breasting down the front of a bank by trenching on to the clay below. Previously, in weak places, this has been done by hand; but now, with a new and more extensive programme, it is being undertaken by trenching machines.

The floods of 1936 and 1937 yielded much valuable data, from which it has been possible to re-design the section for the main river from Littleport to Denver, to which all the other rivers in the South Level are tributary. This stretch is to be widened and some half a million yards will be dredged away. It is calculated that this widening will reduce flood conditions at Littleport, when the river is discharging, by a matter of 10 in. This in turn will provide greater storage capacity for the periods when Denver Sluice is closed by tidal waters.[1] The cost of the scheme will amount to £266,000, and a 75 per cent grant has been obtained from the Ministry.

The Hundred Foot River and the Old Bedford River are also receiving attention. The Middle Level Barrier Bank, which protects the Middle Level area from the flood waters from the Uplands, was heightened under a scheme completed in 1933. The Old Bedford River, too, is now being improved; and, consequent upon damage during the unprecedented flood of 1937, most of the Middle Level Barrier Bank is being protected with clay at the cost of about £60,000.

Denver Sluice was partially remodelled in 1923, when one large eye, 34 ft. in width, was installed instead of two smaller discharging sluices.

[1] See p. 193 above.

The result is that Denver Sluice can now take the full discharge from the South Level without any loss of head.

The outlet from the Washlands between the two Bedford Rivers is by means of Welmore Lake Sluice.[1] This was rebuilt in 1930, and subsequent observations, taken during flood conditions, have shown the advisability of increasing by 50 per cent the discharge capacity at this point, by the installation of a third sluicegate 24 ft. wide, which it is hoped will be constructed next year.

THE TIDAL RIVER SECTION

The so-called tidal river section (i.e. the estuary below Denver Sluice) has been the subject of much controversy and of many reports during past centuries. The Eau Brink Cut of 1821 and the Marsh Cut of 1852 were especially successful because they shortened the length of the river.[2] Much benefit also resulted from the activities of the Norfolk Estuary Co., which was compelled by an Act of Parliament to carry training walls through Vinegar Middle Shoal (in the estuary) before it commenced reclamation. The walls were completed in 1857, by which time the company had spent £250,000 on the improvement of the estuary. The Norfolk Estuary Co. was intended originally to recover land from the Wash. Fig. 53 shows the result of its activity and of similar effort in Lincolnshire during the nineteenth century.

After a series of flood years during the nineteenth century, Mr W. H. Wheeler was consulted by the Denver Sluice Commissioners; and his report, issued in 1883, recommended that the river should be widened from Denver Sluice down as far as the Eau Brink Cut. The Eau Brink and Marsh Cuts were, apparently, in very good condition at that time. There were comparatively low-water levels under normal conditions, but the river was not wide enough to deal with flood waters—hence the necessity for the report. Had Mr Wheeler's scheme been carried out at the time, the result should have been very satisfactory, but unfortunately the banks and channels of the Wash were changing, and, in consequence, the estuary conditions have become steadily worse.

Comparison between Fig. 54 and Fig. 55 will show the change in the channels of the Wash between 1871 and 1936. The tide on the eastern side of the Wash follows a circulatory movement in an anti-clockwise direction and the channels follow this tidal flow. Thus, the water flowed in by the Lynn Channel and, following this circulatory motion, discharged by the Bulldog Channel. The channels were then well defined. At some later

[1] See footnote 4, p. 182 above. [2] See p. 191 above.

period the Teetotal Channel widened and the Daseley Channel broke through, while at the same time, for some cause which is as yet unknown, the inward end of the Bulldog Channel began to silt up. The full implication of these changes is not yet known; further investigation is now proceeding.

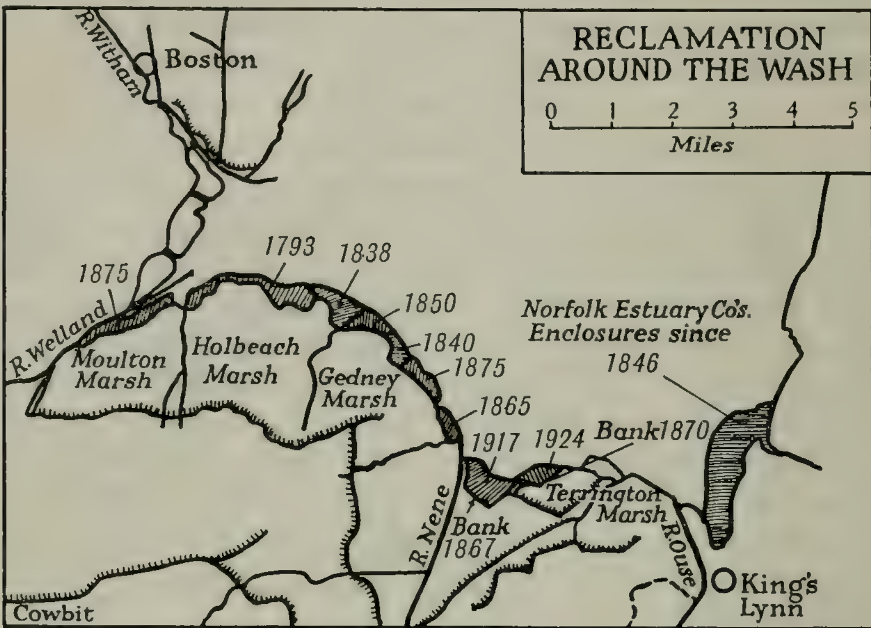

Fig. 53.

The coastal "marshes" were recovered before the nineteenth century. The inner banks, marked by toothed lines, represent the limits of still earlier enclosures.

Further flooding of the Fenland occurred during the period of the Great War, and, at the request of the Lower Ouse Drainage Board, Mr Havelock Case issued a report in 1917. Mr Case, like Mr Wheeler, recommended the widening of the river from Denver to the sea; he also advocated the installation of a larger sluice at Denver, which, as noted above, was not carried out until 1923. He also found that the conditions in the Wash were so bad as to require the further construction of training walls.

The position was again discussed in 1918, when Mr Preston held an enquiry at King's Lynn. A proposal was put forward for the construction of a barrage instead of training walls, but the enquiry showed that there was not sufficient technical evidence to make a decision.

In 1925, a Commission of enquiry was set up by the Ministry of Agriculture, and their technical adviser, Mr Binnie, put forward a scheme for training walls. He believed that the training walls should be sufficiently high to carry the river water through to the Hull Sand Beacon, some

5 miles out to sea. His contention was that the silting of the river was due to material in suspension and that this should be excluded. A further report was subsequently prepared by Sir Alexander Gibb, but the financial burden was impossible for the Ouse Drainage Board to carry.

By 1930, the original short training walls had seriously deteriorated. But the Ouse Drainage Board felt that it could not finance any improvement, which was, therefore, undertaken by the Lynn Conservancy Board with Government assistance. The principle adopted was to protect the toe of the existing walls by means of brushwood mattresses and to heighten the tops by a combination of brushwood and stone. This particularly suitable method of repair was put forward by a firm of contractors of Dutch origin, who were operating in this country. Subsequently, the firm called in two eminent Dutch engineers, and the result was the so-called "Dutch Scheme", which was considered by the Labour Government in 1931. By this time, conditions in the estuary had become so bad that it was estimated that it would cost five and a half million pounds to put it right.

The "Dutch Scheme" provided, as before, for the widening of the river from King's Lynn to Denver and Welmore Lake Sluice. It also advocated the cutting through of Magdalen Bend and the widening of the Hundred Foot River. Because the upper section of the tidal river had probably been silted up by material coming in from the Wash, it was proposed to construct a set of sluices across the Hundred Foot River in the neighbourhood of Welmore Lake Sluice. From here down to the sea the toe of the banks was to be protected by mattresses, and their slopes pitched with concrete blocks. To take the river out to deep water, training walls over 5 miles long were suggested. The lower portion was to be made with mattress work, and the upper portion was to consist of caissons of concrete blocks. The height was to be brought up to at least neap-tide level, and, in order to regulate the depth of channel, a series of groynes was to be constructed inside the new walls. Although the Government offered a 90 per cent grant, it was again felt that the Catchment Board which had just come into existence, in 1930, could not face the financial burden.

But, at this time, the Catchment Board had to face the reconstruction of training walls for a length of 1 mile on the eastern side. This had to be done under difficult financial conditions. Work was undertaken in 1932 and cost the Board £85,000. It was felt advisable to reconstruct a new wall slightly behind the old wall, and to carry it up to a somewhat higher level.[1]

During the dry summers of 1934–35, silt travelled[2] steadily up-river

[1] Both the east and the west training walls were extended a short distance in 1937.
[2] See p. 189 above.

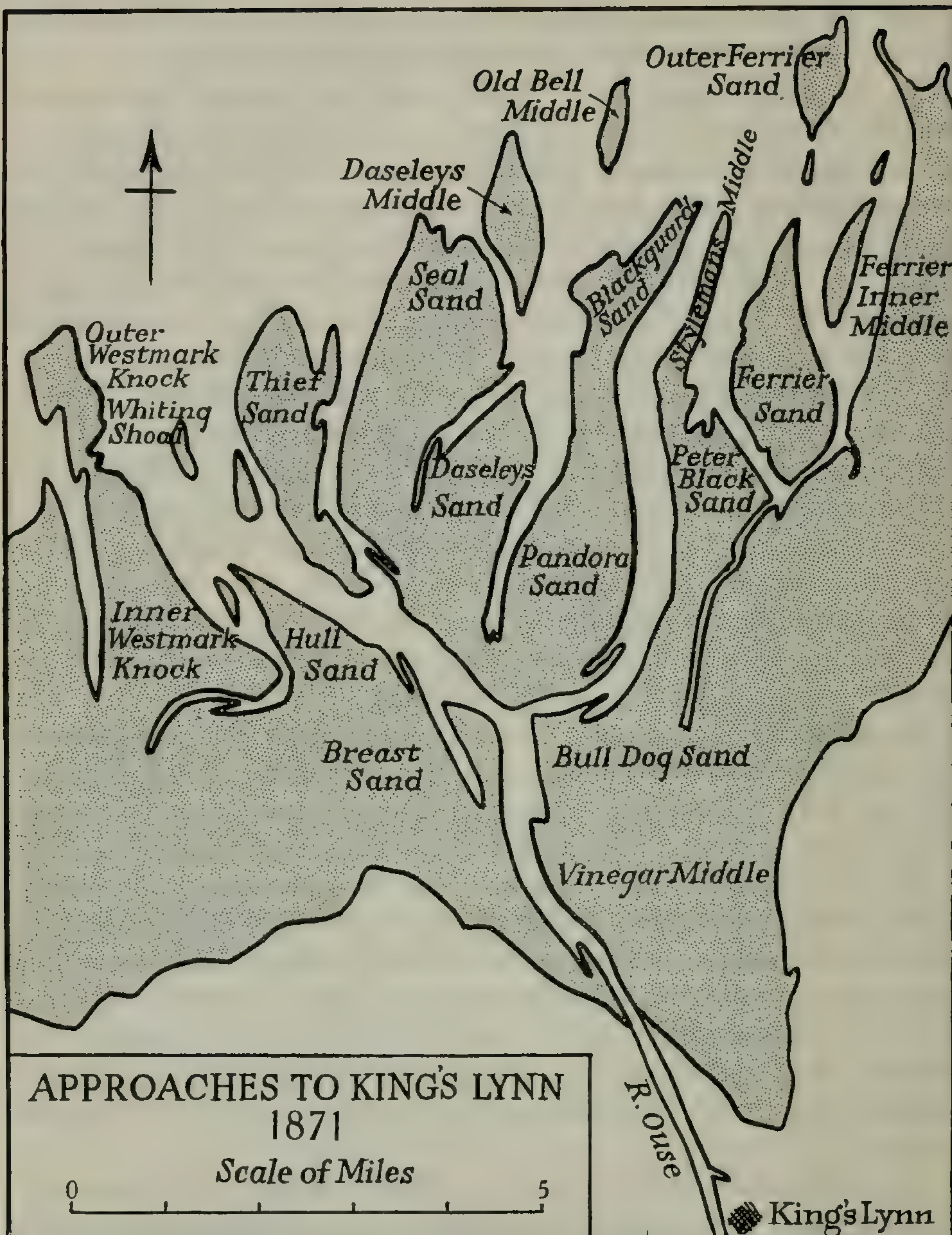

Fig. 54.

Based upon British Admiralty Charts.

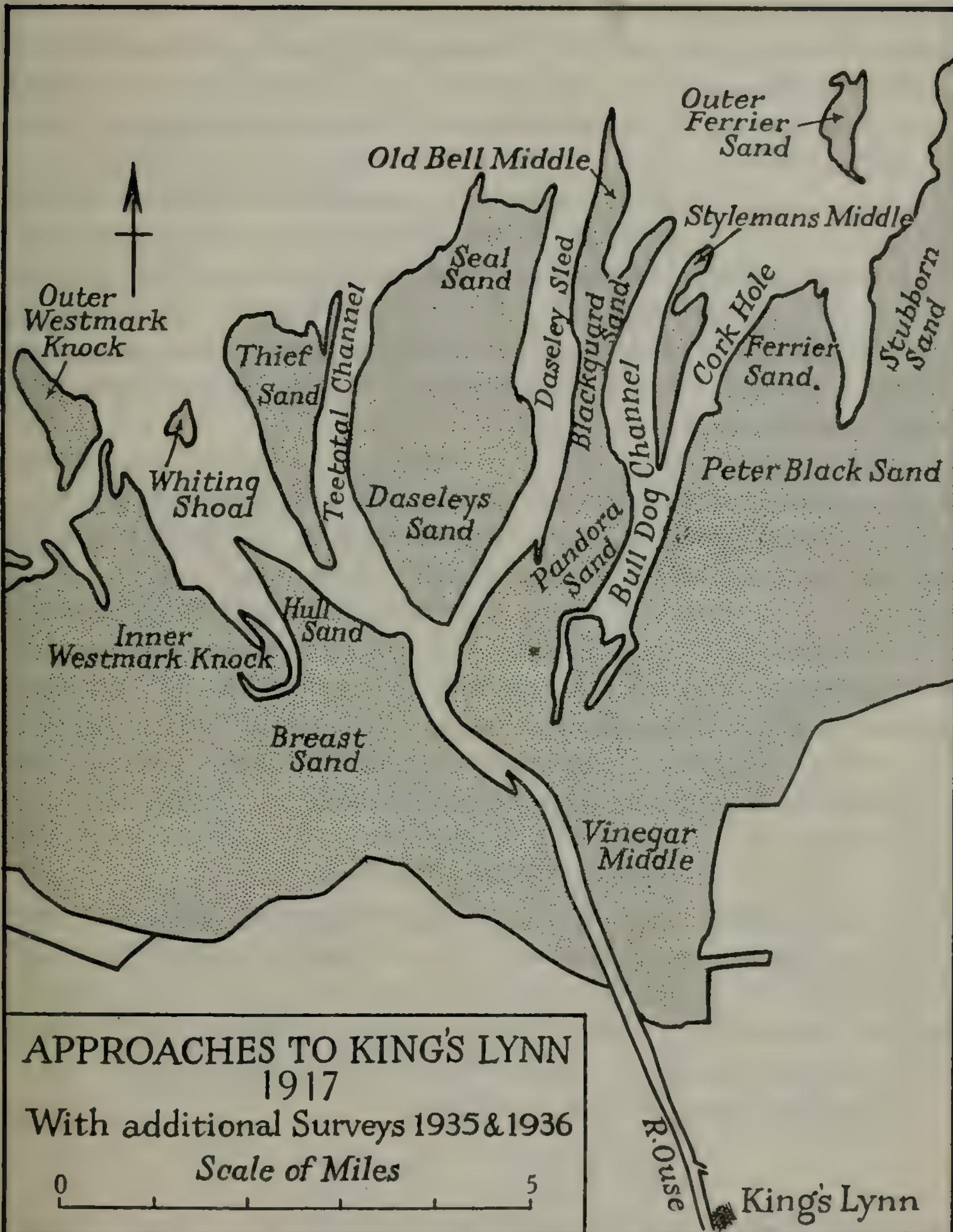

Fig. 55.

The 1917 data were based upon British Admiralty Charts. The data of 1935 and 1936 were obtained from the surveys of the River Great Ouse Catchment Board.

until the bed at Denver had risen some 8 ft.; indeed, at certain inlets the bed had risen a matter of 13 ft. This meant a blocking-up of the river by over a million cubic yards. The flood conditions of 1936 scoured out a certain amount of this accumulation, otherwise the flood of 1937 would have been disastrous. The general effect of this heavy flood of 1937 was to scour down the river bed for a distance of about 8 miles below Denver. A great quantity of silt must have been carried out to sea, although the percentage of silt in suspension in the river current in the Wash seems to have been somewhat low. A vast quantity of silt, however, did not reach the sea, but was deposited in Marsh Cut, which, due to neglect, has been steadily widening since its original construction. In fact, the section of the Marsh Cut is now about 60 ft. wider than it was in 1860, and its bed has risen some 8 to 10 ft.

The bed of Marsh Cut is now higher than the bed of the river upstream; and, in order to remedy this state of affairs, the Board (with the assistance of the Ministry) has agreed to a scheme (1) for lining the banks with stone pitching to prevent further erosion, and (2) for the construction of groynes throughout the length of the Cut to provide a narrowing channel at low-water level. This work, extending for about 4 miles to the Free Bridge at King's Lynn, is being done by contract at a cost of one-quarter million pounds. Although, as the result of deposition, the bed between the training walls out to sea rose a certain amount, it is still lower than that of the Marsh Cut, thus showing the benefit of the training walls.

THE TIDAL MODEL

Since 1932, the Catchment Board has been engaged on an investigation of the many problems of the estuary. For this purpose, a large tidal model has been built at Cambridge; this, at the time of its construction, was the largest tidal model in the world. It is essentially a model of the Wash out to a line drawn from Hunstanton to Friskney Flat near Skegness.

The technical particulars[1] are as follows:

Horizontal scale	... 1 : 2500
Vertical scale ...	... 1 : 60
Vertical exaggeration	... 1 : 41·7
Time scale ...	... 1 : 324
Velocity scale ...	... 1 : 7·7
Tidal period ...	... 138 seconds
One year of tides	... 27·1 hours

The tides are produced by a plunger, weighing 14 tons, which displaces water from a large trough; the water, flowing over the model area of the

[1] These particulars are taken from the brochure issued in connection with the Model.

Wash, correctly reproduces the tide. The mechanism operating the plunger incorporates a special cam which enables the production of the correct tidal cycle to be obtained. The flow from the various rivers is controlled by valves which may be set, so that a correct volume of water flows down the model channels which accurately represent the actual rivers.

The correlation of the model hydraulic conditions with those found in nature was undertaken first from data collected by the Admiralty in 1917, and again from data collected by the Board in 1935. The model was moulded to reproduce these conditions respectively. The satisfactory results of these correlation tests enabled those in charge of investigations to take a step forward, and to determine the changes in hydraulic conditions (and in the configuration of the channels) likely to result from schemes carried out in the rivers of the Wash. Tests of certain proposals have been made.

Results have shown that the problem is much more involved than appears at first sight. A careful study is being made not only of the conditions in the Wash, but of similar works which have been undertaken elsewhere, particularly in Holland and Germany. Investigation has been somewhat handicapped by the lack of finance. For the design of the groynes in the Marsh Cut, together with other alterations proposed in the section running through to Denver, a secondary model is under construction. This will have a horizontal scale of 1 : 240 and a vertical scale of 1 : 100. As the model includes the length of river up to Denver, it will be about 360 ft. long, and must be constructed in the open air. The plunger for the creation of the tide weighs $4\frac{1}{2}$ tons; and the tidal period will be 31 minutes. This secondary model will enable experimental work to be done on a larger scale.

CHAPTER FIFTEEN

THE BRECKLAND

By R. R. Clarke, J. Macdonald, and A. S. Watt

(A) HISTORICAL AND ECONOMIC BACKGROUND

By R. R. Clarke, B.A.

BRECKLAND IS A NATURAL REGION UNIQUE IN BRITAIN BUT paralleled in Western Europe by the heaths of Denmark, Holland, north-west Germany, and the Rhine Valley.[1] Roughly speaking, it covers some 400 square miles in the counties of Norfolk and Suffolk, while its south-western extremity impinges on the eastern boundary of Cambridgeshire.[2] The precise limits of the region are not easy to define, for, save on the south-west where it marches abruptly with the peat and clay of the Fens, the Breck district shades imperceptibly into the regions of gravel and chalk that elsewhere surround it. The borders of Breckland therefore present mixed physical characters with many outliers (Fig. 57), but Fig. 56 will serve to indicate the location of the main area.

This main area is mostly a low plateau rising between 100 and 200 ft. above sea level. It owes its geographical personality to a remarkable pall of sand that covers its complex sub-soil of chalk, gravel, sand, loam and chalky boulder clay out of which the lime has been dissolved by rainwater. The Breck soil is, with a few insignificant exceptions, arid and highly permeable. Combined with a relatively dry climate,[3] these characteristics have produced its peculiar vegetation and fauna, and have controlled human activity in the district. But for human agency, Breckland would be, under present climatic conditions, largely a treeless steppe, and any woodland that might flourish would be open in character and free from scrub. Besides its characteristic vegetation, and insects,[4] the Breckland heaths are important ornithologically,[5] and form one of the chief strongholds of the stone curlew and the ringed plover, while other rare birds nest by its heathland pools and meres.

In this arid region, human settlement is very dependent upon water

[1] Full bibliographies of recent work on the district are contained in: (1) W. G. Clarke, *In Breckland Wilds*, second revised edition by R. R. Clarke (1937); (2) H. Schober, *Das Breckland: eine Charakterlandschaft Ost-Englands* (Breslau, 1937).

[2] Naturally, any boundary-line must be arbitrary. Country with some "breck" characteristics can be found outside the Breckland proper.

[3] See p. 43 above. [4] See p. 70 above. [5] See p. 62 above.

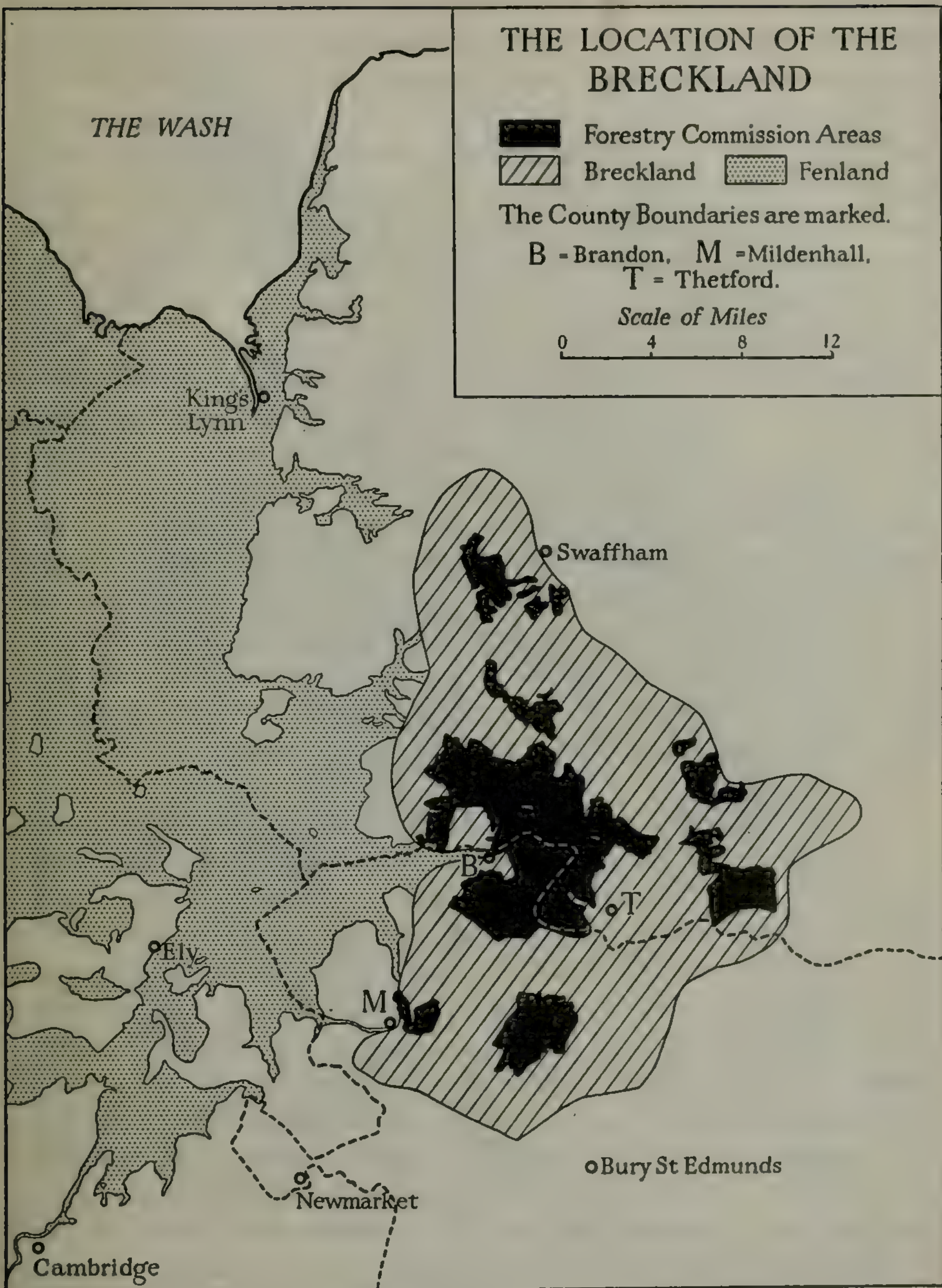

Fig. 56.

This figure shows the main location of the Breckland. Outliers with some typical Breck features occur beyond this arbitrary frontier, e.g. in Cambridgeshire, to the north of Newmarket.

supply provided by the valleys of the Wissey and the Little Ouse-Thet that run through the middle of the district, and by those of the Nar and the Lark, near its northern and southern margins—all of which empty into the Fens and so to the Wash. From prehistoric times, settlement has been focused on these valleys and their tributaries, and only four parishes (Swaffham, Elveden, Ingham, and Wordwell) appear never, in historic times, to have had access to stream or fen or mere. The meres of Breckland provide a tolerable substitute in the absence of rivers. The biggest and most typical of these curious sheets of water lie in five parishes in Norfolk, and the best known are Fowlmere, Langmere, Ringmere, and the Devil's Punchbowl; the largest of all, Mickle Mere ($29\frac{1}{4}$ acres), is near-by in West Wretham Park. With one exception, the water level in these meres has no visible inlet or outlet, and is subject to remarkable fluctuations. At times, the meres are completely dry for several years; at other times, they overflow adjacent roads. There can be little doubt that their waters are derived from the surrounding chalk, and that they rise and fall with the saturation level in the underlying rock. Rainfall is thus solely responsible for their fluctuating levels. Some at least of the meres may have been formed from "pipes" in the chalk filled with drift-sand. The importance of the meres as sources of water is shown by the numerous parish boundaries which meet at them. At Rymer Point, 4 miles south of Thetford, no less than nine parishes meet, and here, formerly, was a considerable natural sheet of water.

The palaeolithic flint implements found in its gravels and brickearths; the important neolithic flint mines at Grime's Graves; the flint implements scattered by the million over the surface of its heaths and arable fields;[1] its extensive mileage of primitive trackways; its impressive dykes and its numerous barrows and other relics of early cultures which are constantly being discovered—all these evidences indicate that in some of the prehistoric periods Breckland must have been one of the most thickly populated districts in Britain.[2] The principal attraction of the region to early man lay in the absence of heavy woods which he was unable to clear. In addition, the margins of the Fenland and of the heathland meres yielded fish and fowl; while, for tool-making, the chalk provided unlimited quantities of the finest flint in Britain. Then, too, the Icknield Way,[3] along the chalk

[1] The working of flint in this district has probably been continuous from prehistoric times. To-day, Brandon supports the last surviving flint-knapping industry in Britain. The mines at Lingheath still produce some of the raw material required for the manufacture of gun-flints—see R. R. Clarke, "The Flint-Knapping Industry at Brandon", *Antiquity*, ix, 38 (1935).

[2] Well shown by the distribution maps of Sir Cyril Fox. See (1) *The Archaeology of the Cambridge Region* (1923), (2) *Proc. Prehist. Soc. E. Anglia*, vii, 149 (1933).

[3] See p. 85 above.

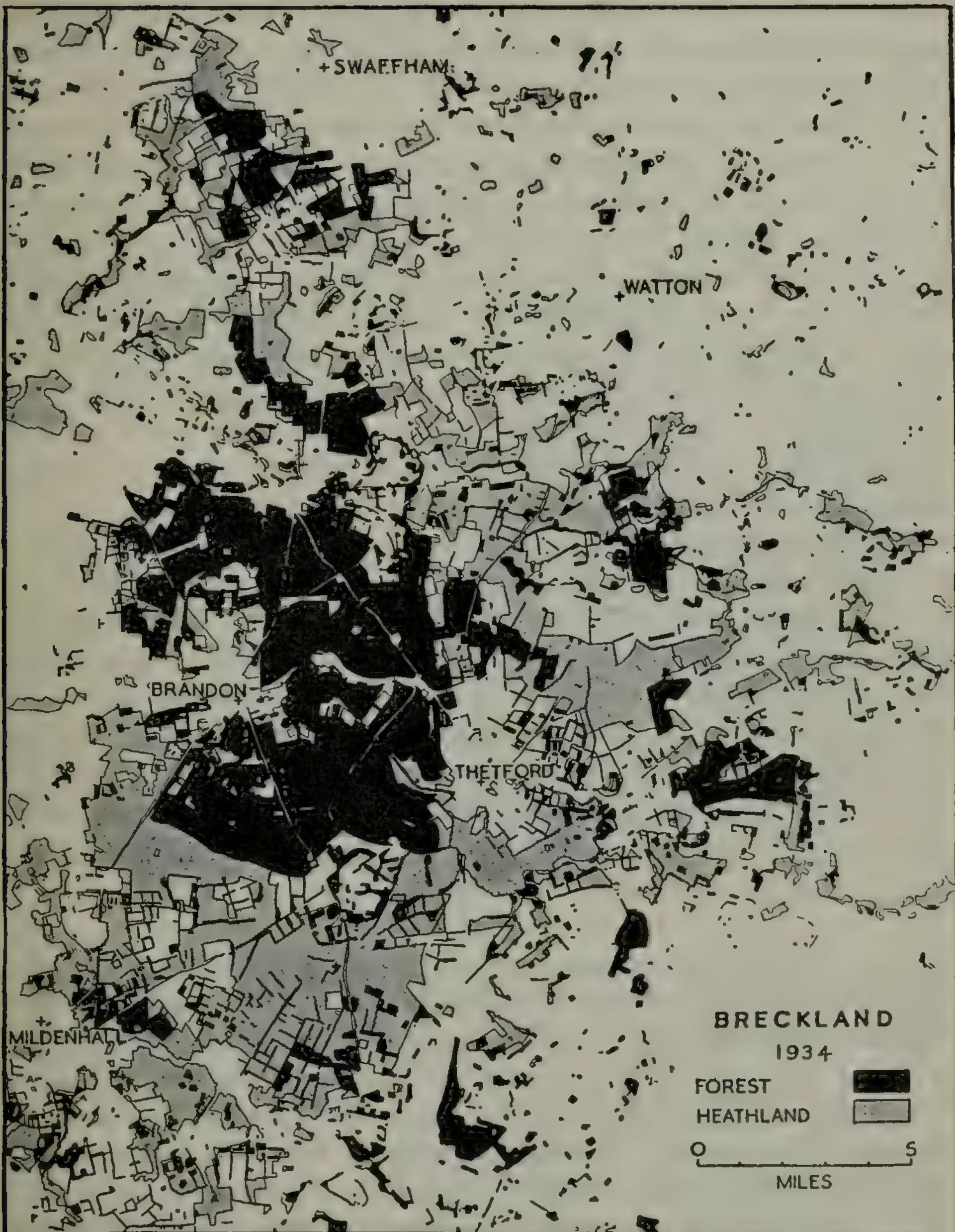

Fig. 57.

Reproduced by the courtesy of the Land Utilisation Survey of Britain.

ridge, offered easy intercourse with the rich cultural province of Wessex. It was probably during the Bronze Age that Breckland became one of the chief centres of population in Eastern England, but as Iron Age man acquired the power to subdue and exploit the more stiff but richer soils of adjacent regions, the cultural focus of "East Anglia" moved south-west leaving Breckland as a backwater for a thousand years. Not until the late-Saxon period did Breckland acquire a new strategic status, when the deforestation of the claylands of Norfolk and Suffolk again swung the economic pendulum north-eastward. Though still a poverty-stricken steppe, as the Domesday Book attests,[1] it was now the gatehouse of a wealthy East Anglia commanding the Icknield Way, still the main line of approach from the civilised south. Although a waste-land it was a frontier zone through which communication was essential. The rise of Thetford[2] to the zenith of its importance as the eleventh-century capital of East Anglia, with its cathedral and its mint, was due to its location on this highway, at the confluence of the Rivers Little Ouse and Thet.

Place-names indicate that most of the present primary settlements of Breckland are of Anglian origin; there are 8 *-ings* and *-inghams*, 20 *-hams*, 13 *-tons*, and 8 *-fords*. The importance of the rivers for water supply is demonstrated by the concentration of these nucleated villages in the valleys. Two villages are associated with the Nar, 28 with the Wissey, 16 with the Little Ouse, 10 with the Thet, and 9 with the Lark, each including its tributaries. Secondary settlements consisting of heathland farms, with their satellite cottages, and isolated houses for warreners and gamekeepers, only came into existence, in most cases, with the growth of enclosures and tree-planting during the nineteenth century.

What were the main features of the economy of Breckland prior to the modern enclosures? Recent investigation has shown that in West Wretham,[3] and also several other parishes in the heart of the region, something akin to the Scottish infield-outfield system was common, though the border parishes are likely to have conformed to the custom of the normal Norfolk and Suffolk village-community. The essence of the system was a division of the arable land of a village into two unequal parts: a small infield probably cropped continuously, near the village; and a larger outfield comprising five to ten temporary enclosures from the waste

[1] See H. C. Darby, "The Domesday Geography of Norfolk and Suffolk", *Geog. Jour.* lxxxv, 432 (1935).

[2] It is interesting to note that the Domesday Book numbers the burgesses of Thetford as 720, compared with 665 at Norwich and 70 at Yarmouth.

[3] J. Saltmarsh and H. C. Darby, "The Infield-Outfield System on a Norfolk Manor", *Economic History*, iii, 30 (1935).

(called brakes, folds or faughs), of which one was broken up every year, cropped continuously for a few seasons (with the aid of sheep manure and marling), and then allowed to revert to its former condition until its turn came to be ploughed again. Fig. 58 shows the fields at West Wretham in the mid-eighteenth century, and illustrates conditions generally. These outfields were large, but few can have equalled those at Northwold in the seventeenth century when men ploughed straight for 12 furlongs.[1]

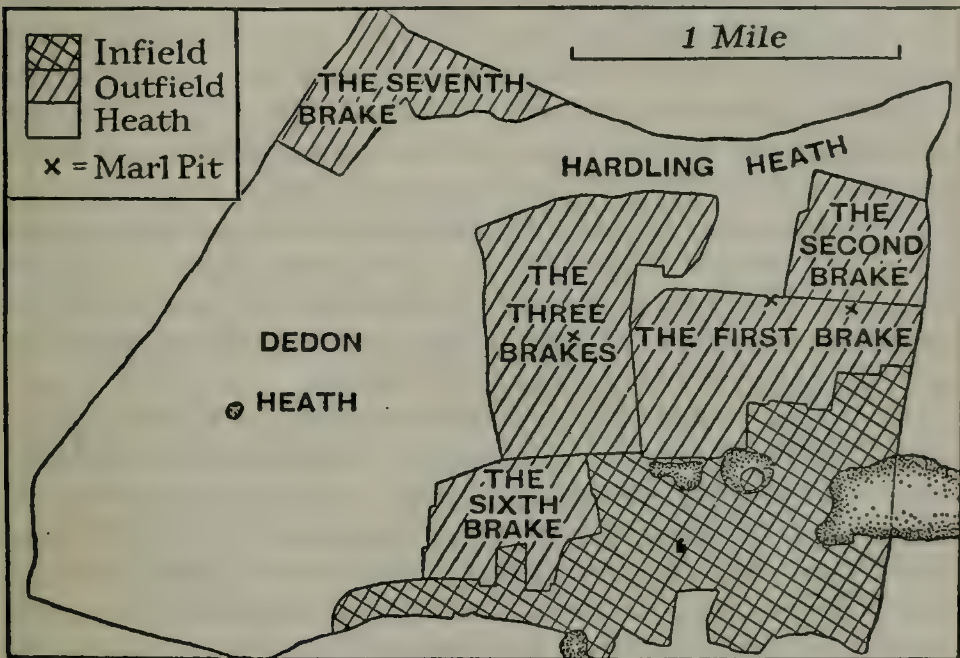

Fig. 58.

Field System at West Wretham (Norfolk), 1741.

From J. Saltmarsh and H. C. Darby, "The Infield-Outfield System on a Norfolk Manor", *Economic History*, iii, 34 (1935). This is diagrammatised from the original map on two sheets of vellum pasted together, and measuring $53\frac{1}{2} \times 36\frac{1}{4}$ in. It is preserved in the Muniment Room of King's College, Cambridge.

According to W. G. Clarke, "parts of almost every area of heathland were at one time cultivated, but have become derelict. Both these areas and the large sandy open fields are known as 'brecks', and their number, and the fact that they are characteristic of all parishes, induced me in 1894 to give the district the name of Breckland."[2] Thus it seems that the name by which the whole area is known may mean nothing other than "the land of outfields".

[1] Mentioned by Sir Philip Shippon, 1671, *Norfolk Archaeology*, xxii, 176 (1925).
[2] W. G. Clarke, *In Breckland Wilds* (1925), p. 22. The term "The Brock District" was used by Prof. A. Newton in the introduction to H. Stevenson's, *The Birds o Norfolk*, vol. i (1866).

The crops of the district were as characteristic as its field system and its waste lands. Rye was the commonest cereal, but the yield of barley was often the largest, with oats next. The wheat crop was small. Large flocks of sheep were kept in every parish for fertilising the soil while there was "no where better Mutton than this barren Land affords, the Sheep being not liable to the Disease called the Rot".[1] Pre-enclosure travellers were very impressed by the abundance of rabbits. "A large portion of this arid country is full of rabbits, of which the numbers astonished me", wrote the Duc de la Rochefoucauld in 1784. "We saw whole troops of them in broad daylight; they were not alarmed by noise and we could almost touch some of them with our whips. I enquired of this prodigious number and was told that there was an immense warren which brought in 200 guineas a year to the owner, being let to a farmer."[2] The penalties for poaching were severe, because the farming of rabbits formed the economic mainstay of many of the landowners. Some farmers still pay their rent from what they realise by the sale of rabbits, but the number caught is rapidly decreasing with the spread of afforestation, which necessitates the extermination of all rabbits within its confines.

One of the common features of Breckland in the pre-enclosure period was the prevalence of disastrous sandstorms. A notable storm in 1668 blew sand for 5 miles from Lakenheath Warren to Santon Downham, almost overwhelming the village and obstructing the navigation of the Little Ouse.[3] John Evelyn, in 1677, also referred to "the Travelling Sands about ten miles wide of Euston, that have so damaged the country, rolling from place to place, and, like the lands in the Deserts of Lybia, quite overwhelmed some gentlemen's whole estates".[4] The open and unrestricted appearance of this region, before enclosure and afforestation wrought such drastic changes in its scenery and economy, is well described by an eighteenth-century traveller, William Gilpin. Between Brandon and Mildenhall, he declared that:

> Nothing was to be seen on either side but sand and scattered gravel without the least vegetation; a mere African desert. In some places this sandy waste occupied the whole scope of the eye; in other places, at a distance we could see a skirting of green with a few straggling bushes which, being surrounded by sand, appear'd like a stretch of low land shooting into the sea. The whole country indeed had the appearance of a beaten sea-coast, but without the beauties which adorn that species

[1] F. Blomefield, *An Essay towards a Topographical History of the County of Norfolk*, i, 553 (1739).

[2] F. de la Rochefoucauld, *A Frenchman in England, 1784* (1933), p. 212.

[3] T. Wright, "A curious and exact relation of a Sand-floud, which hath lately overwhelmed a great tract of land in the County of Suffolk", *Philosophical Transactions*, No. 37 (July 1668).

[4] John Evelyn, *Diary*, 10 Sept. 1677.

> of landscape. In many places we saw the sand even driven into ridges; and the road totally covered, which indeed was everywhere so deep and heavy, that four horses which we were obliged to take could scarce in the slowest pace drag us through it. It was a little surprising to find such a piece of absolute desert almost in the heart of England.[1]

It must be remembered, however, that casual travellers through the district may have exaggerated its wild and barren character, for the main trackways crossed the heathlands remote from the more fertile valleys.

Even so, if this barren soil was ever to be cultivated it was essential to plant trees. The enclosure movement, towards the close of the eighteenth century, was accompanied by the planting of belts of dwarfed hedges of conifers, especially of Scots pine, to shelter the fields from winds. But tree-planting on a large scale only began about 1840. The incidence of enclosure in Breckland varied with the soil, and its effects were more marked in the border parishes. There, holdings were consolidated into large estates, corn production was increased by more intensive cultivation, and population expanded rapidly. Sheep manure and marl had helped to feed the hungry sands of Breckland; the outfield rotation was a device for concentrating upon a small area the "tathe" of a flock supported by the grazing of the whole township.[2] But the introduction of the four-course shift of the new Norfolk husbandry brought changes. Under turnips and artificial grasses, the sand produced more fodder than ever before; more sheep could be carried to the acre; their "tathe" would consequently be richer and the crops heavier. It was probably the introduction of the new convertible husbandry that ousted the infield-outfield system from West Wretham.

But there were yet other changes to come. The agricultural crises of the nineteenth century from 1813 to 1837, from 1874 to 1884, and during the 1890's, saw the decline of arable farming, and the acquisition of vast estates by great landowners, a few of whom owned almost the whole of Breckland. One estate covered 34 square miles, another 20 and a third 18. Many tried to counteract their agricultural losses by developing the leasing of the sporting rights, and, to facilitate their disposal, tree-planting was encouraged as it provided cover for game. To-day, there is less land under the plough than there was one hundred and fifty years ago. This decline is due primarily to economic causes, but it may well have been hastened by soil impoverishment. Artificial manures on these poor soils are not always productive of good crops, while ploughing breaks up the chalk and assists its disappearance from the upper layers of soil. Fertility can then

[1] W. Gilpin, *Observations on several parts of Cambridge, Norfolk, Suffolk and Essex... made in 1769* (1805), p. 28.

[2] J. Saltmarsh and H. C. Darby, *art. cit.* p. 43.

be maintained only by marling or by introducing humus to absorb the artificial manures. Both mustard and lupins are often ploughed in for this purpose.

The tillage of poor land, like that of the Breck country, is lucrative only when prices are high, and this factor has encouraged experimental crops in what is, economically, a marginal area. In recent years, mature tobacco has been grown on the deeper sands at Croxton, Icklingham and Methwold, but the experiment failed, partly because the leaf could not be dried without artificial means. The introduction of sugar beet has been more successful as the sugar content is high, and beet is now the principal crop on soils which have been matured. Quite recently, black currants and asparagus for canning have been grown successfully on a large scale on the Kilverstone estate, where the light soils are fertilised with pig manure. Among the older established crops, barley is of most importance, though its yield is the lowest in East Anglia. Other crops are potatoes, lupins and mustard for sheep feed, buckwheat for game, lucerne and rape, peas, clovers, vetch and sainfoin, swedes, turnips and mangolds. Good pasture is rare even in the small fertile valleys, and so the density of live stock is only about half that of the adjacent districts. Cattle are few, though dairy animals have increased since the war, while sheep are below the average, being grazed usually on mere rough pasture.

Significant as are recent attempts to increase the agricultural and horticultural productivity of Breckland, they are less interesting than the post-war afforestation. This is the most fundamental vegetational change in the region in historic times, equalled only by the planting and enclosing of its treeless, grassy steppes at the close of the eighteenth and the dawn of the nineteenth centuries. To-day, the largest single forest area created in Britain in modern times is growing to maturity, and has wrought a revolution in the natural and economic equilibrium of the region.

Breckland is now the least densely populated region of its size between the Pennines and the New Forest. With the stimulus of enclosures, its population rose during the early nineteenth century, and, despite agricultural depression, this reached a total of over 40,000 in 1851. The subsequent depressions helped to depopulate the countryside, though the towns of Thetford, Brandon, Mildenhall, and Swaffham maintained the position they gained in the earlier part of the century. By 1931, the total population of the area was only just over 30,000; and, if the urban population of about 12,000 is subtracted, the remaining 18,000 are scattered over its heaths and valleys at about sixty to the square mile—less than one-tenth the average density for England and Wales.

(B) AFFORESTATION IN THE BRECKLAND

By J. Macdonald, B.SC.

Divisional Officer, H.M. Forestry Commission

The Forestry Act of 1919 initiated something new in the rural economy of this country when it set in motion the work of afforesting large areas of land under the control of the state. The Forestry Commission, established under the Act, was set the task of safeguarding the national supplies of timber and other forest produce by creating in Great Britain an area of woodland large enough to tide the country over a period of emergency of about three years. For this purpose it was estimated that, in addition to the existing areas of woodland, mostly privately owned, it would be necessary to create about one and three-quarter million acres of entirely new forest.

The land for this enterprise must be capable of growing trees to a size at which they can be utilised. This means that large areas of land in this country, at present contributing little to the national resources, have had to be excluded from consideration because they are too exposed and high lying and because their soils are too poor even for the less exacting species of tree. On the other hand, it would not be in the national interest to include good agricultural land, although this is generally capable of growing excellent trees, particularly hardwoods. Consequently, the land for planting has been sought where possible in areas which are not too exposed or too high lying, and yet which are uncultivated or on the margin of economic cultivation.

The Breckland is a good example of the type of country into which forestry can be introduced without displacing, or threatening, any vital national interest. So far as can be known, the district has always been poorly wooded, although the plantations which have been made since the middle of the eighteenth century have shown that numerous species are capable of making good growth and reaching timber size. A great part of the area has not been cultivated within memory but has remained as open heath, formerly a pasturage for sheep but given over latterly to game and rabbits.[1] It is estimated that about 19 per cent of the total area planted by the Commissioners in Breckland has been at one time or another under the plough. Much of this, however, has in recent years been cropped only for game feed, while a considerable part was broken up during the war under the "Food Production" schemes.

[1] See p. 214 above.

Work began in the district in the winter of 1921–22 south of Brandon and at Cockley Cley near Swaffham. Since that time, the acquisition, and planting, of land have gone on steadily, and at the present time the Commissioners are in control of an area of 52,807 acres almost wholly in Breckland proper (see Fig. 56). There are three Forest units—Thetford, Swaffham, and the King's Forest; and details of these are given in the following table.

Forest	Total area acquired	Total area planted to 30. iv. 1938
Thetford	40,869	28,875
Swaffham	5,948	4,879
The King's	5,990	2,421

Thetford Forest is now the largest planted area in England. It is composed of a central block which extends from Methwold, on the north, to Elveden, and from Hockwold, on the west, to Croxton, together with outlying areas at Hockham, West Harling, and Mildenhall. Swaffham Forest consists of various blocks to the south-west and south of the town of Swaffham, as well as of an area at Didlington, north of Mundford. The King's Forest lies to the north of Bury St Edmunds. It was acquired in 1935 and owes its present name to its selection as one of the forests chosen to commemorate the Silver Jubilee of King George V.

In all three forests planting has been carried out mainly with coniferous species, among which Scots pine and Corsican pine preponderate. There are several reasons for this concentration on conifers. In the first place the Commissioners must pay attention to the type of timber that is most required in industry. At the present time, more than 90 per cent of the timber and wood products used in this country comes from softwood or coniferous trees, and there is no sign that in the future there will be any marked change in this proportion. It is reasonable therefore that most of the planting, not only in Breckland, but all over the country, should be done with conifers.

In the second place, the soils of Breckland are generally suitable for the growth of conifers. These soils, it is true, vary widely and form, roughly, a series running from a very thin sand over chalk, to deep, podsolised sands on some of the heaths where chalk is a long way from the surface.[1] On the last type, conifers are the only choice. On soils where chalk is at a moderate depth, and where sufficient soil moisture is available, good crops of oak could be raised; while on the thin soils, immediately over the chalk, beech would grow, although it is nowhere very vigorous in this district.

[1] See p. 223 below.

But, for reasons connected with the local climate, these and other broad-leaved species, as well as conifers such as Douglas fir and European larch, are often extremely difficult to establish when planted in the open. The two principal factors which work against them are frost and drought. Frosts in the late spring and early summer are a normal feature of the climate of Breckland. These are often quite sharp (up to 10 degrees of frost is not uncommon in the second half of May), and they fall with great severity on oak, beech, and other hardwoods. Douglas fir and European larch also suffer in the same way though less severely.

Drought also plays an important part in checking the growth of young broad-leaved trees and of conifers such as the larches. The rainfall is low and the soil unretentive of moisture, while, on the grass-covered areas, the dense sward leads to an intense local competition for supplies of moisture and, at the same time, acts as a covering which prevents much of the rainfall from actually reaching the soil. The high temperatures of the summer also tend to have an injurious effect on beech planted in the open, as this species is apt to suffer from sun scorch. The pines, and especially the Scots pine, are remarkably resistant to frost and drought, and for this reason alone they are likely to remain the principal species used in new planting in Breckland.

At the same time the importance of establishing broad-leaved trees on those soils which are really suited to them has not been overlooked and much experimental work is being carried out on this subject. In particular, methods of introducing beech into young plantations of pine have been studied in some detail.

The earlier plantations of Scots pine dating from 1922 and 1923 are developing rapidly, although there is much variation in growth, which can probably be correlated with variations in the soil. Already, there are trees up to 25 ft. in height, and a preliminary thinning has yielded produce in the form of fencing stakes, pit-props, small poles, and firewood. The pine plantations have not suffered much damage from fungi apart from the death of small groups of trees apparently killed by *Fomes annosus*, and by the fairy-ring fungus, *Paxillus*.[1] Damage by insects has however been more severe, and, in the Scots pine plantations, the pine shoot moth *Evetria buoliana* has been a dangerous pest for a number of years.[2] The attacks of this insect have led to the distortion of a large number of trees, and, although the damage has turned out to be less serious than was at one time feared, it has been sufficiently important to make special treatment of

[1] T. R. Peace, "Destructive fairy rings associated with *Paxillus giganteus* in young pine plantations", *Forestry*, x, 74 (1936).

[2] *Studies on the Pine Shoot Moth*, Forestry Commission Bulletin, No. 16 (1936).

the crops a necessity. The Corsican pine, although not immune to attack, is much less frequently damaged, and plantations of this species are full of vigorous, straight poles.

Most of the plants used in the afforestation in Breckland have been raised locally in the Commission's own nurseries, the most important of which are situated at Weeting, Lynford, Harling, and Santon Downham. These extend to 88 acres, and at the end of September 1937 they contained five million transplants and fourteen million seedlings. About eight thousand pounds of seed are sown annually.

One important side of the work is protection against fire, which, on account of the dry climate and the inflammable nature of the crop, is a serious menace particularly in the spring and summer. There are two observation towers manned in periods of danger, and connected by telephone with the central office at Santon Downham; while patrols are also put along the roads. In order to prevent fires spreading from road or railway, strips are ploughed and kept free from vegetation, while similar strips are also ploughed along rides through the forest. These ploughed strips are generally sufficient to prevent the spread of a ground fire in its early stages. Broad-leaved crops are less inflammable than conifers, and for some years it has been the policy to plant belts of hardwood trees along roadsides to serve as a protection. These will also have the effect of adding to the amenity of the countryside, and to promote this, various ornamental trees like the red oak and the wild cherry are now being planted, in addition to the common species such as oak, birch, beech, and sycamore.

In addition to their programme of afforestation, the Commissioners were charged with land settlement, which they have been carrying out by means of their forestry-workers' holding schemes. Holdings are generally created in the proportion of one for every 200 acres. Each holding consists of a house, buildings, and land which does not as a rule extend to more than ten acres. The holders are guaranteed 150 days' work in the year, but many of them obtain almost full-time employment. There are at present 188 of these holdings in the Breckland forests, and they house 630 persons, of whom 134 are workers in the plantations. At the end of 1937, the value of the live stock on these holdings was estimated at £7980. The number of workmen employed by the Commissioners in Breckland during 1937 varied between 300 in the winter months and 225 during the summer months, when the amount of work available normally falls off. In addition to the holdings there are within the boundaries of the forests twenty-four farms let on agricultural tenancies. These are not likely to be planted.

(C) THE ECOLOGY OF BRECKLAND

By A. S. Watt, PH.D.

From west to east in England, as the oceanic influence decreases, there is a fall in the Atlantic element of our flora, and a new element—not homogeneous, but commonly referred to as the "continental" element—becomes significant. Breckland is its headquarters in this country;[1] on the Continent it ranges from the far north of Europe to the south, and eastwards to the steppes of Russia and beyond. In a climate which is permissive to it, two other sets of factors condition its survival, namely, a soil with a high base status and/or freedom from competition. This last is freely offered by abandoned arable fields, disturbed soil, and open communities.

Freedom from competition very likely explains the presence of the liverworts, *Lophozia barbata* and *L. hatcheri*, and the lichens, *Cladina rangiferina* and *Stereocaulon evolutum*, in this outpost to the south-east of the main area of their occurrence in this country. It also explains the high percentage of annuals in the flora: half the "continental" element are annuals, and so are 40 per cent of the flora of the grasslands described later in this chapter.

The flora is essentially heliophilous and xerophytic. The annuals are drought evading, the perennials drought resistant. In soil preferences, there is a wide range represented, but calcicoles and species of slightly acid soils are numerous, while calcifuges are few and there are noteworthy absentees, e.g. *Erica cinerea*. The same numerical representation characterises both the continental element and the annuals: the bulk of each class is found on soils with a relatively high base status, a few only grow on very acid soils. But interest in the rarer species ought not to blind us to the fact that most of the species in Breckland have a wide English and British distribution.

THE VEGETATION

While the interest of the flora of Breckland is enhanced by the presence and frequency of the continental contingent, the dominants of the vegetation do not suggest continentality: rather the reverse. These dominants are *Festuca ovina* and *Agrostis*[2] spp., the chief constituents of the variable

[1] See p. 43 above; and also A. S. Watt, "Studies in the Ecology of Breckland. I. Climate, Soil and Vegetation", *Jour. Ecol.* xxiv, 117 (1936).

[2] The species of *Agrostis* require revision as the result of W. R. Philipson's work. *Jour. Linn. Soc.* li, 73 (1937).

"grass-heath", *Carex arenaria, Calluna vulgaris, Pteridium aquilinum* and, locally, *Ulex europaeus*. Of these species, *Carex arenaria* and *Ulex europaeus* are West European; while *Calluna vulgaris*, although it stretches far eastwards to the plains of Russia, attains its best development in the west; and the cosmopolitan *Pteridium aquilinum* tends, in those parts of Europe with a continental climate, to become a woodland plant. But the behaviour of some of these plants shows an insecurity of tenure suggesting that as dominants they are near their limit.

With the exception of the community dominated by *Ulex europaeus*, which is local and has not been studied, there are four major easily recognisable plant communities forming a somewhat bewildering patchwork, whose pattern formed the subject of the first ecological investigation of Breckland. Farrow in a series of illuminating papers[1] dismissed soil variability as the primary cause, and from experimental and detailed observational evidence he explained the pattern in terms of the intensity of rabbit-grazing. All the dominants except *Pteridium* are grazed. Their palatability and power of withstanding grazing vary, and the differential effects of diminishing intensity of grazing can be seen in a series of zones with grass-heath the most heavily grazed, followed by a zone of *Carex*, and that in turn by *Calluna*.

In interpreting the vegetation of Breckland, the importance of the biotic factor must be recognised, but too great emphasis upon it obscures primary relationships between the different dominants. By taking cognisance of soil variation and the varying behaviour of the dominants on different soil types, the way is opened to a more exact understanding of plant behaviour and the distribution of the plant communities. The soils of Breckland have this in common that their physical properties vary within a rather narrow range. Open, porous, with a high percentage of coarse particles, and with an almost negligible amount of silt and clay, they have a low water-holding capacity, although this varies with the amount of chalk stones present. And primarily because of the chalk there is considerable chemical variation.

The soil over much of Breckland is derived from the chalky boulder clay, which contains roughly 50 per cent of $CaCO_3$ and 50 per cent of sand with small amounts of silt and clay. By leaching, the $CaCO_3$ is removed from the surface downwards. Following its removal the change in acidity brings about the initiation of podsolisation, the leaching of bases, the mobilisation of the sesquioxides of iron and aluminium and their transference to lower layers. These changes result in a complete series of stages in the development of a podsol, from shallow and highly calcareous

[1] E. P. Farrow, *Plant-Life on East Anglian Heaths* (1925).

soils at one extreme to well-developed podsols at the other. Seven stages in this series may be recognised.

Besides the soil variation brought about in this way, there is a further variation resulting from erosion. The leached soils, having lost their binding material, and supporting a vegetation inadequate to maintain stability, are eroded, often in the form of blow-outs, thereby exposing at the surface different horizons of the podsol profile.[1] The transported sand forms a blanket of variable thickness covering considerable areas and overlying intact as well as truncated profiles.

GRASSLAND TYPES

The recognition of soil variation throws great light upon the distribution and behaviour of the four major communities. They can be illustrated by a brief account of the variation shown by grass-heath on the seven stages in the development of a podsol, and by reference to the communities dominated by *Calluna*, *Carex*, and *Pteridium*. The "grass-heaths" (grasslands) and the corresponding soils are provisionally designated by the letters A to G: these symbols have nothing to do with the notation used in soil science.

The chief features of these seven stages are summarised on the following page. The perfectness of the series is spoiled in the last four members by the deposition of blown sand, but the soil has been stable for some time and the blanket of sand seems to have assumed properties appropriate to the underlying soil. The first five stages show a well-marked gradient of fertility: F and G are similar to E. The grassland communities described occur on Lakenheath Warren. They are all heavily grazed by rabbits and are thus comparable within themselves; and they differ in some important respects from ungrazed grassland.

Grassland A. The highly calcareous shallow soil bears an open vegetation of species tolerant of chalk or exclusive to it. *Festuca ovina* is the most abundant species, *Agrostis* is occasional only. Several species are confined or almost confined to this type: *Botrychium lunaria, Calamintha acinos, Galium anglicum, Ditrichum flexicaule* var. *densum, Bilimbia aromatica, Lecanora lentigera, Placodium fulgens,* and *Psora decipiens.* There are no liverworts. Locally there is more sand, and *Cladonia silvatica* occurs.

Grassland B. Of all seven types, this is the richest in species, and its close turf is the nearest approach to chalk pasture found in Breckland. Characteristic species include *Avena pratensis, Arabis hirsuta, Cirsium acaule, Daucus carota,* and *Hypnum chrysophyllum.* The bulk of the turf consists of

[1] A. S. Watt, "Studies in the Ecology of Breckland. II. On the origin and development of blow-outs", *Jour. Ecol.* xxv, 91 (1937).

The seven stages in the development of a podsol in Breckland.

Stage in development of profile	A	B	C	D	E	F	G
% $CaCO_3$ in surface 6 in. soil sample	17·90	1·610	0·129	0·00	0·00	0·00	0·00
pH in surface 6 in. soil sample	8·20	7·81	6·18	4·36	3·95	3·77	3·82
% humus in surface 6 in. soil sample ($=C\times 1{\cdot}724$)	1·551	2·129	1·962	2·198	2·482	4·074	4·046
Exchangeable Ca in M.E. in surface 6 in. soil sample	51·20	34·37	6·29	0·76	0·13	0·07	0·00
Average depth in inches of soil over chalky boulder clay	7·5	13	18	34	Over 60	Over 60	Over 60

General notes on the profile:
A. Highly calcareous throughout.
B. Calcareous throughout; $CaCO_3$ low at the surface increasing rapidly downwards.
C. Surface soil slightly bleached: a reddish band above $CaCO_3$-containing lower layers.
D. A brown forest soil, overlaid by blown sand slightly podsolised.
E, F, G. Podsol profiles in three stages of development, the last a well-marked podsol: all three overlaid by blown sand.

Data of the total number of species, of significant species, and of annuals, in seven variants of grassland.

Stage	A	B	C	D	E	F	G	A–G
Total number of species of vascular plants	50	80	59	37	22	16	9	92
Total number of species of bryophytes	10	31	32	15	11	12	8	42
Total number of species of lichens	15	11	12	12	13	12	12	24
Number of significant species of vascular plants	28	49	34	21	8	8	8	55
Number of significant species of bryophytes	7	16	11	12	6	5	6	26
Number of significant species of lichens	10	7	8	9	8	7	7	16
Number of annuals (higher plants)	20	31	22	13	10	6	2	36

Festuca ovina and *Agrostis* spp.; but an effective, and sometimes colourful, contribution is made by *Asperula cynanchica, Astragalus danicus, Campanula rotundifolia, Carex praecox, C. ericetorum, Galium verum, Koeleria gracilis, Linum catharticum, Lotus corniculatus*, and *Thymus serpyllum*. There are many species of bryophytes but both they and the lichens play a subsidiary part. *Cladonia silvatica* is frequent as "individuals", but never forms pure patches.

Grassland C. This type, briefly, is Grassland B without its large calcicolous element; but many exacting species remain. The surface soil is now acid, and *Galium saxatile, Rumex acetosella, Teesdalia nudicaulis* and *Hypnum schreberi* appear in considerable numbers. The turf is more grassy and coarser than in B: *Festuca ovina* and *Agrostis* spp. make up its bulk, but *Campanula rotundifolia, Carex praecox*, and *Galium verum*, are frequent to abundant. There are many species of bryophytes including the characteristic *Bryum roseum*. The most abundant lichen is *Cladonia silvatica*, and it occasionally forms small pure patches under which are found the dead remains of higher plants.

Grassland D. A further drop in soil fertility is reflected in the absence of many exacting species, leaving only thirty-seven higher plants. Moreover, the grassy turf is not continuous, and the vegetational cover consists essentially of patches of higher plants and patches of lichen with *Cladonia silvatica* dominant. Again, *Festuca ovina* and *Agrostis* spp. are the chief plants: the relatively exacting *Campanula rotundifolia* and *Galium verum* are less frequent than in C, while the calcifuges *Galium saxatile, Rumex acetosella* and *Teesdalia nudicaulis* are more frequent than in C. There are many fewer species of bryophytes.

Grasslands E, F and G. These three types are essentially the same. Eight tolerant significant species of higher plants grow in patches, or scattered in a carpet of lichen composed almost entirely of *Cladonia silvatica*. These species are *Agrostis* spp., *Aira praecox, Festuca ovina, Galium saxatile, Luzula campestris, Rumex acetosella* and *Teesdalia nudicaulis*. The greater number of species (higher plants and bryophytes) which differentiate E and F from G are found largely on soil thrown up by rabbits, thus expressing the finer chemical differences in the three stages of podsolisation represented.

Everywhere, underneath the lichen carpet, occur the remains of grasses, *Rumex, Luzula*, and occasionally *Calluna vulgaris*.

A study of the full lists of species and their distribution among the types of grassland brings out very clearly that from B to G the flora is an attenuating one: there is a fractional elimination of species rather than a radical change. First the calcicoles go; then the more exacting, followed by the less exacting, until finally in G only the tolerants survive. Only a small part of the change is due to the appearance of calcifuges. *Deschampsia flexuosa*, *Nardus stricta* and *Potentilla erecta*, although present in Breckland, are absent from E, F, and G, and also from large areas of Breckland where the soils are certainly acid enough for them. The intimate relation between the soil and the grassland community it bears is thus established. Similar relationships can be seen in Breckland between the soil and communities of bracken, heather, and sand sedge.

CYCLIC PHENOMENA

Grass-heath. In Grassland G, local disintegration of the lichen mat leaves the soil exposed. The rebuilding of the plant cover is initiated by *Aira praecox*, *Festuca ovina* (seedlings) and *Agrostis* spp. (vegetative spread). These afford anchorage for the lichens, which re-establish a continuous cover. In time, the grasses at the centre of a patch die, and death spreads centrifugally until the lichen mat, after a temporary dominance (which may last some years), disrupts.

This cycle of change is a feature of Breckland, and can be well seen in the plant-succession upon bare almost humus-free soil exposed by local erosion in the form of blow-outs.[1] Details cannot be given here, but periodically during the succession there is built up a stage with *Festuca ovina* and *Agrostis* spp. set in a carpet of lichen. As in Grassland G, the grasses die from the centre of a patch outwards, and the pure lichen carpet eventually disintegrates, exposing the soil to erosion. On a partially eroded soil the full succession is telescoped; a new lichen-grass community is built up only to disintegrate once again; and the wave-like advance is repeated until a relatively stable grass-heath emerges upon soil containing about 3 per cent of humus.

But even in a relatively stable grass-heath there is variation from place to place and from year to year. Thus the number of *Agrostis* shoots counted per square metre, in fourteen plots of 0·05 sq.m. selected at random over a uniform grass-heath was 2320, 870, 3004 in the years 1935, 1936, and 1937 respectively: and in two other types of grass-heath the same sequence was obtained. Over the same period the number of shoots of *Rumex* varied inversely, 263, 1299, and 276.

Calluna vulgaris, absent from Grassland A, is present on the remaining soil types, and is capable of assuming dominance from C to G. Its ultimate height varies from about 6 in. to about 30 in. according to the soil. The plant lives to an age of about twenty-five years, and (it is important to note) on the poorer soils, at least, the *Calluna* community goes through a

[1] A. S. Watt, "Studies in the Ecology of Breckland. III. The origin and development of the Festuco-Agrostidetum on eroded sand", *Jour. Ecol.* xxvi, 1 (1938).

cycle of change—a stage of invasion, followed by dominance for a number of years, then widespread death. During the tenure of the ground, humus accumulates forming a black peaty *mor* up to 2 in. thick. With the death of the heather and the decay of its stems, the *mor* disintegrates and erosion exposes the mineral soil, which itself may be eroded until the process is checked by the accumulation of flints forming an erosion pavement. On this bare soil a series of communities leads eventually to the establishment of grass-heath, which, if the biotic factor permits, is invaded and replaced by heather. Here there is a retreat of heather for which rabbits are not responsible. Large areas of grass-heath of the poorer types occur, where the only evidence of the former dominance of *Calluna* is the occasional dead stems under the lichen carpet together with the purple stain typical of *Calluna*-heath soils.

Pteridium aquilinum. The distribution of bracken in Breckland very strongly suggests a spread from nuclei moist enough for its establishment by spores. Large circumscribed areas of bracken contain either woods, or houses surrounded by trees, from which the spread may have taken place. It is excluded from some areas of cold-air drainage by frost, or it invades them marginally only with extreme slowness, and its vigour varies with the microclimate. But even on soils with similar microclimates variation in height and behaviour is found. Bracken grows in all the seven stages; in height, it varies from approximately 15–18 in. on soil A, increasing through stages B and C to a maximum of about 50 in. in stage D, falling again to about 14 in. in stage G. Incidentally, on the same soil type there is variation from year to year. The bracken also shows a curious patchiness, the patches varying in size from soil type to soil type, but in any one type forming a series in a cycle of change. Some patches have few, deep-set fronds; others have more numerous taller fronds with the part of the petioles showing above ground of intermediate length; while still others carry dense tall fronds with long petioles. In series the average depth of origin of the fronds in the soil becomes less and less. In the last type, death spreads centrifugally from the centre of a patch outwards, and the vacated ground is occupied once more by a scattered population of fronds deep set in the soil.

It has been shown that the rhizome system of bracken is sympodial and that numerous relatively small individual plants make up an area of bracken. It is a typical travelling geophyte: as the rhizome advances in front, it dies away behind, throwing off live branches which thus become independent plants. The number of fronds carried by any one plant is small—approximately one frond to 8 ft. of rhizome.

Carex arenaria. The main areas of *Carex* in Breckland lie in the parts of the Little Ouse and Lark Valleys next the Fenland, and upon the blown sand between the large blow-out on Lakenheath Warren and the village of Santon Downham. But it is widely distributed in small and large patches, and it grows, although it does not necessarily become dominant, on all the seven soil stages.

Light has been thrown on this interesting distribution by Mr C. E. M. Tidmarsh,[1] of the Botany School, Cambridge, who has shown that for the successful germination of the seeds a continuous 12 to 20 days' water supply (depending on the temperature) is necessary, and that for the successful establishment of the seedlings similar moist conditions are needed. These requirements limit the establishment of *Carex* to the neighbourhood of water—of rivers like the Lark and Little Ouse, of meres, or of temporary (but not too transient) bodies of water appearing in lower-lying parts when the water table is high.[2] Even if these conditions are satisfied, the establishment of a seedling will be checked by rapid recession of the water table leaving the soil too dry for its survival: thus temporary water-logging offers a somewhat precarious start for *Carex*.

From these *points d'appui*, *Carex* spreads to soils that are essentially dry. The recognition of its early behaviour (the retention by seed and seedling of needs that once may also have characterised the adult), not only explains much of its distribution on Breckland, but also its development on sand dunes near the coast, where it becomes established first in the slacks and later spreads to the dunes. Once established, it spreads freely by rhizomes, and most successfully on loose soil. There are, however, patches of *Carex* in Breckland whose relation to a place suitable for its establishment is not clear. These may be scattered vestiges of a former continuous area in which retrogression has taken place through the activity of rabbits. Just how far *Carex* may degenerate, like *Calluna* and *Pteridium*, without the help of animals like rabbits and mice, is not known. Its behaviour on sand dunes along the coast certainly suggests a loss of vigour with age, but whether this proceeds to the point of annihilation in small or large patches is not yet determined.

The varying height of bracken from year to year; the changing density of *Agrostis*; the results from the application of water to *Agrostis* during dry years; the negative results obtained during the abnormally wet year 1937; and the periodic phenomena already described—all these suggest a causal relation with climate and, in particular, with the rainfall. There is abundant evidence suggesting that scarcity of water is a major difficulty to plant life in Breckland. But the relation between the cyclic phenomena and rainfall

[1] In unpublished work.
[2] As, for example, in the spring of 1937 after a long wet spell.

is not simple, for at any one time cycles in all stages are found, and the period of the rhythm varies from species to species. Up to the present, the data suggest a rhythm explicable partly in terms of the structure and biology of the species, and partly in terms of the effect produced by its own accumulated humus and litter (and for *Agrostis* by the carpet of lichen) on the penetration of rainfall during the summer, when the absolute rainfall is low and the evaporation high. Reversal of the soil-moisture gradient in summer is, in fact, a common occurrence. It is relevant to note that *Agrostis*, *Calluna*, *Pteridium*, and *Carex* form a series with increasing rooting depths; in suitable soils the roots of *Carex* descend to 11 ft., and it is the only species for which a cycle of change has not been demonstrated.

SUCCESSIONAL RELATIONSHIPS

In the edaphic series A to G, nothing has been said about the causes of the change. This does not imply that long-continued occupation of the ground by grassland could bring about the change from a calcareous soil to a well-developed podsol. On the other hand, the presence of the remains of *Calluna* in D, E, F, and G, and the purple colour of the soils in E, F, and G (slight stain in D), strongly suggest podsolisation under heather, although the heather no longer dominates. In other places, eroded and similarly free from heather, truncated podsols with recognisable remains of heather have been sealed up by a deposit of blown sand. It may be, therefore, that heather was much more widespread than it is now. What is put forward here as a working hypothesis is that leaching has proceeded to produce a brown forest soil, whose further change to a podsol is the work of *Calluna*, that may, in the last analysis, be dominant owing to man.

The varying behaviour of the dominants of the four major communities on the different soils makes it impossible to put forward any simple scheme outlining their relationships to each other. Further work is needed. At the moment, all that can usefully be said is that the relations *Festuca-Agrostis/Carex*, *Calluna*, *Pteridium*, and *Carex/Calluna*, *Pteridium*, and *Calluna/Pteridium*, vary according to the soil, and for bracken, at least, according to microclimate.

But the work of E. P. Farrow[1] and the facts presented in this account make it plain that these communities are not in stable equilibrium with their inorganic environment. The exclusion of rabbits is followed by vegetational change. In different parts of Breckland there are places free, or relatively free, from rabbits and on these areas woody plants have colonised. Of shrubs, the most important is the gorse (*Ulex europaeus*), and of trees the most important are pine, oak and birch. There is little doubt that on certain soils, at least, woodland of some kind would eventually be formed.

[1] E. P. Farrow, *Plant-Life on East Anglian Heaths* (1925).

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* *Indicates that the title only of a communication is given.*

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