British Association

for the

Advancement of Science

Report

of the

Eighty-Eighth Meeting

Cardiff—1920

BRITISH ASSOCIATION
FOR THE ADVANCEMENT OF SCIENCE

—

REPORT

of the

EIGHTY - EIGHTH MEETING

CARDIFF—1920
AUGUST 24—28

LONDON
JOHN MURRAY, ALBEMARLE STREET

1920

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* Local Sectional Secretaries

Table showing the Attendances and Receipts

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	TheMarquisofNorthampton,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. Daubeny, 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	SirWilliam 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
1900, Sept. 5	Bradford	Sir William Turner, D.C.L., F.R.S.	267	13

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

at Annual Meetings of the Association.

Old Annual Members	New Annual Members	Associates	Ladies	Foreigners	Total	Amount received during the Meeting	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
—	—	—	1100*	—	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	68‡	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
297	45	801	482	9	1915	1801 0 0	1072 10 0	1900

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

Table showing the Attendances and Receipts

Date of Meeting	Where held	Presidents	Old Life Members	New Life Members
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

¶ Including 848 Members of the South African Association.
‡‡ Grants from the Caird Fund are not included in this and subsequent sums.

at Annual Meetings of the Association—(continued).

Old Annual Members	New Annual Members	Associates	Ladies	Foreigners	Total	Amount received during the Meeting	Sums paid on account of Grants for Scientific Purposes	Year
574	131	794	246	20	1912	£2046 0	£920 9 11	1901
314	86	647	305	6	1620	1644 0	947 0 0	1902
319	90	688	365	21	1754	1762 0	845 13 2	1903
449	113	1338	317	121	2789	2650 0	887 18 11	1904
937¶	411	430	181	16	2130	2422 0	928 2 2	1905
356	93	817	352	22	1972	1811 0	882 0 9	1906
339	61	659	251	42	1647	1561 0	757 12 10	1907
465	112	1166	222	14	2297	2317 0	1157 18 8	1908
290**	162	789	90	7	1468	1623 0	1014 9 9	1909
379	57	563	123	8	1449	1439 0	963 17 0	1910
349	61	414	81	31	1241	1176 0	922 0 0	1911
368	95	1292	359	88	2504	2349 0	845 7 6	1912
480	149	1287	291	20	2643	2756 0	978 17 1‡‡	1913
139	4160§	539‖	—	21	5044‖	4873 0	1086 16 4	1914
287	116	628*	141	8	1441	1406 0	1159 2 8	1915
250	76	251*	73	—	826	821 0	715 18 10	1916
—	—	—	—	—	—	—	427 17 2	1917
—	—	—	—	—	—	—	220 13 3	1918
254	102	688*	153	3	1482	1736 0	160 0 0	1919

| Old Annual (Regular) Members | Annual Members | | Transferable Tickets | Students' Tickets | Foreigners | Total | Amount received | Sums paid | Year |
	Meeting and Report	Meeting only							
136	192	571	42	120	20	1380	1272 10	959 13 9	1920

** Including 137 Members of the American Association.

‖ 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.

* Including Students' Tickets, 10s.

REPORT OF THE COUNCIL, 1919–20.

I. Sir T. E. Thorpe, C.B., has been unanimously nominated by the Council to fill the office of President of the Association for the year 1921-22 (Edinburgh Meeting).

II. Resolutions referred by the General Committee, at the Bournemouth Meeting, for consideration, and, if desirable, for action, were dealt with as follows :—

(a) The Council adopted a resolution from Section D, that in the case of persons applying for membership of the General Committee who are not known to the Council, the matter should be referred to the Organising Committee of the Section concerned.

(b) The Council collaborated with the Conjoint Board of Scientific Societies in laying before the Prime Minister, H.M. Secretaries of State for the Colonies and for India, and the Governments of the Australian Commonwealth and the Union of South Africa, proposals for the collection and publication of scientific data relating to ex-German colonies (Resolutions of Sections E and H).

(c) The Council expressed to H.M. Government the Association's approval of the proposal to establish a British Institute of Archæology in Egypt. (Resolution of Section H.)

(d) The Council forwarded to the Board of Agriculture a representation on the desirability of securing the uniform description and nomenclature of ancient remains on Ordnance Survey Maps, and after correspondence with the Director-General of the Ordnance Survey have learnt that measures have been taken to this end. (Resolution of Section H.)

(e) The Council referred back to the Committee of Section I a proposal that that Section should be entitled ' Physiology and Psychology,' and that the Presidents in alternate years should represent the two branches of the Section.

(f) The Council, after enquiry, felt unable to take action recommended by the Conference of Delegates in the matter of a representation to H.M. Government on the use of taxes derived from motor-spirit and carriages for the improvement of roads.

(*g*) A proposal from the Conference of Delegates, that the Board of Education should be asked to hold an enquiry on the teaching of geography, was referred to Section E.

(*h*) The General Officers, on the instruction of the General Committee, forwarded resolutions urging upon H.M. Government the necessity for supporting an organised scheme of scientific research to the Prime Minister, the Chancellor of the Exchequer, the First Lord of the Admiralty, the Secretary of State for War, the President of the Board of Trade, the Food Controller, and the Minister of Health.

The Council have received from the Admiralty and from the War Office information on proposals for research. At the invitation of the Master-General of the Ordnance, the General Officers attended a Conference at the War Office, at which the Master-General, Lieut.-General Sir J. P. Du Cane, the Quartermaster-General, Lieut.-General Sir T. E. Clarke, and the Director of Medical Services, Lieut.-General Sir T. Goodwin, explained the organisation which has been adopted for scientific research in connection with military services.

III. The Council nominated as their representatives on the Joint Committee of the General Committee and Council on Grants, under the chairmanship of the President (Sir C. Parsons), Profs. W. A. Herdman, J. Perry, H. H. Turner, and J. L. Myres. This Committee was directed to report to the General Committee as well as to the Council, and its report, which the Council has approved, is appended:

The Committee would favour the following procedure: That Research Committees proposed by the Sectional Committees of the British Association and approved by the Committee of Recommendations be recommended by the Council for support by the Department of Scientific and Industrial Research, the Medical Research Board, or other bodies entrusted with the distribution of public funds, and that all Committees, the work of which may be aided by such bodies, remain Committees of the Association responsible as before to the Sectional Committees.

IV. The Council resumed consideration (deferred owing to the War) of certain resolutions referred to them by the General Committee in Australia in 1914.

(*a*) The Council forwarded to the Australian Government a resolution urging the need for legalising in Australia the metric system of weights and measures as an alternative (optional) system. (Resolution of Section A.)

(*b*) The Council found it inexpedient to forward a resolution proposing a gravity survey in Australia. (Resolution of Section C.)

(*c*) The Council forwarded to the Australian Government a resolution urging the early production of the Australian sheets of the Carte du Monde au Millionième. (Resolution of Section E.)

(*d*) The Council has still under consideration the proposal for the establishment of Bench-marks on Coral Islands, in the Pacific. (Resolutions of Sections C and E.)

V. The Department of Scientific and Industrial Research made a grant of £600 to the Association to meet the cost of certain specified researches for which Committees were appointed at the Bournemouth Meeting.

VI. The Research Fund initiated at Bournemouth now amounts to £1,888 16s. 6d.

VII. CAIRD FUND.—The Council made the following grants during the year, additional to annual grants previously made:—

Fuel Economy Committee (additional to grant made by General Committee at Bournemouth)	£10
Committee on Training in Citizenship	10
Geophysical Committee of Royal Astronomical Society	10
Conjoint Board of Scientific Societies	10

VIII. CONFERENCE OF DELEGATES and CORRESPONDING SOCIETIES COMMITTEE:—

The following Nominations are made by the Council:—

Conference of Delegates.—Mr. T. Sheppard (*President*), Mr. T. W. Sowerbutts (*Vice-President*), Mr. W. Mark Webb (*Secretary*).

Corresponding Societies Committee.—Mr. W. Whitaker (*Chairman*), Mr. W. Mark Webb (*Secretary*), Mr. P. J. Ashton, Dr. F. A. Bather, Rev. J. O. Bevan, Sir Edward Brabrook, Sir H. G. Fordham, Mr. A. L. Lewis, Mr. T. Sheppard, Rev. T. R. Stebbing, Mr. Mark L. Sykes, and the President and General Officers of the Association.

On the proposal of a sub-committee of the Corresponding Societies Committee the Council, in the interests of economy, propose that the bibliography of scientific publications in the transactions of Corresponding Societies be not printed in future in the Annual Report, and therefore recommend the following change in the Rules:—

Rule Chap. XI., 3 (ii.):—

"There shall be inserted in the Annual Report of the Association a list of the papers published by the Corresponding Societies . . ."
to read as follows:—

"A list shall be prepared of the papers published by the Corresponding Societies. . . ."

IX. The Council have received reports from the General Treasurer during the past year. His accounts have been audited and are presented to the General Committee.

The Hon. Sir Charles Parsons has been nominated a Trustee of the Association, in the room of the late Lord Rayleigh.

X. Power having been delegated to the Council by the General Committee to appoint ordinary members of Council to the vacancies caused by the resignation of Sir E. F. im Thurn and the appointment of Prof. J. L. Myres as General Secretary, Sir R. Hadfield and Sir J. Scott Keltie were appointed.

The retiring members of the Council are:—

By seniority.—Sir Dugald Clerk, Prof. A. Dendy.

By least attendance.—Prof. W. H. Perkin, Dr. E. J. Russell, Prof. E. H. Starling.

The Council nominated the following members:—

> Mr. J. Barcroft,
> Prof. J. Stanley Gardiner,
> Sir W. J. Pope,

leaving two vacancies to be filled by the General Committee without nomination by the Council.

The full list of nominations of ordinary members is as follows:—

Dr. E. F. Armstrong.
Mr. J. Barcroft.
Prof. W. A. Bone.
Dr. F. A. Dixey.
Sir F. W. Dyson.
Prof. A. Fowler.
Prof. J. Stanley Gardiner.
Sir R. A. Gregory.
Dr. E. H. Griffiths.
Sir R. Hadfield.
Sir S. F. Harmer.
Prof. J. H. Jeans.

Prof. A. Keith.
Sir J. Scott Keltie.
Prof. A. W. Kirkaldy.
Sir Daniel Morris.
Sir W. J. Pope.
Dr. W. H. R. Rivers.
Miss E. R. Saunders.
Prof. W. R. Scott.
Sir A. Strahan.
Mr. W. Whitaker.
Dr. A. Smith Woodward.

XI. THE GENERAL SECRETARIES have been nominated by the Council as follows:—

> Prof. H. H. Turner.
> Prof. J. L. Myres.

XII. The General Treasurer and one or other of the General Secretaries have been appointed representatives of the Association on the Conjoint Board of Scientific Societies.

XIII. Prof. H. A. Lorentz has been appointed an Honorary Corresponding Member of the Association.

XIV. The following have been admitted as members of the General Committee:—

Mr. W. B. Brierley.
Dr. F. D. Chattaway.
Mr. W. N. Cheesman.
Miss M. C. Crosfield.
Miss A. C. Davies.
Prof. J. E. Duerden.
Prof. A. J. Ewart.
Mr. C. B. Fawcett.
Dr. A. Holmes.
Prof. F. Horton.
Mr. A. Pearse Jenkin.
Prof. W. Neilson Jones.

Prof. A. A. Lawson.
Prof. J. W. MacBain.
Dr. R. MacDowall.
Dr. J. S. Owens.
Mr. H. J. E. Peake.
Dr. Mabel C. Rayner.
Prof. E. W. Skeats.
Mr. C. E. Stromeyer.
Dr. W. M. Tattersall.
Mr. Edwin Thompson.
Lieut.-Col. Marett Tims.

XV. A Meeting of Recorders and Local Sectional Secretaries for the Cardiff Meeting, together with the General Secretaries and Dr. W. E. Hoyle, Local Secretary, was held in New College, Oxford, on April 10-12, 1920. Though of an informal character, it was fruitful in discussion of arrangements at Cardiff and of other details in the working of the Association, and the Council hope that such a meeting may become an annual institution.

XVI. The Council received from the General Secretaries a detailed memorandum on the increased cost of printing, showing that the Association could not hope to maintain printing at the level maintained before the war. The Council have put into force a number of alterations in the practice of the Association in this connection, and hope that the General Committee, after experience, will approve them. Taken together, it is hoped that they will save the Association over £600 a year.

XVII. Finally, the Council record with deep regret the death of Mr. H. C. Stewardson, on May 1, 1920, after a short illness. His devoted service to the Association began in 1873, and being in his eightieth year he had intended to retire at the close of the financial year 1919-20.

The Council have instructed the Assistant Secretary to carry on the financial duties undertaken by Mr. Stewardson as Assistant Treasurer.

ADDENDUM.

A verbal addition was made to the above report, when it was presented to the General Committee, expressing the profound regret of the Council at the death of Prof. J. Perry, General Treasurer, which took place on August 4, 1920.*

The Council, at the same time, recorded their regret at the death of Sir Norman Lockyer, President of the Association in 1903.

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* The General Committee, after receiving this report and expressing concurrence with the sentiments of the Council, delegated to the Council the appointment of a General Treasurer for the year 1920-21, and appointed Prof. H. H. Turner as Acting Treasurer in the meantime.

The Council, at its meeting on November 5, 1920, elected Dr. E. H. Griffiths, Sc.D., D.Sc., LL.D., F.R.S., to be General Treasurer for the year 1920-21.

GENERAL MEETINGS AT CARDIFF.

On Tuesday, August 24, at 8 P.M., in the Park Hall, the Hon. Sir Charles Parsons, K.C.B., F.R.S., resigned the office of President to Prof. W. A. Herdman, C.B.E., F.R.S. (See p. xxxi.)

Prof. W. A. Herdman then assumed the chair and delivered an address, for which see p. 1.

On Wednesday, August 25, at 8 P.M., a Reception was given in the City Hall by the Right Hon. the Lord Mayor of Cardiff.

On Thursday, August 26, at 5 P.M., a Conference took place in the Assembly Hall, Technical College, on Science Applied to Public Services, arising out of communications which had passed between the Association and Government Departments as the result of resolutions adopted by the General Committee at the Bournemouth Meeting (see Report, 1919, pp. lxxiii-iv). The Conference was addressed by Mr. F. E. Smith, O.B.E., Director of Scientific Research, Admiralty, and others.

On Thursday, August 26, at 8 P.M., in the Park Hall, Sir R. T. Glazebrook, K.C.B., F.R.S., delivered a discourse on ' Some Requirements of Modern Aircraft.' (See p. 384.)

On Friday, August 27, at 8 P.M., the concluding General Meeting was held in the Park Hall.

Sir Daniel Hall, K.C.B., F.R.S., delivered a discourse on ' A Grain of Wheat from the Field to the Table.' (See p. 389.)

After the above discourse the following resolution was unanimously adopted on the motion of the President:—

That the cordial thanks of the British Association be extended to the Rt. Hon. the Lord Mayor and Corporation and the citizens of the city of Cardiff for their hearty welcome and for the facilities so generously afforded to the Association at the City Hall; to the Governing Bodies of the University of Wales, the University College of South Wales and Monmouthshire, the Technical College, the South Wales Institute of Engineers, and other institutions which have kindly placed their buildings and resources at the disposal of the Association; and, finally, to the Local Executive Committee, the Local Treasurers and Secretaries for their exertions in collecting the necessary funds and for the hospitality which has been freely offered to many members of the Association, as well as for the admirable arrangements made for the eighty-eighth annual meeting of the Association.

PUBLIC OR CITIZENS' LECTURES.

The following public lectures were given in the Park Hall at 8 P.M. on the days stated:—

August 23, Prof. J. Lloyd Williams on ' Light and Life.'

August 25, Prof. A. W. Kirkaldy on ' Present Industrial Conditions.'

August 28, Dr. Vaughan Cornish on ' The Geographical Position of the British Empire.'

Dr. THE GENERAL TREASURER IN ACCOUNT
ADVANCEMENT OF SCIENCE,

RECEIPTS.

	£ s. d.	£ s. d.	£ s. d.
To Balance brought forward :—			
Lloyds Bank, Birmingham		1,728 17 3	
Bank of England—Western Branch :—			
On 'Caird Fund'	508 19 8		
„ General Account	172 4 2		
		681 3 10	
Cash in hand		0 1 11	
		2,410 3 0	
Less Petty Cash overdrawn		2 5 2	
			2,407 17 10
Life Compositions (including Transfers)			734 0 0
Annual Subscriptions			707 0 0
New Annual Members			216 0 0
Sale of Associates' Tickets			645 10 0
Sale of Ladies' Tickets			152 0 0
Life Members (old) Additional Subscriptions			446 13 0
Donations for Research :—			
C. Read		0 10 0	
Rev. F. Smith		2 2 0	
Sir Hugh Bell		100 0 0	
Sir Richard Robinson		25 0 0	
Sir Robert Hadfield		250 0 0	
Sir Charles Parsons		1,000 0 0	
Sir Alfred Yarrow		500 0 0	
Sir C. Bright		1 1 0	
Scientific and Industrial Research Department		600 0 0	
Scientific Research Association		10 13 6	
			2,489 6 6
Sale of Publications			224 11 10
Interest on Deposits :—			
Lloyds Bank, Birmingham		17 2 11	
„ „ 'Caird Gift'		36 5 1	
London County Westminster and Parr's Bank		36 3 9	
			89 11 9
Unexpended Balances of Grants returned		51 19 3	
„ „ 'Caird Fund'		50 0 0	
			101 19 3
Dividends on Investments:—			
Consols 2½ per Cent.		81 8 0	
India 3 per Cent.		75 12 0	
Great Indian Peninsula Railway 'B' Annuity		23 7 2	
War Stock 5 per Cent.		43 3 0	
War Bonds 5 per Cent.		49 0 0	
			272 10 2
Dividends on 'Caird Fund' Investments:—			
India 3½ per Cent.		64 7 4	
Canada 3½ per Cent. (including extra ½ per Cent.)		70 0 0	
London and South-Western Railway Consolidated 4 per Cent. Preference Stock		70 0 0	
London and North-Western Railway Consolidated 4 per Cent. Preference Stock		58 16 0	
			263 3 4

Investments.

£ s. d.	
4,651 10 5	Consolidated 2½ per Cent. Stock
3,600 0 0	India 3 per Cent. Stock
879 14 9	Great Indian Peninsula Railway £43 'B' Annuity
2,627 0 10	India 3½ per Cent. Stock, 'Caird Fund'
2,100 0 0	London and North-Western Railway Consolidated 4 per Cent. Preference Stock, 'Caird Fund'
2,500 0 0	Canada 3½ per Cent. (1930-50) Registered Stock 'Caird Fund'
2,500 0 0	London and South-Western Railway Consolidated 4 per Cent. Preference Stock, 'Caird Fund'
100 19 3	Sir Frederick Bramwell's Gift of 2½ per Cent. Self-Cumulating Consolidated Stock
863 2 10	War Loan 5 per Cent. Stock
1,400 0 0	War Loan 5 per Cent., 1929-47
1,000 0 0	Lloyds Bank, Birmingham—Deposit Account, Sir J. Caird's Gift for Radio-Activity Investigation, included in Balance at Bank

£22,222 8 1	£8,750 3 8

Value at 30th June, 1920, £13,416 8s. 1d.

WITH THE BRITISH ASSOCIATION FOR THE   *Cr.*
July 1, 1919, to June 30, 1920.

PAYMENTS.

		£	s.	d.
By Rent and Office Expenses		292	14	8
Salaries and Travelling Expenses		877	0	0
Printing, Binding, etc.		859	15	3

Grants to Research Committees :—

	£	s.	d.		s.	d.
Liverpool Tidal Institute	150	0	0			
Bronze Implements Committee	100	0	0			
Mathematical Tables	30	0	0			
Geology of Coal Seams	1	13	0			
Free Places in Secondary Education	10	0	0			
Stress Distribution	80	0	0			
Effects of War on Credit	100	0	0			
Replacement of Men by Women	30	0	0			
Breeding Experiments on Œnothera, &c.	30	0	0			
Radiotelegraph Investigations	100	0	0			
Palaeolithic Site in Jersey	5	0	0			
Rude Stone Monuments	20	0	0			
Annual Tables of Constants	40	0	0			
Museums Committee	15	0	0			
Railway Committee	5	0	0			
Heredity Committee	40	0	0			
Palaeozoic Rocks Committee	30	0	0			
Committee on Lepidoptera	50	0	0			
Absorption Spectra Committee	10	0	0			
International Language Committee	5	0	0			
Charts and Pictures Committee	10	0	0			
Kiltorcan Rocks Committee	15	0	0			
Zoological Bibliography	10	0	0			
Seismological Committee	100	0	0			
Stone Circles Committee	15	0	0			
Malta Site	10	0	0			

	£	s.	d.
	1,011	13	0
Expenses Bournemouth Meeting	260	8	7
„ Oxford Meeting	50	5	4
Grants made from 'Caird Fund'	240	0	0

Balance at Lloyds Bank, Birmingham (with Interest accrued), including
Sir James Caird's Gift, Radio-Activity Investigation, of £1,000 and
Interest accrued thereon ... 1,782 5 3
London County Westminster and Parr's Bank, Ltd. 1,854 10 9

Balance at Bank of England, Western Branch :—

	£	s.	d.		s.	d.
On 'Caird Fund'	582	3	0			
„ General Account	938	7	1			
				1,520	10	1

	£	s.	d.
	5,157	6	1
Petty Cash Balance	1	0	9
	5,158	6	10

	£	s.	d.
	£8,750	3	8

I have examined the above Account with the Books and Vouchers of the Association, and certify the same to be correct. I have also verified the Balances at the Bankers, and have ascertained that the Investments are registered in the names of the Trustees, or held by the Bank of England on account of the Association.

APPROVED—
    EDWARD BRABROOK, } *Auditors.*
    ARTHUR L. BOWLEY, }
23 Queen Victoria Street, E.C. 4,
    August 13, 1920.

W. B. KEEN, *Chartered Accountant.*

RESEARCH COMMITTEES, Etc.,

Appointed by the General Committee, Meeting in Cardiff:
August, 1920.

1. **(a)** *Receiving grants of money from the Association for expenses connected with research.* **(b)** *Receiving grants of money from the Association specifically for cost of printing Report.* **(c)** *Grant to be applied for from Public Funds.*

SECTION A.—MATHEMATICS AND PHYSICS.

Seismological Investigations.—Prof. H. H. Turner (*Chairman*), Mr. J. J. Shaw (*Secretary*), Mr. C. Vernon Boys, Dr. J. E. Crombie, Sir H. Darwin, Dr. C. Davison, Sir F. W. Dyson, Sir R. T. Glazebrook, Prof. C. G. Knott, Prof. H. Lamb, Sir J. Larmor, Prof. A. E. H. Love, Prof. H. M. Macdonald, Prof. H. C. Plummer, Mr. W. E. Plummer, Prof. R. A. Sampson, Sir A. Schuster, Sir Napier Shaw, Dr. G. T. Walker, Mr. G. W. Walker. **(b) £10, (c) £90.** †

To assist work on the Tides.—Prof. H. Lamb (*Chairman*), Dr. A. T. Doodson (*Secretary*), Colonel Sir C. F. Close, Dr. P. H. Cowell, Sir H. Darwin, Dr. G. H. Fowler, Admiral F. C. Learmonth, Sir J. E. Petavel, Prof. J. Proudman, Major G. I. Taylor, Prof. D'Arcy W. Thompson, Sir J. J. Thomson, Prof. H. H. Turner. **(b) £35, (c) £165.**

Annual Tables of Constants and Numerical Data, chemical, physical, and technological. —Sir E. Rutherford (*Chairman*), Prof. A. W. Porter (*Secretary*), Mr. A. E. G. Egerton. **(a) £40.**

Determination of Gravity at Sea.—Prof. A. E. H. Love (*Chairman*), Dr. W. G. Duffield (*Secretary*), Mr. T. W. Chaundy, Sir H. Darwin, Prof. A. S. Eddington, Major E. O. Henrici, Sir A. Schuster, and Prof. H. H. Turner. **(a) £10.**

Calculation of Mathematical Tables.—Prof. J. W. Nicholson (*Chairman*), Dr. J. R. Airey (*Secretary*), Mr. T. W. Chaundy, Prof. L. N. G. Filon, Sir G. Greenhill, Colonel Hippisley, Prof. E. W. Hobson, Mr. G. Kennedy, and Profs. Alfred Lodge, A. E. H. Love, H. M. Macdonald, G. B. Mathews, G. N. Watson, and A. G. Webster. **(a) £30, (c) £270.**

SECTION B.—CHEMISTRY.

Colloid Chemistry and its Industrial Applications.—Prof. F. G. Donnan (*Chairman*), Mr. W. Clayton (*Secretary*), Mr. E. Ardern, Dr. E. F. Armstrong, Prof. W. M. Bayliss, Prof. C. H. Desch, Dr. A. E. Dunstan, Mr. H. W. Greenwood, Mr. W. Harrison, Mr. E. Hatschek, Mr. G. King, Prof. W. C. McC. Lewis, Prof. J. W. McBain, Dr. R. S. Morell, Profs. H. R. Proctor and W. Ramsden, Dr. E. J. Russell, Mr. A. B. Searle, Dr. S. A. Shorter, Dr. R. E. Slade, Mr. Sproxton, Dr. H. P. Stevens, Mr. H. B. Stocks, Mr. R. Whymper. **(a) £5, (c)** For printing Report.

Fuel Economy ; Utilisation of Coal ; Smoke Prevention.—Prof. W. A. Bone (*Chairman*), Mr. H. James Yates (*Vice-Chairman*), Mr. Robert Mond (*Secretary*), Mr. A. H. Barker, Prof. P. P. Bedson, Dr. W. S. Boulton, Mr. E. Bury, Prof. W. E. Dalby, Mr. E. V. Evans, Dr. W. Galloway, Sir Robert Hadfield, Bart., Dr. H. S. Hele-Shaw, Mr. D. H. Helps, Dr. G. Hickling, Mr. D. V. Hollingworth, Mr. A. Hutchinson, Principal G. Knox, Mr. Michael Longridge, Prof. Henry Louis, Mr. G. E. Morgans, Mr. W. H. Patchell, Mr. E. D. Simon, Mr. A. T. Smith, Dr. J. E. Stead, Mr. C. E. Stromeyer, Mr. G. Blake Walker, Sir Joseph Walton, Prof. W. W. Watts, Mr. W. B. Woodhouse, and Mr. C. H. Wordingham. **(a) £15, (b) £20.**

† The Committee receives a grant of £100 from the Caird Fund.

Absorption Spectra and Chemical Constitution of Organic Compounds.—Sir J. J. Dobbie (*Chairman*), Prof. E. E. C. Baly (*Secretary*), Dr. A. W. Stewart. **(a) £10, (b) £25.**

Research on Non-Aromatic Diazonium Salts.—Dr. F. D. Chattaway (*Chairman*), Prof. G T. Morgan (*Secretary*), Mr. P. G. W. Bayly and Dr. N. V. Sidgwick. **(a) £10.**

To report on the present state of knowledge in regard of Infra-red Spectra.—Prof. E. E. C. Baly (*Chairman*), (vacant) (*Secretary*), Prof. W. C. McC. Lewis, Prof. F. A. Lindemann, Prof. T. W. Lowry, Prof. T. R. Merton. **(a) £5.**

SECTION C.—GEOLOGY.

The Old Red Sandstone Rocks of Kiltorcan, Ireland.—Prof. Grenville Cole (*Chairman*), Prof. T. Johnson (*Secretary*), Dr. J. W. Evans, Dr. R. Kidston, and Dr. A. Smith Woodward. **(a) £15.**

To excavate Critical Sections in the Palæozoic Rocks of England and Wales.—Prof. W. W. Watts (*Chairman*), Prof. W. G. Fearnsides (*Secretary*), Prof. W. S. Boulton, Mr. E. S. Cobbold, Prof. E. J. Garwood, Mr. V. C. Illing, Dr. Lapworth, Dr. J. E. Marr, and Dr. W. K. Spencer. **(a) £30, (b) £12.**

To consider the Nomenclature of the Carboniferous, Permo-carboniferous, and Permian Rocks of the Southern Hemisphere.—Prof. T. W. Edgeworth David (*Chairman*), Prof. E. W. Skeats (*Secretary*), Mr. W. S. Dun, Prof. J. W. Gregory, Sir T. H. Holland, Messrs. W. Howchin, A. E. Kitson, and G. W. Lamplugh, Dr. A. W. Rogers, Prof. A. C. Seward, Mr. D. M. S. Watson, and Prof. W. G. Woolnough. **(a) £25, (b) £5.**

SECTION D.—ZOOLOGY.

To nominate competent Naturalists to perform definite pieces of work at the Marine Laboratory, Plymouth.—Prof. A. Dendy (*Chairman and Secretary*), Prof. E. S. Goodrich, Prof. J. P. Hill, Prof. S. J. Hickson, Sir E. Ray Lankester. **(a) £200.**

Experiments in Inheritance in Silkworms.—Prof. W. Bateson (*Chairman*), Mrs. Merritt Hawkes (*Secretary*), Dr. F. A. Dixey, Prof. E. B. Poulton, Prof. R. C. Punnett. **(a) £17 2s. 1d.**

Experiments in Inheritance of Colour in Lepidoptera.—Prof. W. Bateson (*Chairman*), The Hon. H. Onslow (*Secretary*), Dr. F. A. Dixey, Prof. E. B. Poulton. **(a) £24, (b) £1.**

Zoological Bibliography and Publication.—Prof. E. B. Poulton (*Chairman*), Dr. F. A. Bather (*Secretary*), Mr. E. Heron-Allen, Dr. W. E. Hoyle, and Dr. P. Chalmers Mitchell. **(a) £1.**

To summon meetings in London or elsewhere for the consideration of matters affecting the interests of Zoology, and to obtain by correspondence the opinion of Zoologists on matters of a similar kind, with power to raise by subscription from each Zoologist a sum of money for defraying current expenses of the organisation.—Prof. S. J. Hickson (*Chairman*), Dr. W. M. Tattersall (*Secretary*), Profs. G. C. Bourne, A. Dendy, J. Stanley Gardiner, W. Garstang, Marcus Hartog, W. A. Herdman, J. Graham Kerr, R. D. Laurie, F. W. MacBride, A. Meek, Dr. P. Chalmers Mitchell, and Prof. E. B. Poulton. **(b) £20.**

Section F.—ECONOMIC SCIENCE AND STATISTICS.

The Effects of the War on Credit, Currency, Finance, and Foreign Exchanges.— Prof. W. R. Scott (*Chairman*), Mr. J. E. Allen (*Secretary*), Prof C. F. Bastable, Sir E. Brabrook, Prof. L. R. Dicksee, Mr. B. Ellinger, Mr. E. L. Franklin, Mr. A. H. Gibson, Mr. C. W. Guilleband, Mr. F. W. Hirst, Prof. A. W. Kirkaldy, Mr. F. Lavington, Mr. E. Sykes, Sir J. C. Stamp, Mr. Hartley Withers, Mr. Hilton Young. **(a) £50.**

Section G.—ENGINEERING.

To report on certain of the more complex Stress Distributions in Engineering Materials. —Prof. E. G. Coker (*Chairman*), Prof. L. N. G. Filon and Prof. A. Robertson (*Secretary*), Prof. A. Barr, Dr. Chas. Chree, Dr. Gilbert Cook, Prof. W. E. Dalby, Sir J. A. Ewing, Messrs. A. R. Fulton and J. J. Guest, Dr. B. P. Haigh, Profs. Sir J. B. Henderson, C. E. Inglis, F. C. Lea, A. E. H. Love, and W. Mason, Sir J. E. Petavel, Dr. F. Rogers, Dr. W. A. Scoble, Mr. R. V. Southwell, Dr. T. E. Stanton, Mr. C. E. Stromeyer, and Mr. J. S. Wilson. (**b**) **£50.**

The Investigation of Gaseous Explosions, with special reference to temperature.— Sir Dugald Clerk (*Chairman*), (Vacant) (*Secretary*), Profs. W. A. Bone, F. W. Burstall, H. L. Callendar, and E. G. Coker, Mr. D. L. Chapman, Prof. H. B. Dixon, Prof. A. H. Gibson, Sir R. T. Glazebrook, Dr. J. A. Harker, Colonel Sir H. C. L. Holden, Sir J. E. Petavel, Mr. D. R. Pye, Mr. H. R. Ricardo, Captain H. R. Sankey, Prof. A. Smithells, and Mr. H. Wimperis. (**b**) **£50.**

Section H.—ANTHROPOLOGY.

To excavate Early Sites in Macedonia.—Prof. Sir W. Ridgeway (*Chairman*), Mr. A. J. B. Wace (*Secretary*), Prof. R. C. Bosanquet, Mr. L. H. D. Buxton, Mr. S. Casson, Dr. W. L. H. Duckworth, Prof. J. L. Myres. (**a**) **£50.**

To excavate a Palæolithic Site in Jersey.—Dr. R. R. Marett (*Chairman*), Mr. G. de Gruchy (*Secretary*), Dr. C. W. Andrews, Mr. H. Balfour, Prof. A. Keith, and Colonel Warton. (**b**) **£1.**

To report on the Classification and Distribution of Rude Stone Monuments.—Dr. R. R. Marett (*Chairman*), Prof. H. J. Fleure (*Secretary*), Mr. L. H. D. Buxton, Prof. J. L. Myres, Mr. H. Peake. (**a**) **£25,** (**b**) **£1.**

To report on the Distribution of Bronze Age Implements.—Prof. J. L. Myres (*Chairman*), Mr. H. Peake (*Secretary*), Dr. E. C. R. Armstrong, Dr. H. A. Auden, Mr. H. Balfour, Mr. L. H. D. Buxton, Mr. O. G. S. Crawford, Sir W. Boyd Dawkins, Prof. H. J. Fleure, Mr. G. A. Garfitt, Dr. R. R. Marett, Mr. R. Mond, Sir C. H. Read, Sir W. Ridgeway. (**a**) **£100,** (**b**) **£1.**

To conduct Archæological Investigations in Malta.—Prof. J. L. Myres (*Chairman*), Prof. A. Keith (*Secretary*), Dr. T. Ashby, Mr. H. Balfour, Dr. A. C. Haddon, Dr. R. R. Marett, and Mr. H. Peake. (**a**) **£50.**

Section I.—PHYSIOLOGY.

Ductless Glands.—Sir E. Sharpey Schafer (*Chairman*), Prof. Swale Vincent (*Secretary*), Dr. R. J. S. McDowall. (**c**) **£30.**

Section K.—BOTANY.

Experimental Studies in the Physiology of Heredity.—Dr. F. F. Blackman (*Chairman*), Miss E. R. Saunders (*Secretary*), Profs. Bateson and Keeble. (**a**) **£10,** (**c**) **£90.**

To continue Breeding Experiments on Oenothera and other Genera.—Dr. A. B. Rendle (*Chairman*), Dr. R. R. Gates (*Secretary*), Prof. W. Bateson, Mr. W. Brierley, Prof. O. V. Darbishire, Dr. M. C. Rayner. (**a**) **£25.**

Primary Botanical Survey in Wales.—Dr. E. N. Miles Thomas (*Chairman*), Miss Wortham (*Secretary*), Miss Davey, Prof. F. W. Oliver, Prof. Stapledon, Principal A. H. Trow. (**a**) **£20.**

SECTION L.—EDUCATIONAL SCIENCE.

Training in Citizenship.—Rt. Rev. J. E. C. Welldon (*Chairman*), Lady Shaw (*Secretary*), Sir R. Baden-Powell, Mr. C. H. Blakiston, Mr. G. D. Dunkerley, Mr. W. D. Eggar, Mr. C. R. Fay, Principal J. C. Maxwell Garnett, Sir R. A. Gregory, and Sir T. Morison. **(a) £15, (b) £10, (c) £50.**

To inquire into the provision of Educational Pictures for display in schools.—Sir R. A. Gregory (*Chairman*), Mr. G. D. Dunkerley (*Secretary*), Mr. C. E. Browne, Miss L. J. Clarke, Mr. C. B. Fawcett, Mr. E. N. Fallaize, Prof. S. J. Hickson, Mr. O. J. R. Howarth, Mr. C. G. T. Morison, Mr. H. J. E. Peake, Prof. S. H. Reynolds, Prof. H. E. Roaf, Sir Napier Shaw, Dr. T. W. Woodhead. **(a) £6. 10s., (b) £16.**

To inquire into the work being done by University bureaux in furthering the interchange of Students (particularly post-graduates) between home and foreign Universities, and to consider what steps can be taken to increase their spheres of action.—Mr. D. Berridge (*Secretary*). **(a) £5.**

To inquire into the Practicability of an International Auxiliary Language.—Mr. W. B. Hardy (*Chairman*), Dr. E. H. Tripp (*Secretary*), Mr. E. Bullough, Prof. J. J. Findlay, Sir Richard Gregory, Dr. C. W. Kimmins, Dr. H. Foster Morley, Sir E. Cooper Perry, Prof. W. Ripman, Mr. F. Nowell Smith, Mr. A. E. Twentyman. **(a) £7. 10s., (b) £15.**

CORRESPONDING SOCIETIES.

Corresponding Societies Committee for the preparation of their Report.—Mr. W. Whitaker (*Chairman*), Mr. W. Mark Webb (*Secretary*), Mr. P. J. Ashton, Dr. F. A. Bather, Rev. J. O. Bevan, Sir Edward Brabrook, Sir H. G. Fordham, Mr. A. L. Lewis, Mr. T. Sheppard, Rev. T. R. R. Stebbing, Mr. Mark L. Sykes, and the President and General Officers of the Association. **(a) £40, (b) £30.**

2. *Not receiving Grants of Money.*

SECTION A.—MATHEMATICAL AND PHYSICAL SCIENCE.

Radiotelegraphic Investigations.—Sir Oliver Lodge (*Chairman*), Dr. W. H. Eccles (*Secretary*), Mr. S. G. Brown, Dr. C. Chree, Sir F. W. Dyson, Prof. A. S. Eddington, Dr. Erskine-Murray, Profs. J. A. Fleming, G. W. O. Howe, H. M. Macdonald, and J. W. Nicholson, Sir H. Norman, Captain H. R. Sankey, Sir A. Schuster, Sir Napier Shaw, and Prof. H. H. Turner.

Investigation of the Upper Atmosphere.—Sir Napier Shaw (*Chairman*), Mr. C. J. P. Cave (*Secretary*), Prof. S. Chapman, Mr. J. S. Dines, Mr. W. H. Dines, Sir R. T. Glazebrook, Col. E. Gold, Dr. H. Jeffreys, Sir J. Larmor, Mr. R. G. K. Lempfert, Prof. F. A. Lindemann, Dr. W. Makower, Sir J. E. Petavel, Sir A. Schuster, Dr. G. C. Simpson, Mr. F. J. W. Whipple, Prof. H. H. Turner.

To aid the work of Establishing a Solar Observatory in Australia.—Prof. H. H. Turner, (*Chairman*), Dr. W. G. Duffield (*Secretary*), Rev. A. L. Cortie, Dr. W. J. S. Lockyer, Mr. F. McClean, and Sir A. Schuster.

SECTION C.—GEOLOGY.

The Collection, Preservation, and Systematic Registration of Photographs of Geological Interest.—Prof. E. J. Garwood (*Chairman*), Prof. S. H. Reynolds (*Secretary*), Mr. G. Bingley, Dr. T. G. Bonney, Messrs. C. V. Crook, R. Kidston, and A. S. Reid, Sir J. J. H. Teall, Prof. W. W. Watts, and Messrs. R. Welch and W. Whitaker.

To consider the preparation of a List of Characteristic Fossils.—Prof. P. F. Kendall (*Chairman*), Dr. W. T. Gordon (*Secretary*), Prof. W. S. Boulton, Dr. A. R. Dwerryhouse, Profs. J. W. Gregory, Sir T. H. Holland, and S. H. Reynolds, Dr. Marie C. Stopes, Dr. J. E. Marr, Prof. W. W. Watts, Mr. H. Woods, and Dr. A. Smith Woodward.

To investigate the Flora of Lower Carboniferous times as exemplified at a newly-discovered locality at Gullane, Haddingtonshire.—Dr. R. Kidston (*Chairman*), Dr. W. T. Gordon (*Secretary*), Dr. J. S. Flett, Prof. E. J. Garwood, Dr. J. Horne, and Dr. B. N. Peach.

SECTION D.—ZOOLOGY.

To aid competent Investigators selected by the Committee to carry on definite pieces of work at the Zoological Station at Naples.— Mr. E. S. Goodrich (*Chairman*), Prof. J. H. Ashworth (*Secretary*), Dr. G. P. Bidder, Prof. F. O. Bower, Drs. W. B. Hardy, Sir S. F. Harmer, Prof. S. J. Hickson, Sir E. Ray Lankester, Prof. W. C. McIntosh, Dr. A. D. Waller.†

The collection of Marsupials for work upon (*a*) the reproductive apparatus and development, (*b*) the brain.—Prof. A. Dendy (*Chairman*), Dr. G. E. Nicholls (*Secretary*), Profs. W. J. Dakin, T. Flynn, J. P. Hill, E. B. Poulton, and G. Elliot Smith, Dr. Marett Tims.

Section F.—ECONOMIC SCIENCE AND STATISTICS.

Replacement of Men by Women in Industry.—Prof. W. R. Scott (*Chairman*), Miss Grier (*Secretary*), Miss Ashley, Mr. J. Cunnison, Mr. Daniels, Mr. C. R. Fay, Mr. J. E. Highton, and Professor A. W. Kirkaldy.

Section H.—ANTHROPOLOGY.

The Collection, Preservation, and Systematic Registration of Photographs of Anthropological Interest.—Sir C. H. Read (*Chairman*), Mr. E. N. Fallaize (*Secretary*), Dr. G. A. Auden, Dr. H. O. Forbes, Mr. E. Heawood, and Prof. J. L. Myres.

To conduct Explorations with the object of ascertaining the Age of Stone Circles.— Sir C. H. Read (*Chairman*), Mr. H. Balfour (*Secretary*), Dr. G. A. Auden, Prof. Sir W. Ridgeway, Dr. J. G. Garson, Sir Arthur Evans, Sir W. Boyd Dawkins, Prof. J. L. Myres, Mr. A. L. Lewis, and Mr. H. Peake.

To conduct Archæological and Ethnological Researches in Crete.—Mr. D. G. Hogarth (*Chairman*), Prof. J. L. Myres (*Secretary*), Prof. R. C. Bosanquet, Dr. W. L. H. Duckworth, Sir A. Evans, Sir W. Ridgeway, Dr. F. C. Shrubsall.

To conduct Anthropometric Investigations in the Island of Cyprus.—Prof. J. L. Myres (*Chairman*), Dr. F. C. Shrubsall (*Secretary*), Mr. L. H. Dudley Buxton, Dr. A. C. Haddon.

To co-operate with Local Committees in excavation on Roman Sites in Britain.— Sir W. Ridgeway (*Chairman*), Mr. H. J. E. Peake (*Secretary*), Dr. T. Ashby, Mr. Willoughby Gardner, Prof. J. L. Myres.

To report on the present state of knowledge of the Ethnography and Anthropology of the Near and Middle East.—Dr. A. C. Haddon (*Chairman*), Mr. L. H. Dudley Buxton (*Secretary*), Mr. S. Casson, Prof. H. J. Fleure, Mr. H. J. E. Peake.

† Grant of £100 from Caird Fund : see p. xxx.

To report on the present state of knowledge of the relation of early Palæolithic Instruments to Glacial Deposits.—Mr. H. J. E. Peake (*Chairman*), Mr. E. N. Fallaize (*Secretary*), Mr. H. Balfour.

SECTION I.—PHYSIOLOGY.

Electromotive Phenomena in Plants.—Dr. A. D. Waller (*Chairman*), Mrs. Waller (*Secretary*), Prof. J. B. Farmer, Mr. J. C. Waller.

Food Standards and Man-power.—Prof. W. D. Halliburton (*Chairman*), Dr. A. D. Waller (*Secretary*), Prof. E. H. Starling.

SECTION K.—BOTANY.

To consider the possibilities of investigation of the Ecology of Fungi, and assist Mr. J. Ramsbottom in his initial efforts in this direction.—Mr. H. W. T. Wager (*Chairman*), Mr. J. Ramsbottom and Miss A. Lorrain Smith (*Secretaries*), Mr. W. B. Brierley, Mr. F. T. Brooks, Mr. W. N. Cheesman, Prof. T. Johnson, Prof. M. C. Potter, Mr. L. Carleton Rea, and Mr. E. W. Swanton.

SECTION L.—EDUCATION.

The Influence of School Books upon Eyesight.—Dr. G. A. Auden (*Chairman*), Mr. G. F. Daniell (*Secretary*), Mr. C. H. Bothamley, Mr. W. D. Eggar, Sir R. A. Gregory, Dr. N. Bishop Harman, Mr. J. L. Holland, Dr. W. E. Sumpner, and Mr. Trevor Walsh.

CORRESPONDING SOCIETIES COMMITTEE.

To take steps to obtain Kent's Cavern for the Nation.—Mr. W. Whitaker (*Chairman*), Mr. W. M. Webb (*Secretary*), Prof. Sir W. Boyd Dawkins, Mr. Mark L. Sykes.

Research Committees 'in Suspense.'

The work of the following Committees is in suspense until further notice. The *personnel* of these Committees will be found in the Report for 1917.

SECTION D.—ZOOLOGY.

An investigation of the Biology of the Abrolhos Islands and the North-west Coast of Australia (north of Shark's Bay to Broome), with particular reference to the Marine Fauna.

Nomenclator Animalium Genera et Sub-genera.

SECTION H.—ANTHROPOLOGY.

To investigate the Physical Characters of the Ancient Egyptians.

To prepare and publish Miss Byrne's Gazetteer and Map of the Native Tribes of Australia.

To investigate the Lake Villages in the neighbourhood of Glastonbury in connection with a Committee of the Somerset Archæological and Natural History Society.

SECTION K.—BOTANY.

The Renting of Cinchona Botanic Station in Jamaica.

RESOLUTIONS AND RECOMMENDATIONS.

The following Resolutions and Recommendations were referred to the Council (unless otherwise stated) by the General Committee at Cardiff for consideration and, if desirable, for action :—

From Section A.

That H.M. Stationery Office be asked to print the Tables on Congruence Solutions prepared by Lieut.-Col. A. Cunningham and Mr. T. G. Creak.

From Sections A and E.

(1) That this joint meeting of Sections A and E strongly urges upon the General Committee the desirability of printing in the report of the Association the paper read before it by Principal E. H. Griffiths and Major E. O. Henrici on ' The Need for a Central British Institute for Training and Research in Surveying, Hydrography, and Geodesy ' *; and (2) that the meeting calls the attention of the Council to the urgency of the question at the present time, and begs that the Council will again give attention to the subject.

From Section B.

That Section B requests the Council to recommend to the appropriate authorities the great desirability of continuing the experiments on the production of industrial alcohol now in progress, by aid of the installations now existing in Government establishments.

From Section C.

That the Committee of Section C intimate to the Council that it regards the forecasting of the length of Committee reports as in many cases impossible.

From Section D.

Unanimously agreed by the Committee of Section D (thirty-nine present) that it be a recommendation to the Zoology Organisation Committee that no scheme of payment of professional zoologists in the service of the State is satisfactory which places them on a lower level than that of the higher grade of the Civil Service.

(The above Resolution received the support of representatives of other Sections, and the General Committee directed that its consideration and any action upon it should take account of the position of workers in other branches of science.)

From Section D.

That Section D is profoundly impressed with the importance of urging the initiation of a further National Expedition for the Exploration of the Ocean, and requests the Council of the British Association to appoint a Committee to take the necessary steps to impress this need upon His Majesty's Government and the nation.

(The above Resolution was supported by the Committees of all Sections concerned.)

* This Recommendation was sanctioned by the General Committee.

From Section E.

That this meeting of Section E of the British Association, being convinced by the results already obtained of the value as an educational instrument and as a work of national importance of the scheme recently initiated by the Welsh Department of the Board of Education for the collection of Rural Lore and Regional Survey material through the medium of the elementary and secondary schools and colleges, heartily approves the same, and expresses the earnest hope that the scheme may be widely taken up throughout the country.

From Section H (see preceding Resolution).

That the Committee of Section H, Cardiff, August 1920, views with interest and appreciation the scheme of the Welsh Department of the Board of Education for the collection of Rural Lore through the agency of the schools, and hopes that steps may be taken to apply the scheme, mutatis mutandis, to other parts of Great Britain.

From Section E.

That the Committee of Section E (Geography) of the British Association for the Advancement of Science begs leave to ask the President of the Board of Education to give schools permission to include geography as a subject on a level with the other subjects in advanced courses of suitable type in mathematics and science, in classics, and in modern studies.

From Section E.

The Committee of Section E of the British Association meeting at Cardiff (1920) expresses its appreciation of the opportunity of co-operation in the work of the National Committee on Geographical Research afforded by the Royal Society, but it begs leave to suggest that the purpose might be served more fully if the Section were permitted to nominate a representative for a period of two or three years in place of the nomination of the President of the Section who retires annually.

The Committee begs to suggest, if their recommendation be adopted, that Prof. J. L. Myres be nominated as their representative.

From Section H.

That the following Committees be authorised to obtain financial assistance from sources other than the Association * :

(a) Archæological Investigations in Malta.
(b) Bronze Age Implements.
(c) Palæolithic Site in Jersey.
(d) Rude Stone Monuments.

From Section H.

That this Association urges upon the Government of the Union of South Africa the desirability of instituting an Ethnological Bureau for the purpose of studying the racial characteristics, languages, institutions, and beliefs of the native population of South Africa, in order that any attempt which may be made to bring this population into closer touch with the course of social and economic development in South Africa may be based upon a scientific knowledge and an understanding of its psychology, mode of life, and institutions.

* This Recommendation was sanctioned by the General Committee.

From Section H.

That this Association would urge upon the Government of Western Australia the desirability of instituting forthwith an anthropological survey of the aboriginal population now living under Government protection on Government reservations, stations, and elsewhere in Western Australia, in order that a record may be made of the physical measurements, languages, customs, and beliefs of these tribes, before this material, of great scientific importance, is lost by the death of the older members of the tribes or impaired in value by contact with civilisation.

From Section H.

That the attention of this Association having been called to the present deplorable condition of the aboriginal population of Central Australia, it would urge upon the Federal Government of the Commonwealth of Australia, the Government of South Australia, and the Government of Western Australia the necessity for (1) the declaration of an absolute reservation on some part of the lands at present inhabited by these tribes, such as, for instance, the Musgrave, Mann, and Tomkinson Ranges, upon which all may be located under State protection and supervision; and (2) the institution of a medical service for the aborigines to check the ravages of tuberculosis and other diseases now rife among them.

From Section H.

That in future years Associations for the Advancement of Science in the Dominions and in Foreign Countries be invited to send official representatives to attend the annual meetings of this Association.

From Section H.

Recommendations * in reference to printing of reports of Research Committees 1919-20 :

(a) Archæological Investigations in Malta :—That the Government of Malta be asked to contribute £50 towards the cost of printing this report in the Journal of the Royal Anthropological Institute on the condition that copies of the report will be available for sale in the Island of Malta.

(b) That Mr. Willoughby Gardner's report on the Excavations at Dinorben in 1919-20 be printed, *in abstract* only, as an appendix to the report of the Roman Sites Committee for 1919-20.

From Sections H and L.

That this Association, while viewing with approbation the recent regulation of the Board of Education (Circular 1153, March 31, 1908), where anthropometric observations may be included in the medical inspection of Continuation Schools, would urge upon the Board the desirability of extending this provision to all schools in receipt of Government grant for a limited period of, say, five years, in order that, as a result of such a survey, standards of comparison may be available in the future for the purpose of both medical inspection and scientific investigation.

From Section I.

The Committee of Section I recommend to the General Committee of the British Association that a separate Section of Psychology be formed.

(The above Recommendation was supported by representatives of Section L, and was approved by the General Committee subject to the approval of the Council.)

* These Recommendations were sanctioned by the General Committee.

From Section K.

That Government support is desired for the afforestation experiments on pit-mounds being conducted by the Midland Reafforesting Committee.

From Section L.

Section L ask the Council to give power to the Organising Committee of Section L, if they think fit, to allow a book upon Citizenship, based upon the syllabus in Appendix I. of the 1920 Report of the Committee upon Training in Citizenship, to be published, with a foreword to the effect that the book has the approval of the Organising Committee of Section L of the British Association.

From Section L.

That 500 short copies of the Reports on Museums and on Training in Citizenship (1920) be printed from the standing type.*

* This Recommendation was sanctioned by the General Committee.

THE CAIRD FUND.

An unconditional gift of 10,000*l.* was made to the Association at the Dundee Meeting, 1912, by Mr. (afterwards Sir) J. K. Caird, LL.D., of Dundee.

The Council, in its report to the General Committee at the Birmingham Meeting, made certain recommendations as to the administration of this Fund. These recommendations were adopted, with the Report, by the General Committee at its meeting on September 10, 1913.

The following allocations have been made from the Fund by the Council to August 1920 :—

Naples Zoological Station Committee (p. xxiv).—50*l.* (1912–13) ; 100*l.* (1913–14) ; 100*l.* annually in future, subject to the adoption of the Committee's report. (Reduced to 50*l.* during war.)

Seismology Committee (p. xx).—100*l.* (1913–14) ; 100*l.* annually in future, subject to the adoption of the Committee's report.

Radiotelegraphic Committee (p. xxiii).—500*l.* (1913–14).

Magnetic Re-survey of the British Isles (in collaboration with the Royal Society).—250*l.*

Committee on Determination of Gravity at Sea (p. xx).—100*l.* (1914–15).

Mr. F. Sargent, Bristol University, in connection with his Astronomical Work.—10*l.* (1914).

Organising Committee of Section F (*Economics*), *towards expenses of an Inquiry into Outlets for Labour after the War.*—100*l.* (1915).

Rev. T. E. R. Phillips, for aid in transplanting his private observatory.—20*l.* (1915).

Committee on Fuel Economy (p. xx). —25*l.* (1915–16), 10*l.* (1919–20).

Committee on Training in Citizenship (*p.* xxiii).—10*l.* (1919–20).

Geophysical Committee of Royal Astronomical Society.—10*l.* (1920).

Conjoint Board of Scientific Societies.—10*l.* (1920).

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Sir J. K. Caird, on September 10, 1913, made a further gift of 1,000*l.* to the Association, to be devoted to the study of Radio-activity.

INAUGURAL GENERAL MEETING.

Tuesday, August 24, 1920.

In the course of his speech introducing his successor, the President, the Hon. Sir Charles Parsons, K.C.B., F.R.S., said:—

The General Committee have authorised me to send the following telegram to His Majesty the King :—

YOUR MAJESTY,

The members of the British Association for the Advancement of Science desire to express their loyal devotion to your Majesty, and at this their meeting in the Principality of Wales hope that they may be permitted to congratulate your Majesty on the splendid work done by the Prince of Wales, which has drawn towards him the thoughts and the hearts of the whole Empire.

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We have to record with deep regret that since our meeting at Bournemouth last year the Association has lost two of its most devoted and valued officers.

Professor John Perry, F.R.S., General Treasurer of the Association since 1904, died at his London residence on August 4 at the age of seventy. He had only returned two months ago from a long voyage round South America, undertaken for the benefit of his health. It had, however, not produced the desired result; the affection of his heart increased, and the end came suddenly. Professor Perry was widely known as an eminent mathematician, and as one who had directed most of his life to introducing mathematics as a practical science—his numerous books are well known in this country and America, and have been translated into many foreign languages. He was at one time assistant to Lord Kelvin, and helped in the perfecting of the Kelvin electrostatic voltmeter. In association with Ayrton he was a pioneer in the early developments of electrical instruments, storage batteries, and on the applications of electricity. He was a Past-President of the Institute of Electrical Engineers and of the Physical Society. One of his most famous lectures was on ' Spinning Tops,' delivered at the British Association meeting at Leeds in 1890, and his recent work in the perfecting of the gyroscopic compass is well known. His genial, warm-hearted kindness endeared him not only to his wide circle of friends, but also to his colleagues and students, and there are few members of this Association who do not feel a blank that it is difficult to fill.

Henry Charles Stewardson, Assistant Treasurer of the British Association for many years, entered the services of the Association in 1873 in a clerical capacity, but, through his ability for finance, soon became Assistant Treasurer, and the Association undoubtedly owes much to his careful economies and to his accurate forecasts of the balance available for grants to research, which guided the Committee of Recommendations each year. He missed no annual meeting, and many members gratefully remember his help and courtesy in the Reception Room. His health was failing at the last meeting, but he continued to discharge his duties until within four days of his death, on May 1 last, in his eightieth year.

The death of Sir Norman Lockyer, F.R.S., on Monday of last week, deprives the world of a great astronomer, and the nation of a force which it can ill afford to lose. By applying the spectroscope to the sun he furnished the means of studying its surface without waiting for an eclipse; revealed in 1868 the prominences as local disturbances in the chromosphere; and observed in the sun the gas, named by him helium, and afterwards identified on the earth by Sir William Ramsay. More than half a century ago Sir Norman founded that admirable scientific journal ' Nature.' He also founded the British Science

Guild in 1905.   His Presidential Address to the British Association at South-port sixteen years ago, on 'The Influence of Brain Power on History,' attracted wide attention, but it has taken the greatest war in history to awaken national consciousness to its significance.

I have now the pleasure of introducing to you my successor in this chair, an eminent biologist who has directed his great talents with indefatigable energy to the study of the life that exists in the vast spaces and depths of the ocean, which covers nearly three-fourths of our globe.   Few people give much thought to the ocean beyond the fact that it carries our ships and is the source of most of the fish which we eat.   But the work of investigating what goes on within the ocean, a work in which Professor Herdman has taken so arduous and prominent a part, has revealed a life within it, both vegetable and animal, of great complexity and of enormous magnitude, but governed by laws chemical and physical which are being gradually discovered.   It is indeed difficult to realise, as Professor Herdman has stated, that in some seas a cubic mile of water may contain as much as 30,000 tons of living organisms whose life history depends on the light of the sun, thermal currents in the ocean, and seasonal changes, and that those organisms form the staple food of the fishes which we eat.   The difficulties of these investigations must have been enormous, requir-ing the resources of science, consummate skill, and indefatigable energy to overcome them.   Many years ago Professor Herdman created a fisheries labora-tory in the University of Liverpool, created and brought into co-operation with it a biological station at Port Erin, and arranged periodical ocean trips for dredging and collecting marine organisms.   A year ago he endowed a chair of oceanography at Liverpool, the first on this subject in the British Isles.   He also founded, two years earlier, the chair of geology in memory of his only son George Herdman, one of those young men of brilliant promise killed in the war.   His enthusiasm and sympathy have made him beloved by his pupils, as indeed by zoologists in general, and his work has led to the throwing of much additional light on the marine life of our globe.

The President-Elect, Professor W. A. Herdman, C.B.E., D.Sc., LL.D., F.R.S., then took the chair, and delivered the Presidential Address, which is printed below (pp. 1-33).

The following gracious reply was received from His Majesty the King to the telegram quoted on p. xxxi:—

I have received with much pleasure and satisfaction the message which you have addressed to me on behalf of the members of the British Association, and in thanking them for their loyal assurances to myself I feel greatly touched at the kind references to my son, which are the more appreciated coming as these do from the members of this distinguished Society assembled in the Principality of Wales.   I shall follow your deliberations with close interest, and I gratefully recognise all that is being done for the advancement of civilisation by the men of science.   GEORGE R.I.

CARDIFF: 1920.

ADDRESS

BY

WILLIAM A. HERDMAN, C.B.E., D.Sc., Sc.D., LL.D., F.R.S.,

Professor of Oceanography in the University of Liverpool,

PRESIDENT.

Oceanography and the Sea-Fisheries.

IT has been customary, when occasion required, for the President to offer a brief tribute to the memory of distinguished members of the Association lost to Science during the preceding year. These, for the most part, have been men of advanced years and high reputation, who had completed their life-work and served well in their day the Association and the sciences which it represents. Such are our late General Treasurer, Professor Perry, and our Past-President, Sir Norman Lockyer, of whom the retiring President has just spoken.[1] We have this year no other such losses to record; but it seems fitting on the present occasion to pause for a moment and devote a grateful thought to that glorious band of fine young men of high promise in science who, in the years since our Australian meeting in 1914, gave, it may be, in brief days and months of sacrifice, greater service to humanity and the advance of civilisation than would have been possible in years of normal time and work. A few names stand out already known and highly honoured—Moseley, Jenkinson, Geoffrey Smith, Keith Lucas, Hopkinson, Gregory, and more recently Leonard Doncaster—all grievous losses; but there are also others, younger members of our Association, who had not yet had opportunity for showing accomplished work, but who equally gave up all for a great ideal. I prefer to offer a collective rather than an individual tribute. Other young men of science will arise and carry on their work—but the gap in our ranks remains. Let their successors remember that it serves as a reminder of a great example and of high endeavour worthy of our gratitude and of permanent record in the annals of Science.

AT the last Cardiff Meeting of the British Association in 1891 you had as your President the eminent astronomer Sir William Huggins, who discoursed upon the then recent discoveries of the spectroscope in relation to the chemical nature, density, temperature, pressure and even the motions of the stars. From the sky to the sea is a long drop; but the sciences of both have this in common, that they deal with

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[1] See p. xxx., *ante.*

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fundamental principles and with vast numbers. Over three hundred years ago Spenser in the 'Faerie Queene' compared ' the seas abundant progeny ' with ' the starres on hy,' and recent investigations show that a litre of sea-water may contain more than a hundred times as many living organisms as there are stars visible to the eye on a clear night.

During the past quarter of a century great advances have been made in the science of the sea, and the aspects and prospects of sea-fisheries research have undergone changes which encourage the hope that a combination of the work now carried on by hydrographers and biologists in most civilised countries on fundamental problems of the ocean may result in a more rational exploitation and administration of the fishing industries.

And yet even at your former Cardiff Meeting thirty years ago there were at least three papers of oceanographic interest—one by Professor Osborne Reynolds on the action of waves and currents, another by Dr. H. R. Mill on seasonal variation in the temperature of lochs and estuaries, and the third by our Honorary Local Secretary for the present meeting, Dr. Evans Hoyle, on a deep-sea tow-net capable of being opened and closed under water by the electric current.

It was a notable meeting in several other respects, of which I shall merely mention two. In Section A, Sir Oliver Lodge gave the historic address in which he expounded the urgent need, in the interests of both science and the industries, of a national institution for the promotion of physical research on a large scale. Lodge's pregnant idea put forward at this Cardiff Meeting, supported and still further elaborated by Sir Douglas Galton as President of the Association at Ipswich, has since borne notable fruit in the establishment and rapid development of the National Physical Laboratory. The other outstanding event of that meeting is that you then appointed a committee of eminent geologists and naturalists to consider a project for boring through a coral reef, and that led during following years to the successive expeditions to the atoll of Funafuti in the Central Pacific, the results of which, reported upon eventually by the Royal Society, were of great interest alike to geologists, biologists, and oceanographers.

Dr. Huggins, on taking the Chair in 1891, remarked that it was over thirty years since the Association had honoured Astronomy in the selection of its President. It might be said that the case of Oceanography is harder, as the Association has never had an Oceanographer as President—and the Association might well reply ' Because until very recent years there has been no Oceanographer to have.' If Astronomy is the oldest of the sciences, Oceanography is probably the youngest. Depending as it does upon the methods and results of other sciences, it was not until our knowledge of Physics, Chemistry, and **Biology** was

relatively far advanced that it became possible to apply that knowledge to the investigation and explanation of the phenomena of the ocean. No one man has done more to apply such knowledge derived from various other subjects and to organise the results as a definite branch of science than the late Sir John Murray, who may therefore be regarded as the founder of modern Oceanography.

It is, to me, a matter of regret that Sir John Murray was never President of the British Association. I am revealing no secret when I tell you that he might have been. On more than one occasion he was invited by the Council to accept nomination, and he declined for reasons that were good and commanded our respect. He felt that the necessary duties of this post would interfere with what he regarded as his primary life-work—oceanographical explorations already planned, the last of which he actually carried out in the North Atlantic in 1912, when over seventy years of age, in the Norwegian steamer *Michael Sars*, along with his friend Dr. Johan Hjort.

Anyone considering the subject-matter of this new science must be struck by its wide range, overlapping as it does the borderlands of several other sciences and making use of their methods and facts in the solution of its problems. It is not only world-wide in its scope but extends beyond our globe and includes astronomical data in their relation to tidal and certain other oceanographical phenomena. No man in his work, or even thought, can attempt to cover the whole ground—although Sir John Murray, in his remarkably comprehensive ' Summary ' volumes of the *Challenger* Expedition and other writings, went far towards doing so. He, in his combination of physicist, chemist, geologist and biologist, was the nearest approach we have had to an all-round Oceanographer. The International Research Council probably acted wisely at the recent Brussels Conference in recommending the institution of two International Sections in our subject, the one of physical and the other of biological Oceanography—although the two overlap and are so interdependent that no investigator on the one side can afford to neglect the other.[2]

On the present occasion I must restrict myself almost wholly to the latter division of the subject, and be content, after brief reference to the

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[2] The following classification of the primary divisions of the subject may possibly be found acceptable :—

Physiography

Oceanography   Geography

Hydrography	Metabolism	Bionomics	Tidology
(Physics, &c.)	(Bio-Chemistry)	(Biology)	(Mathematics)
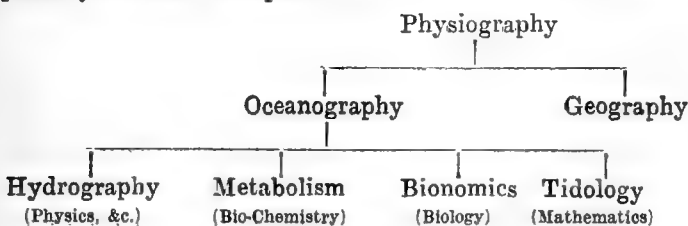
founders and pioneers of our science, to outline a few of those investigations and problems which have appeared to me to be of fundamental importance, of economic value, or of general interest.

Although the name Oceanography was only given to this branch of science by Sir John Murray in 1880, and although according to that veteran oceanographer Mr. J. Y. Buchanan, the last surviving member of the civilian staff of the *Challenger*, the science of Oceanography was born at sea on February 15, 1873,[3] when, at the first official dredging station of the expedition, to the westward of Teneriffe, at 1525 fathoms, everything that came up in the dredge was new and led to fundamental discoveries as to the deposits forming on the floor of the ocean, still it may be claimed that the foundations of the science were laid by various explorers of the ocean at much earlier dates. Aristotle, who took all knowledge for his province, was an early oceanographer on the shores of Asia Minor. When Pytheas passed between the Pillars of Hercules into the unknown Atlantic and penetrated to British seas in the fourth century B.C., and brought back reports of Ultima Thule and of a sea to the North thick and sluggish like a jelly-fish, he may have been recording an early planktonic observation. But passing over all such and many other early records of phenomena of the sea, we come to surer ground in claiming, as founders of Oceanography, Count Marsili, an early investigator of the Mediterranean, and that truly scientific navigator Captain James Cook, who sailed to the South Pacific on a Transit of Venus expedition in 1769 with Sir Joseph Banks as naturalist, and by subsequently circumnavigating the South Sea about latitude 60° finally disproved the existence of a great southern continent; and Sir James Clerk Ross, who, with Sir Joseph Hooker as naturalist, first dredged the Antarctic in 1840.

The use of the naturalist's dredge (introduced by O. F. Müller, the Dane, in 1799) for exploring the sea-bottom was brought into prominence almost simultaneously in several countries of North-West Europe —by Henri Milne-Edwards in France in 1830, Michael Sars in Norway in 1835, and our own Edward Forbes about 1832.

The last-mentioned genial and many-sided genius was a notable figure in several sections of the British Association from about 1836 onwards, and may fairly be claimed as a pioneer of Oceanography. In 1839 he and his friend the anatomist, John Goodsir, were dredging

[3] Others might put the date later. Significant publications are Sir John Murray's Summary Volumes of the *Challenger* (1895), the inauguration of the 'Musée Océanographique' at Monaco in 1910, the foundation of the 'Institut Océanographique' at Paris in 1906 (see the Prince of Monaco's letter to the Minister of Public Instruction), and Sir John Murray's little book *The Ocean* (1913), where the superiority of the term 'Oceanography' to 'Thalassography' (used by Alexander Agassiz) is discussed.

in the Shetland seas, with results which Forbes made known to the meeting of the British Association at Birmingham that summer, with such good effect that a ' Dredging Committee ' [4] of the Association was formed to continue the good work. Valuable reports on the discoveries of that Committee appear in our volumes at intervals during the subsequent twenty-five years.

It has happened over and over again in history that the British Association, by means of one of its research committees, has led the way in some important new research or development of science and has shown the Government or an industry what wants doing and how it can be done. We may fairly claim that the British Association has inspired and fostered that exploration of British seas which through marine biological investigations and deep-sea expeditions has led on to modern Oceanography. Edward Forbes and the British Association Dredging Committee, Wyville Thomson, Carpenter, Gwyn Jeffreys, Norman, and other naturalists of the pre-*Challenger* days—all these men in the quarter-century from 1840 onwards worked under research committees of the British Association, bringing their results before successive meetings; and some of our older volumes enshrine classic reports on dredging by Forbes, McAndrew, Norman, Brady, Alder, and other notable naturalists of that day. These local researches paved the way for the *Challenger* and other national deep-sea expeditions. Here, as in other cases, it required private enterprise to precede and stimulate Government action.

It is probable that Forbes and his fellow-workers on this ' Dredging Committee ' in their marine explorations did not fully realise that they were opening up a most comprehensive and important department of knowledge. But it is also true that in all his expeditions—in the British seas from the Channel Islands to the Shetlands, in Norway, in the Mediterranean as far as the Ægean Sea—his broad outlook on the problems of nature was that of the modern oceanographer, and he was the spiritual ancestor of men like Sir Wyville Thomson of the *Challenger* Expedition and Sir John Murray, whose accidental death a few years ago, while still in the midst of active work, was a grievous loss to this new and rapidly advancing science of the sea.

Forbes in these marine investigations worked at border-line problems, dealing for example with the relations of Geology to Zoology.

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[4] ' For researches with the dredge, with a view to the investigation of the marine zoology of Great Britain, the illustration of the geographical distribution of marine animals, and the more accurate determination of the fossils of the pleistocene period : under the superintendence of Mr. Gray, Mr. Forbes, Mr. Goodsir, Mr. Patterson, Mr. Thompson of Belfast, Mr. Ball of Dublin, Dr. George Johnston, Mr. Smith of Jordan Hill, and Mr. A. Strickland, £60.' Report for 1839, p. xxvi.

and the effect of the past history of the land and sea upon the distribution of plants and animals at the present day, and in these respects he was an early oceanographer. For the essence of that new subject is that it also investigates border-line problems and is based upon and makes use of all the older fundamental sciences—Physics, Chemistry and Biology—and shows for example how variations in the great ocean currents may account for the movements and abundance of the migratory fishes, and how periodic changes in the physico-chemical characters of the sea, such as variations in the hydrogen-ion and hydroxyl-ion concentration, are correlated with the distribution at the different seasons of the all-important microscopic organisms that render our oceanic waters as prolific a source of food as the pastures of the land.

Another pioneer of the nineteenth century who, I sometimes think, has not yet received sufficient credit for his foresight and initiative, is Sir Wyville Thomson, whose name ought to go down through the ages as the leader of the scientific staff on the famous *Challenger* Deep-Sea Exploring Expedition. It is due chiefly to him and to his friend Dr. W. B. Carpenter that the British Government, through the influence of the Royal Society, was induced to place at the disposal of a committee of scientific experts first the small surveying steamer *Lightning* in 1868, and then the more efficient steamer *Porcupine* in the two succeeding years, for the purpose of exploring the deep water of the Atlantic from the Faroes in the North to Gibraltar and beyond in the South, in the course of which expeditions they got successful hauls from the then unprecedented depth of 2435 fathoms, nearly three statute miles.

It will be remembered that Edward Forbes, from his observations in the Mediterranean (an abnormal sea in some respects), regarded depths of over 300 fathoms as an azoic zone. It was the work of Wyville Thomson and his colleagues Carpenter and Gwyn Jeffreys on these successive dredging expeditions to prove conclusively what was beginning to be suspected by naturalists, that there is no azoic zone in the sea, but that abundant life belonging to many groups of animals extends down to the greatest depths of from four to five thousand fathoms—nearly six statute miles from the surface.

These pioneering expeditions in the *Lightning* and *Porcupine*—the results of which are not even yet fully made known to science—were epoch-making, inasmuch as they not only opened up this new region to the systematic marine biologist, but gave glimpses of world-wide problems in connection with the physics, the chemistry and the biology of the sea which are only now being adequately investigated by the modern oceanographer. These results, which aroused intense interest amongst the leading scientific men of the time, were so rapidly surpassed and overshadowed by the still greater achievements of the

Challenger and other national exploring expeditions that followed in the 'seventies and 'eighties of last century, that there is some danger of their real importance being lost sight of; but it ought never to be forgotten that they first demonstrated the abundance of life of a varied nature in depths formerly supposed to be azoic, and, moreover, that some of the new deep-sea animals obtained were related to extinct forms belonging to the Jurassic, Cretaceous and Tertiary periods.

It is interesting to recall that our Association played its part in promoting the movement that led to the *Challenger* Expedition. Our General Committee at the Edinburgh Meeting of 1871 recommended that the President and Council be authorised to co-operate with the Royal Society in promoting ' a Circumnavigation Expedition, specially fitted out to carry the Physical and Biological Exploration of the Deep Sea into all the Great Oceanic Areas '; and our Council subsequently appointed a committee consisting of Dr. Carpenter, Professor Huxley and others to co-operate with the Royal Society in carrying out these objects.

It has been said that the *Challenger* Expedition will rank in history with the voyages of Vasco da Gama, Columbus, Magellan and Cook. Like these it added new regions of the globe to our knowledge, and the wide expanses thus opened up for the first time, the floors of the oceans, though less accessible, are vaster than the discoveries of any previous exploration. Has not the time come for a new *Challenger* expedition?

Sir Wyville Thomson, although leader of the expedition, did not live to see the completed results, and Sir John Murray will be remembered in the history of science as the *Challenger* naturalist who brought to a successful issue the investigation of the enormous collections and the publication of the scientific results of that memorable voyage: these two Scots share the honour of having guided the destinies of what is still the greatest oceanographic exploration of all times.

In addition to taking his part in the general work of the expedition, Murray devoted special attention to three subjects of primary importance in the science of the sea, viz.: (1) the plankton or floating life of the oceans, (2) the deposits forming on the sea bottoms, and (3) the origin and mode of formation of coral reefs and islands. It was characteristic of his broad and synthetic outlook on nature that, in place of working at the speciography and anatomy of some group of organisms, however novel, interesting and attractive to the naturalist the deep-sea organisms might seem to be, he took up wide-reaching general problems with economic and geological as well as biological applications.

Each of the three main lines of investigation—deposits, plankton and coral reefs—which Murray undertook on board the *Challenger* has been most fruitful of results both in his own hands and those of

others. His plankton work has led on to those modern planktonic researches which are closely bound up with the scientific investigation of our sea-fisheries.

His work on the deposits accumulating on the floor of the ocean resulted, after years of study in the laboratory as well as in the field, in collaboration with the Abbé Renard of the Brussels Museum, afterwards Professor at Ghent, in the production of the monumental ' Deep-Sea Deposits ' volume, one of the *Challenger* Reports, which first revealed to the scientific world the detailed nature and distribution of the varied submarine deposits of the globe and their relation to the rocks forming the crust of the earth.

These studies led, moreover, to one of the romances of science which deeply influenced Murray's future life and work. In accumulating material from all parts of the world and all deep-sea exploring expeditions for comparison with the *Challenger* series, some ten years later, Murray found that a sample of rock from Christmas Island in the Indian Ocean, which had been sent to him by Commander (now Admiral) Aldrich, of H.M.S. *Egeria*, was composed of a valuable phosphatic material. This discovery in Murray's hands gave rise to a profitable commercial undertaking, and he was able to show that some years ago the British Treasury had already received in royalties and taxes from the island considerably more than the total cost of the *Challenger* Expedition.

That first British circumnavigating expedition on the *Challenger* was followed by other national expeditions (the American *Tuscarora* and *Albatross*, the French *Travailleur*, the German *Gauss*, *National*, and *Valdivia*, the Italian *Vettor Pisani*, the Dutch *Siboga*, the Danish *Thor* and others) and by almost equally celebrated and important work by unofficial oceanographers such as Alexander Agassiz, Sir John Murray with Dr. Hjort in the *Michael Sars*, and the Prince of Monaco in his magnificent ocean-going yacht, and by much other good work by many investigators in smaller and humbler vessels. One of these supplementary expeditions I must refer to briefly because of its connection with sea-fisheries. The *Triton*, under Tizard and Murray, in 1882, while exploring the cold and warm areas of the Faroe Channel separated by the Wyville-Thomson ridge, incidentally discovered the famous Dubh-Artach fishing-grounds, which have been worked by British trawlers ever since.

Notwithstanding all this activity during the last forty years since Oceanography became a science, much has still to be investigated in all seas in all branches of the subject. On pursuing any line of investigation one very soon comes up against a wall of the unknown or a maze of controversy. Peculiar difficulties surround the subject. The

matters investigated are often remote and almost inaccessible. Unknown factors may enter into every problem. The samples required may be at the other end of a rope or a wire eight or ten miles long, and the oceanographer may have to grope for them literally in the dark and under other difficult conditions which make it uncertain whether his samples when obtained are adequate and representative, and whether they have undergone any change since leaving their natural environment. It is not surprising then that in the progress of knowledge mistakes have been made and corrected, that views have been held on what seemed good scientific grounds which later on were proved to be erroneous. For example, Edward Forbes, in his division of life in the sea into zones, on what then seemed to be sufficiently good observations in the Ægean, but which we now know to be exceptional, placed the limit of life at 300 fathoms, while Wyville Thomson and his fellow-workers on the *Porcupine* and the *Challenger* showed that there is no azoic zone even in the great abysses.

Or, again, take the celebrated myth of 'Bathybius.' In the 'sixties of last century samples of Atlantic mud, taken when surveying the bottom for the first telegraph cables and preserved in alcohol, were found when examined by Huxley, Haeckel and others to contain what seemed to be an exceedingly primitive protoplasmic organism, which was supposed on good evidence to be widely extended over the floor of the ocean. The discovery of this Bathybius was said to solve the problem of how the deep-sea animals were nourished in the absence of sea-weeds. Here was a widespread protoplasmic meadow upon which other organisms could graze. Belief in Bathybius seemed to be confirmed and established by Wyville Thomson's results in the *Porcupine* Expedition of 1869, but was exploded by the naturalists on the *Challenger* some five years later. Buchanan in his recently published 'Accounts Rendered' tells us how he and his colleague Murray were keenly on the look-out for hours at a time on all possible occasions for traces of this organism, and how they finally proved, in the spring of 1875 on the voyage between Hong-Kong and Yokohama, that the all-pervading substance like coagulated mucus was an amorphous precipitate of sulphate of lime thrown down from the sea-water in the mud on the addition of a certain proportion of alcohol. He wrote to this effect from Japan to Professor Crum Brown, and it is in evidence that after receiving this letter Crum Brown interested his friends in Edinburgh by showing them how to make Bathybius in the chemical laboratory. Huxley at the Sheffield Meeting of the British Association in 1879 handsomely admitted that he had been mistaken, and it is said that he characterised Bathybius as 'not having fulfilled the promise of its youth.' Will any of our present oceanographic beliefs

share the fate of Bathybius in the future? Some may, but even if they do they may well have been useful steps in the progress of science. Although like Bathybius they may not have fulfilled the promise of their youth, yet, we may add, they will not have lived in the minds of man in vain.

Many of the phenomena we encounter in oceanographic investigations are so complex, are or may be affected by so many diverse factors, that it is difficult, if indeed possible, to be sure that we are unravelling them aright and that we see the real causes of what we observe.

Some few things we know approximately—nothing completely. We know that the greatest depths of the ocean, about six miles, are a little greater than the highest mountains on land, and Sir John Murray has calculated that if all the land were washed down into the sea the whole globe would be covered by an ocean averaging about two miles in depth.[5] We know the distribution of temperatures and salinities over a great part of the surface and a good deal of the bottom of the oceans, and some of the more important oceanic currents have been charted and their periodic variations, such as those of the Gulf Stream, are being studied. We know a good deal about the organisms floating or swimming in the surface waters (the epi-plankton), and also those brought up by our dredges and trawls from the bottom in many parts of the world—although every expedition still makes large additions to knowledge. The region that is least known to us, both in its physical conditions and also its inhabitants, is the vast zone of intermediate waters lying between the upper few hundred fathoms and the bottom. That is the region that Alexander Agassiz from his observations with closing tow-nets on the *Blake* Expedition supposed to be destitute of life, or at least, as modified by his later observations on the *Albatross*, to be relatively destitute compared with the surface and the bottom, in opposition to the contention of Murray and other oceanographers that an abundant meso-plankton was present, and that certain groups of animals, such as the Challengerida and some kinds of Medusæ, were characteristic of these deeper zones. I believe that, as sometimes happens in scientific controversies, both sides were right up to a point, and both could support their views upon observations from particular regions of the ocean under certain circumstances.

But much still remains unknown or only imperfectly known even in matters that have long been studied and where practical applications

[5] It was possibly in such a former world-wide ocean of ionised water that according to the recent speculations of A. H. Church (*Thalassiophyta*, 1919) the first living organisms were evolved to become later the floating unicellular plants of the primitive plankton.

of great value are obtained—such as the investigation and prediction of tidal phenomena. We are now told that theories require re-investigation and that published tables are not sufficiently accurate. To take another practical application of oceanographic work, the ultimate causes of variations in the abundance, in the sizes, in the movements and in the qualities of the fishes of our coastal industries are still to seek, and notwithstanding volumes of investigation and a still greater volume of discussion, no man who knows anything of the matter is satisfied with our present knowledge of even the best-known and economically most important of our fishes, such as the Herring, the Cod, the Plaice and the Salmon.

Take the case of our common fresh-water eel as an example of how little we know and at the same time of how much has been discovered. All the eels of our streams and lakes of N.-W. Europe live and feed and grow under our eyes without reproducing their kind—no spawning eel has ever been seen. After living for years in immaturity, at last near the end of their lives the large male and female yellow eels undergo a change in appearance and in nature. They acquire a silvery colour and their eyes enlarge, and in this bridal attire they commence the long journey which ends in maturity, reproduction and death. From all the fresh waters they migrate in the autumn to the coast, from the inshore seas to the open ocean and still westward and south to the mid-Atlantic and we know not how much further—for the exact locality and manner of spawning has still to be discovered. The youngest known stages of the Leptocephalus, the larval stage of eels, have been found by the Dane, Dr. Johs. Schmidt, to the west of the Azores where the water is over 2000 fathoms in depth. These were about one-third of an inch in length and were probably not long hatched. I cannot now refer to all the able investigators—Grassi, Hjort and others—who have discovered and traced the stages of growth of the Leptocephalus and its metamorphosis into the 'elvers' or young eels which are carried by the North Atlantic drift back to the coasts of Europe and ascend our rivers in spring in countless myriads; but no man has been more indefatigable and successful in the quest than Dr. Schmidt, who in the various expeditions of the Danish Investigation Steamer *Thor* from 1904 onwards found successively younger and younger stages, and who is during the present summer engaged in a traverse of the Atlantic to the West Indies in the hope of finding the missing link in the chain, the actual spawning fresh-water eel in the intermediate waters somewhere above the abysses of the open ocean.[6]

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[6] According to Schmidt's results the European fresh-water eel, in order to be able to propagate, requires a depth of at least 500 fathoms, a salinity of more than 35.20 per mille and a temperature of more than 7° C. in the required depth.

Again, take the case of an interesting oceanographic observation which, if established, may be found to explain the variations in time and amount of important fisheries. Otto Pettersson in 1910 discovered by his observations in the Gullmar Fjord the presence of periodic submarine waves of deeper salter water in the Kattegat and the fjords of the west coast of Sweden, which draw in with them from the Jutland banks vast shoals of the herrings which congregate there in autumn. The deeper layer consists of 'bankwater' of salinity 32 to 34 per thousand, and as this rolls in along the bottom as a series of huge undulations it forces out the overlying fresher water, and so the herrings living in the bankwater outside are sucked into the Kattegat and neighbouring fjords and give rise to important local fisheries. Pettersson connects the crests of the submarine waves with the phases of the moon. Two great waves of salter water which reached up to the surface took place in November 1910, one near the time of full moon and the other about new moon, and the latter was at the time when the shoals of herring appeared inshore and provided a profitable fishery. The coincidence of the oceanic phenomena with the lunar phases is not, however, very exact, and doubts have been expressed as to the connection; but if established, and even if found to be due not to the moon but to prevalent winds or the influence of ocean currents, this would be a case of the migration of fishes depending upon mechanical causes, while in other cases it is known that migrations are due to spawning needs or for the purpose of feeding, as in the case of the cod and the herring in the west and north of Norway and in the Barents Sea.

Then, turning to a very fundamental matter of purely scientific investigation, we do not know with any certainty what causes the great and all-important seasonal variations in the plankton (or floating minute life of the sea) as seen, for example, in our own home seas, where there is a sudden awakening of microscopic plant life, the Diatoms, in early spring when the water is at its coldest. In the course of a few days the upper layers of the sea may become so filled with organisms that a small silk net towed for a few minutes may capture hundreds of millions of individuals. And these myriads of microscopic forms, after persisting for a few weeks, may disappear as suddenly as they came, to be followed by swarms of Copepoda and many other kinds of minute animals, and these again may give place in the autumn to a second maximum of Diatoms or of the closely related Peridiniales. Of course there are theories as to all these more or less periodic changes in the plankton, such as Liebig's 'law of the minimum,' which limits the production of an organism by the amount of that necessity of existence which is present in least quantity, it may be nitrogen or silicon or

phosphorus. According to Raben it is the accumulation of silicic acid in the sea-water that determines the great increase of Diatoms in spring and again in autumn. Some writers have considered these variations in the plankton to be caused largely by changes in temperature supplemented, according to Ostwald, by the resulting changes in the viscosity of the water; but Murray and others are more probably correct in attributing the spring development of phyto-plankton to the increasing power of the sunlight and its value in photosynthesis.

Let us take next the fact—if it be a fact—that the genial warm waters of the tropics support a less abundant plankton than the cold polar seas. The statement has been made and supported by some investigators and disputed by others, both on a certain amount of evidence. This is possibly a case like some other scientific controversies where both sides are partly in the right, or right under certain conditions. At any rate there are marked exceptions to the generalisation. The German Plankton Expedition in 1889 showed in its results that much larger hauls of plankton per unit volume of water were obtained in the temperate North and South Atlantic than in the tropics between, and that the warm Sargasso Sea had a remarkably scanty microflora. Other investigators have since reported more or less similar results. Lohmann found the Mediterranean plankton to be less abundant than that of the Baltic, gatherings brought back from tropical seas are frequently very scanty, and enormous hauls on the other hand have been recorded from Arctic and Antarctic seas. There is no doubt about the large gatherings obtained in northern waters. I have myself in a few minutes' haul of a small horizontal net in the North of Norway collected a mass of the large Copepod *Calanus finmarchicus* sufficient to be cooked and eaten like potted shrimps by half a dozen of the yacht's company, and I have obtained similar large hauls in the cold Labrador current near Newfoundland. On the other hand, Kofoid and Alexander Agassiz have recorded large hauls of plankton in the Humboldt current off the west coast of America, and during the *Challenger* Expedition some of the largest quantities of plankton were found in the equatorial Pacific. Moreover, it is common knowledge that on occasions vast swarms of some planktonic organism may be seen in tropical waters. The yellow alga Trichodesmium, which is said to have given its name to the Red Sea and has been familiarly known as ' sea-sawdust ' since the days of Cook's first voyage,[7] may cover the entire surface over considerable areas of the Indian and South Atlantic Oceans; and some pelagic animals such as Salpæ, Medusæ and Ctenophores are also commonly present in abundance in the tropics. Then, again, American

[7] See Journal of Sir Joseph Banks. This and other swarms were also noticed by Darwin during the voyage of the *Beagle*.

biologists [8] have pointed out that the warm waters of the West Indies and Florida may be noted for the richness of their floating life for periods of years, while at other times the pelagic organisms become rare and the region is almost a desert sea.

It is probable, on the whole, that the distribution and variations of oceanic currents have more than latitude or temperature alone to do with any observed scantiness of tropical plankton. These mighty rivers of the ocean in places teem with animal and plant life, and may sweep abundance of food from one region to another in the open sea.

But even if it be a fact that there is this alleged deficiency in tropical plankton there is by no means agreement as to the cause thereof. Brandt first attributed the poverty of the plankton in the tropics to the destruction of nitrates in the sea as a result of the greater intensity of the metabolism of denitrifying bacteria in the warmer water; and various other writers since then have more or less agreed that the presence of these denitrifying bacteria, by keeping down to a minimum the nitrogen concentration in tropical waters, may account for the relative scarcity of the phyto-plankton, and consequently of the zoo-plankton, that has been observed. But Gran, Nathansohn, Murray, Hjort and others have shown that such bacteria are rare or absent in the open sea, that their action must be negligible, and that Brandt's hypothesis is untenable. It seems clear, moreover, that the plankton does not vary directly with the temperature of the water. Furthermore, Nathansohn has shown the influence of the vertical circulation in the water upon the nourishment of the phyto-plankton—by rising currents bringing up necessary nutrient materials, and especially carbon dioxide from the bottom layers; and also possibly by conveying the products of the drainage of tropical lands to more polar seas so as to maintain the more abundant life in the colder water.

Pütter's view is that the increased metabolism in the warmer water causes all the available food materials to be rapidly used up, and so puts a check to the reproduction of the plankton.

According to Van't Hoff's law in Chemistry, the rate at which a reaction takes place is increased by raising the temperature, and this probably holds good for all bio-chemical phenomena, and therefore for the metabolism of animals and plants in the sea. This has been verified experimentally in some cases by J. Loeb. The contrast between the plankton of Arctic and Antarctic zones, consisting of large numbers of small Crustaceans belonging to comparatively few species, and that of tropical waters, containing a great many more species generally of smaller size and fewer in number of individuals, is to be

[8] A. Agassiz, A. G. Mayer, and H. B. Bigelow.

accounted for, according to Sir John Murray and others, by the rate of metabolism in the organisms. The assemblages captured in cold polar waters are of different ages and stages, young and adults of several generations occurring together in profusion,[9] and it is supposed that the adults ' may be ten, twenty or more years of age.' At the low temperature the action of putrefactive bacteria and of enzymes is very slow or in abeyance, and the vital actions of the Crustacea take place more slowly and the individual lives are longer. On the other hand, in the warmer waters of the tropics the action of the bacteria is more rapid, metabolism in general is more active, and the various stages in the life-history are passed through more rapidly, so that the smaller organisms of equatorial seas probably only live for days or weeks in place of years.

This explanation may account also for the much greater quantity of living organisms which has been found so often on the sea floor in polar waters. It is a curious fact that the development of the polar marine animals is in general ' direct ' without larval pelagic stages, the result being that the young settle down on the floor of the ocean in the neighbourhood of the parent forms, so that there come to be enormous congregations of the same kind of animal within a limited area, and the dredge will in a particular haul come up filled with hundreds, it may be, of an Echinoderm, a Sponge, a Crustacean, a Brachiopod, or an Ascidian; whereas in warmer seas the young pass through a pelagic stage and so become more widely distributed over the floor of the ocean. The *Challenger* Expedition found in the Antarctic certain Echinoderms, for example, which had young in various stages of development attached to some part of the body of the parents, whereas in temperate or tropical regions the same class of animals set free their eggs and the development proceeds in the open water quite independently of, and it may be far distant from, the parent.

Another characteristic result of the difference in temperature is that the secretion of carbonate of lime in the form of shells and skeletons proceeds more rapidly in warm than in cold water. The massive shells of molluscs, the vast deposits of carbonate of lime formed by corals and by calcareous seaweeds, are characteristic of the tropics; whereas in polar seas, while the animals may be large, they are for the most part soft-bodied and destitute of calcareous secretions. The calcareous pelagic Foraminifera are characteristic of tropical and sub-tropical plankton, and few, if any, are found in polar waters. Globigerina

[9] Whether, however, the low temperature may not also retard reproduction is worthy of consideration.

ooze, a calcareous deposit, is abundant in equatorial seas, while in the Antarctic the characteristic deposit is siliceous Diatomaceous ooze.

The part played by bacteria in the metabolism of the sea is very important and probably of wide-reaching effect, but we still know very little about it. A most promising young Cambridge biologist, the late Mr. G. Harold Drew, now unfortunately lost to science, had already done notable work at Jamaica and at Tortugas, Florida, on the effects produced by a bacillus which is found in the surface waters of these shallow tropical seas and in the mud at the bottom ; and which denitrifies nitrates and nitrites, giving off free nitrogen. He found that this *Bacillus calcis* also caused the precipitation of soluble calcium salts in the form of calcium carbonate (' drewite ') on a large scale, in the warm shallow waters. Drew's observations tend to show that the great calcareous deposits of Florida and the Bahamas previously known as ' coral muds ' are not, as was supposed by Murray and others, derived from broken-up corals, shells, nullipores, &c., but are minute particles of carbonate of lime which have been precipitated by the action of these bacteria.[10]

The bearing of these observations upon the formation of oolitic limestones and the fine-grained unfossiliferous Lower Palæozoic limestones of New York State, recently studied in this connection by R. M. Field,[11] must be of peculiar interest to geologists, and forms a notable instance of the annectant character of Oceanography, bringing the metabolism of living organisms in the modern sea into relation with palæozoic rocks.

The work of marine biologists on the plankton has been in the main *qualitative*, the identification of species, the observation of structure, and the tracing of life-histories. The oceanographer adds to that the *quantitative* aspect when he attempts to estimate numbers and masses per unit volume of water or of area. Let me lay before you a few thoughts in regard to some such attempts, mainly for the purpose of showing the difficulties of the investigation. Modern quantitative methods owe their origin to the ingenious and laborious work of Victor Hensen, followed by Brandt, Apstein, Lohmann, and others of the Kiel school of quantitative planktologists. We may take their well-known estimations of fish eggs in the North Sea as an example of the method.

The floating eggs and embryos of our more important food fishes may occur in quantities in the plankton during certain months in spring, and Hensen and Apstein have made some notable calculations

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[10] *Journ. Mar. Biol. Assoc.*, October 1911.
[11] Carnegie Institute of Washington, Year Book for 1919, p. 197.

based on the occurrence of these in certain hauls taken at intervals across the North Sea, which led them to the conclusion that, taking six of our most abundant fish, such as the cod and some of the flat fish, the eggs present were probably produced by about 1200 million spawners, enabling them to calculate that the total fish population of the North Sea (of these six species), at that time (spring of 1895), amounted to about 10,000 millions. Further calculations led them to the result that the fishermen's catch of these fishes amounted to about one-quarter of the total population. Now all this is not only of scientific interest, but also of great practical importance if we could be sure that the samples upon which the calculations are based were adequate and representative, but it will be noted that these samples only represent one square metre in 3,465,968,354. Hensen's statement, repeated in various works in slightly differing words, is to the effect that, using a net of which the constants are known hauled vertically through a column of water from a certain depth to the surface, he can calculate the volume of water filtered by the net and so estimate the quantity of plankton under each square metre of the surface; and his whole results depend upon the assumption, which he considers justified, that the plankton is evenly distributed over large areas of water which are under similar conditions. In these calculations in regard to the fish eggs he takes the whole of the North Sea as being an area under similar conditions, but we have known since the days of P. T. Cleve and from the observations of Hensen's own colleagues that this is not the case, and they have published chart-diagrams showing that at least three different kinds of water under different conditions are found in the North Sea, and that at least five different planktonic areas may be encountered in making a traverse from Germany to the British Isles. If the argument be used that wherever the plankton is found to vary there the conditions cannot be uniform, then few areas of the ocean of any considerable size remain as cases suitable for population-computation from random samples. It may be doubted whether even the Sargasso Sea, which is an area of more than usually uniform character, has a sufficiently evenly distributed plankton to be treated by Hensen's method of estimation of the population.

In the German Plankton Expedition of 1889 Schütt reports that in the Sargasso Sea, with its relatively high temperature, the twenty-four catches obtained were uniformly small in quantity. His analysis of the volumes of these catches shows that the average was 3·33 c.c., but the individual catches ranged from 1·5 c.c. to 6·5 c.c., and the divergence from the average may be as great as $+3·2$ c.c.; and, after deducting 20 per cent. of the divergence as due to errors of the experiment,

Schütt estimates the mean variation of the plankton at about 16 per cent. above or below. This does not seem to me to indicate the uniformity that might be expected in this ' halistatic ' area occupying the centre of the North Atlantic Gulf Stream circulation. Hensen also made almost simultaneous hauls with the same net in quick succession to test the amount of variation, and found that the average error was about 13 per cent.

As so much depends in all work at sea upon the weather, the conditions under which the ship is working, and the care taken in the experiment, with the view of getting further evidence under known conditions I carried out some similar experiments at Port Erin on four occasions during last April and on a further occasion a month later, choosing favourable weather and conditions of tide and wind, so as to be able to maintain an approximate position. On each of four days in April the Nansen net, with No. 20 silk, was hauled six times from the same depth (on two occasions 8 fathoms and on two occasions 20 fathoms), the hauls being taken in rapid succession and the catches being emptied from the net into bottles of 5 per cent. formaline, in which they remained until examined microscopically.

The results were of interest, for although they showed considerable uniformity in the amount of the catch—for example, six successive hauls from 8 fathoms being all of them 0·2 c.c. and four out of five from 20 fathoms being 0·6 c.c.—the volume was made up rather differently in the successive hauls. The same organisms are present for the most part in each haul, and the chief groups of organisms are present in much the same proportion. For example, in a series where the Copepoda average about 100 the Dinoflagellates average about 300 and the Diatoms about 8000, but the percentage deviation of individual hauls from the average may be as much as *plus* or *minus* 50. The numbers for each organism (about 40) in each of the twenty-six hauls have been worked out, and the details will be published elsewhere, but the conclusion I come to is that if on each occasion one haul only, in place of six, had been taken, and if one had used that haul to estimate the abundance of any one organism in that sea-area, one might have been about 50 per cent. wrong in either direction.

Successive improvements and additions to Hensen's methods in collecting plankton have been made by Lohmann, Apstein, Gran, and others, such as pumping up water of different layers through a hose-pipe and filtering it through felt, filter-paper, and other materials which retain much of the micro-plankton that escapes through the meshes of the finest silk. Use has even been made of the extraordinarily minute and beautifully regular natural filter spun by the pelagic animal *Appendicularia* for the capture of its own food. This grid-like trap,

when dissected out and examined under the microscope, reveals a surprising assemblage of the smallest protozoa and protophyta, less than 30 micro-millimetres in diameter, which would all pass easily through the meshes of our finest silk nets.

The latest refinement in capturing the minutest-known organisms of the plankton (excepting the bacteria) is a culture method devised by Dr. E. J. Allen, Director of the Plymouth Laboratory.[12] By diluting half a cubic centimetre of the sea-water with a considerable amount (1500 c.c.) of sterilised water treated with a nutrient solution, and distributing that over a large number (70) of small flasks in which after an interval of some days the number of different kinds of organisms which had developed in each flask were counted, he calculates that the sea contains 464,000 of such organisms per litre; and he gives reasons why his cultivations must be regarded as minimum results, and states that the total per litre may well be something like a million. Thus every new method devised seems to multiply many times the probable total population of the sea. As further results of the quantitative method it may be recorded that Brandt found about 200 diatoms per drop of water in Kiel Bay, and Hensen estimated that there are several hundred millions of diatoms under each square metre of the North Sea or the Baltic. It has been calculated that there is approximately one Copepod in each cubic inch of Baltic water, and that the annual consumption of these Copepoda by herring is about a thousand billion; and that in the 16 square miles of a certain Baltic fishery there is Copepod food for over 530 millions of herring of an average weight of 60 grammes.

There are many other problems of the plankton in addition to quantitative estimates—probably some that we have not yet recognised—and various interesting conclusions may be drawn from recent planktonic observations. Here is a case of the introduction and rapid spread of a form new to British seas.

Biddulphia sinensis is an exotic diatom which, according to Ostenfeld, made its appearance at the mouth of the Elbe in 1903, and spread during successive years in several directions. It appeared suddenly in our plankton gatherings at Port Erin in November 1909, and has been present in abundance each year since. Ostenfeld, in 1908, when tracing its spread in the North Sea, found that the migration to the north along the coast of Denmark to Norway corresponded with the rate of flow of the Jutland current to the Skagerrak—viz., about 17 cm. per second—a case of plankton distribution throwing light on hydrography—and he predicted that it would soon be found in the English

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[12] *Journ. Mar. Biol. Assoc.* xii. 1, July 1919.

Channel. Dr. Marie Lebour, who recently examined the store of plankton gatherings at the Plymouth Laboratory, finds that as a matter of fact this form did appear in abundance in the collections of October 1909, within a month of the time when according to our records it reached Port Erin. Whether or not this is an Indo-Pacific species brought accidentally by a ship from the Far East, or whether it is possibly a new mutation which appeared suddenly in our seas, there is no doubt that it was not present in our Irish Sea plankton gatherings previous to 1909, but has been abundant since that year, and has completely adopted the habits of its English relations—appearing with *B. mobiliensis* in late autumn, persisting during the winter, reaching a maximum in spring, and dying out before summer.

The Nauplius and Cypris stages of *Balanus* in the plankton form an interesting study. The adult barnacles are present in enormous abundance on the rocks round the coast, and they reproduce in winter, at the beginning of the year. The newly emitted young are sometimes so abundant as to make the water in the shore pools and in the sea close to shore appear muddy. The Nauplii first appeared at Port Erin, in 1907, in the bay gatherings on February 22 (in 1908 on February 13), and increased with ups and downs to their maximum on April 15, and then decreased until their disappearance on April 26. None were taken at any other time of the year. The Cypris stage follows on after the Nauplius. It was first taken in the bay on April 6, rose to its maximum on the same day with the Nauplii, and was last caught on May 24. Throughout, the Cypris curve keeps below that of the Nauplius, the maxima being 1740 and 10,500 respectively. Probably the difference between the two curves represents the death-rate of *Balanus* during the Nauplius stage. That conclusion I think we are justified in drawing, but I would not venture to use the result of any haul, or the average of a number of hauls, to multiply by the number of square yards in a zone round our coast in order to obtain an estimate of the number of young barnacles, or of the old barnacles that produced them—the irregularities are too great.

To my mind it seems clear that there must be three factors making for irregularity in the distribution of a plankton organism :—

1. The sequence of stages in its life-history—such as the Nauplius and Cypris stages of *Balanus*.

2. The results of interaction with other organisms—as when a swarm of *Calanus* is pursued and devoured by a shoal of herring.

3. Abnormalities in time or abundance due to the physical environment—as in favourable or unfavourable seasons.

And these factors must be at work in the open ocean as well as in coastal waters.

In many oceanographical inquiries there is a double object. There is the scientific interest and there is the practical utility—the interest, for example, of tracing a particular swarm of a Copepod like *Calanus*, and of making out why it is where it is at a particular time, tracing it back to its place of origin, finding that it has come with a particular body of water, and perhaps that it is feeding upon a particular assemblage of Diatoms; endeavouring to give a scientific explanation of every stage in its progress. Then there is the utility—the demonstration that the migration of the *Calanus* has determined the presence of a shoal of herrings or mackerel that are feeding upon it, and so have been brought within the range of the fisherman and have constituted a commercial fishery.

We have evidence that pelagic fish which congregate in shoals, such as herring and mackerel, feed upon the Crustacea of the plankton and especially upon Copepoda. A few years ago when the summer herring fishery off the south end of the Isle of Man was unusually near the land, the fishermen found large red patches in the sea where the fish were specially abundant. Some of the red stuff, brought ashore by the men, was examined at the Port Erin Laboratory and found to be swarms of the Copepod *Temora longicornis*; and the stomachs of the herring caught at the same time were engorged with the same organism. It is not possible to doubt that during these weeks of the herring fishery in the Irish Sea the fish were feeding mainly upon this species of Copepod. Some ten years ago Dr. E. J. Allen and Mr. G. E. Bullen published [13] some interesting work, from the Plymouth Marine Laboratory, demonstrating the connection between mackerel and Copepoda and sunshine in the English Channel; and Farran [14] states that in the spring fishery on the West of Ireland the food of the mackerel is mainly composed of *Calanus*.

Then again at the height of the summer mackerel fishery in the Hebrides, in 1913, we found [15] the fish feeding upon the large Copepod *Calanus finmarchicus*, which was caught in the tow-net at the rate of about 6000 in a five-minutes' haul, and 6000 was also the average number found in the stomachs of the fish caught at the same time.

These were cases where the fish were feeding upon the organism that was present in swarms—a monotonic plankton—but in other cases the fish are clearly selective in their diet. If the sardine of the French coast can pick out from the micro-plankton the minute Peridiniales in preference to the equally minute Diatoms which are present in the sea at the same time, there seems no reason why the herring and the

[13] *Journ. Mar. Biol. Assoc.* vol. viii. (1909), pp. 394-406.
[14] *Conseil Internat. Bull. Trimestr.* 1902-8, 'Planktonique,' p. 89.
[15] 'Spolia Runiana,' iii. *Linn. Soc. Journ., Zoology*, vol. xxxiv. p. 95, 1918.

mackerel should not be able to select particular species of Copepoda or other large organisms from the macro-plankton, and we have evidence that they do. Nearly thirty years ago the late Mr. Isaac Thompson, a constant supporter of the Zoological Section of this Association and one of the Honorary Local Secretaries for the last Liverpool meeting, showed me in 1893 that young plaice at Port Erin were selecting one particular Copepod, a species of Jonesiella, out of many others caught in our tow-nets at the time. H. Blegvad [16] showed in 1916 that young food fishes and also small shore fishes pick out certain species of Copepoda (such as Harpacticoids) and catch them individually —either lying in wait or searching for them. A couple of years later [17] Dr. Marie Lebour published a detailed account of her work at Plymouth on the food of young fishes, proving that certain fish undoubtedly do prefer certain planktonic food.

These Crustacea of the plankton feed upon smaller and simpler organisms—the Diatoms, the Peridinians, and the Flagellates—and the fish themselves in their youngest post-larval stages are nourished by the same minute forms of the plankton. Thus it appears that our sea-fisheries ultimately depend upon the living plankton which no doubt in its turn is affected by hydrographic conditions. A correlation seems to be established between the Cornish pilchard fisheries and periodic variations in the physical characters (probably the salinity) of the water of the English Channel between Plymouth and Jersey.[18] Apparently a diminished intensity in the Atlantic current corresponds with a diminished fishery in the following summer. Possibly the connection in these cases is through an organism of the plankton.

It is only a comparatively small number of different kinds of organisms—both plants and animals—that make up the bulk of the plankton that is of real importance to fish. One can select about half-a-dozen species of Copepoda which constitute the greater part of the summer zoo-plankton suitable as food for larval or adult fishes, and about the same number of generic types of Diatoms which similarly make up the bulk of the available spring phyto-plankton year after year. This fact gives great economic importance to the attempt to determine with as much precision as possible the times and conditions of occurrence of these dominant factors of the plankton in an average year. An obvious further extension of this investigation is an inquiry into the degree of coincidence between the times of appearance in the sea of the plankton organisms and of the young fish, and the possible effect of any marked absence of correlation in time and quantity.

Just before the war the International Council for the Exploration

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[16] *Rep. Danish Biol. Stat.* xxiv. 1916.

[17] *Journ. Mar. Biol. Assoc.* May 1918.

[18] See E. C. Jee, *Hydrography of the English Channel*, 1904-17.

of the Sea [19] arrived at the conclusion that fishery investigations indicated the probability that the great periodic fluctuations in the fisheries are connected with the fish larvæ being developed in great quantities only in certain years. Consequently they advised that plankton work should be directed primarily to the question whether these fluctuations depend upon differences in the plankton production in different years. It was then proposed to begin systematic investigation of the fish larvæ and the plankton in spring and to determine more definitely the food of the larval fish at various stages.

About the same time Dr. Hjort [20] made the interesting suggestion that possibly the great fluctuations in the number of young fish observed from year to year may not depend wholly upon the number of eggs produced, but also upon the relation in time between the hatching of these eggs and the appearance in the water of the enormous quantity of Diatoms and other plant plankton upon which the larval fish after the absorption of their yolk depend for food. He points out that if even a brief interval occurs between the time when the larvæ first require extraneous nourishment and the period when such food is available, it is highly probable that an enormous mortality would result. In that case even a rich spawning season might yield but a poor result in fish in the commercial fisheries of successive years for some time to come. So that, in fact, the numbers of a year-class may depend not so much upon a favourable spawning season as upon a coincidence between the hatching of the larvæ and the presence of abundance of phyto-plankton available as food. [21]

The curve for the spring maximum of Diatoms corresponds in a general way with the curve representing the occurrence of pelagic fish eggs in our seas. But is the correspondence sufficiently exact and constant to meet the needs of the case? The phyto-plankton may still be relatively small in amount during February and part of March in some years, and it is not easy to determine exactly when, in the open sea, the fish eggs have hatched out in quantity and the larvæ have absorbed their food-yolk and started feeding on Diatoms.

If, however, we take the case of one important fish—the plaice—we can get some data from our hatching experiments at the Port Erin Biological Station which have now been carried on for a period of seventeen years. An examination of the hatchery records for these years in comparison with the plankton records of the neighbouring sea, which have been kept systematically for the fourteen years from 1907

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[19] *Rapports et Proc. Verb.* xix. December 1913.
[20] *Rapports et Proc. Verb.* xx. 1914, p. 204.
[21] For the purpose of this argument we may include in ' phyto-plankton ' the various groups of Flagellata and other minute organisms which may be present with the Diatoms.

to 1920 inclusive, shows that in most of these years the Diatoms were present in abundance in the sea a few days at least before the fish larvæ from the hatchery were set free, and that it was only in four years (1908, '09, '13, and '14) that there was apparently some risk of the larvæ finding no phyto-plankton food, or very little. The evidence so far seems to show that if fish larvæ are set free in the sea as late as March 20, they are fairly sure of finding suitable food;[22] but if they are hatched as early as February they run some chance of being starved.

But this does not exhaust the risks to the future fishery. C. G. Joh. Petersen and Boysen-Jensen in their valuation of the Limfjord[23] have shown that in the case not only of some fish but also of the larger invertebrates on which they feed there are marked fluctuations in the number of young produced in different seasons, and that it is only at intervals of years that a really large stock of young is added to the population.

The prospects of a year's fishery may therefore depend primarily upon the rate of spawning of the fish, affected no doubt by hydrographic and other environmental conditions, secondarily upon the presence of a sufficient supply of phyto-plankton in the surface layers of the sea at the time when the fish larvæ are hatched, and that in its turn depends upon photosynthesis and physico-chemical changes in the water, and finally upon the reproduction of the stock of molluscs or worms at the bottom which constitute the fish food at later stages of growth and development.

The question has been raised of recent years—Is there enough plankton in the sea to provide sufficient nourishment for the larger animals, and especially for those fixed forms such as sponges that are supposed to feed by drawing currents of plankton-laden water through the body? In a series of remarkable papers from 1907 onwards Pütter and his followers put forward the views (1) that the carbon requirements of such animals could not be met by the amount of plankton in the volume of water that could be passed through the body in a given time, and (2) that sea-water contained a large amount of dissolved organic carbon compounds which constitute the chief if not the only food of a large number of marine animals. These views have given rise to much controversy and have been useful in stimulating further research, but I believe it is now admitted that Pütter's samples of water from the Bay of Naples and at Kiel were probably polluted, that his figures were erroneous, and that his conclusions

[22] All dates and statements as to occurrence refer to the Irish Sea round the south end of the Isle of Man. For further details see *Report Lancs. Sea-Fish. Lab.* for 1919.

[23] *Report of Danish Biol. Station* for 1919.

must be rejected, or at least greatly modified. His estimates of the plankton were minimum ones, while it seems probable that his figures for the organic carbon present represent a variable amount of organic matter arising from one of the reagents used in the analysis.[24] The later experimental work of Henze, of Raben, and of Moore shows that the organic carbon dissolved in sea-water is an exceedingly minute quantity, well within the limits of experimental error. Moore puts it, at the most, at one-millionth part, or 1 mgm. in a litre. At the Dundee meeting of the Association in 1912 a discussion on this subject took place, at which Pütter still adhered to a modified form of his hypothesis of the inadequacy of the plankton and the nutrition of lower marine animals by the direct absorption of dissolved organic matter. Further work at Port Erin since has shown that, while the plankton supply as found generally distributed would prove sufficient for the nutrition of such sedentary animals as Sponges and Ascidians, which require to filter only about fifteen times their own volume of water per hour, it is quite inadequate for active animals such as Crustaceans and Fishes. These latter are, however, able to seek out and capture their food, and are not dependent on what they may filter or absorb from the sea-water. This result accords well with recorded observations on the irregularity in the distribution of the plankton, and with the variations in the occurrence of the migratory fishes which may be regarded as following and feeding upon the swarms of planktonic organisms.

This then, like most of the subjects I am dealing with, is still a matter of controversy, still not completely understood. Our need, then, is Research, more Research, and *still more Research*.

Our knowledge of the relations between plankton productivity and variation and the physico-chemical environment is still in its infancy, but gives promise of great results in the hands of the bio-chemist and the physical chemist.

Recent papers by Sörensen, Palitzsch, Witting, Moore, and others have made clear that the amount of hydrogen-ion concentration as indicated by the relative degree of alkalinity and acidity in the sea-water may undergo local and periodic variations and that these have an effect upon the living organisms in the water and can be correlated with their presence and abundance. To take an example from our own seas, Professor Benjamin Moore and his assistants in their work at the Port Erin Biological Station in successive years from 1912 onwards have shown[25] that the sea around the Isle of Man is a good deal more alkaline in spring (say April) than it is in summer (say

[24] See Moore, etc., *Bio.-Chem. Journ.* vi. p. 266, 1912.
[25] 'Photosynthetic phenomena in sea-water,' *Trans. Liverpool Biol. Soc.* xxix. 233, 1915.

July).   The alkalinity, which gets low in summer, increases somewhat in autumn, and then decreases rapidly, to disappear during the winter; and then once more, after several months of a minimum, begins to come into evidence again in March, and rapidly rises to its maximum in April or May.   This periodic change in alkalinity will be seen to correspond roughly with the changes in the living microscopic contents of the sea represented by the phyto-plankton annual curve, and the connection between the two will be seen when we realise that the alkalinity of the sea is due to the relative absence of carbon dioxide. In early spring, then, the developing myriads of diatoms in their metabolic processes gradually use up the store of carbon dioxide accumulated during the winter, or derived from the bi-carbonates of calcium and magnesium, and so increase the alkalinity of the water, till the maximum of alkalinity, due to the fixation of the carbon and the reduction in amount of carbon dioxide, corresponds with the crest of the phyto-plankton curve in, say, April.   Moore has calculated that the annual turnover in the form of carbon which is used up or converted from the inorganic into an organic form probably amounts to something of the order of 20,000 or 30,000 tons of carbon per cubic mile of sea-water, or, say, over an area of the Irish Sea measuring 16 square miles and a depth of 50 fathoms; and this probably means a production each season of about two tons of dry organic matter, corresponding to at least ten tons of moist vegetation, per acre—which suggests that we may still be very far from getting from our seas anything like the amount of possible food-matters that are produced annually.

Testing the alkalinity of the sea-water may therefore be said to be merely ascertaining and measuring the results of the photosynthetic activity of the great phyto-plankton rise in spring due to the daily increase of sunlight.

The marine biologists of the Carnegie Institute, Washington, have made a recent contribution to the subject in certain observations on the alkalinity of the sea (as determined by hydrogen-ion concentration), during which they found in tropical mid-Pacific a sudden change to acidity in a current running eastwards.   Now in the Atlantic the Gulf Stream, and tropical Atlantic waters generally, are much more alkaline than the colder coastal water running south from the Gulf of St. Lawrence.   That is, the colder Arctic water has more carbon dioxide. This suggests that the Pacific easterly set may be due to deeper water, containing more carbon dioxide (=acidity), coming to the surface at that point.   The alkalinity of the sea-water can be determined rapidly by mixing the sample with a few drops of an indicator and observing the change of colour; and this method of detecting ocean currents by observing the hydrogen-ion concentration of the water might be useful to navigators as showing the time of entrance to a known current.

Oceanography has many practical applications—chiefly, but by no means wholly, on the biological side. The great fishing industries of the world deal with living organisms, of which all the vital activities and the inter-relations with the environment are matters of scientific investigation. Aquiculture is as susceptible of scientific treatment as agriculture can be; and the fisherman who has been in the past too much the nomad and the hunter—if not, indeed, the devastating raider—must become in the future the settled farmer of the sea if his harvest is to be less precarious. Perhaps the nearest approach to cultivation of a marine product, and of the fisherman reaping what he has actually sown, is seen in the case of the oyster and mussel industries on the west coast of France, in Holland, America, and to a less extent on our own coast. Much has been done by scientific men for these and other similar coastal fisheries since the days when Professor Coste in France in 1859 introduced oysters from the Scottish oyster-beds to start the great industry at Arcachon and elsewhere. Now we buy back the descendants of our own oysters from the French ostreicul-turists to replenish our depleted beds.

It is no small matter to have introduced a new and important food-fish to the markets of the world. The remarkable deep-water 'tile-fish,' new to science and described as *Lopholatilus chamæleonticeps*, was discovered in 1879 by one of the United States fishing schooners to the south of Nantucket, near the 100-fathom line. Several thousand pounds weight were caught, and the matter was duly investigated by the United States Fish Commission. For a couple of years after that the fish was brought to market in quantity, and then something unusual happened at the bottom of the sea, and in 1882 millions of dead tile-fish were found floating on the surface over an area of thousands of square miles. The schooner *Navarino* sailed for two days and a night through at least 150 miles of sea, thickly covered as far as the eye could reach with dead fish, estimated at 256,000 to the square mile. The Fish Commission sent a vessel to fish systematically over the grounds known as the 'Gulf Stream slope,' where the tile-fish had been so abundant during the two previous years, but she did not catch a single fish, and the associated sub-tropical invertebrate fauna was also practically obliterated.

This wholesale destruction was attributed by the American oceano-graphers to a sudden change in the temperature of the water at the bottom, due in all probability to a withdrawal southwards of the warm Gulf Stream water and a flooding of the area by the cold Labrador current.

I am indebted to Dr. C. H. Townsend, Director of the celebrated New York Aquarium, for the latest information in regard to the

reappearance in quantity of this valuable fish upon the old fishing grounds off Nantucket and Long Island, at about 100 miles from the coast to the east and south-east of New York. It is believed that the tile-fish is now abundant enough to maintain an important fishery, which will add an excellent food-fish to the markets of the United States. It is easily caught with lines at all seasons of the year, and reaches a length of over three feet and a weight of 40 to 50 pounds. During July 1915 the product of the fishery was about two and a half million pounds weight, valued at 55,000 dollars, and in the first few months of 1917 the catch was four and a half million pounds, for which the fishermen received 247,000 dollars.

We can scarcely hope in European seas to add new food-fishes to our markets, but much may be done through the co-operation of scientific investigators of the ocean with the Administrative Departments to bring about a more rational conservation and exploitation of the national fisheries.

Earlier in this address I referred to the pioneer work of the distinguished Manx naturalist, Professor Edward Forbes. There are many of his writings and of his lectures which I have no space to refer to which have points of oceanographic interest. Take this, for example, in reference to our national sea fisheries. We find him in 1847 writing to a friend: ' On Friday night I lectured at the Royal Institution. The subject was the bearing of submarine researches and distribution matters on the fishery question. I pitched into Government mismanagement pretty strong, and made a fair case of it. It seems to me that at a time when half the country is starving we are utterly neglecting or grossly mismanaging great sources of wealth and food. . . . Were I a rich man I would make the subject a hobby, for the good of the country and for the better proving that the true interests of Government are those linked with and inseparable from Science.' We must still cordially approve of these last words, while recognising that our Government Department of Fisheries is now being organised on better lines, is itself carrying on scientific work of national importance, and is, I am happy to think, in complete sympathy with the work of independent scientific investigators of the sea and desirous of closer co-operation with University laboratories and biological stations.

During recent years one of the most important and most frequently discussed of applications of fisheries investigation has been the productivity of the trawling grounds, and especially those of the North Sea. It has been generally agreed that the enormous increase of fishing power during the last forty years or so has reduced the number of large plaice, so that the average size of that fish caught in our home

waters has become smaller, although the total number of plaice landed had continued to increase up to the year of the outbreak of war. Since then, from 1914 to 1919, there has of necessity been what may be described as the most gigantic experiment ever seen in the closing of extensive fishing grounds. It is still too early to say with any certainty exactly what the results of that experiment have been, although some indications of an increase of the fish population in certain areas have been recorded. For example, the Danes, A. C. Johansen and Kirstine Smith, find that large plaice landed in Denmark are now more abundant, and they attribute this to a reversal of the pre-war tendency, due to less intensive fishing. But Dr. James Johnstone has pointed out that there is some evidence of a natural periodicity in abundance of such fish and that the results noticed may represent phases in a cyclic change. If the periodicity noted in Liverpool Bay[26] holds good for other grounds it will be necessary in any comparison of pre-war and post-war statistics to take this natural variation in abundance into very careful consideration.

In the application of oceanographic investigations to sea-fisheries problems, one ultimate aim, whether frankly admitted or not, must be to obtain some kind of a rough approximation to a census or valuation of the sea—of the fishes that form the food of man, of the lower animals of the sea-bottom on which many of the fishes feed, and of the planktonic contents of the upper waters which form the ultimate organised food of the sea—and many attempts have been made in different ways to attain the desired end.

Our knowledge of the number of animals living in different regions of the sea is for the most part relative only. We know that one haul of the dredge is larger than another, or that one locality seems richer than another, but we have very little information as to the actual numbers of any kind of animal per square foot or per acre in the sea. Hensen, as we have seen, attempted to estimate the number of food-fishes in the North Sea from the number of their eggs caught in a comparatively small series of hauls of the tow-net, but the data were probably quite insufficient and the conclusions may be erroneous. It is an interesting speculation to which we cannot attach any economic importance. Heincke says of it: ' This method appears theoretically feasible, but presents in practice so many serious difficulties that no positive results of real value have as yet been obtained.'

All biologists must agree that to determine even approximately the number of individuals of any particular species living in a known area is a contribution to knowledge which may be of great economic value

[26] See Johnstone, *Report Lancs. Sea-Fish Lab.* for 1917, p. 60; and Daniel, *Report* for 1919, p. 51.

in the case of the edible fishes, but it may be doubted whether Hensen's methods, even with greatly increased data, will ever give us the required information. Petersen's method, of setting free marked plaice and then assuming that the proportion of these recaught is to the total number marked as the fishermen's catch in the same district is to the total population, will only hold good in circumscribed areas where there is practically no migration and where the fish are fairly evenly distributed. This method gives us what has been called 'the fishing coefficient,' and this has been estimated for the North Sea to have a probable value of about 0'33 for those sizes of fish which are caught by the trawl. Heincke,[27] from an actual examination of samples of the stock on the ground obtained by experimental trawling (' the catch coefficient '), supplemented by the market returns of the various countries, estimates the adult plaice at about 1,500 millions, of which about 500 millions are caught or destroyed by the fishermen annually.

It is difficult to imagine any further method which will enable us to estimate any such case as, say, the number of plaice in the North Sea where the individuals are so far beyond our direct observation and are liable to change their positions at any moment. But a beginning can be made on more accessible ground with more sedentary animals, and Dr. C. G. Joh. Petersen, of the Danish Biological Station, has for some years been pursuing the subject in a series of interesting Reports on the ' Evaluation of the Sea.'[28] He uses a bottom-sampler, or grab, which can be lowered down open and then closed on the bottom so as to bring up a sample square foot or square metre (or in deep water one-tenth of a square metre) of the sand or mud and its inhabitants. With this apparatus, modified in size and weight for different depths and bottoms, Petersen and his fellow-workers have made a very thorough examination of the Danish waters, and especially of the Kattegat and the Limfjord, have described a series of ' animal communities ' characteristic of different zones and regions of shallow water, and have arrived at certain numerical results as to the quantity of animals in the Kattegat expressed in tons—such as 5,000 tons of plaice requiring as food 50,000 tons of ' useful animals ' (mollusca and polychaet worms), and 25,000 tons of starfish using up 200,000 tons of useful animals which might otherwise serve as food for fishes, and the dependence of all these animals directly or indirectly upon the great beds of Zostera, which make up 24,000,000 tons in the Kattegat. Such estimates are obviously of great biological interest, and even if only rough approximations are a valuable contribution to our under-

[27] F. Heincke, *Cons. Per. Internat. Explor. de la Mer*, ' Investigations on the Plaice,' Copenhagen, 1913.

[28] See *Reports of the Danish Biological Station*, and especially the *Report* for 1918 ' The Sea Bottom and its Production of Fish Food.'

standing of the metabolism of the sea and of the possibility of increasing the yield of local fisheries.

But on studying these Danish results in the light of what we know of our own marine fauna, although none of our seas have been examined in the same detail by the bottom-sampler method, it seems probable that the animal communities as defined by Petersen are not exactly applicable on our coasts and that the estimates of relative and absolute abundance may be very different in different seas under different conditions. The work will have to be done in each great area, such as the North Sea, the English Channel, and the Irish Sea, independently. This is a necessary investigation, both biological and physical, which lies before the oceanographers of the future, upon the results of which the future preservation and further cultivation of our national sea-fisheries may depend.

It has been shown by Johnstone and others that the common edible animals of the shore may exist in such abundance that an area of the sea may be more productive of food for man than a similar area of pasture or crops on land. A Lancashire mussel bed has been shown to have as many as 16,000 young mussels per square foot, and it is estimated that in the shallow waters of Liverpool Bay there are from twenty to 200 animals of sizes varying from an amphipod to a plaice on each square metre of the bottom.[29]

From these and similar data which can be readily obtained, it is not difficult to calculate totals by estimating the number of square yards in areas of similar character between tide-marks or in shallow water. And from weighings of samples some approximation to the number of tons of available food may be computed. But one must not go too far. Let all the figures be based upon actual observation. Imagination is necessary in science, but in calculating a population of even a very limited area it is best to believe only what one can see and measure.

Countings and weighings, however, do not give us all the information we need. It is something to know even approximately the number of millions of animals on a mile of shore and the number of millions of tons of possible food in a sea-area, but that is not sufficient. All food-fishes are not equally nourishing to man, and all plankton and bottom invertebrata are not equally nourishing to a fish. At this point the biologist requires the assistance of the physiologist and the bio-chemist. We want to know next the value of our food matters in proteids, carbohydrates, and fats, and the resulting calories. Dr. Johnstone, of the Oceanography Department of the University of Liverpool, has already shown us how markedly a fat summer herring

[29] *Conditions of Life in the Sea*, Cambridge Univ. Press. 1908.

differs in essential constitution from the ordinary white fish, such as the cod, which is almost destitute of fat.

Professor Brandt, at Kiel, Professor Benjamin Moore, at Port Erin, and others have similarly shown that plankton gatherings may vary greatly in their nutrient value according as they are composed mainly of Diatoms, of Dinoflagellates, or of Copepoda. And, no doubt, the animals of the ' benthos,' the common invertebrates of our shores, will show similar differences in analysis.[30] It is obvious that some contain more solid flesh, others more water in their tissues, others more calcareous matter in the exoskeleton, and that therefore weight for weight we may be sure that some are more nutritious than the others ; and this is probably at least one cause of that preference we see in some of our bottom-feeding fish for certain kinds of food, such as polychaet worms, in which there is relatively little waste, and thin-shelled lamellibranch molluscs, such as young mussels, which have a highly nutrient body in a comparatively thin and brittle shell.

My object in referring to these still incomplete investigations is to direct attention to what seems a natural and useful extension of faunistic work, for the purpose of obtaining some approximation to a quantitative estimate of the more important animals of our shores and shallow water and their relative values as either the immediate or the ultimate food of marketable fishes.

Each such fish has its ' food-chain ' or series of alternative chains, leading back from the food of man to the invertebrates upon which it preys and then to the food of these, and so down to the smallest and simplest organisms in the sea, and each such chain must have all its links fully worked out as to seasonal and quantitative occurrence back to the Diatoms and Flagellates which depend upon physical conditions and take us beyond the range of biology—but not beyond that of oceanography. The Diatoms and the Flagellates are probably more important than the more obvious sea-weeds not only as food, but also in supplying to the water the oxygen necessary for the respiration of living protoplasm. Our object must be to estimate the rate of production and rate of destruction of all organic substances in the sea.

To attain to an approximate census and valuation of the sea— remote though it may seem—is a great aim, but it is not sufficient. We want not only to observe and to count natural objects, but also to understand them. We require to know not merely what an organism is—in the fullest detail of structure and development and affinities—

[30] Moore and others have made analyses of the protein, fat, etc., in the soft parts of Sponge, Ascidian, Aplysia, Fusus, Echinus and Cancer at Port Erin, and find considerable differences—the protein ranging, for example, from 8 to 51 per cent., and the fat from 2 to 14 per cent. (see *Bio-Chemical Journ.* vi. p. 291).

and also where it occurs—again in full detail—and in what abundance under different circumstances, but also *how* it lives and what all its relations are to both its physical and its biological environment, and that is where the physiologist, and especially the bio-chemist, can help us. In the best interests of biological progress the day of the naturalist who merely collects, the day of the anatomist and histologist who merely describe, is over, and the future is with the observer and the experimenter animated by a divine curiosity to enter into the life of the organism and understand how it lives and moves and has its being. 'Happy indeed is he who has been able to discover the causes of things.'

Cardiff is a sea-port, and a great sea-port, and the Bristol Channel is a notable sea-fisheries centre of growing importance. The explorers and merchant venturers of the South-West of England are celebrated in history. What are you doing now in Cardiff to advance our knowledge of the ocean? You have here an important university centre and a great modern national museum, and either or both of these homes of research might do well to establish an oceanographical department, which would be an added glory to your city and of practical utility to the country. This is the obvious centre in Wales for a sea-fisheries institute for both research and education. Many important local movements have arisen from British Association meetings, and if such a notable scientific development were to result from the Cardiff meeting of 1920, all who value the advance of knowledge and the application of science to industry would applaud your enlightened action.

But in a wider sense, it is not to the people of Cardiff alone that I appeal, but to the whole population of these Islands, a maritime people who owe everything to the sea. I urge them to become better informed in regard to our national sea-fisheries and take a more enlightened interest in the basal principles that underlie a rational regulation and exploitation of these important industries. National efficiency depends to a very great extent upon the degree in which scientific results and methods are appreciated by the people and scientific investigation is promoted by the Government and other administrative authorities. The principles and discoveries of science apply to aquiculture no less than to agriculture. To increase the harvest of the sea the fisheries must be continuously investigated, and such cultivation as is possible must be applied, and all this is clearly a natural application of the biological and hydrographical work now united under the science of Oceanography.

ADDRESS

TO THE

MATHEMATICAL AND PHYSICAL SCIENCE SECTION

BY

PROFESSOR A. S. EDDINGTON, M.A., M.Sc., F.R.S.,
PRESIDENT OF THE SECTION.

The Internal Constitution of the Stars.

LAST year at Bournemouth we listened to a proposal from the President of the Association to bore a hole in the crust of the earth and discover the conditions deep down below the surface. This proposal may remind us that the most secret places of Nature are, perhaps, not 10 to the *n*-th miles above our heads, but 10 miles below our feet. In the last five years the outward march of astronomical discovery has been rapid, and the most remote worlds are now scarcely safe from its inquisition. By the work of H. Shapley the globular clusters, which are found to be at distances scarcely dreamt of hitherto, have been explored, and our knowledge of them is in some respects more complete than that of the local aggregation of stars which includes the Sun. Distance lends not enchantment but precision to the view. Moreover, theoretical researches of Einstein and Weyl make it probable that the space which remains beyond is not illimitable; not merely the material universe, but space itself, is perhaps finite; and the explorer must one day stay his conquering march for lack of fresh realms to invade. But to-day let us turn our thoughts inwards to that other region of mystery—a region cut off by more substantial barriers, for, contrary to many anticipations, even the discovery of the fourth dimension has not enabled us to get at the inside of a body. Science has material and non-material appliances to bore into the interior, and I have chosen to devote this address to what may be described as analytical boring devices—*absit omen* !

The analytical appliance is delicate at present, and, I fear, would make little headway against the solid crust of the earth. Instead of letting it blunt itself against the rocks, let us look round for something easier to penetrate. The Sun ? Well, perhaps. Many have struggled to penetrate the mystery of the interior of the Sun; but the difficulties are great, for its substance is denser than water. It may not be quite so bad as Biron makes out in *Love's Labour's Lost* :—

> The heaven's glorious sun,
> That will not be deep-searched with saucy looks;
> Small have continual plodders ever won
> Save base authority from others' books.

But it is far better if we can deal with matter in that state known as a perfect gas, which charms away difficulties as by magic. Where shall it be found?

A few years ago we should have been puzzled to say where, except perhaps in certain nebulæ; but now it is known that abundant material of this kind awaits investigation. Stars in a truly gaseous state exist in great numbers, although at first sight they are scarcely to be discriminated from dense stars like our Sun. Not only so, but the gaseous stars are the most powerful light-givers, so that they force themselves on our attention. Many of the familiar stars are of this kind—Aldebaran, Canopus, Arcturus, Antares; and it would be safe to say that three-quarters of the naked-eye stars are in this diffuse state. This remarkable condition has been made known through the researches of H. N. Russell [1] and E. Hertzsprung; the way in which their conclusions, which ran counter to the prevailing thought of the time, have been substantiated on all sides by overwhelming evidence, is the outstanding feature of recent progress in stellar astronomy.

The diffuse gaseous stars are called *giants*, and the dense stars are called *dwarfs*. During the life of a star there is presumably a gradual increase of density through contraction, so that these terms distinguish the earlier and later stages of stellar history. It appears that a star begins its effective life as a giant of comparatively low temperature—a red or M-type star. As this diffuse mass of gas contracts its temperature must rise, a conclusion long ago pointed out by Homer Lane. The rise continues until the star becomes too dense, and ceases to behave as a perfect gas. A maximum temperature is attained, depending on the mass, after which the star, which has now become a dwarf, cools and further contracts. Thus each temperature-level is passed through twice, once in an ascending and once in a descending stage—once as a giant, once as a dwarf. Temperature plays so predominant a part in the usual spectral classification that the ascending and descending stars were not originally discriminated, and the customary classification led to some perplexities. The separation of the two series was discovered through their great difference in luminosity, particularly striking in the case of the red and yellow stars, where the two stages fall widely apart in the star's history. The bloated giant has a far larger surface than the compact dwarf, and gives correspondingly greater light. The distinction was also revealed by direct determinations of stellar densities, which are possible in the case of eclipsing variables like Algol. Finally, Adams and Kohlschütter have set the seal on this discussion by showing that there are actual spectral differences between the ascending and descending stars at the same temperature-level, which are conspicuous enough—when they are looked for.

Perhaps we should not too hastily assume that the direction of evolution is necessarily in the order of increasing density, in view of our ignorance of the origin of a star's heat, to which I must allude later. But, at any rate, it is a great advance to have disentangled what

[1] *Nature*, vol. **93**, pp. 227, 252, 281.

is the true order of continuous increase of density, which was hidden by superficial resemblances.

The giant stars, representing the first half of a star's life, are taken as material for our first boring experiment. Probably, measured in time, this stage corresponds to much less than half the life, for here it is the ascent which is easy and the way down is long and slow. Let us try to picture the conditions inside a giant star. We need not dwell on the vast dimensions—a mass like that of the Sun, but swollen to much greater volume on account of the low density, often below that of our own atmosphere. It is the star as a storehouse of heat which especially engages our attention. In the hot bodies familiar to us the heat consists in the energy of motion of the ultimate particles, flying at great speeds hither and thither. So too in the stars a great store of heat exists in this form; but a new feature arises. A large proportion, sometimes more than half the total heat, consists of imprisoned radiant energy—ether-waves travelling in all directions trying to break through the material which encages them. The star is like a sieve, which can only retain them temporarily; they are turned aside, scattered, absorbed for a moment, and flung out again in a new direction. An element of energy may thread the maze for hundreds of years before it attains the freedom of outer space. Nevertheless the sieve leaks, and a steady stream permeates outwards, supplying the light and heat which the star radiates all round.

That some ethereal heat as well as material heat exists in any hot body would naturally be admitted; but the point on which we have here to lay stress is that in the stars, particularly in the giant stars, the ethereal portion rises to an importance which quite transcends our ordinary experience, so that we are confronted with a new type of problem. In a red-hot mass of iron the ethereal energy constitutes less than a billionth part of the whole; but in the tussle between matter and ether the ether gains a larger and larger proportion of the energy as the temperature rises. This change in proportion is rapid, the ethereal energy increasing rigorously as the fourth power of the temperature, and the material energy roughly as the first power. But even at the temperature of some millions of degrees attained inside the stars there would still remain a great disproportion; and it is the low density of material, and accordingly reduced material energy per unit volume in the giant stars, which wipes out the last few powers of 10. In all the giant stars known to us, widely as they differ from one another, the conditions are just reached at which these two varieties of heat-energy have attained a rough equality; at any rate one cannot be neglected compared with the other. Theoretically there could be conditions in which the disproportion was reversed and the ethereal far out-weighed the material energy; but we do not find them in the stars. It is as though the stars had been measured out—that their sizes had been determined—with a view to this balance of power; and one cannot refrain from attributing to this condition a deep significance in the evolution of the cosmos into separate stars.

To recapitulate. We are acquainted with heat in two forms—the energy of motion of material atoms and the energy of ether waves. In

familiar hot bodies the second form exists only in insignificant quantities. In the giant stars the two forms are present in more or less equal proportions. That is the new feature of the problem.

On account of this new aspect of the problem the first attempts to penetrate the interior of a star are now seen to need correction. In saying this we do not depreciate the great importance of the early researches of Lane, Ritter, Emden, and others, which not only pointed the way for us to follow, but achieved conclusions of permanent value. One of the first questions they had to consider was by what means the heat radiated into space was brought up to the surface from the low level where it was stored. They imagined a bodily transfer of the hot material to the surface by currents of convection, as in our own atmosphere. But actually the problem is, not how the heat can be brought to the surface, but how the heat in the interior can be held back sufficiently—how it can be barred in and the leakage reduced to the comparatively small radiation emitted by the stars. Smaller bodies have to manufacture the radiant heat which they emit, living from hand to mouth; the giant stars merely leak radiant heat from their store. I have put that much too crudely; but perhaps it suggests the general idea.

The recognition of ethereal energy necessitates a twofold modification in the calculations. In the first place, it abolishes the supposed convection currents; and the type of equilibrium is that known as radiative instead of convective. This change was first suggested by R. A. Sampson so long ago as 1894. The detailed theory of radiative equilibrium is particularly associated with K. Schwarzschild, who applied it to the Sun's atmosphere. It is perhaps still uncertain whether it holds strictly for the atmospheric layers, but the arguments for its validity in the interior of a star are far more cogent. Secondly, the outflowing stream of ethereal energy is powerful enough to exert a *direct mechanical effect* on the equilibrium of a star. It is as though a strong wind were rushing outwards. In fact we may fairly say that the stream of radiant energy *is* a wind; for though ether waves are not usually classed as material, they have the chief mechanical properties of matter, viz. mass and momentum. This wind distends the star and relieves the pressure on the inner parts. The pressure on the gas in the interior is not the full weight of the superincumbent columns, because that weight is partially borne by the force of the escaping ether waves beating their way out. This force of radiation-pressure, as it is called, makes an important difference in the formulation of the conditions for equilibrium of a star.

Having revised the theoretical investigations in accordance with these considerations,[2] we are in a position to deduce some definite numerical results. On the observational side we have fairly satisfactory knowledge of the masses and densities of the stars and of the total radiation emitted by them; this knowledge is partly individual and partly statistical. The theoretical analysis connects these observational data on the one hand with the physical properties of the material inside

[2] *Astrophysical Journal*, vol. 48, p. 205.

the star on the other hand.   We can thus find certain information as to the inner material, as though we had actually bored a hole.   So far as can be judged there are only two physical properties of the material which can concern us—always provided that it is sufficiently rarefied to behave as a perfect gas—viz. the average molecular weight and the transparency or permeability to radiant energy.   In connecting these two unknowns with the quantities given directly by astronomical observation we depend entirely on the well-tried principles of conservation of momentum and the second law of thermodynamics.   If any element of speculation remains in this method of investigation, I think it is no more than is inseparable from every kind of theoretical advance.

We have, then, on the one side the mass, density and output of heat, quantities as to which we have observational knowledge; on the other side, molecular weight and transparency, quantities which we want to discover.

To find the transparency of stellar material to the radiation traversing it is of particular interest because it links on this astronomical inquiry to physical investigations now being carried on in the laboratory, and to some extent it extends those investigations to conditions unattainable on the earth.   At high temperatures the ether waves are mainly of very short wave-length, and in the stars we are dealing mainly with radiation of wave-length 3 to 30 Angström units, which might be described as very soft x-rays.   It is interesting, therefore, to compare the results with the absorption of the harder x-rays dealt with by physicists.   To obtain an exact measure of this absorption in the stars we have to assume a value of the molecular weight; but fortunately the extreme range possible for the molecular weight gives fairly narrow limits for the absorption.   The average weight of the ultimate independent particles in a star is probably rather low, because in the conditions prevailing there the atoms would be strongly ionised; that is to say, many of the outer electrons of the system of the atom would be broken off; and as each of these free electrons counts as an independent molecule for the present purposes, this brings down the average weight.   In the extreme case (probably not reached in a star) when the whole of the electrons outside the nucleus are detached the average weight comes down to about 2, *whatever the material*, because the number of electrons is about half the atomic weight for all the elements (except hydrogen).   We may, then, safely take 2 as the extreme lower limit.   For an upper limit we might perhaps take 200; but to avoid controversy we shall be generous and merely assume that the molecular weight is not greater than— infinity.   Here is the result:—

> For molecular weight 2, mass-coefficient of absorption=10 C.G.S. units.
>
> For molecular weight ∞, mass-coefficient of absorption =130 C.G.S. units.

The true value, then, must be between 10 and 130.   Partly from thermodynamical considerations, and partly from further comparisons of astronomical observation with theory, the most likely value seems to be about 35 C.G.S. units, corresponding to molecular weight 3·5.

Now this is of the same order of magnitude as the absorption of x-rays measured in the laboratory. I think the result is in itself of some interest, that in such widely different investigations we should approach the same kind of value of the opacity of matter to radiation. The penetrating power of the radiation in the star is much like that of x-rays; more than half is absorbed in a path of 20 cms. at atmospheric density. Incidentally, this very high opacity explains why a star is so nearly heat tight, and can store vast supplies of heat with comparatively little leakage.

So far this agrees with what might have been anticipated; but there is another conclusion which physicists would probably not have foreseen. The giant series comprises stars differing widely in their densities and temperatures, those at one end of the series being on the average about ten times hotter throughout than those at the other end. By the present investigation we can compare directly the opacity of the hottest stars with that of the coolest stars. The rather surprising result emerges that the opacity is the same for all; at any rate there is no difference large enough for us to detect. There seems no room for doubt that at these high temperatures the absorption-coefficient is approaching a limiting value, so that over a wide range it remains practically constant. With regard to this constancy, it is to be noted that the temperature is concerned twice over: it determines the character and wave-length of the radiation to be absorbed, as well as the physical condition of the material which is absorbing. From the experimental knowledge of x-rays we should have expected the absorption to vary very rapidly with the wave length, and therefore with the temperature. It is surprising, therefore, to find a nearly constant value.

The result becomes a little less mysterious when we consider more closely the nature of absorption. Absorption is not a continuous process, and after an atom has absorbed its quantum it is put out of action for a time until it can recover its original state. We know very little of what determines the rate of recovery of the atom, but it seems clear that there is a limit to the amount of absorption that can be performed by an atom in a given time. When that limit is reached no increase in the intensity of the incident radiation will lead to any more absorption. There is in fact a saturation effect. In the laboratory experiments the radiation used is extremely weak; the atom is practically never caught unprepared, and the absorption is proportional to the incident radiation. But in the stars the radiation is very intense and the saturation effect comes in.

Even granting that the problem of absorption in the stars involves this saturation effect, which does not affect laboratory experiments, it is not very easy to understand theoretically how the various conditions combine to give a constant absorption-coefficient independent of temperature and wave-length. But the astronomical results seem conclusive. Perhaps the most hopeful suggestion is one made to me a few years ago by C. G. Barkla. He suggested that the opacity of the stars may depend mainly on *scattering* rather than on true atomic absorption. In that case the constancy has a simple explanation, for it is known that the coefficient of scattering (unlike true absorption)

approaches a definite constant value for radiation of short wave-length. The value, moreover, is independent of the material. Further, scattering is a continuous process, and there is no likelihood of any saturation effect; thus for very intense streams of radiation its value is maintained, whilst the true absorption may sink to comparative insignificance. The difficulty in this suggestion is a numerical discrepancy between the known theoretical scattering and the values already given as deduced from the stars. The theoretical coefficient is only 0.2 compared with the observed value 10 to 130. Barkla further pointed out that the waves here concerned are not short enough to give the ideal coefficient; they would be scattered more powerfully, because under their influence the electrons in any atom would all vibrate in the same phase instead of haphazard phases. This might help to bridge the gap, but not sufficiently. It must be remembered that many of the electrons have broken loose from the atom and do not contribute to the increase.[3] Making all allowances for uncertainties in the data, it seems clear that the astronomical opacity is definitely higher than the theoretical scattering. Very recently, however, a new possibility has opened up which may possibly effect a reconciliation. Later in the address I shall refer to it again.

Astronomers must watch with deep interest the investigations of these short waves, which are being pursued in the laboratory, as well as the study of the conditions of ionisation both by experimental and theoretical physics, and I am glad of this opportunity of bringing before those who deal with these problems the astronomical bearing of their work.

I can only allude very briefly to the purely astronomical results which follow from this investigation;[4] it is here that the best opportunity occurs for checking the theory by comparison with observation, and for finding out in what respects it may be deficient. Unfortunately, the observational data are generally not very precise, and the test is not so stringent as we could wish. It turns out that (the opacity being constant) the total radiation of a giant star should be a function of its mass only, independent of its temperature or state of diffuseness. The total radiation (which is measured roughly by the luminosity) of any one star thus remains constant during the whole giant stage of its history. This agrees with the fundamental feature, pointed out by Russell in introducing the giant and dwarf hypothesis, that giant stars of every spectral type have nearly the same luminosity. From the range of luminosity of these stars it is now possible to find their range of mass. The masses are remarkably alike—a fact already suggested by work on double stars. Limits of mass in the ratio 3 : 1 would cover the great majority of the giant stars. Somewhat tentatively we are able to extend the investigation to dwarf stars, taking account of the

[3] *E.g.*, for iron non-ionised the theoretical scattering is 5.2, against an astronomical value 120. If 16 electrons (2 rings) are broken off the theoretical coefficient is 0.9 against an astronomical value 35. For different assumptions as to ionisation the values chase one another, but cannot be brought within reasonable range.

[4] *Monthly Notices*, vol. 77, pp. 16, 596; vol. 79, p. 2.

deviations of dense gas from the ideal laws and using our own Sun to supply a determination of the unknown constant involved. We can calculate the maximum temperature reached by different masses; for example, a star must have at least $\frac{1}{7}$ the mass of the Sun in order to reach the lowest spectral type, M; and in order to reach the hottest type, B, it must be at least $2\frac{1}{2}$ times as massive as the Sun. Happily for the theory no star has yet been found with a mass less than $\frac{1}{7}$ of the Sun's; and it is a well-known fact, discovered from the study of spectroscopic binaries, that the masses of the B stars are large compared with those of other types. Again, it is possible to calculate the difference of brightness of the giant and dwarf stars of type M, *i.e.* at the beginning and end of their career; the result agrees closely with the observed difference. In the case of a class of variable stars in which the light changes seem to depend on a mechanical pulsation of the star, the knowledge we have obtained of the internal conditions enables us to predict the period of pulsation within narrow limits. For example, for δ Cephei, the best-known star of this kind, the theoretical period is between 4 and 10 days, and the actual period is $5\frac{1}{3}$ days. Corresponding agreement is found in all the other cases tested.

Our observational knowledge of the things here discussed is chiefly of a rather vague kind, and we can scarcely claim more than a general agreement of theory and observation. What we have been able to do in the way of tests is to offer the theory a considerable number of opportunities to 'make a fool of itself,' and so far it has not fallen into our traps. When the theory tells us that a star having the mass of the Sun will at one stage in its career reach a maximum effective temperature of 9,000° (the Sun's effective temperature being 6,000°) we cannot do much in the way of checking it; but an erroneous theory might well have said that the maximum temperature was 20,000° (hotter than any known star), in which case we should have detected its error. If we cannot feel confident that the answers of the theory are true, it must be admitted that it has shown some discretion in lying without being found out.

It would not be surprising if individual stars occasionally depart considerably from the calculated results, because at present no serious attempt has been made to take into account rotation, which may modify the conditions when sufficiently rapid. That appears to be the next step needed for a more exact study of the question.

Probably the greatest need of stellar astronomy at the present day, in order to make sure that our theoretical deductions are starting on the right lines, is some means of measuring the apparent angular diameters of stars. At present we can calculate them approximately from theory, but there is no observational check. We believe we know with fair accuracy the apparent surface brightness corresponding to each spectral type; then all that is necessary is to divide the total apparent brightness by this surface brightness, and the result is the angular area subtended by the star. The unknown distance is not involved, because surface brightness is independent of distance. Thus the estimation of the angular diameter of any star seems to be a very simple matter. For instance, the star with the greatest apparent diameter is almost certainly

Betelgeuse, diameter ·051″. Next to it comes Antares, ·043″. Other examples are Aldebaran ·022″, Arcturus ·020″, Pollux ·013″. Sirius comes rather low down with diameter ·007″. The following table may be of interest as showing the angular diameters expected for stars of various types and visual magnitudes:—

Probable Angular Diameters of Stars.

Vis. Mag.	A	F	G	K	M
m.	″	″	″	″	″
0·0	·0034	·0054	·0098	·0219	·0859
2·0	·0014	·0022	·0039	·0087	·0342
4·0	·0005	·0009	·0016	·0035	·0136

However confidently we may believe in these values, it would be an immense advantage to have this first step in our deductions placed beyond doubt. If the direct measurement of these diameters could be made with any accuracy it would make a wonderfully rapid advance in our knowledge. The prospects of accomplishing some part of this task are now quite hopeful. We have learnt with great interest this year that work is being carried out by interferometer methods with the 100-inch reflector at Mount Wilson, and the results are most promising. At present the method has only been applied to measuring the separation of close double stars, but there seems to be no doubt that an angular diameter of ·05″ is well within reach. Although the great mirror is used for convenience, the interferometer method does not in principle require great apertures, but rather two small apertures widely separated as in a range-finder. Prof. Hale has stated, moreover, that successful results were obtained on nights of poor seeing. Perhaps it would be unsafe to assume that ' poor seeing ' at Mount Wilson means quite the same thing as it does for us, and I anticipate that atmospheric disturbance will ultimately set the limit to what can be accomplished. But even if we have to send special expeditions to the top of one of the highest mountains in the world the attack on this far-reaching problem must not be allowed to languish.

I spoke earlier of the radiation-pressure exerted by the outflowing heat, which has an important effect on the equilibrium of a star. It is quite easy to calculate what proportion of the weight of the material is supported in this way; it depends neither on the density nor opacity, but solely on the star's total mass and on the molecular weight. No astronomical data are needed; the calculation involves only fundamental physical constants found by laboratory researches. Here are the figures, first for average molecular weight 3·0:—

For mass $\frac{1}{2}$ × Sun, fraction of weight supported by radiation-pressure = ·044.

For mass 5 × Sun, fraction of weight supported by radiation-pressure = ·457.

For molecular weight 5·0 the corresponding fractions are ·182 and ·645.

The molecular weight can scarcely go beyond this range,[5] and for the conclusions I am about to draw it does not much matter which limit we take. Probably 90 per cent. of the giant stars have masses between $\frac{1}{2}$ and 5 times the Sun's, and we see that this is just the range in which radiation-pressure rises from unimportance to importance. It seems clear that a globe of gas of larger mass, in which radiation-pressure and gravitation are nearly balancing, would be likely to be unstable. The condition may not be strictly unstable in itself, but a small rotation or perturbation would make it so. It may therefore be conjectured that, if nebulous material began to concentrate into a mass much greater than 5 times the Sun's, it would probably break up, and continue to redivide until more stable masses resulted. Above the upper limit the chances of survival are small; when the lower limit is approached the danger has practically disappeared, and there is little likelihood of any further breaking-up. Thus the final masses are left distributed almost entirely between the limits given. To put the matter slightly differently, we are able to predict from general principles that the material of the stellar universe will aggregate primarily into masses chiefly lying between 10^{33} and 10^{34} grams; and this is just the magnitude of the masses of the stars according to astronomical observation.[6]

This study of the radiation and internal conditions of a star brings forward very pressingly a problem often debated in this Section: What is the source of the heat which the Sun and stars are continually squandering? The answer given is almost unanimous—that it is obtained from the gravitational energy converted as the star steadily contracts. But almost as unanimously this answer is ignored in its practical consequences. Lord Kelvin showed that this hypothesis, due to Helmholtz, necessarily dates the birth of the Sun about 20,000,000 years ago; and he made strenuous efforts to induce geologists and biologists to accommodate their demands to this time-scale. I do not think they proved altogether tractable. But it is among his own colleagues, physicists and astronomers, that the most outrageous violations of this limit have prevailed. I need only refer to Sir George Darwin's theory of the earth-moon system, to the present Lord Rayleigh's determination of the age of terrestrial rocks from occluded helium, and to all modern discussions of the statistical equilibrium of the stellar system. No one seems to have any hesitation, if it suits him, in carrying back the history of the earth long before the supposed date of formation of the solar system; and in some cases at least this appears to be justified

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[5] As an illustration of these limits, iron has 26 outer electrons; if 10 break away the average molecular weight is 5; if 18 break away the molecular weight is 3. Eggert (*Phys. Zeits.* 1919, p. 570) has suggested by thermodynamical reasoning that in most cases the two outer rings (16 electrons) would break away in the stars. The comparison of theory and observation for the dwarf stars also points to a molecular weight a little greater than 3.

[6] By admitting plausible assumptions closer limits could be drawn. Taking the molecular weight as 3.5, and assuming that the most critical condition is when $\frac{1}{3}$ of gravitation is counterbalanced (by analogy with the case of rotating spheroids, in which centrifugal force opposes gravitation and creates instability), we find that the critical mass is just twice that of the Sun, and stellar masses may be expected to cluster closely round this value.

by experimental evidence which it is difficult to dispute. Lord Kelvin's date of the creation of the Sun is treated with no more respect than Archbishop Ussher's.

The serious consequences of this contraction hypothesis are particularly prominent in the case of giant stars, for the giants are prodigal with their heat and radiate at least a hundred times as fast as the Sun. The supply of energy which suffices to maintain the Sun for 10,000,000 years would be squandered by a giant star in less than 100,000 years. The whole evolution in the giant stage would have to be very rapid. In 18,000 years at the most a typical star must pass from the initial M stage to type G. In 80,000 years it has reached type A, near the top of the scale, and is about to start on the downward path. Even these figures are probably very much over-estimated.[7] Most of the naked-eye stars are still in the giant stage. Dare we believe that they were all formed within the last 80,000 years? The telescope reveals to us objects not only remote in distance but remote in time. We can turn it on a globular cluster and behold what was passing 20,000, 50,000, even 200,000 years ago—unfortunately not all in the same cluster, but different clusters representing different epochs of the past. As Shapley has pointed out, the verdict appears to be ' no change.' This is perhaps not conclusive, because it does not follow that individual stars have suffered no change in the interval; but it is difficult to resist the impression that the evolution of the stellar universe proceeds at a slow, majestic pace, with respect to which these periods of time are insignificant.

There is another line of astronomical evidence which appears to show more definitely that the evolution of the stars proceeds far more slowly than the contraction hypothesis allows; and perhaps it may ultimately enable us to measure the true rate of progress. There are certain stars, known as Cepheid variables, which undergo a regular fluctuation of light of a characteristic kind, generally with a period of a few days. This light change is *not* due to eclipse. Moreover, the colour quality of the light changes between maximum and minimum, evidently pointing to a periodic change in the physical condition of the star. Although these objects were formerly thought to be double stars, it now seems clear that this was a misinterpretation of the spectroscopic evidence. There is in fact no room for the hypothetical companion star; the orbit is so small that we should have to place it inside the principal star. Everything points to the period of the light pulsation being something intrinsic in the star; and the hypothesis advocated by Shapley, that it represents a mechanical pulsation of the star, seems to be the most plausible. I have already mentioned that the observed period does in fact agree with the calculated period of mechanical pulsation, so that the pulsation explanation survives one fairly stringent test. But whatever the cause of the variability, whether pulsation or rotation, provided only that it is intrinsic in the

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[7] I have taken the ratio of specific heats at the extreme possible value, $\frac{5}{3}$; that is to say, no allowance has been made for the energy needed for ionisation and internal vibrations of the atoms, which makes a further call on the scanty supply available.

star, and not forced from outside, the density must be the leading factor in determining the period. If the star is contracting so that its density changes appreciably, the period cannot remain constant. Now, on the contraction hypothesis the change of density must amount to at least 1 per cent. in 40 years. (I give the figures for δ Cephei, the best-known variable of this class.) The corresponding change of period should be very easily detectable. For δ Cephei the period ought to decrease 40 seconds annually.

Now δ Cephei has been under careful observation since 1785, and it is known that the change of period, if any, must be very small. S. Chandler found a decrease of period of $\frac{1}{20}$ second per annum, and in a recent investigation E. Hertzsprung has found a decrease of $\frac{1}{10}$ second per annum. The evidence that there is any decrease at all rests almost entirely on the earliest observations made before 1800, so that it is not very certain; but in any case the evolution is proceeding at not more than $\frac{1}{400}$ of the rate required by the contraction hypothesis. There must at this stage of the evolution of the star be some other source of energy which prolongs the life of the star 400-fold. The time-scale so enlarged would suffice for practically all reasonable demands.

I hope the dilemma is plain. Either we must admit that whilst the density changes 1 per cent. a certain period intrinsic in the star can change no more than $\frac{1}{800}$ of 1 per cent., or we must give up the contraction hypothesis.

If the contraction theory were proposed to-day as a novel hypothesis I do not think it would stand the smallest chance of acceptance. From all sides—biology, geology, physics, astronomy—it would be objected that the suggested source of energy was hopelessly inadequate to provide the heat spent during the necessary time of evolution; and, so far as it is possible to interpret observational evidence confidently, the theory would be held to be definitely negatived. Only the inertia of tradition keeps the contraction hypothesis alive—or rather, not alive, but an unburied corpse. But if we decide to inter the corpse, let us frankly recognise the position in which we are left. A star is drawing on some vast reservoir of energy by means unknown to us. This reservoir can scarcely be other than the sub-atomic energy which, it is known, exists abundantly in all matter; we sometimes dream that man will one day learn how to release it and use it for his service. The store is well-nigh inexhaustible, if only it could be tapped. There is sufficient in the Sun to maintain its output of heat for 15 billion years.

Certain physical investigations in the past year, which I hope we may hear about at this meeting, make it probable to my mind that some portion of this sub-atomic energy is actually being set free in the stars. F. W. Aston's experiments seem to leave no room for doubt that all the elements are constituted out of hydrogen atoms bound together with negative electrons. The nucleus of the helium atom, for example, consists of 4 hydrogen atoms bound with 2 electrons. But Aston has further shown conclusively that the mass of the helium atom is less than the sum of the masses of the 4 hydrogen atoms which enter into it; and in this at any rate the chemists agree with him. There is a loss of mass in the synthesis amounting to about 1 part in 120, the

atomic weight of hydrogen being 1·008 and that of helium just 4. I will not dwell on his beautiful proof of this, as you will no doubt be able to hear it from himself. Now mass cannot be annihilated, and the deficit can only represent the mass of the electrical energy set free in the transmutation. We can therefore at once calculate the quantity of energy liberated when helium is made out of hydrogen. If 5 per cent. of a star's mass consists initially of hydrogen atoms, which are gradually being combined to form more complex elements, the total heat liberated will more than suffice for our demands, and we need look no further for the source of a star's energy.

But is it possible to admit that such a transmutation is occurring? It is difficult to assert, but perhaps more difficult to deny, that this is going on. Sir Ernest Rutherford has recently been breaking down the atoms of oxygen and nitrogen, driving out an isotope of helium from them; and what is possible in the Cavendish laboratory may not be too difficult in the Sun. I think that the suspicion has been generally entertained that the stars are the crucibles in which the lighter atoms which abound in the nebulæ are compounded into more complex elements. In the stars matter has its preliminary brewing to prepare the greater variety of elements which are needed for a world of life. The radio-active elements must have been formed at no very distant date; and their synthesis, unlike the generation of helium from hydrogen, is endothermic. If combinations requiring the addition of energy can occur in the stars, combinations which liberate energy ought not to be impossible.

We need not bind ourselves to the formation of helium from hydrogen as the sole reaction which supplies the energy, although it would seem that the further stages in building up the elements involve much less liberation, and sometimes even absorption, of energy. It is a question of accurate measurement of the deviations of atomic weights from integers, and up to the present hydrogen is the only element for which Mr. Aston has been able to detect the deviation. No doubt we shall learn more about the possibilities in due time. The position may be summarised in these terms: the atoms of all elements are built of hydrogen atoms bound together, and presumably have at one time been formed from hydrogen; the interior of a star seems as likely a place as any for the evolution to have occurred; whenever it did occur a great amount of energy must have been set free; in a star a vast quantity of energy is being set free which is hitherto unaccounted for. You may draw a conclusion if you like.

If, indeed, the sub-atomic energy in the stars is being freely used to maintain their great furnaces, it seems to bring a little nearer to fulfilment our dream of controlling this latent power for the well-being of the human race—or for its suicide.

So far as the immediate needs of astronomy are concerned, it is not of any great consequence whether in this suggestion we have actually laid a finger on the true source of the heat. It is sufficient if the discussion opens our eyes to the wider possibilities. We can get rid of the obsession that there is no other conceivable supply besides contraction, but we need not again cramp ourselves by adopting prematurely

what is perhaps a still wilder guess. Rather we should admit that the source is not certainly known, and seek for any possible astronomical evidence which may help to define its necessary character. One piece of evidence of this kind may be worth mentioning. It seems clear that it must be the high temperature inside the stars which determines the liberation of energy, as H. N. Russell has pointed out.[8] If so the supply may come mainly from the hottest region at the centre. I have already stated that the general uniformity of the opacity of the stars is much more easily intelligible if it depends on scattering rather than on true absorption; but it did not seem possible to reconcile the deduced stellar opacity with the theoretical scattering coefficient. Within reasonable limits it makes no great difference in our calculations at what parts of the star the heat energy is supplied, and it was assumed that it comes more or less evenly from all parts, as would be the case on the contraction theory. The possibility was scarcely contemplated that the energy is supplied entirely in a restricted region round the centre. Now, the more concentrated the supply, the lower is the opacity requisite to account for the observed radiation. I have not made any detailed calculations, but it seems possible that for a sufficiently concentrated source the deduced and the theoretical coefficients could be made to agree, and there does not seem to be any other way of accomplishing this. Conversely, we might perhaps argue that the present discrepancy of the coefficients shows that the energy supply is not spread out in the way required by the contraction hypothesis, but belongs to some new source only available at the hottest, central part of the star.

I should not be surprised if it is whispered that this address has at times verged on being a little bit speculative; perhaps some outspoken friend may bluntly say that it has been highly speculative from beginning to end. I wonder what is the touchstone by which we may test the legitimate development of scientific theory and reject the idly speculative. We all know of theories which the scientific mind instinctively rejects as fruitless guesses; but it is difficult to specify their exact defect or to supply a rule which will show us when we ourselves do err. It is often supposed that to speculate and to make hypotheses are the same thing; but more often they are opposed. It is when we let our thoughts stray outside venerable, but sometimes insecure, hypotheses that we are said to speculate. Hypothesis limits speculation. Moreover, distrust of speculation often serves as a cover for loose thinking; wild ideas take anchorage in our minds and influence our outlook; whilst it is considered too speculative to subject them to the scientific scrutiny which would exorcise them.

If we are not content with the dull accumulation of experimental facts, if we make any deductions or generalisations, if we seek for any theory to guide us, some degree of speculation cannot be avoided. Some will prefer to take the interpretation which seems to be most immediately indicated and at once adopt that as an hypothesis; others will rather seek to explore and classify the widest possibilities which are not definitely inconsistent with the facts. Either choice has its dangers;

8 *Pub. Act. Soc. Pacific.* August 1919.

the first may be too narrow a view and lead progress into a cul-de-sac; the second may be so broad that it is useless as a guide, and diverges indefinitely from experimental knowledge.   When this last case happens, it must be concluded that the knowledge is not yet ripe for theoretical treatment and speculation is premature.   The time when speculative theory and observational research may profitably go hand in hand is when the possibilities, or at any rate the probabilities, can be narrowed down by experiment, and the theory can indicate the tests by which the remaining wrong paths may be blocked up one by one.

The mathematical physicist is in a position of peculiar difficulty. He may work out the behaviour of an ideal model of material with specifically defined properties, obeying mathematically exact laws, and so far his work is unimpeachable.   It is no more speculative than the binomial theorem.   But when he claims a serious interest for his toy, when he suggests that his model is like something going on in Nature, he inevitably begins to speculate.   Is the actual body really like the ideal model?   May not other unknown conditions intervene? He cannot be sure, but he cannot suppress the comparison; for it is by looking continually to Nature that he is guided in his choice of a sub-ject.   A common fault, to which he must often plead guilty, is to use for the comparison data over which the more experienced observer shakes his head; they are too insecure to build extensively upon.   Yet even in this, theory may help observation by showing the kind of data which it is especially important to improve.

I think that the more idle kinds of speculation will be avoided if the investigation is conducted from the right point of view.   When the properties of an ideal model have been worked out by rigorous mathe-matics, all the underlying assumptions being clearly understood, then it becomes possible to say that such and such properties and laws lead precisely to such and such effects.   If any other disregarded factors are present, they should now betray themselves when a comparison is made with Nature.   There is no need for disappointment at the failure of the model to give perfect agreement with observation; it has served its purpose, for it has distinguished what are the features of the actual phenomena which require new conditions for their explanation.   A general preliminary agreement with observation is necessary, otherwise the model is hopeless; not that it is necessarily wrong so far as it goes, but it has evidently put the less essential properties foremost.   We have been pulling at the wrong end of the tangle, which has to be un-ravelled by a different approach.   But after a general agreement with observation is established, and the tangle begins to loosen, we should always make ready for the next knot.   I suppose that the applied mathematician whose theory has just passed one still more stringent test by observation ought not to feel satisfaction, but rather disappointment —' Foiled again!   This time I *had* hoped to find a discordance which would throw light on the points where my model could be improved.' Perhaps that is a counsel of perfection; I own that I have never felt very keenly a disappointment of this kind.

Our model of Nature should not be like a building—a handsome

structure for the populace to admire, until in the course of time someone takes away a corner-stone and the edifice comes toppling down. It should be like an engine with movable parts. We need not fix the position of any one lever; that is to be adjusted from time to time as the latest observations indicate. The aim of the theorist is to know the train of wheels which the lever sets in motion—that binding of the parts which is the soul of the engine.

In ancient days two aviators procured to themselves wings. Dædalus flew safely through the middle air across the sea, and was duly honoured on his landing. Young Icarus soared upwards towards the Sun till the wax melted which bound his wings, and his flight ended in fiasco. In weighing their achievements perhaps there is something to be said for Icarus. The classic authorities tell us that he was only ' doing a stunt,' but I prefer to think of him as the man who certainly brought to light a constructional defect in the flying-machines of his day. So too in science. Cautious Dædalus will apply his theories where he feels most confident they will safely go; but by his excess of caution their hidden weaknesses cannot be brought to light. Icarus will strain his theories to the breaking-point till the weak joints gape. For a spectacular stunt? Perhaps partly; he is often very human. But if he is not yet destined to reach the Sun and solve for all time the riddle of its constitution, yet he may hope to learn from his journey some hints to build a better machine.

ADDRESS

TO THE

CHEMICAL SECTION

BY

C. T. HEYCOCK, M.A., F.R.S.,

PRESIDENT OF THE SECTION.

DURING its past eighty-nine years of useful life the British Association has, in the course of its evolution, established certain traditions; among these is the expectation that the sectional President shall deliver an address containing a summary of that branch of natural knowledge with which he has become especially acquainted.

The rapid accumulation of experimental observations during the last century, and the consequent necessity for classifying the observed facts with the aid of hypotheses and theories of ever-increasing complexity, make such summaries of knowledge essential, not only to the student of science, but also to the person of non-specialised education who desires to realise something of the tendencies and of the results of modern science.

At the present moment, when the whole world is in pause after having overcome the greatest peril which has ever threatened civilisation; when all productive effort, social, artistic, and scientific, is undergoing reorganisation preparatory to an advance which will eclipse in importance the progress made during the nineteenth century, such attempts to visualise the present condition of knowledge as are made in our Presidential Addresses are of particular value. It is, therefore, hardly necessary for me to apologise for an endeavour to place before you a statement upon the particular branch of science to which I have myself paid special attention; whatever faults may attend the mode of presentation, such a survey of a specific field of knowledge cannot but be of value to some amongst us.

I propose to deal to-day with the manner in which our present rather detailed knowledge of metallic alloys has been acquired, starting from the sparse information which was available thirty or forty years ago;

to show the pitfalls which have been avoided in the theoretical interpretation of the observed facts, and to sketch very briefly the present position of our knowledge.

The production of metals and their alloys undoubtedly constitutes the oldest of those chemical arts which ultimately expanded into the modern science of chemistry, with all its overwhelming mass of experimental detail and its intricate interweaving of theoretical interpretation of the observed facts. Tubal-Cain lived during the lifetime of our common ancestor, and was 'an instructor of every artificer in brass and iron'; and although it may be doubted whether the philologists have yet satisfactorily determined whether Tubal-Cain was really acquainted with the manufacture of such a complex metallic alloy as brass, it is certain that chemical science had its beginnings in the reduction of metals from their ores and in the preparation of useful alloys from those metals. In fact, metallic alloys, or mixtures of metals, have been used by mankind for the manufacture of implements of war and of agriculture, of coinage, statuary, cooking vessels, and the like from the very earliest times.

In the course of past ages an immense amount of practical information has been accumulated concerning methods of reducing metals, or mixtures of metals, from their ores, and by subsequent treatment, usually by heating and cooling, of adapting the resulting metallic product to the purpose for which it was required. Until quite recent times, however, the whole of this knowledge was entirely empirical in character, because it had no foundation in general theoretical principles; it was collected in haphazard fashion in accordance with that method of trial and error which led our forerunners surely, but with excessive expenditure of time and effort, to valuable results.

To-day I purpose dealing chiefly with the non-ferrous alloys, not because any essential difference in type exists between the ferrous and non-ferrous alloys, but merely because the whole field presented by the chemistry of the metals and their alloys is too vast to be covered in any reasonable length of time.

The earliest recorded scientific investigations on alloys were made in 1722 by Reaumur, who employed the microscope to examine the fractured surfaces of white and grey cast iron and steel.

In 1808 Widmanstatten cut sections from meteorites, which he polished and etched.

The founder, however, of modern metallography is undoubtedly H. C. Sorby, of Sheffield. Sorby's early petrographic work on the examination of thin sections of rock under the microscope led him to a study of meteorites and of iron and steel, and in a paper read before the British Association in 1864 he describes briefly (I quote his own words) how sections 'of iron and steel may be prepared for the microscope so as to exhibit their structure to a perfection that leaves little to be desired. They show various mixtures of iron, and two or three well-defined compounds of iron and carbon, graphite, and slag; these constituents being present in different proportions and arranged in various manners, give rise to a large number of varieties of iron and steel, differing by well-marked and very striking peculiarities

of structure.'   The methods described by Sorby for polishing and
etching alloys and his method of vertical illumination (afterwards
improved by Beck) are employed to-day by all who work at this branch
of metallography.

The lantern-slides, now shown, were reproduced from his original
photographs; they form a lasting memorial to his skill as an investi-
gator and his ability as a manipulator.   In 1887 Dr. Sorby published
a paper on the microscopical structure of iron and steel in the Journal
of the Iron and Steel Institute.   This masterpiece of clear writing
and expression, even with our present knowledge, needs but little
emendation.   In this paper he describes Free Iron (ferrite) carbon as
graphite, the pearly constituent as a very fine laminar structure (pearlitic
structure), combined iron as the chief constituent of white cast iron
(cementite), slag inclusions, effect of tempering steel, effect of working
iron and steel, cementation of wrought iron, and the decarbonisation
of cast iron by haematite.   A truly remarkable achievement for one
man.

From 1854-68 Mattheisen published in the Reports of the British
Association and in the Proceedings and Transactions of the Royal
Society, a large number of papers on the electrical conductivity,
tenacity, and specific gravity of pure metals and alloys.   He concluded
that alloys are either mixtures of definite chemical compounds with an
excess of one or other metal, or solutions of the definite alloy in the
excess of one of the metals employed, forming, in their solid condition,
what he called a solidified solution.   This idea of a solidified solution
has developed into a most fruitful theory upon which much of our
modern notions of alloys depends.   Although, at the time, the experi-
ments on the electrical conductivity did not lead to very definite con-
clusions, the method has since been used with great success in testing
for the presence of minute quantities of impurities in the copper used
for conductors.

In the *Philosophical Magazine* for 1875, F. Guthrie, in a
remarkable paper, quite unconnected with alloys, gave an account of
his experiments on salt solutions and attached water.   He was led to
undertake this work by a consideration of a paper by Dr. J. Rea, the
Arctic explorer, on the comparative saltness of freshly formed and of
older ice floes.   Guthrie showed that the freezing-point of solutions was
continuously diminished as the percentage of common salt increased,
and that this lowering increased up to 23.6 per cent. of salt, when the
solution solidified as a whole at about 22° C.   He further showed,
and this is of great importance, that the substance which first separated
from solutions more dilute than 23.6 was pure ice.   To the substance
which froze as a whole, giving crystals of the same composition as the
mother liquor, he gave the name cryohydrate.   At the time he thought
that the cryohydrate of salt containing 23.6 per cent. NaCl and 76.4 per
cent. of water was a chemical compound $2NaCl.21H_2O$.   In suc-
ceeding years he showed that a large number of other salts gave solu-
tions which behaved in a similar manner to common salt.   He
abandoned the idea that the cryohydrates were chemical compounds.

How clear his views were will be seen by quotations from his

paper in the *Phil. Mag.* (5) I. and II., 1876, in which he states: (i.) When a solution weaker than the cryohydrate loses heat, ice is formed. (ii.) Ice continues to form and the temperature to fall until the cryohydrate is reached. (iii.) At the point of saturation ice and salt separate simultaneously and the solid and liquid portions are identical in composition.

These results can be expressed in the form of a simple diagram as shown in the slide.

In a subsequent paper, *Phil. Mag.* (5) 17, he extends his experiments to solvents other than water, and states that the substances which separate at the lowest temperature are neither atomic nor molecular; this lowest melting-point mixture of two bodies he names the eutectic mixture. In the same paper he details the methods of obtaining various eutectic alloys of bismuth, lead, tin, and cadmium.

We have, in these papers of Guthrie's, the first important clue to what occurs on cooling a fused mixture of metals. The researches of Sorby and Guthrie, undertaken as they were for the sake of investigating natural phenomena, are a remarkable example of how purely scientific experiment can lead to most important practical results. It is not too much to claim for these investigators the honour of being the originators of all our modern ideas of metallurgy. Although much valuable information had been accumulated, no rapid advance could be made until some general theory of solution had been developed. In 1878 Raoult first began his work on the depression of the freezing-point of solvents due to the addition of dissolved substances, and he continued, at frequent intervals, to publish the results of his experiments up to the time of his death in 1901. He established for organic solvents certain general laws: (i.) that for moderate concentrations the fall of the freezing-point is proportional to the weight of the dissolved substance present in a constant weight of solvent; (ii.) that when the falls produced in the same solvent by different dissolved substances are compared, it is found that a molecular weight of a dissolved substance produces the same fall of the freezing-point, whatever the substance is. When, however, he applied the general laws which he had established for organic solvents to aqueous solutions of inorganic acids, bases, and salts, the results obtained were hopelessly discrepant. In a paper in the *Zeit. Physikal. Chem.* for 1888 on 'Osmotic Pressure in the analogy between solutions and gases,' Van't Hoff showed that the experiments of Pfeffer on osmotic pressure could be explained on the theory that dissolved substances were, at any rate for dilute solutions, in a condition similar to that of a gas; that they obeyed the laws of Boyle, Charles, and Avogadro, and that on this assumption the depression of the freezing-point of a solvent could be calculated by means of a simple formula. He also showed that the exceptions which occurred to Raoult's laws, when applied to aqueous solutions of electrolytes, could be explained by the assumption, first made by Arrhenius, that these latter in solution are partly dissociated into their ions. The result of all this work was to establish a general theory applicable to all solutions which has been widespread in its applications. It is true that Van't Hoff's theory has been violently attacked;

but it enables us to calculate the depression of the freezing-points of a large number of solvents. To do this it is necessary to know the latent heat of fusion of the pure solvent and the absolute temperature of the freezing-point of the solution. That the numbers calculated are in very close accord with the experimental values constitutes a strong argument in favour of the theory. From this time the study of alloys began to make rapid progress. Laurie (*Chem. Soc. Jour.* 1888), by measuring the potential difference of voltaic cells composed of plates of alloy and the more negative element immersed in a solution of a salt of one of the component metals, obtained evidence of the existence of compounds such as $CuZn_3.Cu_3Sn$. In 1889 F. H. Neville and I, whilst repeating Raoult's experiments on the lowering of the freezing-point of organic solvents, thought that it was possible that the well-known fact that alloys often freeze at a lower temperature than either of their constituents might be explained in a similar way. In a preliminary note communicated to the Chemical Society on March 21, 1889, on the same evening that Professor Ramsay read his paper on the molecular weights of metals as determined by the depression of the vapour pressure, we showed that the fall produced in the freezing-point of tin by dissolving metals in it was for dilute solutions directly proportional to the concentration. We also showed that the fall produced in the freezing-point of tin by the solution of one atomic weight of metal in 100 atomic weights of tin was a constant.

G. Tannman about the same time (*Zeit. Physikal. Chemie*, III., 44, 1889) arrived at a similar conclusion, using mercury as a solvent.

These experiments helped to establish the similarity between the behaviour of metallic solutions or alloys and that of aqueous and other solutions of organic compounds in organic solvents. That our experiments were correct seemed probable from the agreement between the observed depression of the freezing-point and the value calculated from Van't Hoff's formula for the case of those few metals whose latent heats of fusion had been determined with any approach to accuracy.

Our experiments, subsequently extended to other solvents, led to the conclusion that in the case of most metals dissolved in tin the molecular weight is identical with the atomic weight; in other words, that the metals in solution are monatomic. This conclusion, however, involves certain assumptions. Prof. Ramsay's experiments on the lowering of the vapour pressure of certain amalgams point to a similar conclusion.

So far our work had been carried out with mercury thermometers, standardised against a platinum resistance pyrometer, but it was evident that, if it was to be continued, we must have some method of extending our experiments to alloys which freeze at high temperatures. The thermo couple was not at this stage a reliable instrument; fortunately, however, Callendar and Griffiths had brought to great perfection the electrical resistance pyrometer (*Phil. Trans.* A, 1887 and 1891). Dr. E. H. Griffiths kindly came to our aid, and with his help we installed a complete electrical resistance set. As at this time the freezing-points of pure substances above 300° were not known with any degree of accuracy, we began by making these measurements :—

Table of Freezing-points.

---	Carnelly's Tables	Holborn & Wien, 1892	Callendar & Griffiths, 1892	Neville & Heycock, 1895	Burgess & Le Chatelier, 1912. High Temperature Measurements
Tin	—	—	231·7	231·9	231·9
Zinc	433	—	417·6	419·0	419·4
Lead	—	—	—	327·6	327·4
Antimony	432	—	—	629·5	630·7 & 629·2
Magnesium	—	—	—	[1]632·6	650
Aluminium	700	—	—	[2]654·5	658
Silver	954	968	972	960·7	960·9
Gold	1,045	1,072	1,037	1,061·7	1,062·4
Copper	1,054	1,082	—	1,080·5	1,083
Sulphur B.P.	448	—	444·53	—	444·7

[1] Contaminated with silicon.   [2] Known to be impure.

With the exception of silver and gold, these metals were the purest obtainable in commerce.

Two facts are evident from the consideration of this table: (a) the remarkable accuracy of Callendar's formula connecting the Temperature Centigrade with the change of resistance of a pure platinum wire; (b) the accuracy of Callendar and Griffiths' determination of the boiling-point of sulphur. Although the platinum resistance pyrometer had at this time only been compared with the air thermometer up to 600° C., it will be noted that the exterpolation from 600° to nearly 1,100 was justified.

I cannot leave the subject of high-temperature measurements without referring to the specially valuable work of Burgess, and also to Eza Griffiths' book on high-temperature measurements, which contains an excellent summary of the present state of our knowledge of this important subject.

During the period that the above work on non-ferrous alloys was being done, great progress was being made in the study of iron and steel by Osmond and Le Chatelier. In 1890 the Institute of Mechanical Engineers, not apparently without considerable misgivings on the part of some of its members, formed an Alloys Research Committee. This Committee invited Professor (afterwards Sir William) Roberts-Austen to undertake research work for them. The results of his investigations are contained in a series of five valuable reports, extending from 1891 to 1899, published in the Journal of the Institute. The first report contained a description of an improved form of the Le Chatelier recording pyrometer, and the instrument has since proved a powerful weapon of research. In the second report, issued in 1893, the effects on the properties of copper of small quantities of arsenic, bismuth, and antimony were discussed. Whilst some engineers advocated, others as strongly controverted, the beneficial results of small quantities of

arsenic on the copper used for the fireboxes of locomotives. The report showed that the presence of from ·5-1 per cent. of arsenic was highly beneficial. The third report dealt with electric welding and the production of alloys of iron and aluminium. The fourth report is particularly valuable, as it contains a *résumé* of the Bakerian Lecture given by Roberts-Austen on the diffusion of metals in the solid state, in which he showed that gold, even at as low a temperature as 100°, could penetrate into lead, and that iron became carbonised at a low red heat by contact with a diamond in a vacuum. In 1899 the fifth report appeared, on the effects of the addition of carbon to iron. This report is of especial importance, because, besides a description of the thermal effects produced by carbon, which he carefully plotted and photographed, he described the microscopical appearance of the various constituents of iron. The materials of this report, together with the work of Osmond and others on steel and iron, provided much of the material on which Professor Bakhuis Roozeboom founded the iron carbon equilibrium diagram. Reference should also be made to the very valuable paper by Stansfield on the present position of the solution theory of carbonised iron (*Journ. Iron and Steel Inst.*, 11, 1900, p. 317). It may be said of this fifth report, and the two papers just referred to, that they form the most important contribution to the study of iron and steel that has ever been published. Although the diagram for the equilibrium of iron and carbon does not represent the whole of the facts, it affords the most important clue to these alloys, and undoubtedly forms the basis of most of the modern practice of steel manufacture. (Slide showing iron carbon diagram.)

Many workers, both at home and abroad, were now actively engaged in metallurgical work—Stead, Osmond, Le Chatelier, Arnold, Hadfield, Carpenter, Ewing, Rosenhain, and others too numerous to mention.

In 1897 Neville and I determined the complete freezing-point curve of the copper-tin alloys, confirming and extending the work of Roberts-Austen, Stansfield, and Le Chatelier; but the real meaning of the curve remained as much of a mystery as ever. Early in 1900 Sir G. Stokes suggested to us that we should make a microscopic examination of a few bronzes as an aid to the interpretation of the singularities of the freezing-point curve. An account of this work, which occupied us for more than two years, was published as the Bakerian Lecture of the Royal Society in February 1903. Whilst preparing a number of copper-tin alloys of known composition we were struck by the fact that the crystalline pattern which developed on the free surface of the slowly cooled alloys was entirely unlike the structure developed by polishing and etching sections cut from the interior; it therefore appeared probable that changes were going on within the alloys as they cooled. In the hope that, as Sorby had shown in the case of steel, we could stereotype or fix the change by sudden cooling, we melted small ingots of the copper-tin alloys and slowly cooled them to selected temperatures and then suddenly chilled them in water. The results of this treatment were communicated to the Royal Society and published in the Proceedings, February 1901. (Slides showing effects of chilling alloys.)

To apply this method to a selected alloy we first determined its cooling curve by means of an automatic recorder, the curve usually showing several halts or steps in it. The temperature of the highest of these steps corresponded with a point on the liquidus, *i.e.*, when solid first separated out from the molten mass. To ascertain what occurred at the subsequent halts, ingots of the melted alloy were slowly cooled to within a few degrees above and below the halt and then chilled, with the result just seen on the screen.

The method of chilling also enabled us to fix, with some degree of accuracy, the position of points on the solidus. If an alloy, chilled when it is partly solid and partly liquid, is polished and etched, it will be seen to consist of large primary combs embedded in a matrix consisting of mother liquor, in which are disseminated numerous small combs, which we called ' chilled primary.' By repeating the process at successively lower and lower temperatures we obtained a point at which the chilled primary no longer formed, *i.e.*, the upper limit of the solidus.

Although we made but few determinations of the physical properties of the alloys, it is needless to say how much they vary with the temperature and with the rapidity with which they are heated or cooled.

From a consideration of the singularities in the liquidus curve, coupled with the microscopic examination of slowly cooled and chilled alloys, we were able to divide the copper-tin alloys into certain groups having special qualities. It would take far too long to discuss these divisions. In interpreting our result we were greatly assisted not only by the application of the phase rule, but also by the application of Roozeboom's theory of solid solution (unfortunately Professor Roozeboom's letters were destroyed by fire in June 1910) and by the advice he kindly gave us. At the time the paper was published we expressly stated that we did not regard all our results as final, as much more work was required to clear up points still obscure. Other workers—Shepherd and Blough, Giolitti and Tavanti—have somewhat modified the diagram. (Slides shown.)

Neither Shepherd and Blough nor Hoyt have published the photomicrographs upon which their results are based, so that it is impossible to criticise their conclusions. Giolitti and Tavanti have published some microphotographs, from which it seems that they had not allowed sufficient time for equilibrium to be established. In this connection I must call attention to the excellent work of Haughton on the constitution of the alloys of copper and tin (*Journ. Institute of Metals*, March 1915). He investigated the alloys rich in tin, and illustrated his conclusions by singularly beautiful microphotographs, and has done much to clear up doubtful points in this region of the diagram. I have dwelt at some length on this work, for copper-tin is probably the first of the binary alloys on which an attempt had been made to determine the changes which take place in passing from one pure constituent to the other. I would again call attention to the fact that without a working theory of solution the interpretation of the results would have been impossible.

Since 1900, many complete equilibrium diagrams have been pub-

lished; amongst them may be mentioned the work of Rosenhain and Tucker on the lead-tin alloys (*Phil. Trans.*, 1908), in which they describe hitherto unsuspected changes on the lead rich side which go on when these alloys are at quite low temperatures, also the constitution of the alloys of aluminium and zinc; the work of Rosenhain and Archbutt (*Phil. Trans.*, 1911), and quite recently the excellent work of Vivian, on the alloys of tin and phosphorus, which has thrown an entirely new light on this difficult subject.

So far I have called attention to some of the difficulties encountered in the examination of binary alloys. When we come to ternary alloys the difficulties of carrying out an investigation are enormously increased, whilst with quaternary alloys they seem almost insurmountable; in the case of steels containing always six, and usually more, constituents, we can only hope to get information by purely empirical methods.

Large numbers of the elements and their compounds which originally were laboriously prepared and investigated in the laboratory and remained dormant as chemical curiosities for many years have, in the fulness of time, taken their places as important and, indeed, essential articles of commerce. Passing over the difficulties encountered by Davy in the preparation of metallic sodium and by Faraday in the production of benzene (both of which materials are manufactured in enormous quantities at the present time), I may remark that even during my own lifetime I have seen a vast number of substances transferred from the category of rare laboratory products to that which comprises materials of the utmost importance to the modern metallurgical industries. A few decades ago, aluminium, chromium, cerium, thorium, tungsten, manganese, magnesium, molybdenum, nickel, calcium and calcium carbide, carborundum, and acetylene were unknown outside the chemical laboratory of the purely scientific investigator; to-day these elements, their compounds and alloys, are amongst the most valuable of our industrial metallic products. They are essential in the manufacture of high-speed steels, of armour-plate, of filaments for the electric bulb lamp, of incandescent gas mantles, and of countless other products of modern scientific industry.

All these metallic elements and compounds were discovered, and their industrial uses foreshadowed, during the course of the purely academic research work carried out in our Universities and Colleges; all have become the materials upon which great and lucrative industries have been built up. Although the scientific worker has certainly not exhibited any cupidity in the past—although he has been content to rejoice in his own contributions to knowledge, and to see great manufacturing enterprises founded upon his work—it is clear that the obligation devolves upon those who have reaped in the world's markets the fruit of scientific discovery to provide from their harvest the financial aid without which scientific research cannot be continued.

The truth of this statement is well understood by those of our great industrial leaders who are engaged in translating the results of scientific research into technical practice. As evidence of this I may quote the magnificent donation of 210,000*l.* by the British Oil Companies towards the endowment of the School of Chemistry in the University of Cam-

bridge, the noble bequest of the late Dr. Messel, one of the most en-
lightened of our technical chemists, for defraying the cost of scientific
research, the gifts of the late Dr. Ludwig Mond towards the upkeep
and expansion of the Royal Institution, one of the strongholds of British
chemical research, and the financial support given by the Goldsmiths'
and others of the great City of London Livery Companies (initiated
largely by the late Sir Frederick Abel, Sir Frederick Bramwell, and
Mr. George Matthey), to the foundation of the Imperial College of
Science and Technology. The men who initiated these gifts have been
themselves intimately associated with developments both in science
and industry; they have understood that the field must be prepared
before the crop can be reaped. Fortunately our great chemical indus-
tries are, for the most part, controlled and administered by men fully
conversant with the mode in which technical progress and prosperity
follow upon scientific achievement; and it is my pleasant duty to record
that within the last few weeks the directors of one of our greatest
chemical-manufacturing concerns have, with the consent of their
shareholders, devoted £100,000 to research. Doubtless other chemical
industries will in due course realise what they have to gain by an ade-
quate appreciation of pure science.

If the effort now being made to establish a comprehensive scheme
for the resuscitation of chemical industry within our Empire is to
succeed, financial support on a very liberal scale must be forthcoming,
from the industry itself, for the advancement of purely scientific
research. This question has been treated recently in so able a fashion
by Lord Moulton that nothing now remains but to await the results of
his appeal for funds in aid of the advancement of pure science.

In order to prevent disappointment, and a possible reaction in the
future, in those who endow pure research, it is necessary to give a word
of warning. It must be remembered that the history of science abounds
in illustrations of discoveries, regarded at the time as trivial, which have
in after years become epoch-making.

In illustration I would cite Faraday's discovery of electro-magnetic
induction. He found that when a bar magnet was thrust into the
core of a bobbin of insulated copper wire, whose terminals were con-
nected with a galvanometer, a momentary current was produced;
whilst on withdrawing the magnet a momentary reverse current
occurred; a purely scientific experiment destined in later years to
develop into the dynamo and with it the whole electrical industry.
Another illustration may be given: Guyton de Morveau, Northmore,
Davy, Faraday and Cagniard Latour between 1800 and 1850 were
engaged in liquefying many of the gases. Hydrogen, oxygen, nitrogen,
marsh gas, carbon-monoxide, and nitric oxide, however, resisted all
efforts, until the work of Joule and Andrews gave the clue to the causes
of failure. Some thirty years later by careful application of the
theoretical considerations all the gases were liquefied. The liquefaction
of oxygen and nitrogen now forms the basis of a very large and
important industry.

Such cases can be multiplied indefinitely in all branches of
science.

Perhaps the most pressing need of the present day lies in the cultivation of a better understanding between our great masters of productive industry, the shareholders to whom they are in the first degree responsible, and our scientific workers; if, by reason of any turbidity of vision, our large manufacturing corporations fail to discern that, in their own interest, the financial support of purely scientific research should be one of their first cares, technical advance will slacken and other nations, adopting a more far-sighted policy, will forge ahead in science and technology. It should, I venture to think, be the bounden duty of everyone who has at heart the aims and objects of the British Association to preach the doctrine that in closer sympathy between all classes of productive labour, manual and intellectual, lies our only hope for the future. I cannot do better than conclude by quoting the words of Pope, one of our most characteristically British poets :

> ‘ By mutual confidence and mutual aid
> Great deeds are done and great discoveries made.’

ADDRESS

TO THE

GEOLOGICAL SECTION

BY

FRANCIS ARTHUR BATHER, M.A., D.Sc., F.R.S.,

PRESIDENT OF THE SECTION.

FOSSILS AND LIFE.

OF the many distinguished men who have preceded me in this chair only eight can be described as essentially palaeontologists; and among them few seized the occasion to expound the broader principles of their science. I propose, then, to consider the Relations of Palaeontology to the other Natural Sciences, especially the Biological, to discuss its particular contribution to biological thought, and to inquire whether its facts justify certain hypotheses frequently put forward in its name. Several of those hypotheses were presented to you in his usual masterly manner by Dr. Smith Woodward in 1909, and yet others are clearly elucidated in two Introductions to Palaeontology which we have been delighted to welcome as British products: the books by Dr. Morley Davies and Dr. H. L. Hawkins. If I subject those attractive speculations to cold analysis it is from no want of admiration or even sympathy, for in younger days I too have sported with Vitalism in the shade and been caught in the tangles of Transcendental hair.

The Differentia of Palaeontology.

Like Botany and Zoology, Palaeontology describes the external and internal form and structure of animals and plants; and on this description it bases, first, a systematic classification of its material; secondly, those broader inductions of comparative anatomy which constitute morphology, or the science of form. Arising out of these studies are the questions of relation—real or apparent kinship, lines of descent, the how and the why of evolution—the answers to which reflect their light back on our morphological and classificatory systems. By a different approach we map the geographical distribution of genera and species, thus helping to elucidate changes of land and sea, and so barring out one hypothesis of racial descent or unlocking the door to another. Again, we study collective faunas and floras, unravelling the interplay of their component animals and plants, or inferring from each assemblage the climatic and other physical agents that favoured, selected, and delimited it.

All this, it may be said, is nothing more than the Botany and Zoology of the past. True, the general absence of any soft tissues, and the obscured or fragmentary condition of those harder parts which alone are preserved, make the studies of the palaeontologist more difficult, and drive him to special methods. But the result is less complete: in short, an inferior and unattractive branch of Biology. Let us relegate it to Section C!

Certainly the relation of Palaeontology to Geology is obvious. It is a part of that general history of the Earth which is Geology. And it is an essential part even of physical geology, for without life not merely would our series of strata have lacked the coal measures, the mountain limestones, the chalks, and the siliceous earths, but the changes of land and sea would have been far other. To the scientific interpreter of Earth-history, the importance of fossils lies first in their value as date-markers; secondly, in the light which they cast on barriers and currents, on seasonal and climatic variation. Conversely, the history of life has itself been influenced by geologic change. But all this is just as true of the present inhabitants of the globe as it is of their predecessors. It does not give the *differentia* of Palaeontology.

That which above all distinguishes Palaeontology—the study of ancient creatures, from Neontology—the study of creatures now living, that which raises it above the mere description of extinct assemblages of life-forms, is the concept of Time. Not the quasi-absolute time of the clock, or rather, of the sun; not various unrelated durations; but an orderly and related succession, coextensive, in theory at least, with the whole history of life on this planet. The bearing of this obvious statement will appear from one or two simple illustrations.

Effect of the Time-concept on Principles of Classification.

Adopting the well-tried metaphor, let us imagine the tree of life buried, except for its topmost twigs, beneath a sand-dune. The neontologist sees only the unburied twigs. He recognises certain rough groupings, and constructs a classification accordingly. From various hints he may shrewdly infer that some twigs come from one branch, some from another; but the relations of the branches to the main stem are matters of speculation, and when branches have become so interlaced that their twigs have long been subjected to the same external influences, he will probably be led to incorrect conclusions. The palaeontologist then comes, shovels away the sand, and by degrees exposes the true relations of branches and twigs. His work is not yet accomplished, and probably he never will reveal the root and lower part of the tree; but already he has corrected many natural, if not inevitable, errors of the neontologist.

I could easily occupy the rest of this hour by discussing the profound changes wrought by this conception on our classification. It is not that Orders and Classes hitherto unknown have been discovered, not that some erroneous allocations have been corrected, but the whole basis of our system is being shifted. So long as we were dealing with

a horizontal section across the tree of life—that is to say, with an assemblage of approximately contemporaneous forms—or even with a number of such horizontal sections, so long were we confined to simple description. Any attempt to frame a causal connection was bound to be speculative. Certain relations of structure, as of cloven hooves with horns and with a ruminant stomach, were observed, but, as Cuvier himself insisted, the laws based on such facts were purely empirical. Huxley, then, was justified in maintaining, as he did in 1863 and for long after, that a zoological classification could be based with profit on 'purely structural considerations' alone. 'Every group in that [kind of] classification is such in virtue of certain structural characters, which are not only common to the members of that group, but distinguish it from all others; and the statement of these constitutes the definition of the group.' In such a classification the groups or categories—from species and genera up to phyla—are the expressions of an arbitrary intellectual decision. From Linnaeus downwards botanists and zoologists have sought for a classification that should be not arbitrary but natural, though what they meant by 'natural' neither Linnaeus nor his successors either could or would say. Not, that is, until the doctrine of descent was firmly established, and even now its application remains impracticable, except in those cases where sufficient proof of genetic connection has been furnished—as it has been mainly by palaeontology. In many cases we now perceive the causal connection; and we recognise that our groupings, so far as they follow the blood-red clue, are not arbitrary but tables of natural affinity.

Fresh difficulties, however, arise. Consider the branching of a tree. It is easy to distinguish the twigs and the branches each from each, but where are we to draw the line along each ascending stem? To convey the new conception of change in time we must introduce a new set of systematic categories, called grades or series, keeping our old categories of families, orders, and the like for the vertical divisions between the branches. Thus, many crinoids with pinnulate arms arose from others in which the arms were non-pinnulate. We cannot place them in an Order by themselves, because the ancestors belonged to two or three Orders. We must keep them in the same Orders as their respective ancestors, but distinguish a Grade Pinnata from a Grade Impinnata.

This sounds fairly simple, and for the larger groups so it is. But when we consider the genus, we are met with the difficulty that many of our existing genera represent grades of structure affecting a number of species, and several of those species can be traced back through previous grades. This has long been recognised, but I take a modern instance from H. F. Osborn's 'Equidae' (1918, Mem. Amer. Mus. N.H., n.s. II. 51): 'The line between such species as *Miohippus (Mesohippus) meteulophus* and *M. brachystylus* of the Leptauchenia zone and *M. (Mesohippus) intermedius* of the Protoceras zone is purely arbitrary. It is obvious that members of more than one phylum [*i.e.*, lineage] are passing from one genus into the next, and *Mesohippus meteulophus* and *M. brachystylus* may with equal consistency be referred to *Miohippus*.'

The problem is reduced to its simplest elements in the following scheme :—

a	*b*	*c*	*d*	*e*	*f*	Italics.
a	b	c	d	e	f	Lower-case Roman.
A	B	C	D	E	F	Capitals Roman.
α	β	γ	δ	ε	φ	Greek.

Our genera are equivalent to the forms of letters: Italics, Roman, Greek, and so forth. The successive species are the letters themselves. Are we to make each species a genus? Or would it not be better to confess that here, as in the case of many larger groups, our basis of classification is wrong? For the palaeontologist, at any rate, the lineage *a*, A, a, *a*, is the all-important concept. Between these forms he finds every gradation; but between *a* and *b* he perceives no connection.

In the old classification the vertical divisions either were arbitrary, or were gaps due to ignorance. We are gradually substituting a classification in which the vertical divisions are based on knowledge, and the horizontal divisions, though in some degree arbitrary, often coincide with relatively sudden or physiologically important changes of form.

This brings us to the last point of contrast. Our definitions can no longer have the rigid character emphasised by Huxley. They are no longer purely descriptive. When it devolved on me to draw up a definition of the great group Echinoderma, a definition that should include all the fossils, I found that scarcely a character given in the textbooks could certainly be predicated of every member of the group. The answer to the question, ' What is an Echinoderm?' (and you may substitute Mollusc, or Vertebrate, or what name you please) has to be of this nature: An Echinoderm is an animal descended from an ancestor possessed of such-and-such characters differentiating it from other animal forms, and it still retains the imprint of that ancestor, though modified and obscured in various ways according to the class, order, family, and genus to which it belongs. The definitions given by Professor Charles Schuchert in his classification of the Brachiopoda (1913, Eastman's ' Zittel ') represent an interesting attempt to put these principles into practice. The Family Porambonitidae, for instance, is thus defined: ' Derived (out of Syntrophiidae), progressive, semi-rostrate Pentamerids, with the deltidia and chilidia vanishing more and more in time. Spondylia and cruralia present, but the former tends to thicken and unite with the ventral valve.'

The old form of diagnosis was *per genus et differentiam*. The new form is *per proavum et modificationem*.

Even the conception of our fundamental unit, the species, is insecure owing to the discovery of gradual changes. But this is a difficulty which the palaeontologist shares with the neontologist.

Let us consider another way in which the time-concept has affected biology.

Effect of the Time-concept on Ideas of Relationship.

Etienne Geoffroy-Saint Hilaire was the first to compare the embryonic stages of certain animals with the adult stages of animals considered

inferior. Through the more precise observations of Von Baer, Louis Agassiz, and others, the idea grew until it was crystallised by the poetic imagination of Haeckel in his fundamental law of the reproduction of life—namely, that every creature tends in the course of its individual development to pass through stages similar to those passed through in the history of its race. This principle is of value if applied with the necessary safeguards. If it was ever brought into disrepute, it was owing to the reckless enthusiasm of some embryologists, who unwarrantably extended the statement to all shapes and structures observed in the developing animal, such as those evoked by special conditions of larval existence, sometimes forgetting that every conceivable ancestor must at least have been capable of earning its own livelihood. Or, again, they compared the early stages of an individual with the adult structure of its contemporaries instead of with that of its predecessors in time. Often, too, the searcher into the embryology of creatures now living was forced to study some form that really was highly specialised, such as the unstalked Crinoid *Antedon*, and he made matters worse by comparing its larvae with forms far too remote in time. Allman, for instance, thought he saw in the developing *Antedon* a Cystid stage, and so the Cystids were regarded as the ancestors of the Crinoids; but we now find that stage more closely paralleled in some Crinoids of Carboniferous and Permian age, and we realise that the Cystid structure is quite different.

Such errors were due to the ignoring of time relations or to lack of acquaintance with extinct forms, and were beautifully illustrated in those phylogenetic trees which, in the 'eighties, every dissector of a new or striking animal thought it his duty to plant at the end of his paper. The trees have withered, because they were not rooted in the past.

A similar mistake was made by the palaeontologist who, happening on a new fossil, blazoned it forth as a link between groups previously unconnected—and in too many cases unconnected still. This action, natural and even justifiable under the old purely descriptive system, became fallacious when descent was taken as the basis. In those days one heard much of generalised types, especially among the older fossils; animals were supposed to combine the features of two or three classes. This mode of thought is not quite extinct, for in the last American edition of Zittel's 'Palaeontology' *Stephanocrinus* is still spoken of as a Crinoid related to the Blastoids, if not also to the Cystids. Let it be clear that these so-called 'generalised' or 'annectant' types are not regarded by their expositors as ancestral. Of course, a genus existing at a certain period may give rise to two different genera of a succeeding period, as possibly the Devonian *Coelocrinus* evolved into *Agaricocrinus*, with concave base, and into *Dorycrinus*, with convex base, both Carboniferous genera. But, to exemplify the kind of statement here criticised, perhaps I may quote from another distinguished writer of the present century: ' The new genus is a truly annectant form uniting the Melocrinidae and the Platycrinidae.' Now the genus in question appeared, so far as we know, rather late in the Lower Carboniferous, whereas both Platycrinidae and Melocrinidae were already

established in Middle Silurian time. How is it possible that the far later form should unite these two ancient families? Even a *mésalliance* is inconceivable. In a word, to describe any such forms as ' annectant ' is not merely to misinterpret structure but to ignore time.

As bold suggestions calling for subsequent proof these speculations had their value, and they may be forgiven in the neontologist, if not in the palaeontologist, if we regard them as erratic pioneer tracks blazed through a tangled forest. As our acquaintance with fossils enlarged, the general direction became clearer, and certain paths were seen to be impossible. In 1881, addressing this Association at York, Huxley could say: ' Fifty years hence, whoever undertakes to record the progress of palaeontology will note the present time as the epoch in which the law of succession of the forms of the higher animals was determined by the observation of palaeontological facts. He will point out that, just as Steno and as Cuvier were enabled from their knowledge of the empirical laws of co-existence of the parts of animals to conclude from a part to a whole, so the knowledge of the law of succession of forms empowered their successors to conclude, from one or two terms of such a succession, to the whole series, and thus to divine the existence of forms of life, of which, perhaps, no trace remains, at epochs of inconceivable remoteness in the past.'

Descent Not a Corollary of Succession.

Note that Huxley spoke of succession, not of descent. Succession undoubtedly was recognised, but the relation between the terms of the succession was little understood, and there was no proof of descent. Let us suppose all written records to be swept away, and an attempt made to reconstruct English history from coins. We could set out our monarchs in true order, and we might suspect that the throne was hereditary; but if on that assumption we were to make James I. the son of Elizabeth—well, but that's just what palaeontologists are constantly doing. The famous diagram of the Evolution of the Horse which Huxley used in his American lectures has had to be corrected in the light of the fuller evidence recently tabulated in a handsome volume by Professor H. F. Osborn and his coadjutors. *Palaeotherium*, which Huxley regarded as a direct ancestor of the horse, is now held to be only a collateral, as the last of the Tudors were collateral ancestors of the Stuarts. The later *Anchitherium* must be eliminated from the true line as a side-branch—a Young Pretender. Sometimes an apparent succession is due to immigration of a distant relative from some other region—' The glorious House of Hanover and Protestant Succession.' It was, you will remember, by such migrations that Cuvier explained the renewal of life when a previous fauna had become extinct. He admitted succession but not descent. If he rejected special creation, he did not accept evolution.

Descent, then, is not a corollary of succession. Or, to broaden the statement, history is not the same as evolution. History is a succession of events. Evolution means that each event has sprung from the preceding one. Not that the preceding event was the active cause of its successor, but that it was a necessary condition of it. For the evolu-

tionary biologist, a species contains in itself and its environment the possibility of producing its successor. The words ' its environment ' are necessary, because a living organism cannot be conceived apart from its environment. They are important, because they exclude from the idea of organic evolution the hypothesis that all subsequent forms were implicit in the primordial protoplast alone, and were manifested either through a series of degradatións, as when Thorium by successive disintegrations transmutes itself to Lead, or through fresh developments due to the successive loss of inhibiting factors. I say ' a species contains the possibility ' rather than ' the potentiality,' because we cannot start by assuming any kind of innate power.

Huxley, then, forty years ago, claimed that palaeontologists had proved an orderly succession. To-day we claim to have proved evolution by descent. But how do we prove it? The neontologist, for all his experimental breeding, has scarcely demonstrated the transmutation of a species. The palaeontologist cannot assist at even a single birth. The evidence remains circumstantial.

Recapitulation as Proof of Descent.

Circumstantial evidence is convincing only if inexplicable on any other admissible theory. Such evidence is, I believe, afforded by palaeontological instances of Haeckel's law—*i.e.*, the recapitulation by an individual during its growth of stages attained by adults in the previous history of the race. You all know how this has been applied to the ammonites ; but any creatures with a shell or skeleton that grows by successive additions and retains the earlier stages unaltered can be studied by this method. If we take a chronological series of apparently related species or mutations, a^1, a^2, a^3, a^4, and if in a^4 we find that the growth stage immediately preceding the adult resembles the adult a^3, and that the next preceding stage resembles a^2, and so on ; if this applies *mutatis mutandis* to the other species of the series ; and if, further, the old age of each species foreshadows the adult character of its successor ; then we are entitled to infer that the relation between the species is one of descent. Mistakes are liable to occur for various reasons, which we are learning to guard against. For example, the perennial desire of youth to attain a semblance of maturity leads often to the omission of some steps in the orderly process. But this and other eccentricities affect the earlier rather than the later stages, so that it is always possible to identify the immediate ancestor, if it can be found. Here we have to remember that the ancestor may not have lived in the same locality, and that therefore a single cliff-section does not always provide a complete or simple series. An admirable example of the successful search for a father is provided by R. G. Carruthers in his paper on the evolution of *Zaphrentis delanouei* (1910, *Quart. Journ. Geol. Soc.*, lxvi., 523). Surely when we get a clear case of this kind we are entitled to use the word ' proof,' and to say that we have not merely observed the succession, but have proved the filiation.

It has, indeed, been objected to the theory of recapitulation that the stages of individual growth are an inevitable consequence of an

animal's gradual development from the embryo to the adult, and therefore prove nothing. Even now there are those who maintain that the continuity of the germ-plasm is inconsistent with any true recapitulation. Let us try to see what this means. Take any evolutionary series, and consider the germ-plasm at any early stage in it. The germ, it is claimed, contains the factors which produce the adult characters of that stage. Now proceed to the next stage of evolution. The germ has either altered or it has not. If it has not altered, the new adult characters are due to something outside the germ, to factors which may be in the environment but are not in the germ. In this case the animal must be driven by the inherited factors to reproduce the ancestral form; the modifications due to other factors will come in on the top of this, and if they come in gradually and in the later stages of growth, then there will be recapitulation. There does not seem to be any difficulty here. You may deny the term 'character' to these modifications, and you may say that they are not really inherited, that they will disappear entirely if the environment reverts to its original condition. Such language, however, does not alter the fact, and when we pass to subsequent stages of evolution and find the process repeated, and the recapitulation becoming longer, then you will be hard put to it to imagine that the new environment produces first the effects of the old and then its own particular effect.

Even if we do suppose that the successive changes in, say, an ammonite as it passes from youth to age are adaptations to successive environments, this must mean that there is a recapitulation of environment. It is an explanation of structural recapitulation, but the fact remains. There is no difficulty in supposing an individual to pass through the same succession of environments as were encountered in the past history of its race. Every common frog is an instance. The phenomenon is of the same nature as the devious route followed in their migrations by certain birds, a route only to be explained as the repetition of past history. There are, however, many cases, especially among sedentary organisms, which cannot readily be explained in this way.

Let us then examine the other alternative and suppose that every evolutionary change is due to a change in the germ—how produced we need not now inquire. Then, presumably, it is claimed that at each stage of evolution the animal will grow from the egg to the adult along a direct path. For present purposes we ignore purely larval modifications, and admit that the claim appears reasonable. The trouble is that it does not harmonise with facts. The progress from youth to age is not always a simple advance. The creature seems to go out of its way to drag in a growth-stage that is out of the straight road, and can be explained only by the fact that it is inherited from an ancestor. Thus, large ammonites of the *Xipheroceras planicosta* group, beginning smooth, pass through a ribbed stage, which may be omitted, through unituberculate and bituberculate stages, back to ribbed and smooth again. The anal plate of the larval *Antedon*, which ends its course and finally disappears above the limits of the cup, begins life in that lower position which the similar plate occupied in most of the older crinoids.

Here, then, is a difficulty. It can be overcome in two ways. A view held by many is that there are two kinds of characters: first, those that arise from changes in the germ, and appear as sudden or discontinuous variations; second, those that are due to external (*i.e.*, nongerminal) factors. It seems a corollary of this view that the external characters should so affect the germ-plasm as ultimately to produce in it the appropriate factors. This is inheritance of acquired characters. The other way out of the difficulty is to suppose that all characters other than fluctuations or temporary modifications are germinal; that changes are due solely to changes in the constitution of the germ; and that, although a new character may not manifest itself till the creature has reached old age, nevertheless it was inherent in the germ and latent through the earlier growth-stages. This second hypothesis involves two further difficulties. It is not easy to formulate a mechanism by which a change in the constitution of the germ shall produce a character of which no trace can be detected until old age sets in; such a character, for instance, as the tuberculation of the last-formed portion of an ammonite shell. Again, it is generally maintained that characters due to this change of germinal factors, however minute they may be, make a sudden appearance. They are said to be discontinuous. They act as integral units. Now the characters we are trying to explain seem to us palaeontologists to appear very gradually, both in the individual and in the race. Their beginnings are small, scarcely perceptible; they increase gradually in size or strength; and gradually they appear at earlier and earlier stages in the life-cycle. It appears least difficult to suppose that characters of this kind are not initiated in the germ, and that they, if no others, may be subject to recapitulation. It may not yet be possible to visualise the whole process by which such characters are gradually established, or to refer the phenomena of recapitulation back to more fundamental principles. But the phenomena are there, and if any hypothesis is opposed to them so much the worse for the hypothesis. However they be explained, the instances of recapitulation afford convincing proof of descent, and so of genetic evolution.

The ' Line upon Line ' Method of Palaeontology.

You will have observed that the precise methods of the modern palaeontologist, on which this proof is based, are very different from the slap-dash conclusions of forty years ago. The discovery of *Archaeopteryx*, for instance, was thought to prove the evolution of Birds from Reptiles. No doubt it rendered that conclusion extremely probable, especially if the major premiss—that evolution *was* the method of nature—were assumed. But the fact of evolution is precisely what men were then trying to prove. These jumpings from Class to Class or from Era to Era, by aid of a few isolated stepping-stones, were what Bacon calls Anticipations, ' hasty and premature ' but ' very effective, because as they are collected from a few instances, and mostly from those which are of familiar occurrence, they immediately dazzle the intellect and fill the imagination ' (*Nov. Org.* I. 28). No secure step was taken until the modern palaeontologist began to affiliate mutation with mutation and species with species, working his way back, literally

inch by inch, through a single small group of strata.   Only thus could he base on the laboriously collected facts a single true Interpretation; and to those who preferred the broad path of generality his Interpretations seemed, as Bacon says they always 'must seem, harsh and discordant—almost like mysteries of faith.'

It is impossible to read these words without thinking of one 'naturae minister et interpres,' whose genius was the first in this country to appreciate and apply to palaeontology the Novum Organon. Devoting his whole life to abstruse research, he has persevered with this method in the face of distrust and has produced a series of brilliant studies which, whatever their defects, have illuminated the problems of stratigraphy and gone far to revolutionise systematic palaeontology. Many are the workers of to-day who acknowledge a master in Sydney Savory Buckman.

I have long believed that the only safe mode of advance in palaeontology is that which Bacon counselled and Buckman has practised, namely, 'uniformly and step by step.'   Was this not indeed the principle that guided Linnaeus himself?   Not till we have linked species into lineages, can we group them into genera; not till we have unravelled the strands by which genus is connected with genus can we draw the limits of families.   Not till that has been accomplished can we see how the lines of descent diverge or converge, so as to warrant the establishment of Orders.   Thus by degrees we reject the old slippery stepping-stones that so often toppled us into the stream, and foot by foot we build a secure bridge over the waters of ignorance.

The work is slow, for the material is not always to hand, but as we build we learn fresh principles and test our current hypotheses.   To some of these I would now direct your attention.

Continuity in Development.

Let us look first at this question of continuity.   Does an evolving line change by discontinuous steps (saltations), as when a man mounts a ladder; or does it change continuously, as when a wheel rolls uphill?   The mere question of fact is extraordinarily difficult to determine. Considering the gaps in the geological record one would have expected palaeontologists to be the promulgators of the hypothesis of discontinuity.   They are its chief opponents.[1]   The advocates of discontinuity maintain that palaeontologists are misled: that the steps are so minute as to escape the observation of workers handicapped by the obscurities of their material; that many apparent characters are compound and cannot, in the case of fossils, be subjected to Mendelian

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[1] As Dr. W. D. Matthew (1910, *Pop. Sci. Monthly*, p. 473) has well exemplified by the history of the Tertiary oreodont mammals in North America, the known record, taken at its face value, leads to 'the conclusion that new species, new genera and even larger groups have appeared by saltatory evolution, not by continuous development.'   But a consideration of the general conditions controlling evolution and migration among land mammals shows him that such a conclusion is unwarranted. 'The more complete the series of specimens, the more perfect the record in successive strata, and the nearer the hypothetic centre of dispersal, the closer do we come to a phyletic series whose intergrading stages are well within the limits of observed variation of the race.'

analysis; that no palaeontologist can guarantee the genetic purity of the assemblages with which he works, even when his specimens are collected from a single locality and horizon. It is difficult to reply to such negative arguments. One can but give examples of the kind of observation on which palaeontologists rely.

Since Dr. Rowe's elaborate analysis of the species of *Micraster* occurring in the Chalk of S.E. England, much attention has been concentrated on the gradual changes undergone by those sea-urchins in the course of ages. The changes observed affect many characters; indeed, they affect the whole test, and all parts are doubtless correlated. The changes come in regularly and gradually; there is no sign of discontinuity. It is convenient to give names to the successive forms, but they are linked up by innumerable gradations. There does not seem here to be any question of the sudden appearance of a new character, in one or in many individuals; or of the introduction of any character and the gradual extension of its range by cross-breeding until it has become universal and in turn gives way to some new step in advance. The whole assemblage is affected and moves forward in line, not with an advanced scout here and a straggler there. Slight variation between contemporaneous individuals occurs, no doubt, but the limits are such that a trained collector can tell from a single fossil the level at which it has been found. The continuity of the changes is also inferred from such a fact as that in occasional specimens of *Micraster cor-bovis* the distinctness of the ambulacral sutures (which is one of these characters) is greater on one side of the test than on the other.

Such changes as these may profitably be compared with those which Professor Duerden believes to be taking place in the ostrich. He too finds a slow continuous change affecting innumerable parts of the bird, a change that is universal and within slight limits of variation as between individuals. Even on the hypothesis that every barb of every feather is represented by a factor in the germ, he finds it impossible to regard the changes as other than continuous, and he is driven to the supposition (on the hypothesis of germinal factors) that the factors themselves undergo a gradual change, which he regards as due to old age. It is interesting also that he finds an occasional lop-sided change, such as we noted in *Micraster cor-bovis*.

Whatever may be the explanation, the facts do seem to warrant the statement that evolutionary change can be, and often is, continuous. Professor De Vries has unfortunately robbed palaeontologists of the word 'mutation,' by which, following Waagen, they were accustomed to denote such change. I propose, therefore, to speak of it as 'transition.' But here the question may be posed, whether such transitions can progress indefinitely, or whether they should not be compared to those divergences from the norm of a species which we call fluctuations, because, like the waves of the sea piled up by a gale, they return to their original level when the external cause is removed. If every apparent transition in time is of the latter nature, then, when it reaches a limit comparable to that circumscribing contemporary fluctuation, there must, if progress is to persist, be some disturbance provoking

a saltation, and so giving a new centre to fluctuation and a fresh limit to the upward transition. Those who maintain such an hypothesis presumably regard transition as the response of the growing individual body to gradual change of the physical environment (somatic modification). But saltation they ascribe to a change in the composition of the germ. That change may be forced on the germ by the condition of the body, and may therefore be in harmony with the environment, and may produce a new form along that line. The new form may be obviously distinct from its predecessor, or the range of its fluctuation may overlap that of its predecessor, in which case it will be impossible to decide whether the change is one of transition or of saltation. This succession of hypotheses involves a good many difficulties; among others, the mechanism by which the germ is suddenly modified in accordance with the transition of the body remains obscure. But the facts before us seem to necessitate either perpetual transition or saltation acting in this manner. Transition, we must admit, also involves a change of the germ *pari passu* with the change of the body. Consequently the difference between the two views seems to be narrowed down to a point which, if not trivial, is at any rate minute.

The particular saltation-hypothesis which I have sketched may remind some hearers of the ' expression points ' of E. D. Cope. That really was quite a different conception. Cope believed that, in several cases, generic characters, after persisting for a long time, changed with relative rapidity. This took place when the modifications of adult structure were pushed back so far prior to the period of reproduction as to be transmitted to the offspring. The brief period of time during which this rapid change occurred in any genus was an expression-point, and was compared by Cope to the critical temperature at which a gas changes into a liquid, or a liquid into a solid. The analogy is not much more helpful than Galton's comparison of a fluctuating form to a rocking polyhedron, which one day rocks too much and topples over on to another face. It is, however, useful to note Cope's opinion that these points were ' attained without leaps, and abandoned without abruptness.' He did not believe that ' sports ' had ' any considerable influence on the course of evolution ' (1887, ' Origin of Fittest,' pp. 39, 79 ; 1896, ' Factors Org. Evolution,' pp. 24, 25).

The Direction of Change: Seriation.

The conception of connected change, whether by transition or by scarcely perceptible saltation, or by a combination of the two processes, leads us to consider the Direction of the Change.

Those who attempt to classify species now living frequently find that they may be arranged in a continuous series, in which each species differs from its neighbours by a little less or a little more; they find that the series corresponds with the geographical distribution of the species; and they find sometimes that the change affects particular genera or families or orders, and not similar assemblages subjected, apparently, to the same conditions. They infer from this that the series represents a genetic relation, that each successive species is the descendant of its preceding neighbour; and in some cases this inference

is warranted by the evidence of recapitulation, a fact which further indicates that the change arises by addition or subtraction at the end of the individual life-cycle. So far as I am aware this phenomenon, at least so far as genera are concerned, was first precisely defined by Louis Agassiz in his 'Essay on Classification,' 1857. He called it 'Serial Connection,' a term which connotes the bare statement of fact. Cope in his 'Origin of Genera,' 1869, extended the observation, in a few cases, to species, and introduced the term 'Successional Relation,' which for him implied descent. We may here use the brief and non-committal term 'Seriation.'

The comparison of the seriation of living species and genera to the seriation of a succession of extinct forms as revealed by fossils was, it seems, first definitely made by Cope, who in 1866 held the zoological regions of to-day to be related to one another 'as the different subdivisions of a geologic period in time' (*Journ. Acad. Nat. Sci. Philadelphia*, 1866, p. 108). This comparison is of great importance. Had we the seriations of living forms alone, we might often be in doubt as to the meaning of the phenomenon. In the first place we might ascribe it purely to climatic and similar environmental influence, and we should be unable to prove genetic filiation between the species. Even if descent were assumed we should not know which end of the series was ancestral, or even whether the starting point might not be near the middle. But when the palaeontologist can show the same, or even analogous, seriation in a time-succession, he indicates to the neontologist the solution of his problem.

Here it is well to remind ourselves that all seriations are not exact. There are seriations of organs or of isolated characters, and the transition has not always taken place at the same rate. Hence numerous examples of what Cope called Inexact Parallelism. The recognition of such cases is largely responsible for the multiplication of genera by some modern palaeontologists. This may or may not be the best way of expressing the facts, but it is desirable that they should be plainly expressed or we shall be unable to delineate the actual lines of genetic descent.

Restricting ourselves to series in which descent may be considered as proved or highly probable, such as the Micrasters of the Chalk, we find then a definite seriation. There is not merely transition, but transition in orderly sequence such as can be represented by a graphic curve of simple form. If there are gaps in the series as known to us, we can safely predict their discovery; and we can prolong the curve backwards or forwards, so as to reveal the nature of ancestors or descendants.

Orthogenesis: Determinate Variation.

The regular, straightforward character of such seriation led Eimer to coin the term Orthogenesis for the phenomenon as a whole. If this term be taken as purely descriptive, it serves well enough to denote certain facts. But Orthogenesis, in the minds of most people, connotes the idea of necessity, of determinate variation, and of predetermined course. Now, just as you may have succession without evolution, so you may have seriation without determination or predetermination.

Let us be clear as to the meaning of these terms.   Variation is said to be determinate, or, as Darwin called it, ' definite,' when all the offspring vary in the same direction.   Such definite variation may be determined by a change in the composition of the germ, due perhaps to some external influence acting on all the parents; or it may express the direct action of an external influence on the growing offspring.   The essential feature is that all the changes are of the same kind, though they may differ in degree.   For instance, all may consist in some addition, as a thickening of skeletal structures, an outgrowth of spines or horns; or all may consist in some loss, as the smaller size of outer digits, the diminution of tubercles, or the disappearance of feathers. A succession of such determinate variations for several generations produces seriation; and when the seriation is in a plus direction it is called progressive (anabatic, anagenetic), when in a minus direction, retrogressive (catabatic, catagenetic).   When successive additions appear late in the life-cycle, each one as it were pushing its predecessors back to earlier stages, then we use Cope's phrase—acceleration of development.   When subtraction occurs in the same way, there is retardation of development.   Now it is clear that if a single individual or generation produces offspring with, say, plus variations differing in degree, then the new generation will display seriation.   Instances of this are well known.   You may draw from them what inferences you please, but you cannot actually prove that there is progression.   Breeding-experiments under natural conditions for a long series of years would be required for such proof.   Here, again, the palaeontologist can point to the records of the process throughout centuries or millennia, and can show that there has been undoubted progression and retrogression. I do not mean to assert that the examples of progressive and retrogressive series found among fossils are necessarily due to the seriation of determinate variations; but the instances of determinate variation known among the creatures now living show the palaeontologist a method that may have helped to produce his series.   Once more the observations of neontologist and palaeontologist are mutually complementary.

Predetermination.

So much for determination : now for Predetermination.   This is a far more difficult problem, discussed when the fallen angels

> ' reasoned high
> Of providence, foreknowledge, will, and fate,
> Fixed fate, free will, foreknowledge absolute,
> And found no end in wandering mazes lost.'

—and it is likely to be discussed so long as a reasoning mind persists. For all that, it is a problem on which many palaeontologists seem to have made up their minds.   They agree (perhaps unwittingly) with Aristotle * that ' Nature produces those things which, being con-

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* φύσει γὰρ [γίνονται] ὅσα ἀπό τινος ἐν αὐτοῖς ἀρχῆς συνεχῶς κινούμενα ἀφικνεῖται εἴς τι τέλος. *Phys.* II., 199b, 15, ed. Bekker.

tinuously moved by a certain principle inherent in themselves, arrive at a certain end.' In other words, a race once started on a certain course, will persist in that course; no matter how conditions may change, no matter how hurtful to the individual its own changes may be, progressive or retrogressive, up hill and down hill, straight as a Roman road, it will go on to that appointed end. Nor is it only palaeontologists who think thus. Professor Duerden has recently written, 'The Nägelian idea that evolutionary changes have taken place as a result of some internal vitalistic force, acting altogether independently of external influences, and proceeding along definite lines, irrespective of adaptive considerations, seems to be gaining ground at the present time among biologists ' (1919, *Journ. Genetics*, viii. p. 193).

The idea is a taking one, but is it really warranted by the facts at our disposal? We have seen, I repeat, that succession does not imply evolution, and (granting evolution) I have claimed that seriation can occur without determinate variation and without predetermination. It is easy to see this in the case of inanimate objects subjected to a controlling force. The fossil-collector who passes his material through a series of sieves, picking out first the larger shells, then the smaller, and finally the microscopic foraminifera, induces a seriation in size by an action which may be compared to the selective action of successive environments. There is, in this case, predetermination imposed by an external mind; but there is no determinate variation. You may see in the museum at Leicester a series beginning with the *via strata* of the Roman occupants of Britain, and passing through all stages of the tramway up to the engineered modern railroad. The unity and apparent inevitability of the series conjures up the vision of a world-mind consciously working to a foreseen end. An occasional experiment along some other line has not been enough to obscure the general trend; indeed, the speedy scrapping of such failures only emphasises the idea of a determined plan. But closer consideration shows that the course of the development was guided simply by the laws of mechanics and economics, and by the history of discovery in other branches of science. That alone was the nature of the determination; and predetermination, there was none. From these instances we see that selection can, indeed must, produce just that evolution along definite lines which is the supposed feature of orthogenesis.

The arguments for orthogenesis are reduced to two: first, the difficulty of accounting for the incipient stages of new structures before they achieve selective value; second, the supposed cases of non-adaptive or even—as one may term it—counter-adaptive growth.

The earliest discernible stage of an entirely new character in an adaptive direction is called by H. F. Osborn a ' rectigradation ' (1907), and the term implies that the character will proceed to develop in a definite direction. As compared with changes of proportion in existing characters (' allometron,' Osborn), rectigradations are rare. Osborn applies the term to the first signs of folding of the enamel in the teeth of the horse. Another of his favourite instances is the genesis of horns in the Titanotheres, which he has summarised as follows: ' (*a*) from excessively rudimentary beginnings, *i.e.* rectigradations, which can

hardly be detected on the surface of the skull; (b) there is some pre-determining law or similarity of potential which governs their first existence, because (c) the rudiments arise independently on the same part of the skull in different phyla [i.e. lineages] at different periods of geologic time; (d) the horn rudiments evolve continuously, and they gradually change in form (i.e. allometrons); (e) they finally become the dominating characters of the skull, showing marked variations of the form in the two sexes; (f) they first appear in late or adult stages of ontogeny, but are pushed forward gradually into earlier and earlier ontogenetic stages until they appear to arise prenatally.'

Osborn says that rectigradations are a result of the principle of determination, but this does not seem necessary. In the first place, the precise distinction between an allometron and a rectigradation fades away on closer scrutiny. When the rudiment of a cusp or a horn changes its form, the change is an allometron; the first swelling is a rectigradation. But both of these are changes in the form of a pre-existing structure; there is no fundamental difference between a bone with an equable curve and one with a slight irregularity of surface. Why may not the original modification be due to the same cause as the succeeding ones? The development of a horn in mammalia is prob-ably a response to some rubbing or butting action which produces changes first in the hair and epidermis. One requires stronger evidence than has yet been adduced to suppose that in this case form precedes function. As Jaekel has insisted, skeletal formation follows the changes in the softer tissues as they respond to strains and stresses. In the evolution of the Echinoid skeleton, any new structures that appear, such as auricles for the attachment of jaw-muscles or notches for the reception of external gills, have at their inception all the character of rectigradations, but it can scarcely be doubted that they followed the growth of their correlated soft parts, and that these latter were already subject to natural selection. But we may go further: in vertebrates as in echinoderms the bony substance is interpenetrated with living matter, which renders it directly responsive to every mechanical force, and modifies it as required by deposition or resorption, so that the skeleton tends continually to a correlation of all its parts and an adapta-tion to outer needs.

The fact that similar structures are developed in the same positions in different stocks at different periods of time is paralleled in probably all classes of animals; Ammonites, Brachiopods, Polyzoa, Crinoids, Sea-urchins present familiar instances. But do we want to make any mystery of it? The words 'predisposition,' 'predetermining law,' 'similarity of potential,' 'inhibited potentiality,' and 'periodicity,' all tend to obscure the simple statement that like causes acting on like material produce like effects. When other causes operate, the result is different. Certainly such facts afford no evidence of predetermination, in the sense that the development must take place willy-nilly. Quite the contrary: they suggest that it takes place only under the influence of the necessary causes. Nor do they warrant such false analogies as 'Environment presses the button: the animal does the rest.'

The resemblance of the cuttle-fish eye to that of a vertebrate has

been explained by the assumption that both creatures are descended, *longo intervallo* no doubt, from a common stock, and that the flesh or the germ of that stock had the internal impulse to produce this kind of eye some day when conditions should be favourable. It is not explained why many other eyed animals, which must also have descended from this remote stock, have developed eyes of a different kind. Nevertheless I commend this hypothesis of Professor Bergson to the advocates of predisposition. To my mind it only shows that a philosopher may achieve distinction by a theory of evolution without a secure knowledge of biology.

When the same stock follows two quite different paths to the same goal, it is impossible to speak of a predetermined course. In the Wenlock beds is a crinoid whose stalk has become flattened and coiled, and the cirri or tendrils of the stalk are no longer set by fives all round it, but are reduced to two rows, one along each side. In one species these cirri are spaced at irregular intervals along the two sides, but as the animal grows there is a tendency for them to become more closely set. In another species, in various respects more developed, the cirri are set quite close together, and the tightly coiled stalk looks like a ribbed ammonite. Closer inspection shows that this species includes two distinct forms. In one each segment of the stalk bears but a single cirrus, first on the right, then on the left; but the segments taper off to the opposite side so that the cirri are brought close together. In the other form two cirri are borne by a single segment, but the next segment bears no cirri. These intervening segments taper to each side, so that here also the cirri are brought close together. Thus the same appearance and the same physiological effect are produced in two distinct ways. Had one of these never existed, the evolution of this curious stem would have offered as good an argument for orthogenesis as many that have been advanced. So much for similarity!

The argument for orthogenesis based on a race-history that marches to inevitable destruction, heedless of environmental factors, has always seemed to me incontrovertible, and so long as my knowledge of palaeontology was derived mainly from books I accepted this premiss as well founded. Greater familiarity with particular groups has led me to doubt whether such cases really occur, for more intensive study generally shows that characters at first regarded as indifferent or detrimental may have been adapted to some factor in the environment or some peculiar mode of life.

Professor Duerden's interesting and valuable studies of the ostrich (1919, 1920, *Journ. Genetics*) lead him to the opinion that retrogressive changes in that bird are destined to continue, and ' we may look forward,' he says, ' to the sad spectacle of a wingless, legless, and featherless ostrich if extinction does not supervene.' Were this so we might at least console ourselves with the thought that the process is a very slow one, for Dr. Andrews tells me that the feet and other known bones of a Pliocene ostrich are scarcely distinguishable from those of the present species. But, after careful examination of Dr. Duerden's arguments, I see no ground for supposing that the changes are other than adaptive. Similar changes occur in other birds of other stocks

when subjected to the requisite conditions, as the flightless birds of
diverse origin found on ocean islands, the flightless and running rails,
geese, and other races of New Zealand, the Pleistocene *Genyornis* of
the dried Lake Callabonna, which, as desert conditions came on, began
to show a reduction of the inner toe. Among mammals the legs and
feet have been modified in the same way in at least three distinct
orders or suborders, during different periods, and in widely separated
regions. Living marsupials in Australia have the feet modified accord-
ing to their mode of life, whether climbing on trees or running over
hard ground; and among the latter we find a series indicating how
the outer toes were gradually lost and the fourth digit enlarged. I
need scarcely remind you of the modifications that resulted in the
horse's hoof with its enlarged third digit, traceable during the Tertiary
Epoch throughout the Northern Hemisphere, whether in one or more
than one stock. I would, however, recall the fact that occasional
races, resuming from time to time a forest habitat, ceased to progress
along the main line. Lastly, there are those early hoofed animals
from South America, made known by Ameghino under the name
Litopterna, which underwent a parallel series of changes and attained
in *Thoatherium* from the Upper Miocene of Patagonia a one-toed foot
with elongate metacarpals essentially similar to that of the horse. In
all these cases the correlation of foot-structure with mode of life (as
also indicated by the teeth) is such that adaptation by selection has
always been regarded as the sole effective cause.

My colleague, Dr. W. D. Lang, has recently published a most
thoughtful paper on this subject (1919, *Proc. Geol. Assoc.* xxx. 102).
His profound studies on certain lineages of Cretaceous Polyzoa
(Cheilostomata) have led him to believe that the habit of secreting
calcium carbonate, when once adopted, persists in an increasing degree.
Thus in lineage after lineage the habit 'has led to a brilliant but
comparatively brief career of skeleton-building, and has doomed the
organism finally to evolve but the architecture of its tomb.' These
creatures, like all others which secrete calcium carbonate, are simply
suffering from a gouty diathesis, to which each race will eventually
succumb. Meanwhile the organism does its best to dispose of the
secretion; if usefully, so much the better; but at any rate where it
will be least in the way. Some primitive polyzoa, we are told, often
sealed themselves up; others escaped this self-immurement by turning
the excess into spines, which in yet other forms fused into a front
wall. But the most successful architects were overwhelmed at last
by superabundance of building-material.

While sympathetic to Dr. Lang's diagnosis of the disease (for in
1888 I hazarded the view that in Cephalopoda lime-deposition was
uncontrollable by the animal, and that its extent was inversely relative
to the rate of formation of chitin or other calcifiable tissue), still I
think he goes too far in postulating an 'insistent tendency.' He
speaks of living matter as if it were the over-pumped inner-tube of
a bicycle tyre, 'tense with potentiality, curbed by inhibitions' [of
the cover] and 'periodically breaking out as inhibitions are removed'
[by broken glass]. A race acquires the lime habit or the drink habit,

and, casting off all restraint, rushes with accelerated velocity down the easy slope to perdition.

A melancholy picture! But is it true? The facts in the case of the Cretaceous Polyzoa are not disputed, but they can be interpreted as a reaction of the organism to the continued abundance of lime-salts in the sea-water. If a race became choked off with lime, this perhaps was because it could not keep pace with its environment. Instead of 'irresistible momentum' from within, we may speak of irresistible pressure from without. Dr. Lang has told us (1919, *Phil. Trans.* B. ccix.) 'that in their evolution the individual characters in a lineage are largely independent of one another.' It is this independence, manifested in differing trends and differing rates of change, that originates genera and species. Did the evolution follow some inner impulse, along lines 'predetermined and limited by innate causes,' one would expect greater similarity, if not identity, of pattern and of tempo.

Many are the races which, seeking only ornament, have (say our historians) perished like Tarpeia beneath the weight of a less welcome gift: oysters, ammonites, hippurites, crinoids, and corals. But I see no reason to suppose that these creatures were ill-adapted to their environment—until the situation changed. This is but a special case of increase in size. In creatures of the land probably, and in creatures of the water certainly (as exemplified by A. D. Mead's experiments on the starfish, 1900, *Amer. Natural.* xxxiv. 17), size depends on the amount of food, including all body- and skeleton-building constituents. When food is plentiful larger animals have an advantage over small. Thus by natural selection the race increases in size until a balance is reached. Then a fall in the food-supply handicaps the larger creatures, which may become extinct. So simple an explanation renders it quite unnecessary to regard size as in itself indicating the old age of the race.

Among the structures that have been most frequently assigned to some blind growth-force are spines or horns, and when they assume a grotesque form or disproportionate size they are dismissed as evidences of senility. Let us take a case.

The Trilobite family Lichadidae is represented in Ordovician and Silurian rocks by species with no or few spines, but in the early part of the Devonian, both in America and in Europe, various unrelated groups in this family begin to grow similarly formed and situated spines, at first short and straight, but soon becoming long curved horns, until the climax is reached in such a troll-like goat-form as *Ceratarges armatus* of the Calceola-beds in the Eifel.

Dr. J. M. Clarke (1913, *Monogr. Serv. Geol. Brazil*, i. p. 142) is among those who have regarded this parallel development as a sign of orthogenesis in the most mystical meaning of the term. Strange though these little monsters may be, I cannot, in view of their considerable abundance, believe that their specialisation was of no use to them, and I am prepared to accept the following interpretation by Dr. Rudolf Richter (1919, 1920).

Such spines have their first origin in the tubercles which form so

common an ornament in Crustacea and other Arthropods and which serve to stiffen the carapace. A very slight projection of any of these tubercles further acts as a protection against such soft-bodied enemies as jelly-fish. Longer out-growths enlarge the body of the trilobite in such a way as to prevent its being easily swallowed. When, as is often the case, the spines stretch over such organs as the eyes, their protective function is obvious. This becomes still more clear when we consider the relation of these spines to the body when rolled up, for then they are seen to form an encircling or enveloping *chevaux-de-frise*. But besides this, the spines in many cases serve as balancers; they throw the centre of gravity back from the weighty head, and thus enable the creature to rise into a swimming posture. Further, by their friction, they help to keep the animal suspended in still water with a comparatively slight motion of its numerous oar-like limbs. Regarded in this light, even the most extravagant spines lose their mystery and appear as consequences of natural selection. A comparison of the curious *Marrella* in the pelagic or still-water fauna of the Middle Cambrian Burgess shale with *Acidaspis radiata* of the Calceola-beds certainly suggests that both of these forms were adapted to a similar life in a similar environment.

The fact that many extreme developments are followed by the extinction of the race is due to the difficulty that any specialised organism or machine finds in adapting itself to new conditions. A highly specialised creature is one adapted to quite peculiar circumstances; very slight external change may put it out of harmony, especially if the change be sudden. It is not necessary to imagine any decline of vital force or exhaustion of potentiality.

When people talk of certain creatures, living or extinct, as obviously unadapted for the struggle of life, I am reminded of Sir Henry de la Beche's drawing of a lecture on the human skull by Professor Ichthyosaurus. ' You will at once perceive,' said the lecturer, ' that the skull before us belonged to one of the lower orders of animals; the teeth are very insignificant, the power of the jaws trifling; and altogether it seems wonderful how the creature could have procured food.'

What, then, is the meaning of ' momentum ' in evolution? Simply this, that change, whatever its cause, must be a change of something that already exists. The changes in evolving lineages are, as a rule, orderly and continuous (to avoid argument the term may for the moment include minute saltations). Environment changes slowly and the response of the organism always lags behind it, taking small heed of ephemeral variations.[2] Suppose a change from shallow to deep water

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[2] The conception of lag in evolution is of some importance. On a hypothesis of selection from fortuitous variations the lag must be considerable. If the variations be determinate and in the direction of the environmental change, the lag will be reduced; but according as the determination departs from the environmental change, the lag will increase. If a change of environment acts on the germ, inducing either greater variation or variation in harmony with the change, there will still be lag, but it will be less. On this hypothesis the lag will depend on the mechanism through which the environment affects the germ. If, with Osborn, we imagine an action on the body, transmitted to the various

—either by sinking of the sea-floor or by migration of the organism. Creatures already capable of becoming acclimatised will be the majority of survivors, and among them those which change most rapidly will soon dominate. Place your successive forms in order, and you will get the appearance of momentum; but the reality is inertia yielding with more or less rapidity to an outer force.

Sometimes a change is exhibited to a greater or less extent by every member of some limited group of animals, and this change may seem to be correlated with the conditions of life in only a few of the genera or species, while in others it manifests no adaptive character and no selective value. Thus the loss of the toes or even limbs in certain lizards is ascribed by Dr. G. A. Boulenger to an internal tendency, although, at any rate in the Skinks, which furnish examples of all stages of loss, it certainly seems connected with a sand-loving and burrowing life. Recently Dr. Boulenger (1920, *Bull. Soc. Zool. France*, xlv. 68) has put forward the East African *Testudo loveridgei*, a ribless tortoise with soft shell that squeezes into holes under rocks, and swells again like an egg in a bottle, as the final stage of a regressive series. The early stages of this regression, such as a decrease in size of the vertebral processes and rib-heads, were long since noticed by him in other members of the same family; but, since they did not occur in other families, and since he could perceive no adaptive value in them, he regarded them as inexplicable, until this latest discovery proved them to be prophetic of a predestined goal. The slightness of my acquaintance with tortoises forbids me to controvert this supreme example of teleology as it appears to so distinguished an authority. But in all these apparent instances we should do well to realise that we are still incompletely informed about the daily life of these creatures and of their ancestors in all stages of growth, and we may remember that structures once adaptive often persist after the need has passed or has been replaced by one acting in a different direction.

The Study of Adaptive Form.

This leads us on to consider a fruitful field of research, which would at first seem the natural preserve of neontologists, but which, as it happens, has of late been cultivated mainly to supply the needs of palaeontology. That field is the influence of the mode of life on the shape of the creature, or briefly, of function on form; and, conversely, the indications that form can give as to habits and habitat. For many a long year the relatively simple mechanics of the vertebrate skeleton have been studied by palaeontologists and anatomists generally, and have been brought into discussions on the effect of use. The investigation of the mechanical conditions controlling the growth of organisms has recently been raised to a higher plane by Professor D'Arcy Thompson's

parts through catalysers and hormones, then the process will involve lag varying with the physico-chemical constitution of the organism. Slight differences in this respect between different races may have some bearing on the rate of change (*vide infra* ' The Tempo of Evolution '), on the correlation of characters, and so on the diversity of form.

suggestive book on 'Growth and Form.' These studies, however, have usually considered the structure of an animal as an isolated machine. We have to realise that an organism should be studied in relation to the whole of its environment, and here form comes in as distinct from structure. That mode of expression, though loose and purely relative, will be generally understood. By 'form' one means those adaptations to the surrounding medium, to food, to the mode of motion, and so forth, which may vary with outer conditions while the fundamental structure persists. Though all structures may, conceivably, have originated as such adaptations, those which we call 'form' are, as a rule, of later origin. Similar adaptive forms are found in organisms of diverse structure, and produce those similarities which we know as 'convergence.' To take but one simple instance from the relations of organisms to gravity. A stalked echinoderm naturally grows upright, like a flower, with radiate symmetry. But in the late Ordovician and in Silurian rocks are many in which the body has a curiously flattened leaf-like shape, in which the two faces are distinct, but the two sides alike, and in which this effect is often enhanced by paired outgrowths corresponding in shape if not in structure. Expansion of this kind implies a position parallel to the earth's surface, *i.e.* at right angles to gravity. The leaf-like form and the balancers are adaptations to this unusual position. Recognition of this enables us to interpret the peculiar features of each genus, to separate the adaptive form from the modified structure, and to perceive that many genera outwardly similar are really of quite different origin.

Until we understand the principles governing these and other adaptations—irrespective of the systematic position of the creatures in which they appear—we cannot make adequate reconstructions of our fossils, we cannot draw correct inferences as to their mode of life, and we cannot distinguish the adaptive from the fundamental characters. No doubt many of us, whether palaeontologists or neontologists, have long recognised the truth in a general way, and have attempted to describe our material—whether in stone or in alcohol—as living creatures; and not as isolated specimens but as integral portions of a mobile world. It is, however, chiefly to Louis Dollo that we owe the suggestion and the example of approaching animals primarily from the side of the environment, and of studying adaptations as such. The analysis of adaptations in those cases where the stimulus can be recognised and correlated with its reaction (as in progression through different media or over different surfaces) affords sure ground for inferences concerning similar forms of whose life-conditions we are ignorant. Thus Othenio Abel (1916) has analysed the evidence as to the living squids and cuttle-fish and has applied it to the belemnites and allied fossils with novel and interesting results. But from such analyses there have been drawn wider conclusions pointing to further extension of the study. It was soon seen that adaptations did not come to perfection all at once, but that harmonisation was gradual, and that some species had progressed further than others. But it by no means follows that these represent chains of descent. The adaptations of all the organs must be considered, and one seriation checked by another. Thus in 1890, in sketching the probable

history of certain crinoids, I pointed out that the seriation due to the migration of the anal plates must be checked by the seriation due to the elaboration of arm-structure, and so on.

In applying these principles we are greatly helped by Dollo's thesis of the Irreversibility of Evolution. It is not necessary to regard this as an absolute Law, subject to no conceivable exception. It is a simple statement of the facts as hitherto observed, and may be expressed thus:

1. In the course of race-history an organism never returns exactly to its former state, even if placed in conditions of existence identical with those through which it has previously passed. Thus, if through adaptation to a new mode of life (as from walking to climbing) a race loses organs which were highly useful to it in the former state, then, if it ever reverts to that former mode of life (as from climbing to walking), those organs never return, but other organs are modified to take their place.

2. But (continues the Law), by virtue of the indestructibility of the past, the organism always preserves some trace of the intermediate stages. Thus, when a race reverts to its former state, there remain the traces of those modifications which its organs underwent while it was pursuing another mode of existence.

The first statement imposes a veto on any speculations as to descent that involve the reappearance of a vanished structure. It does not interfere with the cases in which old age seems to repeat the characters of youth, as in Ammonites, for here the old-age character may be similar, but obviously is not the same. The second statement furnishes a guide to the mode of life of the immediate ancestors, and is applicable to living as well as to fossil forms. It is from such persistent adaptive characters that some have inferred the arboreal nature of our own ancestors, or even of the ancestors of all mammals. To take but a single point, Dr. W. D. Matthew (1904, *Amer. Natural.* xxxviii. 813) finds traces of a former opposable thumb in several early Eocene mammals, and features dependent on this in the same digit of all mammals where it is now fixed.

The Study of Habitat.

The natural history of marine invertebrata is of particular interest to the geologist, but its study presents peculiar difficulties. The marine zoologist has long recognised that his early efforts with trawl and dredge threw little light on the depth in the sea frequented by his captures. The surface floaters, the swimmers of the middle and lower depths, and the crawlers on the bottom were confused in a single haul, and he has therefore devised means for exploring each region separately. The geologist, however, finds all these faunas mixed in a single deposit. He may even find with them the winged creatures of the air, as in the insect beds of Gurnet Bay, or the remains of estuarine and land animals.

Such mixtures are generally found in rocks that seem to have been deposited in quiet land-locked bays. Thus in a Silurian rock near Visby, Gotland, have been found creatures of such diverse habitat as a scorpion, a possibly estuarine *Pterygotus*, a large barnacle, and a crinoid of the delicate form usually associated with clear deep water.

The lagoons of Solenhofen have preserved a strange mixture of land and sea life, without a trace of fresh or brackish water forms. *Archaeopteryx*, insects, flying reptiles, and creeping reptiles represent the air and land fauna; jelly-fish and the crinoid *Saccocoma* are true openwater wanderers; sponges and stalked crinoids were sessile on the bottom; starfish, sea-urchins, and worms crawled on the sea-floor; king-crabs, lobsters, and worms left their tracks on mud-flats; cephalopods swam at various depths; fishes ranged from the bottom mud to the surface waters. The Upper Ordovician Starfish bed of Girvan contains not only the crawling and wriggling creatures from which it takes its name, but stalked echinoderms adapted to most varied modes of life, swimming and creeping trilobites, and indeed representatives of almost all marine levels.

In the study of such assemblages we have to distinguish between the places of birth, of life, of death, and of burial, since, though these may all be the same, they may also be different. The echinoderms of the Starfish bed further suggest that closer discrimination is needed between the diverse habitats of bottom forms. Some of these were, I believe, attached to sea-weed; others grew up on stalks above the bottom; others clung to shells or stones; others lay on the top of the sea-floor; others were partly buried beneath its muddy sand; others may have grovelled beneath it, connected with the overlying water by passages. Here we shall be greatly helped by the investigations of C. G. J. Petersen and his fellow-workers of the Danish Biological Station. (See especially his summary, 'The Sea Bottom and its Production of Fish Food,' Copenhagen, 1918.) They have set an example of intensive study which needs to be followed elsewhere. By bringing up slabs of the actual bottom, they have shown that, even in a small area, many diverse habitats, each with its peculiar fauna, may be found, one superimposed on the other. Thanks to Petersen and similar investigators, exact comparison can now take the place of ingenious speculation. And that this research is not merely fascinating in itself, but illuminatory of wider questions, follows from the consideration that analysis of faunas and their modes of life must be a necessary preliminary to the study of migrations and geographical distribution.

The Tempo of Evolution.

We have not yet done with the results that may flow from an analysis of adaptations. Among the many facts which, when considered from the side of animal structure alone, lead to transcendental theories with Greek names, there is the observation that the relative rate of evolution is very different in races living at the same time. Since their remains are found often side by side, it is assumed that they were subject to the same conditions, and that the differences of speed must be due to a difference of internal motive force. After what has just been said you will at once detect the fallacy in this assumption. Professor Abel has recently maintained that the varying tempo of evolution depends on the changes in outer conditions. He compares the evolution of whales, sirenians, and horses during the Tertiary Epoch, and correlates it with

the nature of the food. Roughly to summarise, he points out that from the Eocene onwards the sirenians underwent a steady, slow change, because, though they migrated from land to sea, they retained their habit of feeding on the soft water-plants. The horses, though they remained on land, display an evolution at first rather quick, then slower, but down to Pliocene times always quicker than that of the sirenians; and this is correlated with their change into eaters of grain, and their adaptation to the plains which furnish such food. The whales, like the sirenians, migrated at the beginning of the Tertiary from land to sea; but how different is their rate of evolution, and into what diverse forms have they diverged! At first they remained near the coasts, keeping to the ancestral diet, and, like the sirenians, changing but slowly. But the whales were flesh-eaters, and soon they took to hunting fish, and then to eating large and small cephalopods; hence from the Oligocene onwards the change was very quick, and in Miocene times the evolution was almost tempestuous. Finally, many whales turned to the swallowing of minute floating organisms, and from Lower Pliocene times, having apparently exhausted the possibilities of ocean provender, they changed with remarkable slowness.

Whether such changes of food or of other habits of life are, in a sense, spontaneous, or whether they are forced on the creatures by changes of climate and other conditions, makes no difference to the facts that the changes of form are a reaction to the stimuli of the outer world, and that the rate of evolution depends on those outer changes.

Whether we have to deal with similar changes of form taking place at different times or in different places, or with diverse changes affecting the same or similar stocks at the same time and place, we can see the possibility that all are adaptations to a changing environment. There is then reason for thinking that ignorance alone leads us to assume some inexplicable force urging the races this way or that, to so-called advance or to apparent degeneration, to life or to death.

The Rhythm of Life.

The comparison of the life of a lineage to that of an individual is, up to a point, true and illuminating; but when a lineage first starts on its independent course (which really means that some individuals of a pre-existing stock enter a new field), then I see no reason to predict that it will necessarily pass through periods of youth, maturity, and old age, that it will increase to an acme of numbers, of variety, or of specialisation, and then decline through a second childhood to ultimate extinction. Still less can we say that, as the individuals of a species have their allotted span of time, long or short, so the species or the lineage has its predestined term. The exceptions to those assertions are indeed recognised by the supporters of such views, and they are explained in terms of rejuvenescence, rhythmic cycles, or a grand despairing outburst before death. This phraseology is delightful as metaphor, and the conceptions have had their value in promoting search for confirmatory or contradictory evidence. But do they lead to any broad and fructifying principle? When one analyses them one is perpetually brought up against some transcendental assumption, some

unknown entelechy that starts and controls the machine, but must for ever evade the methods of our science.

The facts of recurrence, of rhythm, of rise and fall, of marvellous efflorescences, of gradual decline, or of sudden disappearances, all are incontestable. But if we accept the intimate relation of organism and environment, we shall surmise that on a planet with such a geological history as ours, with its recurrence of similar physical changes, the phenomena of life must reflect the great rhythmic waves that have uplifted the mountains and lowered the deeps, no less than every smaller wave and ripple that has from age to age diversified and enlivened the face of our restless mother.

To correlate the succession of living forms with all these changes is the task of the palaeontologist. To attempt it he will need the aid of every kind of biologist, every kind of geologist. But this attempt is not in its nature impossible, and each advance to the ultimate goal will, in the future as in the past, provide both geologist and biologist with new light on their particular problems. When the correlation shall have been completed, our geological systems and epochs will no longer be defined by gaps in our knowledge, but will be the true expression of the actual rhythm of evolution. Lyell's great postulate of the uniform action of nature is still our guide; but we have ceased to confound uniformity with monotony. We return, though with a difference, to the conceptions of Cuvier, to those numerous and relatively sudden revolutions of the surface of the globe which have produced the corresponding dynasties in its succession of inhabitants.

The Future.

The work of a systematic palaeontologist, especially of one dealing with rare and obscure fossils, often seems remote from the thought and practice of modern science. I have tried to show that it is not really so. But still it may appear to some to have no contact with the urgent problems of the world outside. That also is an error. Whether the views I have criticised or those I have supported are the correct ones is a matter of practical importance. If we are to accept the principle of predetermination, or of blind growth-force, we must accept also a check on our efforts to improve breeds, including those of man, by any other means than crossings and elimination of unfit strains. In spite of all that we may do in this way, there remain those decadent races, whether of ostriches or human beings, which ' await alike the inevitable hour.' If, on the other hand, we adopt the view that the life-history of races is a response to their environment, then it follows, no doubt, that the past history of living creatures will have been determined by conditions outside their control, it follows that the idea of human progress as a biological law ceases to be tenable; but, since man has the power of altering his environment and of adapting racial characters through conscious selection, it also follows that progress will not of necessity be followed by decadence; rather that, by aiming at a high mark, by deepening our knowledge of ourselves and of our world, and by controlling our energy and guiding our efforts in the light of that knowledge, we may prolong and hasten our ascent to ages and to heights as yet beyond prophetic vision.

ADDRESS

TO THE

ZOOLOGICAL SECTION

BY

PROFESSOR J. STANLEY GARDINER, M.A., F.R.S.,

PRESIDENT OF THE SECTION.

Where do we stand?

THE public has the right to consider and pass judgment on all that affects its civilisation and advancement, and both of these largely depend on the position and advance of science. I ask its consideration of the science of Zoology, whether or not it justifies its existence as such, and, if it does, what are its needs? It is at the parting of the ways. It either has to justify itself as a science or be altogether starved out by the new-found enthusiasm for chemistry and physics, due to the belief in their immediate application to industries.

It is a truism to point out that the recent developments in chemistry and physics depend, in the main, on the researches of men whose names are scarcely known to the public: this is equally true for all sciences. A list of past Presidents of the Royal Society conveys nothing to the public compared with a list of Captains of Industry who, to do them justice, are the first to recognise that they owe their position and wealth to these scientists. These men of science are unknown to the public, not on account of the smallness of their discoveries, but rather on account of their magnitude, which makes them meaningless to the mass.

Great as have been the results in physical sciences applied to industry, the study of animal life can claim discoveries just as great. Their greatest value, however, lies not in the production of wealth, but rather in their broad applicability to human life. Man is an animal and he is subject to the same laws as other animals. He learns by the experience of his forebears, but he learns, also, by the consideration of other animals in relationship to their fellows and to the world at large. The whole idea of evolution, for instance, is of indescribable value; it permeates all life to-day; and yet Charles Darwin, whose researches did more than any others to establish its facts, is too often only known to the public as 'the man who said we came from monkeys.'

Whilst first and foremost I would base my claim for the study of animal life on this consideration, we cannot neglect the help it has given to the physical welfare of man's body.   It is not out of place to draw attention to the manner in which pure zoological science has worked hand in hand with the science of medicine.   Harvey's experimental discovery of the circulation of the blood laid the foundation for that real knowledge of the working of the human body which is at the basis of medicine; our experience of the history of its development gives us good grounds to hope that the work that is now being carried out by numerous researchers under the term ‘experimental’ will ultimately elevate the art of diagnosis into an exact science.   Harvey's work, too, mostly on developing chicks, was the starting-point for our knowledge of human development and growth.   Instances in medicine could be multiplied wherein clinical treatment has only been rendered possible by laborious research into the life histories of certain parasites preying often on man and other animals alternately.   In this connection there seems reason at present for the belief that the great problem of medical science, cancer, will reach its solution from the zoological side.   A pure zoologist has shown that typical cancer of the stomach of the rat can be produced by a parasitic threadworm (allied to that found in pork, Trichina), this having as a carrying host the American cockroach, brought over to the large warehouses of Copenhagen in sacks of sugar.   Our attack on such parasites is only made effective by what we know of them in lower forms, which we can deal with at will.   Millions of the best of our race owe their lives to the labours of forgotten men of science, who laid the foundations of our knowledge of the generations of insects and flat-worms, the modes of life of lice and ticks, and the physiology of such lowly creatures as *Amœba* and *Paramecium*; parasitic disease—malaria, Bilharziasis, typhus, trench fever and dysentery—was as deadly a foe to us as was the Hun.

Of immense economic importance in the whole domain of domestic animals and plants was the rediscovery, early in the present century, of the completely forgotten work of Gregor Mendel on cross-breeding, made known to the present generation largely by the labours of a former President of this Association, who, true man of science, claims no credit for himself.   We see results already in the few years that have elapsed in special breeds of wheat, in which have been combined with exactitude the qualities man desires.   The results are in the making—and this is true of all things in biology—but can anyone doubt that the breeding of animals is becoming an exact science?   We have got far, perhaps, but we want to get much further in our understanding of the laws governing human heredity; we have to establish immunity to disease.   Without the purely scientific study of chromosomes (the bodies which carry the physical and mental characteristics of parents to children) we could have got nowhere, and to reach our goal we must know more of the various forces which in combination make up what we term life.

In agricultural sciences we are confronted with pests in half a dozen different groups of animals.   We have often to discover which of two or more is the damaging form, and the difficulty is greater

where the damage is due to association between plant and animal pests. Insects are, perhaps, the worst offenders, and our basal knowledge of them as living organisms—they can do no damage when dead, and perhaps pinned in our showcases—is due to Redi, Schwammerdam, and Réaumur in the middle of the seventeenth century. Our present successful honey production is founded on the curiosity of these men in respect to the origin of life and the generations of insects. The fact that most of the dominant insects have a worm (caterpillar or maggot) stage of growth, often of far longer duration than that of the insects, has made systematic descriptive work on the relation of worm and insect of peculiar importance. I hesitate, however, to refer to catalogues in which perhaps a million different forms of adults and young are described. Nowadays we know, to a large degree, with what pests we deal and we are seeking remedies. We fumigate and we spray, spending millions of money, but the next remedy is in the use of free-living enemies or parasites to prey on the insect pests. The close correlation of anatomy with function is of use here in that life histories, whether parasitic, carnivorous, vegetarian, or saprophagous, can be foretold in fly maggots from the structure of the front part of their gut (pharynx); we know whether any maggot is a pest, is harmless, or is beneficial.

I won't disappoint those who expect me to refer more deeply to science in respect to fisheries, but its operations in this field are less known to the public at large. The opening up of our north-western grounds and banks is due to the scientific curiosity of Wyville Thomson and his *confrères* as to the existence or non-existence of animal life in the deep sea. It was sheer desire for knowledge that attracted a host of inquirers to investigate the life history of river eels. The wonder of a fish living in our shallowest pools and travelling two or three thousand miles to breed, very likely on the bottom in 2,000 fathoms, and subjected to pressures varying from 14 lb. to 2 tons per square inch, is peculiarly attractive. It shows its results in regular eel farming, the catching and transplantation of the baby eels out of the Severn into suitable waters, which cannot, by the efforts of Nature alone, be sure of their regular supply. Purely scientific observations on the life histories of flat fish—these were largely stimulated by the scientific curiosity induced by the views of Lamarck and Darwin as to the causes underlying their anatomical development—and on the feeding value and nature of Thisted Bredning and the Dogger Bank, led to the successful experiments on transplantation of young plaice to these grounds and the phenomenal growth results obtained, particularly on the latter. Who can doubt that this 'movement of herds' is one of the first results to be applied in the farming of the North Sea as soon as the conservation of our fish supply becomes a question of necessity?

The abundance of mackerel is connected with the movements of Atlantic water into the British Channel and the North Sea, movements depending on complex astronomical, chemical, and physical conditions. They are further related to the food of the mackerel, smaller animal life which dwells only in these Atlantic waters. These depend, as indeed do all animals, on that living matter which possesses chlorophyll

for its nutrition and which we call plant. In this case the plants are spores of algae, diatoms, etc., and their abundance as food again depends on the amount of the light of the sun—the ultimate source, it might seem, of all life.

A method of ascertaining the age of fishes was sought purely to correlate age with growth in comparison with the growth of air-living vertebrates. This method was found in the rings of growth in the scales, and now the ascertaining of age-groups in herring shoals enables the Norwegian fishermen to know with certainty what possibilities and probabilities are before them in the forthcoming season. From the work on the blending together of Atlantic with Baltic and North Sea water off the Baltic Bight and of the subsequent movements of this Bank water, as it is termed, into the Swedish fiords can be understood, year by year, the Swedish herring fishery. It is interesting that these fisheries have been further correlated with cycles of sun spots, and also with longer cycles of lunar changes.

The mass of seemingly unproductive scientific inquiries undertaken by the United States Bureau of Fisheries, thirty to fifty years ago, was the forerunner of their immense fish-hatching operations, whereby billions of fish eggs are stripped year by year and the fresh waters of that country made into an important source for the supply of food. The study of the growth stages of lobsters and crabs has resulted in sane regulations to protect the egg-carrying females, and in some keeping up of the supply in spite of the enormously increased demand. Lastly, the study of free-swimming larval stages in mollusca, stimulated immensely by their similarity to larval stages in worms and starfishes, has given rise to the establishment of a successful pearl-shell farm at Dongonab, in the Red Sea, and of numerous fresh-water mussel fisheries in the southern rivers of the United States, to supply small shirt buttons.

Fishery investigation was not originally directed to a more ambitious end than giving a reasonable answer to a question of the wisdom or unwisdom of compulsorily restricting commercial fishing, but it was soon found that this answer could not be obtained without the aid of pure zoology. The spread of trawling—and particularly the introduction of steam trawling during the last century—gave rise to grave fears that the stock of fish in home waters might be very seriously depleted by the use of new methods. We first required to know the life histories of the various trawled fish, and Sars and others told us that the eggs of the vast majority of the European marine food species were pelagic; in other words, that they floated, and thus could not be destroyed, as had been alleged. Trawl fishing might have to be regulated all the same, for there might be an insufficient number of parents to keep up the stock. It was clearly necessary to know the habits, movements, and distribution of the fishes, for all were not, throughout their life, or at all seasons, found on the grounds it was practicable to fish. A North Sea plaice of 12 in. in length, a quite moderate size, is usually five years old. The fact that of the female plaice captured in the White Sea, a virgin ground, the vast majority are mature, while less than half the plaice put upon our

markets from certain parts of the southern North Sea in the years immediately before the war had ever spawned, is not only of great interest, but gives rise to grave fears as to the possibility of unrestricted fishing dangerously depleting the stock itself. There is, however, another group of ideas surrounding the question of getting the maximum amount of plaice-meat from the sea; it may be that the best size for catching is in reality below the smallest spawning size. I here merely emphasise that in the plaice we have an instance of an important food fish whose capture it will probably be necessary to regulate, and that in determining how best the stock may be conserved, what sizes should receive partial protection, on what grounds fish congregate and why, and in all the many cognate questions which arise, answers to either can only be given by the aid of zoological science.

But why multiply instances of the applications of zoology as a pure science to human affairs? Great results are asked for on every side of human activities. The zoologist, if he be given a chance to live and to hand on his knowledge and experience to a generation of pupils, can answer many of them. He is increasingly getting done with the collection of anatomical facts, and he is turning more and more to the why and how animals live. We may not know in our generation nor in many generations what life is, but we can know enough to control that life. The consideration of the fact that living matter and water are universally associated opens up high possibilities. The experimental reproduction of animals, without the interposition of the male, is immensely interesting; where it will lead no one can foretell. The association of growth with the acidity and alkalinity of the water is a matter of immediate practical importance, especially to fisheries. The probability of dissolved food material in sea and river water, independent of organised organic life and absorbable over the whole surfaces of animals, is clearly before us. Is it possible that that dissolved material may be even now being created in nature without the assistance of organic life? The knowledge of the existence in food of vitamines, making digestible and usable what in food would otherwise be wasted, may well result in economies of food that will for generations prevent the necessity for the artificial restriction of populations. The parallel between these vitamines and something in sea-water may quite soon apply practically to the consideration of all life in the sea. Finally, what we know of the living matter of germ cells puts before us the not impossible hope that we may influence for the better the generations yet to come.

If it is the possibility in the unknown that makes a science, are there not enough possibilities here? Does Zoology, with these problems before it, look like a decayed and worked-out science? Is it not worthy to be ranked with any other science, and is it not worthy of the highest support? Is it likely to show good value for the money spent upon it? Should we not demand for it a Professorial Chair in every University that wishes to be regarded as an educational institution? And has not the occupant of such a Chair a task at least equal in difficulty to that of the occupant of any other Chair? Surely the zoologist may reasonably claim an equal position and pay to that

of the devotee of any other science! The researcher is not a huckster and will not make this claim on his own behalf, but the occupant of this Chair may be allowed to do so for him.

So far I have devoted my attention primarily, in this survey of the position of Zoology, to the usefulness of the subject. Let us now note where we stand in respect to other subjects and in meeting the real need for wide zoological study.

All sciences are now being reviewed, and zoology has to meet month by month the increasingly powerful claim of physics and chemistry for public support. Both of these sciences are conspicuously applicable to industry, and this, perhaps, is their best claim. The consideration of life as a science would itself be in danger were it not for the economic applications of physiology to medicine. This is the danger from without, but there is another from within, and this lies in the splitting up of the subject into a series of small sections devoted to special economic ends. They are a real danger in that they are forming enclosures within a science, while research is more and more breaking down the walls between sciences. Zoology in many Universities scarcely exists, for what is assimilated by agriculturists and medical men are catalogued lists of pests, while medical students merely acquire the technique of observing dead forms of animals other than human— not the intention of the teachers, it is true, but a melancholy fact all the same. The student, I say again, is merely acquiring in ' Zoology ' a travesty of a noble subject, but to this point I return later.

Let me now give a few facts which have their sweet and bitter for us who make Zoology our life work. During the war we wanted men who had passed the Honours Schools in Zoology—and hence, were presumably capable of doing the work—to train for the diagnosis of protozoal disease. We asked for all names from 1905 to 1914 inclusive, and the average worked out at under fourteen per year from all English Universities: an average of one student per University per year. In the year 1913-14 every student who had done his Honours Course in Zoology in 1913 could, if he had taken entomology as his subject, have been absorbed into the economic applications of that subject. Trained men were wanted to undertake scientific fishery investigations and they could not be found. Posts were advertised in Animal Breeding, in Helminthology, and in Protozoology, three other economic sides of the subject. The Natural History Museum wanted systematists and there were many advertisements for teachers. How many of these posts were filled I don't know, but it is clear that not more than one-half—or even one-third—can have been filled efficiently. Can any zoologist say that all is well with his subject in the face of these deficiencies?

The demands for men in the economic sides of zoology are continually growing, and it is the business of Universities to try and meet these demands. There are Departments of Government at home and in our Colonies, which, in the interests of the people they govern, wish to put into operation protective measures but cannot do so because there are not the men with the requisite knowledge and common sense required for Inspectorates. There are others that wish for research

to develop seas, to conserve existing industries as well as to discover new ones, and they, too, are compelled to mark time.

In default, or in spite of, the efforts of the schools of pure zoology, attempts are being made to set up special training schools in fisheries, in entomology, and in other economic applications of zoology. Each branch is regarded as a science and the supporters of each suppose they can, from the commencement of a lad's scientific training, give specialised instruction in each. The researcher in each has to do the research which the economic side requires. But he can't restrict his education to one science; he requires to know the principles of all sciences; he must attempt to understand what life is. Moreover, his specialist knowledge can seldom be in one science. The economic entomologist, however deep his knowledge of insects may be, will find himself frequently at fault in distinguishing cause and effect unless he has some knowledge of mycology. The protozoologist must have an intimate knowledge of unicellular plants, bacterial and other. The animal-breeder must know the work on cross-fertilisation of plants. The fisheries man requires to understand physical oceanography. The helminthologist and the veterinary surgeon require an intimate knowledge of a rather specialised 'physiology.' All need knowledge of the comparative physiology of animals in other groups beyond those with which they deal, to assist them in their deductions and to aid them to secure the widest outlook. It is surely a mistake, while the greatest scientific minds of the day find that they require the widest knowledge, to endeavour to get great scientific results out of students whose training has been narrow and specialised. Such specialisation requires to come later, and can replace nothing. This short cut is the longest way round. The danger is not only in our science, but in every science.

In face of this highly gratifying need for trained zoologists, independently of medical schools, I ask my colleagues in the teaching of zoology, 'What is wrong with our schools of zoology that they are producing so few men of science? It is not the subject! Can it be our presentation of it, or is it merely a question of inadequate stipends?'

In science schools there can be no standing still. Progress or retrogression in thought, technique, and method are the two alternatives. If we are to progress we must see ever wider vistas of thought, and must use the achievements of our predecessors as the take-off for our own advances. The foundations of our science were well and truly laid, but we must not count the bricks for ever, but add to them. Far be it from me to decry the knowledge and ideas our predecessors have given to us. To have proved the possibility, nay, probability, that all life is one life and that life itself is permanent is an immense achievement. To have catalogued the multitudinous forms that life takes in each country was a herculean task. To have studied with meticulous care the shapes, forms, and developments of organs in so many bodies was equally herculean. It was as much as could be expected in the nineteenth century, during most of which zoology was in advance of all other sciences. But surely for these pioneer workers this docketing, tabulating, and collecting was not the object of their research, but the means to its attainment. The prize they sought was the under-

standing of life itself, the intangible mystery which makes ourselves akin to all these specimens, the common possession which gives to man, as to the lowest creature, the power of growth and reproduction.

To my colleagues I say, let us no longer, in the reconstruction immediately before us, tie ourselves down to the re-chewing of our dry bones. They are but dead bones, and the great mystery which once lived in them has passed from them, and it is that we must now seek. Not in bones, in myriads of named specimens, does that mystery dwell, but in the living being itself, in the growth and reproduction of live creatures. Observation and experiment rather than tabulation and docketing are our technique. What is that life, common to you, to me, to our domestic pets, to animals and to plants alike? Surely this is our goal, and the contents of our museums, means to this end, are in danger of being regarded as the end. There is hope now. Those of us who have the will to look can see zoology in its proper place, the colleague of botany in applying physics and chemistry to the understanding of life itself. The study of life is the oldest of all sciences; it is the science in which the child earliest takes an interest; its study has all the attributes required for education of the highest type, for the appreciation of the beauty of form and of music, of unselfishness, of self-control, of imagination, of love, and constancy. The more we know of life, the more we appreciate its wonders and the more we want to know; it is good to be alive.

Surely the time has now come for us to lift our eyes from our tables of groups and families, and, on the foundations of the knowledge of these, work on the processes going on in the living body, the adaptation to environment, the problems of heredity, and of many another fascinating hunt in unknown country. Let us teach our students not only what is known, but, still more, what is unknown, for in the pursuit of the latter we shall engage eager spirits who care nought for collections of corpses. My own conviction is that we are in danger of burying our live subject along with our specimens in museums.

We see the same evil at work in the teaching of zoology from the very beginning. Those of us who are parents know that the problems of life assail a child almost as soon as it can speak, and that it is the animal side of creation which makes the most natural and immediate appeal to its interest and curiosity. Where such interest is intelligent and constant it is safe to educate truly in the desired direction. You will notice that the child's questions are very fundamental and that, according to my experience, the facts elicited are applied widely, and with perfect simplicity. Thus my own small daughter, having elicited where the baby rabbits came from, said ' Oh! just like eggs from hens.'

The child's own desires show up best what his mind requires for its due development, and I fear no contradiction in claiming that it is animal life in all its living aspects. Yet what is he given? Schools encourage ' natural history,' as it is termed. In some it is nature; but too often it consists in a series of prizes for dates—when the first blooms of wild flowers were found; the first nests, eggs, and young of birds; the records of butterflies and moths, etc. Actual instruction, if there is any beyond this systematic teaching of destruction, frequently

lies solely in a few sheets of the life histories of the cabbage butterfly and other insects. Fossil sea urchins and shells are curiosities and are used to teach names. The whole is taught—there are some striking exceptions—with the minimum requirements of observation and intelligence. Plants too often dominate. The lad can pluck flowers and tear up roots; there is a certain cruelty to be discouraged if animals are treated similarly, but here there is none, for 'they are not alive' as we are. Which one of us would agree to this, and say that there is not a similar 'cruelty' in tearing up plants? The method is the negation of science. The boy must be taught from the other end, from the one animal about which he does know a little, viz., himself. From the commencement he must associate himself with all living matter. The child—boy or girl—shows us the way in that he is invariably keener on the domestic pets, while he has to be bribed by pennies to learn plant names.

As a result of the wrong teaching of zoology we see proposals to make so-called 'nature study' in our schools purely botanical. Is this proposal made in the interests of the teacher or the children? It surely can't be for 'decency' if the teaching is honest, for the phenomena are the same, and there is nothing 'indecent' common to all life. 'The proper study of mankind is man,' and the poor child, athirst for information about himself, is given a piece of moss or duckweed, or even a chaste buttercup. Is the child supposed to get some knowledge it can apply economically? Whatever the underlying ideas may be, this course will not best develop the mind to enable it to grapple with all phenomena, the aim of education. If necessary, the school teacher must go to school; he must bring himself up to date in his own time, as every teacher in science has to do; it is the business of Universities to help him, for nothing is more important to all science than the foundations of knowledge.

Into schools is now moving the teaching required for the first professional examination in medicine, and this profoundly affects the whole attitude of teachers. It has a syllabus approved by the Union of Medicine, the 'apprenticeship' to which is as real and as difficult to alter as that of any expert trade with its own union. It compels the remembering of a number of anatomical facts relating to a miscellaneous selection of animals and plants, and the acquirement of a certain amount of technique. However it may be taught, its examination can almost invariably be passed on memory and manual dexterity; it implies no standard of mental ability. Anatomy without function and knowledge of an organism without reference to its life is surely futile. And yet, too often, this is what our colleagues concerned with the second year of this apprenticeship directly or indirectly compel us to teach in the first year. Surely it is time for us to rebel and insist that what is required is education as to the real meaning of what life is. We shall never reach complete agreement as to a syllabus, but probably we are all at one in regarding reproduction as the most interesting biological phenomenon, and water and air as the most important environments.

Unfortunately most Universities have adopted this in many ways

unscientific and rather useless first Medical Examination as part of their first examination for the B.Sc. degree and for diplomas and degrees in agriculture, dentistry, and other subjects. Zoology is part of a syllabus in which half a dozen professors are concerned, and it cannot change with the times without great difficulty. Our colleagues of other sciences do not want it to change, preferring that a rival subject to attract pupils should remain in a backwash; to be just, each has a firm belief in the subject he knows. For our continuation courses, having choked out the more thinking students, we have to go on as we have begun, and we survey the animal kingdom in a more or less systematic manner. We carefully see that all our beasts are killed before we commence upon them; we deal solely with their comparative anatomy, to which are often added some stories of 'evolution,' the whole an attempted history of the animal kingdom. There are great educational merits in the study of the comparative anatomy of a group of similar animals, but too often we go to group after group, the student attaining all that is educational in the first, only securing from each subsequent group more and more facts which might just as well be culled from text-books.

Students who continue further and take the final honours in zoology specialise in most Universities in their last year in some branch of their science. Such students are usually thinking of the subject from the point of view of their subsequent livelihood. They have to think of what will pay and in what branches there is, in their University, some teacher from whom they can get special instruction. They read up the most modern text-book, examine a few specimens, and are often given the class they desire by examiners who know less of their speciality than they do. They are then supposed to be qualified both to teach and research in zoology. They teach on the same vicious lines, and in research many are satisfied to become mere accumulators of more facts in regard to dead creatures.

I have called this address 'Where do we stand?' and I hope all who are interested will try to answer this question. Personally I feel that we stand in a most uncomfortable position, in which, to use a colloquialism, we must either get on or get out. I am certain that the progress of humanity requires us to 'get on.'

Of you in my audience who are not workers in science I ask a final moment of consideration. There is no knowledge of which it is possible to answer the question, 'What is the use of it?' for only time can disclose what are the full bearings of any piece of knowledge. Let us not starve pure research because we do not see its immediate application. I often think that if Sir Isaac Newton, at the present day, discovered the law of gravity as a result of watching the apples fall, someone would say, 'Oh! interesting, no doubt; but my money will go to the man who can stop the maggots in them.'

On the one side leads the path of economic research, offering more obvious attractions in the way of rapid results and of greater immediate recognition. That path is one trodden by noble steps, full of sacrifice and difficulty, worthy of treading. But let us view with still greater sympathy and understanding the harder path which leads workers,

through years of seemingly unproductive toil, to strive after the solution of the great basal problems of life. Such workers forfeit for themselves the hope of wealth, leisure, and public recognition. As a rule they die in harness, and leave not much beyond honoured names. These are they who worship at the Altar of the Unknown, who at great cost wrest from the darkness its secrets, not recking of the boon they may bring to humanity. It is for these I plead, not for themselves as individuals, but for the means wherewith to keep the flame of pure research burning, for the laboratories and equipment that all Universities need.

ADDRESS

TO THE

GEOGRAPHICAL SECTION

BY

JOHN McFARLANE, M.A.,

PRESIDENT OF THE SECTION.

SINCE the last meeting of the British Association, Treaties of Peace
have been signed with Austria, Hungary, Bulgaria, and Turkey; and,
although there is still much which is unsettled, especially in the East,
it is now possible to obtain some idea of the changes wrought on the
map of Europe by the Great War. These changes are indeed of the
most profound and far-reaching description. Old States have in some
cases either disappeared or suffered an enormous loss of territory, and
new States, with the very names of which we are but vaguely familiar,
have been brought into existence. It has seemed to me, therefore,
that it might not be altogether inappropriate to inquire into the prin-
ciples upon which these territorial changes have been made, and to
consider how far the political units affected by them possess the elements
of stability. A learned but pessimistic historian to whom I confided
my intention shook his head and gravely remarked, ' Whatever you
say on that subject will be writ in water.' But the more I consider
the matter the more do I feel convinced that certain features in the
reconstructed Europe are of great and even of permanent value, and
it is in that belief that I have ventured to disregard the warning which
was given me.

In the rearrangement of European States which has taken place,
geographical conditions have perhaps not always had the consideration
which they deserve, but in an inquiry such as that upon which we are
engaged they naturally occupy the first place. And by geographical
conditions I am not thinking primarily, or even mainly, of defensive
frontiers. It may be true, as Sir Thomas Holdich implies, that they
alone form the true limits of a State. But if they do we ought to
carry our theory to its logical conclusion and crown them with barbed-
wire entanglements. Whether mankind would be happier or even safer
if placed in a series of gigantic compounds I greatly doubt. It is to the
land within the frontier, and not to the frontier itself, that our main
consideration should be given. The factors which we have to take
into account are those which enable a people to lead a common national

life, to develop the economic resources of the region within which they dwell, to communicate freely with other peoples, and to provide not only for the needs of the moment, but as far as possible for those arising out of the natural increase of the population.

The principle of self-determination has likewise played an important, if not always a well-defined, part in the rearrangement of Europe. The basis upon which the new nationalities have been constituted is on the whole ethnical, though it is true that within the main ethnical divisions advantage has been taken of the further differentiation in racial characteristics arising out of differences in geographical environment, history, language, and religion. But no more striking illustration could be adduced of the strength of ethnic relationships at the present time than the union of the Czechs with the Slovaks, or of the Serbs with the Croats and the Slovenes. Economic considerations, of course, played a great part in the settlement arrived at with Germany, but on the whole less weight has been attached to them than to ethnic conditions. In cases where they have been allowed to influence the final decision the result arrived at has not always been a happy one. Nor can more be said for those cases where the motive was political or strategic. Historical claims, which have been urged mainly by Powers anxious to obtain more than that to which they are entitled on other grounds, may be regarded as the weakest of all claims to the possession of new territory.

When we come to examine the application of the principles which I have indicated to the settlement of Europe we shall, I think, find that the promise of stability is greatest in those cases where geographical and ethnical conditions are most in harmony, and least where undue weight has been given to conditions which are neither geographical nor ethnical.

The restoration of Alsace-Lorraine to France has always been treated as a foregone conclusion in the event of a successful termination of the war against Germany. From the geographical point of view, however, there are certainly objections to the inclusion of Alsace within French territory. The true frontier of France in that region is the Vosges, not necessarily because they form the best defensive frontier, but because Alsace belongs to the Rhineland, and the possession of it brings France into a position from which trouble with Germany may arise in the future.

Nor can French claims to Alsace be justified on ethnical grounds. The population of the region contains a strong Teutonic element, as indeed does that of Northern France, and the language spoken by over 90 per cent. of the people is German. On the other hand, it must be borne in mind that during the period between the annexation of Alsace by France in the seventeenth century and its annexation by Germany in the nineteenth French policy appears to have been highly successful in winning over the sympathies of the Alsatians, just as between 1871 and 1914 German policy was no less successful in alienating them. The restoration of Alsace must therefore be defended, if at all, on the ground that its inhabitants are more attached to France than to Germany. That attachment it will be necessary for France to preserve in the future, as economic conditions are not altogether favour-

able. The cotton industry of Alsace may perhaps attach itself to that of France without great difficulty; but the agricultural produce of the Rhine plain will as before be likely to find its best and most convenient market in the industrial regions of Germany.

With regard to Lorraine the position is somewhat different. Physically that region belongs in the main to the country of the Paris basin, and is therefore in a sense part of France. Strategically it commands the routes which enter France from Germany between Belgium and the Vosges, and from that point of view its possession is of the utmost importance to her. Of the native population about one-third speak French, and the German element is mainly concentrated in the more densely populated districts of the north-east. But although in these various aspects Lorraine may be regarded as belonging to France in a sense in which Alsace does not, the real argument for the restoration of the ceded provinces is in both cases the same. Lorraine, no less than Alsace, is French in its civilisation and in its sympathies.

From the economic point of view, however, the great deposits of iron ore in Lorraine constitute its chief attraction for France to-day, just as they appear to have constituted its chief attraction for Germany half a century ago. But the transfer of the province from Germany, which has built up a great industry on the exploitation of its mines, to France, which does not possess in sufficient abundance coal for smelting purposes, together with other arrangements of a territorial or quasi-territorial nature made partly at least in consequence of this transfer, at once raises questions as to the extent to which the economic stability of Germany is threatened. The position of that country with regard to the manufacture of iron and steel will be greatly affected, for not only does she lose, in Lorraine and the Saar, regions in which these manufactures were highly developed, but she loses in them the sources from which other manufacturing regions still left to her, notably the Ruhr, drew considerable quantities either of raw materials or of semi-manufactured goods. For example, in 1913 the Ruhr, which produced over 40 per cent. of the pig iron of the German Empire, obtained 15 per cent. of its iron ore from Lorraine, and it also obtained from there and from the Saar a large amount of pig iron for the manufacture of steel. Unless, therefore, arrangements can be made for a continued supply of these materials a number of its industrial establishments will have to be closed down.

In regard to coal, the position is also serious. We need not, perhaps, be unduly impressed by the somewhat alarmist attitude of Mr. Keynes, who estimates that on the basis of the 1913 figures Germany, as she is now constituted, will require for the pre-war efficiency of her railways and industries an annual output of 110,000,000 tons, and that instead she will have in future only 100,000,000 tons, of which 40,000,000 will be mortgaged to the Allies. In arriving at these figures Mr. Keynes has made an allowance of 18,000,000 tons for decreased production, one-half of which is caused by the German miner having shortened his shift from eight and a half to seven hours per day. This is certainly a deduction which we need not take into account. Mr. Keynes also leaves out of his calculation the fact that previous to the

war about 10,000,000 tons per year were sent from Upper Silesia to other parts of Germany, and there is no reason to suppose that this amount need be greatly reduced, especially in view of Article 90 of the Treaty of Versailles, which provides that 'for a period of fifteen years Poland will permit the produce of the mines of Upper Silesia to be available for sale to purchasers in Germany on terms as favourable as are applicable to like products sold under similar conditions in Poland or in any other country.' We have further to take into account the opportunities for economy in the use of coal, the reduction in the amount which will be required for bunkers, the possibility of renewing imports from abroad—to a very limited extent indeed, but still to some extent—and the fact that the French mines are being restored more rapidly than at one time appeared possible. (On the basis of the production of the first four months of 1920 Germany could already reduce her Treaty obligations to France by 1,000,000 tons per year.) Taking all of these facts into account, it is probably correct to say that when Germany can restore the output of the mines left to her to the 1913 figure, she will, as regards her coal supply for industrial purposes, be in a position not very far removed from that in which she was in 1910, when her total consumption, apart from that at the mines, was about 100,000,000 tons.

The actual arrangements which have been made, it is true, are in some cases open to objection. The Saar is not geographically part of France, and its inhabitants are German by race, language, and sympathy. It is only in the economic necessities of the situation that a defence, though hardly a justification, of the annexation of the coalfield can be found. The coal from it is to be used in the main for the same purposes as before, whereas if it had been left to Germany much of it might have been diverted to other purposes. In 1913 the total production of Alsace-Lorraine and the Saar amounted to about 18,000,000 tons, while their consumption was about 14,000,000 tons. There is thus apparently a net gain to France of about 4,000,000 tons, but from that must be deducted the amount which the North-East of France received from this field in pre-war days. Switzerland also will probably in future continue to draw part of its supplies from the Saar.

The stipulation that Germany should for ten years pay part of her indemnities to France, Belgium, and Italy in kind also indicates an attempt to preserve the pre-war distribution of coal in Europe, though in some respects the scales seem to have been rather unfairly weighted against Germany. France, for example, requires a continuance of Westphalian coal for the metallurgical industries of Lorraine and the Saar, while Germany requires a continuance of Lorraine ore if her ironworks on the Ruhr are not to be closed down. There was therefore nothing unreasonable in the German request that she should be secured her supplies of the latter commodity. Indeed, it would have been to the advantage of both countries if a clause similar to Article 90, which I have already quoted, had been inserted in the Treaty. It is true that temporary arrangements have since been made which will ensure to Germany a considerable proportion of her pre-war consumption of minette ores. But some agreement which enabled the two separate

but complementary natural regions of the Saar and the Ruhr to exchange their surplus products on a business basis would have tended to an earlier restoration of good feeling between the two countries.

One other question which arises in this connection is the extent to which the steel industry of Germany will suffer by the loss of the regions from which she obtained the semi-manufactured products necessary for it.   On this subject it is dangerous to prophesy, but when we take into consideration the length of time required for the construction of modern steelworks, the technical skill involved in their management, and the uncertainties with regard to future supplies of fuel, it seems unlikely that France will attempt any rapid development of her steel industry.   In that case the Ruhr will still continue to be an important market for Lorraine and the Saar.

Our general conclusion, then, is that the territorial arrangements which have been made do not necessarily imperil the economic stability of Germany. The economic consequences of the war are really much more serious than the economic consequences of the peace. Germany has for ten years to make good the difference between the actual and the pre-war production of the French mines which she destroyed. Her own miners are working shorter hours, and as a result her own production is reduced, and as British miners are doing the same she is unable to import from this country. For some years these deductions will represent a loss to her of about 40,000,000 tons per annum, and will undoubtedly make her position a serious one. But to give her either the Saar or the Upper Silesian coalfields would be to enable her to pass on to others the debt which she herself has incurred. The reduction of her annual deliveries of coal to France, Belgium, and Italy was, indeed, the best way in which to show mercy to her.

The position of Poland is geographically weak, partly because its surface features are such that the land has no well-marked individuality, and partly because there are on the east and west no natural boundaries to prevent invasion or to restrain the Poles from wandering far beyond the extreme limits of their State. Polish geographers themselves appear to be conscious of this geographical infirmity, as Vidal de la Blache would have termed it, and in an interesting little work Nalkowski has endeavoured to show that the very transitionality of the land between east and west entitles it to be regarded as a geographical entity. But transitionality is rather the negation of geographical individuality than the basis on which it may be established. And indeed no one has pointed out its dangers more clearly than Nalkowski himself. ' The Polish people,' he says, ' living in this transitional country always were, and still are, a prey to a succession of dangers and struggles. They should be ever alert and courageous, remembering that on such a transitional plain, devoid of strategic frontiers, racial boundaries are marked only by the energy and civilisation of the people. If they are strong they advance those frontiers by pushing forward ; by weakening and giving way they promote their contraction. So the mainland may thrust out arms into the sea, or, being weak, may be breached and even overwhelmed by the ocean floods.' If we bear in mind the constant temptation to a people which

is strong to advance its political no less than its racial frontiers, and the constant danger to which a weakening people is exposed of finding its political frontier contract even more rapidly than its racial, we shall realise some of the evils to which a State basing its existence on transitionality is exposed.

It is, then, to racial feeling, rather than to geographical environment, that we must look for the basis of the new Polish State, but the intensity with which this feeling is likely to operate varies considerably in different parts of the region which it is proposed to include. In the plébiscite area of Upper Silesia there were, according to the census of 1900, which is believed to represent the facts more accurately than that of 1910, seven Poles to three of other nationalities. In Prussian Poland, apart from the western districts which have not been annexed to Poland and the town and district of Bromberg, the Poles number at least 75 per cent. of the total population, and in the ceded and plébiscite areas of East and West Prussia 52 per cent. Russian Poland, which contains rather more than two-thirds of the entire population of what we may call ethnic Poland, has 9,500,000 Poles and over 3,000,000 Jews, Germans, Lithuanians, and others, while West Galicia is almost solidly Polish. Thus out of a total population of 21,000,000 within the regions mentioned the Poles number 15,500,000, or about 75 per cent.

Bearing these facts in mind, it is possible to consider the potentialities of the new State. The population is sufficiently large and the Polish element within it is sufficiently strong to justify its independence on ethnical grounds. Moreover, the alien elements which it contains are united neither by racial ties nor by contiguity of settlement. In Posen, for example, there is in the part annexed to Poland a definitely Polish population with a number of isolated German settlements, while in Russian Poland the Jews are to be found mainly in the towns. Considered as a whole, Poland is at least as pure racially as the United States.

When we consider the economic resources of Poland we see that they also make for a strong and united State. It is true that in the past the country has failed to develop as an economic unit, but this is a natural result of the partitions and of the different economic systems which have prevailed in different regions. Even now, however, we can trace the growth of two belts of industrial activity which will eventually unite these different regions together. One is situated on the coalfield running from Oppeln in Silesia by Cracow and Lemberg, and is engaged in mining, agriculture, and forestry; while the other extends from Posen by Lodz to Warsaw, and has much agricultural wealth and an important textile industry. Moreover, the conditions, geographical and economic, are favourable to the growth of international trade. If Poland obtains Upper Silesia she will have more coal than she requires, and the Upper Silesian fields will, as in the past, export their surplus produce to the surrounding countries, while the manufacturing districts will continue to find their best markets in the Russian area to the east. The outlets of the State are good, for not only has it for all practical purposes

control of the port of Danzig, but it is able to share in the navigation of the Oder and it has easy access to the south by way of the Moravian Gap.

It seems obvious, therefore, that Poland can best seek compensation for the weakness of her geographical position by developing the natural resources which lie within her ethnic frontiers. By such a policy the different parts of the country will be more closely bound to one another than it is possible to bind them on a basis of racial affinity and national sentiment alone. Moreover, Poland is essentially the land of the Vistula, and whatever is done to improve navigation on that river will similarly tend to have a unifying effect upon the country as a whole. The mention of the Vistula, however, raises one point where geographical and ethnical conditions stand in marked antagonism to one another. The Poles have naturally tried to move downstream to the mouth of the river which gives their country what little geographical individuality it possesses, and the Polish corridor is the expression of that movement. On the other hand, the peoples of East and West Prussia are one and the same. The geographical reasons for giving Poland access to the sea are no doubt stronger than the historical reasons for leaving East Prussia united to the remainder of Germany, but strategically the position of the corridor is as bad as it can be, and the solution arrived at may not be accepted as final.

Lastly, we may consider the case of East Galicia, which the Poles claim not on geographical grounds, because it is in reality part of the Ukraine, and not on ethnical grounds, because the great majority of the inhabitants are Little Russians, but on the ground that they are and have for long been the ruling race in the land. It may also be that they are not uninfluenced by the fact that the region contains considerable stores of mineral oil. But as the claim of the Poles to form an independent State is based on the fact that they form a separate race, it is obviously unwise to weaken that claim by annexing a land which counts over 3,000,000 Ruthenes to one-third that number of Poles. Further, the same argument which the Poles use in regard to East Galicia could with no less reason be used by the Germans in Upper Silesia. Mr. Keynes, indeed, suggests that the Allies should declare that in their judgment economic conditions require the inclusion of the coal districts of Upper Silesia in Germany unless the wishes of the inhabitants are decidedly to the contrary. It is not improbable that East Galicia would give a more emphatic vote against Polish rule than Upper Silesia will give for it. If Poland is to ensure her position she must forget the limits of her former empire, turn her back on the Russian plain, with all the temptations which it offers, and resolutely set herself to the development of the basin of the Vistula, where alone she can find the conditions which make for strength and safety.

Czecho-Slovakia is in various ways the most interesting country in the reconstructed Europe. Both geographically and ethnically it is marked by some features of great strength, and by others which are a source of considerable weakness to it. Bohemia by its physical structure and its strategic position seems designed by Nature to be the home of a strong and homogeneous people. Moravia attaches itself

more or less naturally to it, since it belongs in part to the Bohemian massif and is in part a dependency of that massif. Slovakia is Carpathian country, with a strip of the Hungarian plain. Thus, while Bohemia possesses great geographical individuality and Slovakia is at least strategically strong, Czecho-Slovakia as a whole does not possess geographical unity and is in a sense strategically weak, since Moravia, which unites the two upland wings of the State, lies across the great route which leads from the Adriatic to the plains of Northern Europe. The country might easily, therefore, be cut in two as the result of a successful attack, either from the north or from the south. Later I shall endeavour to indicate certain compensations arising out of this diversity of geographical features, but for the moment at least they do not affect our argument.

We have, further, to note that the geographical and ethnical conditions are not altogether concordant. In Bohemia there is in the basin of the Eger in the north-west an almost homogeneous belt of German people, and on the north-eastern and south-western borderlands there are also strips of country in which the Germanic element is in a considerable majority. It is no doubt true, as Mr. Wallis has shown, that the Czechs are increasing in number more rapidly than the Germans, but on ethnical grounds alone there are undoubtedly strong reasons for detaching at least the north-western district from the Czecho-Slovak State. We feel justified in arguing, however, that here at least the governing factors are and must be geographical. To partition a country which seems predestined by its geographical features to be united and independent would give rise to an intolerable sense of injustice. I do not regard the matter either from the strategic or from the economic point of view, though both of these are no doubt important. What I have in mind is the influence which the geographical conditions of a country exercise upon the political ideas of its inhabitants. It is easy to denounce, as Mr. Toynbee does, ' the pernicious doctrine of natural frontiers,' but they will cease to appeal to the human mind only when mountain and river, highland and plain cease to appeal to the human imagination. With good sense on both sides the difficulties in this particular case are not insurmountable. The Germans of the Eger valley, which is known as German Bohemia, have never looked to Germany for leadership nor regarded it as their home, and their main desire has hitherto been to form a separate province in the Austrian Empire. A liberal measure of autonomy might convert them into patriotic citizens, and if they would but condescend to learn the Czech language they might come to play an important part in the government of the country.

In Slovakia also there are racial differences. Within the mountain area the Slovaks form the great majority of the population, but in the valleys, and on the plains of the Danube to which the valleys open out, the Magyar element predominates. Moreover, it is the Magyar element which is racially the stronger, and before which the Slovaks are gradually retiring. Geographical and ethnical conditions therefore unite in fixing the political frontier between Magyar and Slovak at the meeting place of hill and plain. But on the west such a frontier would

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have been politically inexpedient because of its length and irregularity, and economically disadvantageous because the river valleys, of which there are about a dozen, would have had no easy means of communication with one another or with the outside world. Hence the frontier was carried south to the Danube, and about 1,000,000 Magyars were included in the total population of 3,500,000. Nor is the prospect of assimilating these Magyars particularly bright. The Germans in Bohemia are cut off from the Fatherland by mountain ranges, and, as we have seen, it does not appear to present any great attraction to them. It is otherwise in Slovakia, where the Magyars of the lowland live in close touch with those of the Alfold, and it may be long ere they forget their connection with them. The danger of transferring territory not on geographical or ethnical, but on economic, grounds could not be more strikingly illustrated.

With regard to economic development, the future of the new State would appear to be well assured. Bohemia and Moravia were the most important industrial areas in the old Austrian Empire, and Slovakia, in addition to much good agricultural land, contains considerable stores of coal and iron. But if Czecho-Slovakia is to be knit together into a political and economic unit, its communications will have to be developed. We have already suggested that the geographical diversity of the country offers certain compensations for its lack of unity, but these cannot be taken advantage of until its different regions are more closely knit together than they are at present. The north of Bohemia finds its natural outlet both by rail and water through German ports. The south-east of Bohemia and Moravia look towards Vienna. In Slovakia the railways, with only one important exception, converge upon Budapest. The people appear to be alive to the necessity of remedying this state of affairs, and no fewer than fifteen new railways have been projected, which, when completed, will unite Bohemia and Moravia more closely to one another and Slovakia. Moreover, it is proposed to develop the waterways of the country by constructing a canal from the Danube at Pressburg to the Oder. From this canal another will branch off at Prerau and run to Pardubitz on the Elbe, below which point that river has still to be canalised. If these improvements are carried out the position of Czecho-Slovakia will, for an inland State, be remarkably strong. It will have through communication by water with the Black Sea, the North Sea, and the Baltic, and some of the most important land routes of the Continent already run through it. On the other hand, its access to the Adriatic is handicapped by the fact that in order to reach that sea its goods will have to pass through the territory of two, if not of three, other States, and however well the doctrine of economic rights of way may sound in theory, there are undoubted drawbacks to it in practice. Even with the best intentions, neighbouring States may fail to afford adequate means of transport, through defective organisation, trade disputes, or various other reasons. It is probable, therefore, that the development of internal communications will in the end be to the advantage of the German ports, and more especially of Hamburg. But the other outlets of the State will certainly tend towards the preservation of its economic independence.

The extent to which Rumania has improved her position as a result of the war is for the present a matter of speculation. On the one hand she has added greatly to the territory which she previously held, and superficially she has rendered it more compact; but on the other she has lost her unity of outlook, and strategically at least weakened her position by the abandonment of the Carpathians as her frontier. Again, whereas before the war she had a fairly homogeneous population—probably from 90 to 95 per cent. of the 7,250,000 people in the country being of Rumanian stock—she has, by the annexation of Transylvania, added an area of 22,000 square miles of territory, in which the Rumanians number less than one and a half out of a total of two and two-third millions. In that part of the Banat which she has obtained there is also a considerable alien element. It is in this combination of geographical division and ethnic intermixture that we may foresee a danger to Rumanian unity. That part of the State which is ethnically least Rumanian is separated from the remainder of the country by a high mountain range, and in its geographical outlook no less than in the racial sympathies of a great number of its inhabitants is turned towards the west, while pre-war Rumania remains pointed towards the south-east. Economically also there is a diversity of interest, and the historical tie is perhaps the most potent factor in binding the two regions together. It is not impossible, therefore, that two autonomous States may eventually be established, more or less closely united according to circumstances.

The position in the Dobruja is also open to criticism. Geographically the region belongs to Bulgaria, and the Danube will always be regarded as their true frontier by the Bulgarian people. Ethnically its composition is very mixed, and whatever it was originally, it certainly was not a Rumanian land. But after the Rumanians had rather unwillingly been compelled to accept it in exchange for Bessarabia, filched from them by the Russians, their numbers increased and their economic development of the region, and more especially of the port of Constanza, undoubtedly gave them some claims to the northern part of it. As so often happens, however, when a country receives part of a natural region beyond its former boundaries, Rumania is fertile in excuses for annexing more of the Dobruja. To the southern part, which she received after the Balkan wars, and in the possession of which she has been confirmed by the peace terms with Bulgaria, she has neither ethnically nor economically any manner of right. The southern Dobruja is a fertile area which, before its annexation, formed the natural hinterland of the ports of Varna and Ruschuk. Her occupation of it will inevitably draw Rumania on to further intervention in Bulgarian affairs.

The arrangements which have been made with regard to the Banat must be considered in relation to the Magyar position in the Hungarian plain. The eastern country of the Banat, Krasso-Szörény, has a population which is in the main Rumanian, and as it belongs to the Carpathian area it is rightly included with Transylvania in Rumanian territory. In the remainder of the Banat, including Arad, the Rumanians form less than one-third of the total population, which also

comprises Magyars, Germans, and Serbs. The Hungarian plain is a great natural region, capable of subdivision no doubt, but still a great natural region, in which the Magyar element is predominant. The natural limit of that plain is the mountain region which surrounds it, and to that limit at least the Magyar political power will constantly press. But Rumania has been permitted to descend from the mountains and Jugo-Slavia to cross the great river which forms her natural boundary, and both have obtained a foothold on the plain where it may be only too easy for them to seek occasion for further advances. And it cannot be urged that the principle of self-determination would have been violated by leaving the Western Banat to the Magyars. No plébiscite was taken, and it is impossible to say how the German element would have given what in the circumstances would have been the determining vote. Finally, as it was necessary to place nearly a million Magyars in Transylvania under Rumanian rule, it might not have been altogether inexpedient to leave some Rumanians on Hungarian soil.

For the extension of Jugo-Slavia beyond the Danube two pleas have been advanced, one ethnical and the other strategic. Neither is really valid. It is true that there is a Serbian area to the north of Belgrade, but the total number of Serbs within the part assigned to Jugo-Slavia probably does not much exceed 300,000. The strategic argument that the land which they occupy is necessary for the defence of the capital is equally inconclusive. From the military point of view it does not easily lend itself to defensive operations, and when we consider the political needs of the country we cannot avoid the conclusion that a much better solution might have been found in the removal of the capital to some more central position. The Danube is certainly a better defensive frontier than the somewhat arbitrary line which the Supreme Council has drawn across the Hungarian plain.

In fact, it is in the treatment of the Hungarian plain that we feel most disposed to criticise the territorial settlements of the Peace Treaties. Geographical principles have been violated by the dismemberment of a region in which the Magyars were in a majority, and in which they were steadily improving their position. Ethnical principles have been violated, both in the north, where a distinctly Magyar region has been added to Slovakia, and in the south, where the eastern Banat and Bačka have been divided between the Rumanians and the Jugo-Slavs, who together form a minority of the total population. For the transfer of Arad to Rumania and of the Burgenland to Austria more is to be said, but the position as a whole is one of unstable equilibrium, and can only be maintained by support from without. In this part of Europe at least a League of Nations will not have to seek for its troubles.

When we turn to Austria we are confronted with the great tragedy in the reconstruction of Europe. Of that country it could once be said ' Bella gerant alii, tu felix Austria nube,' but to-day, when dynastic bonds have been loosened, the constituent parts of the great but heterogeneous empire which she thus built up have each gone its own way. And for that result Austria herself is to blame. She failed to realise that an empire such as hers could only be permanently retained on a

basis of common political and economic interest. Instead of adopting such a policy, however, she exploited rather than developed the subject nationalities, and to-day their economic, no less than their political, independence of her is vital to their existence. Thus it is that the Austrian capital, which occupies a situation unrivalled in Europe, and which before the war numbered over 2,000,000 souls, finds herself with her occupation gone. For the moment Vienna is not necessary either to Austria or to the so-called Succession States, and she will not be necessary to them until she again has definite functions to perform. I do not overlook the fact that Vienna is also an industrial city, and that it, as well as various other towns in Lower Austria, are at present unable to obtain either raw materals for their industries or foodstuffs for their inhabitants. But there are already indications that this state of affairs will shortly be ameliorated by economic treaties with the neighbouring States. And what I am particularly concerned with is not the temporary but the permanent effects of the change which has taken place. The entire political re-orientation of Austria is necessary if she is to emerge successfully from her present trials, and such a re-orientation must be brought about with due regard to geographical and ethnical conditions. The two courses which are open to her lead in opposite directions. On the one hand she may become a member of a Danubian confederation, on the other she may throw in her lot with the German people. The first would really imply an attempt to restore the economic position which she held before the war, but it is questionable whether it is either possible or expedient for her to make such an attempt. A Danubian confederation will inevitably be of slow growth, as it is only under the pressure of economic necessity that it will be joined by the various nationalities of south-eastern Europe. The suggestions made by Mr. Asquith, Mr. Keynes, and others, for a compulsory free-trade union would, if carried into effect, be provocative of the most intense resentment among most, if not all, of the States concerned. But even if a Danubian confederation were established it does not follow that Austria would be able to play a part in it similar to that which she played in the Dual Monarchy. With the construction of new railways and the growth of new commercial centres it is probable that much of the trade with the south-east of Europe which formerly passed through Vienna will in future go to the east of that city. Even now Pressburg, or Bratislava, to give it the name by which it will hence be known, is rapidly developing at the expense alike of Vienna and Budapest. Finally, Austria has in the past shown little capacity to understand the Slav peoples, and in any case her position in what would primarily be a Slav confederation would be an invidious one. For these reasons we turn to the suggestion that Austria should enter the German Empire, which, both on geographical and on ethnical grounds, would appear to be her proper place. Geographically she is German, because the bulk of the territory left to her belongs either to the Alpine range or to the Alpine foreland. It is only when we reach the basin of Vienna that we leave the mid-world mountain system and look towards the south-east of Europe across the great Hungarian plain. Ethnically, of course, she is essentially

German.   Now although my argument hitherto has rather endeavoured to show that the transfer of territory from one State to another on purely economic grounds is seldom to be justified, it is equally indefensible to argue that two States which are geographically and ethnically related are not to be allowed to unite their fortunes because it would be to their interest to do so.   And that it would be to their interest there seems little doubt.   Austria would still be able to derive some of her raw materials and foodstuffs from the Succession States, and she would have, in addition, a great German area in which she would find scope for her commercial and financial activities.   Even if Naumann were but playing the part of the Tempter, who said ' All these things will I give thee if thou wilt fall down and worship me,' he undoubtedly told the truth when he said ' The whole of Germany is now more open to the Viennese crafts than ever before.   The Viennese might make an artistic conquest extending to Hamburg and Danzig.'   But not only would Austria find a market for her industrial products in Germany, she would become the great trading centre between Germany and south-east Europe, and in that way would once more be, but in a newer and better sense than before, the *Ostmark* of the German people.

The absorption of Austria in Germany is opposed by France, mainly because she cannot conceive that her great secular struggle with the people on the other side of the Rhine will ever come to an end, and she fears the addition of 6,500,000 to the population of her ancient enemy.   But quite apart from the fact that Germany and Austria cannot permanently be prevented from following a common destiny if they so desire, and apart from the fact that politically it is desirable they should do so with at least the tacit assent of the Allied Powers rather than in face of their avowed hostility, there are reasons for thinking that any danger to which France might be exposed by the additional man-power given to Germany would be more than compensated for by the altered political condition in Germany herself.   Vienna would form an effective counterpoise to Berlin, and all the more so because she is a great geographical centre, while Berlin is more or less a political creation.   The South German people have never loved the latter city, and to-day they love her less than ever.   In Vienna they would find not only a kindred civilisation with which they would be in sympathy, but a political leadership to which they would readily give heed.   In such a Germany, divided in its allegiance between Berlin and Vienna, Prussian animosity to France would be more or less neutralised.   Nor would Germany suffer disproportionately to her gain, since in the intermingling of Northern efficiency with Southern culture she would find a remedy for much of the present discontents.   When the time comes, and Austria seeks to ally herself with her kin, we hope that no impassable obstacle will be placed in her way.

The long and as yet unsettled controversy on the limits of the Italian Kingdom illustrates very well the difficulties which may arise when geographical and ethnical conditions are subordinated to considerations of military strategy, history, and sentiment in the determination of national boundaries.   The annexation of the Alto Adige has been generally accepted as inevitable.   It is true that

the population is German, but here, as in Bohemia, geographical conditions appear to speak the final word. Strategically also the frontier is good, and will do much to allay Italian anxiety with regard to the future. Hence, although ethnical conditions are to some extent ignored, the settlement which has been made will probably be a lasting one.

On the east the natural frontier of Italy obviously runs across the uplands from some point near the eastern extremity of the Carnic Alps to the Adriatic. The pre-war frontier was unsatisfactory for one reason because it assigned to Austria the essentially Italian region of the lower Isonzo. But once the lowlands are left on the west the uplands which border them on the east, whether Alpine or Karst, mark the natural limits of the Italian Kingdom, and beyond a position on them for strategic reasons the Italians have no claims in this direction except what they can establish on ethnical grounds. To these, therefore, we turn. In Carniola the Slovenes are in a large majority, and in Gorizia they also form the bulk of the population. On the other hand, in the town and district of Trieste the Italians predominate, and they also form a solid block on the west coast of Istria, though the rest of that country is peopled mainly by Slovenes. It seems to follow, therefore, that the plains of the Isonzo, the district of Trieste, and the west coast of Istria, with as much of the neighbouring upland as is necessary to secure their safety and communications, should be Italian and that the remainder should pass to the Jugo-Slavs. The so-called Wilson line, which runs from the neighbourhood of Tarvis to the mouth of the Arsa, met these requirements fairly well, though it placed from 300,000 to 400,000 Jugo-Slavs under Italian rule, to less than 50,000 Italians, half of whom are in Fiume itself transferred to the Jugo-Slavs. Any additional territory must, by incorporating a larger alien element, be a source of weakness and not of strength to Italy. To Fiume the Italians have no claim beyond the fact that in the town itself they slightly outnumber the Croats, though in the double town of Fiume-Sushak there is a large Slav majority. Beyond the sentimental reasons which they urge in public, however, there is the economic argument, which, perhaps wisely, they keep in the background. So long as Trieste and Fiume belonged to the same empire the limits within which each operated were fairly well defined, but if Fiume become Jugo-Slav it will not only prove a serious rival to Trieste, but will prevent Italy from exercising absolute control over much of the trade of Central Europe. For Trieste itself Italy has in truth little need, and the present condition of that city is eloquent testimony of the extent to which it depended for its prosperity upon the Austrian and German Empires. In the interests, then, not only of Jugo-Slavia but of Europe generally, Fiume must not become Italian, and the idea of constituting it a Free State might well be abandoned. Its development is more fully assured as the one great port of Jugo-Slavia than under any other form of government.

With regard to Italian claims in the Adriatic, little need be said. To the Dalmatian coast Italy has no right either on geographical or on

ethnical grounds, and the possession of Pola, Valona, and some of the islands gives her all the strategic advantages which she has reason to demand. But, after all, the only danger which could threaten her in the Adriatic would come from Jugo-Slavia, and her best insurance against that danger would be an agreement by which the Adriatic should be neutralised. The destruction of the Austro-Hungarian fleet offers Italy a great opportunity of which she would do well to take advantage.

Of the prospects of Jugo-Slavia it is hard to speak with any feeling of certainty. With the exception of parts of Croatia-Slavonia and of Southern Hungary, the country is from the physical point of view essentially Balkan, and diversity rather than unity is its most pronounced characteristic. From this physical diversity there naturally results a diversity in outlook which might indeed be all to the good if the different parts of the country were linked together by a well-developed system of communication. Owing to the structure of the land, however, such a system will take long to complete.

Ethnic affinity forms the real basis of union, but whether that union implies unity is another matter. It is arguable that repulsion from the various peoples—Magyars, Turks, and Austrians—by whom they have been oppressed, rather than the attraction of kinship, is the force which has brought the Jugo-Slavs together. In any case the obstacles in the way of the growth of a strong national feeling are many. Serb, Croat, and Slovene, though they are all members of the Slav family, have each their distinctions and characteristics which political differences may tend to exaggerate rather than obliterate. In Serbian Macedonia, again, out of a total population of 1,100,000, there are 400,000 to 500,000 people who, though Slavs, are Bulgarian in their sympathies, and between Serb and Bulgarian there will long be bitter enmity. Religious differences are not wanting. The Serbs belong to the Orthodox Church, but the Croats are Catholics, and in Bosnia there is a strong Mohammedan element. Cultural conditions show a wide range. The Macedonian Serb, who has but lately escaped from Turkish misrule, the untutored but independent Montenegrin, the Dalmatian, with his long traditions of Italian civilisation, the Serb of the kingdom, a sturdy fighter but without great political insight, and the Croat and Slovene, whose intellectual superiority is generally admitted, all stand on different levels in the scale of civilisation. To build up out of elements in many respects so diverse a common nationality without destroying what is best in each will be a long and laborious task. Economic conditions are not likely to be of much assistance. It is true that they are fairly uniform throughout Jugo-Slavia, and it is improbable that the economic interests of different regions will conflict to any great extent. On the other hand, since each region is more or less self-supporting, they will naturally unite into an economic whole less easily than if there had been greater diversity. What the future holds for Jugo-Slavia it is as yet impossible to say; but the country is one of great potentialities, and a long period of political rest might render possible the development of an important State.

This brings me to my conclusion. I have endeavoured to consider the great changes which have been made in Europe not in regard to

the extent to which they do or do not comply with the canons of boundary-making, for after all there are no frontiers in Europe which can in these days of modern warfare be considered as providing a sure defence, but in regard rather to the stability of the States concerned. A great experiment has been made in the new settlement of Europe, and an experiment which contains at least the germs of success. But in many ways it falls far short of perfection, and even if it were perfect it could not be permanent. The methods which ought to be adopted to render it more equable and to adapt it to changing needs it is not for us to discuss here. But as geographers engaged in the study of the ever-changing relations of man to his environment we can play an important part in the formation of that enlightened public opinion upon which alone a society of nations can be established.

ADDRESS

TO THE

SECTION OF ECONOMIC SCIENCE AND STATISTICS

BY

J. H. CLAPHAM, C.B.E., Litt.D.,

PRESIDENT OF THE SECTION.

It is, I think, a President's first duty to record the losses which economic science has sustained since the Association last met. A year ago we had just lost, on the academic side, Archdeacon Cunningham, and on the side of affairs, Sir Edward Holden. This year, happily, I have no such losses to record in either field. But it is right to name the death of a late enemy, Professor Gustav Cohn, of Göttingen, an economist of the first rank, who had made a special study of English affairs. I believe that no student of our railway history would fail to place Cohn's ' Inquiries into English Railway Policy,' published (in German) so long ago as 1873, first on the unfortunately very short list of scientific works devoted to that side of history. Even when supplemented by an additional volume, issued ten years later, it covers only what seems to-day the prehistoric period of our policy—before the Act of 1888 and very long before our present uncertainties—but it is not yet out of date. Cohn died full of years. He was nearly eighty. I may mention, perhaps, with his name that of a much younger, and possibly more brilliant, German economist, Max Weber, of Munich, who has died at the age of fifty-six. He once tried to explain, by a study of Puritan theology, the economic qualities of the Nonconformist business man—a very fascinating study. But his work as a whole has not roused much interest in England.

By an accident the three scholars whose names I have mentioned were all best known, in England at any rate, as historians. And, with your indulgence, I will do what I think has seldom been done from this chair, in making my address largely historical. History has been my main business in life; and it has occurred to me that some comparisons between the economic condition of Europe after the great wars of a century ago and its condition to-day may not be without interest. Historical situations are never reproduced, even approximately; but it is at least interesting to recall the post-war problems which our grandfathers or great-grandfathers had to face, and how they handled them; to ask how far our sufferings and anxieties have had their parallels in the not remote past; and to note some danger

signals. By ' we ' I mean not the British only, but all the peoples of Western and Central Europe. Of Eastern Europe I will only speak incidentally ; for I am unable as yet to extract truth from the conflicting and biassed evidence as to its economic condition. Moreover, there is still war in the East.

In 1815 France had been engaged in almost continuous wars for twenty-three, England for twenty-two, years. The German States had been at war less continuously ; but they had been fought over, conquered, and occupied by the French. Prussia, for instance, was overthrown in 1806. When the final struggle against Napoleon began, in 1812, there was a French army of occupation of nearly 150,000 men in Prussia alone. From 1806 to 1814 Napoleon's attempt to exclude English trade from the Continent had led to the English blockade—with its striking resemblances to, and its striking differences from, the blockade of 1914-19. Warfare was less horribly intense, and so less economically destructive, than it has become in our day ; but what it lacked in intensity it made up in duration.

Take, for instance, the loss of life. For England it was relatively small—because for us the wars were never people's wars. In France also it was relatively small in the earlier years, when armies of the old size were mainly employed. But under Napoleon it became enormous. Exact figures do not exist, but French statisticians are disposed to place the losses in the ten years that ended with Waterloo at fully 1,500,000. Some place them higher. As the population of France grew about 40 per cent. between 1805-15 and 1904-14, this would correspond to a loss of, say, 2,100,000 on the population of 1914. The actual losses in 1914-18 are put at 1,370,000 killed and missing ; and I believe these figures contain some colonial troops.

Or take the debts accumulated by victors and the requisitions or indemnities extorted from the vanquished. The wars of a century ago left the British debt at 848,000,000l. According to our success or failure in securing repayment of loans made to Dominions and Allies, the Great War will have left us with a liability of from eight to nine times that amount. Whether our debt-carrying capacity is eight or nine times what it was a century ago may be doubted, and cannot be accurately determined. But it is not, I would venture to say, less than six or seven times what it was, and it might well be more. A good deal depends on future price levels. At least the burdens are comparable ; and we understand better now where to look for broad shoulders to bear them.

After Waterloo France was called upon to pay a war indemnity of only 28,000,000l., to be divided among all the victors. With this figure Prussia was thoroughly dissatisfied. Not, I think, without some reason. She reckoned that Napoleon had squeezed out of her alone, between 1806 and 1812, more than twice as much—a tremendous exaction, for she was in those days a very poor land of squires and peasants, whose treasury received only a few millions a year. England, who was mainly responsible—and that for sound political reasons—for the low figure demanded of France, found herself, the victor, in the curious position of being far more heavily burdened with debt than France, who

had lost. England, of course, had acquired much colonial territory; but on the purely financial side the comparison between her and France was most unequal. England's total national debt in 1817 was 848,000,000*l.* France's debt did not reach 200,000,000*l.* until 1830.

The reasons why France came out of the wars so well financially were four. *First,* she had gone bankrupt during the Revolution, and had wiped out most of her old debt. *Second,* under Napoleon she had made war pay for itself, as the case of Prussia shows. *Third,* there was no financial operation known to the world in 1815 by which England's war debt, or even half of it, could have been transferred to France. *Fourth,* England never suggested any such transference, or, so far as I know, ever even discussed it.

France's financial comfort, immediately after her defeat, extended to her currency. During the Revolution she had made a classical experiment in the mismanagement of credit documents, with the assignats issued on the security of confiscated Church property; but after that she had put her currency in good order. Her final defeat in 1812-14, and again in 1815, did not seriously derange it. Indeed, the English currency was in worse order than the French, owing to the suspension of cash payments by the Bank of England; and so rapidly did France's credit recover after 1815 that in 1818 French 5 per cents stood at almost exactly the present-day price of British 5 per cent War Loan. That year she finished the payment of her war indemnity, and the last armies of occupation withdrew.

She had no doubt gained by waging war, and eventually suffering defeat, on foreign soil. No French city had been burnt like Moscow, stormed like Badajoz, or made the heart of a gigantic battle like Leipzig. Napoleon fought one brilliant defensive campaign on French soil, in the valleys of the Marne and the Seine, in 1814. In 1815 his fate was decided in Belgium. Hardly a shot was fired in France; hardly a French cornfield was trampled down. But France, as in 1918, was terribly short of men, and, again as in 1918, her means of communication had suffered. Napoleon's magnificent roads—he was among the greatest of road engineers—had gone out of repair; his great canal works had been suspended. These things, however, were soon set right by the Government which followed him.

France's rapid recovery brings us to one of the essential differences between Western Europe a century ago and Western Europe to-day. In spite of Paris and her other great towns, the France of 1815 was a rural country, a land of peasants and small farmers. Only about 10 per cent. of her population lived in towns of 10,000 inhabitants or more. The town below 10,000, in all countries, is more often a rural market town, ultimately dependent on the prosperity of agriculture, than an industrial centre. Parallels for France's condition must be sought to-day in Eastern Europe—in Serbia or Russia. It is a condition which makes the economics of demobilisation easy. The young peasant goes back from the armies to relieve his father, his mother, and his sisters, who have kept the farm going. Moreover, France maintained a standing army of 240,000 men after 1815; and her losses in the Waterloo campaign had been so heavy that the actual numbers demobilised were

relatively small. Demobilisation left hardly a ripple on the surface of her economic life.

The German States were far more rural in character even than France. There were a few industrial districts, of a sort, in the West and in Saxony; a few trading towns of some size, like Hamburg and Frankfurt; but there was nowhere a city comparable to Paris. In 1819 the twenty-five cities which were to become in our day the greatest of the modern German Empire had not 1,250,000 inhabitants between them. Paris alone at that time had about 700,000. German statesmen, when peace came, were occupied not with problems arising from the situation of the urban wage-earner, though such problems existed, but with how to emancipate the peasants from the condition of semi-servility in which they had lived during the previous century. Here, too, demobilisation presented few of the problems familiar to us. Probably not one man in ten demobilised was a pure wage-earner. The rest had links with the soil. The land, neglected during the war, was crying out for labour, and every man had his place, even if it was a servile place, in rural society.

Things were different in England; but our demobilisation problem was smaller than that of our Continental allies or enemies, who had mobilised national armies, though not of the modern size. On the other hand, we had kept an immense fleet in commission, the crews of which were rapidly discharged. Early in 1817 Lord Castlereagh stated in Parliament that 300,000 soldiers and sailors had been discharged since the peace. In proportion to population, that would be equivalent, for the whole United Kingdom, to nearly 750,000 to-day. For these men no provision whatever was made. They were simply thrown on the labour market; and the vast majority of them were ex-wage-earners or potential wage-earners, industrial, mercantile, or agricultural. The United Kingdom was not urbanised as it is to-day; but the census of 1821 showed that 21 per cent. of the population lived in cities of 20,000 inhabitants and upwards, and probably about 27 per cent. (as compared with France's 10 per cent.) lived in places of 10,000 and upwards. As industry in various forms, especially coal-mining, spinning, and weaving, was extensively carried on in rural or semi-rural districts, it is certain that at least one demobilised man of working age in every three was a potential wage-earner of industry or commerce. And as Great Britain had lost most of her peasant-holders, whether owners or small working farmers, the remainder of the demobilised rank and file were nearly all of the agricultural labourer class. They had to find employment; there was not a place in rural society waiting for them, as there was for the average French or German peasant soldier. It is not surprising that the years from 1815 to 1820 were, both economically and politically, probably the most wretched, difficult, and dangerous in modern English history.

Things were at their worst in 1816-17, both for England and for her Continental neighbours. Western Europe was very near starvation. Had the harvest of 1815 not been excellent, so providing a carry-over of corn, or had the harvest of 1817 been much below the average, there must have been widespread disaster; so thorough and universal

was the harvest failure of 1816. In the latter part of 1815 (December) wheat fell in England to 55s. 7d., although no grain imports were allowed, except of oats. Early in 1816 the United Kingdom was actually exporting a little wheat. Then came a terrible spring—a long frost; snow lying about Edinburgh in May; all the rivers of Western Europe in flood. An equally disastrous summer followed. There was dearth, in places amounting to real famine, everywhere—worst of all in Germany. Unlike France, the German States of a century ago were extraordinarily ill-provided with roads. What roads there were had gone to pieces in the wars. In winter even the mails could hardly get through with sixteen and twenty horses. Food supplies could not be moved over long distances by land; and the slightly more favoured regions could not help the most unfortunate. There was a far wider gap between prices in Eastern and Western Germany in 1816 than there had been in the last bad famine year (1772). Each German State, in its anxiety, began to forbid export early in 1816, thus making things worse. At Frankfurt, the representatives of the German States, gathered for the Diet, could hardly feed their horses. Prices rose amazingly and quite irregularly, with the varying food conditions of the various provinces. In the spring of 1817 pallid, half-starved people were wandering the fields, hunting for and grubbing up overlooked and rotten potatoes of the last year's crop.

In England the harvest failure of 1816 drove wheat up to 103s. 7d. a quarter for December of that year, and to 112s. 8d. for June of 1817. In Paris the June price in 1817 was equivalent to 122s. 5d. At Stuttgart the May price was equivalent to 138s. 7d. These are only samples. Think what these figures mean at a time when an English agricultural labourer's wage was about 9s. 6d., and a French or German unskilled wage far less. It must be recalled that there were no special currency causes of high prices either in France or Germany. These were real dearth prices. In the spring of 1817 the French Government was buying corn wherever it could find it—in England, North Africa, America—as another bad harvest was feared. Happily, the 1817 harvest was abundant, here and on the Continent. By September the Mark Lane price of wheat was 77s. 7d., and the Paris price 71s. 9d.

I have gone into price details for the purpose of drawing a contrast between a century ago and to-day. Except for the damage done to the German roads, the wars had very little to do with these food troubles of 1816-17. High and fluctuating food prices were the natural consequence of the general economic position of Western Europe a century ago. It was only in the most comfortable age in all history— the late nineteenth and early twentieth centuries—that low and stable food prices came to be regarded as normal. In the eighteenth century, when England fed herself and often had an exportable surplus, fluctuations were incessant. Take the ten years 1750-1760. The mean price of wheat at Eton in 1752 was 45 per cent. above the mean price in 1750. The mean price in 1757 was nearly 100 per cent. above the mean price of 1750. On Lady Day 1757 the price was 60s. 5¼d. On Lady Day 1759 it was 37s. 4d. On Lady Day 1761 it was 26s. 8d. The 1761 mean price was exactly half the 1757 mean price.

Eighteenth-century England was too well organised economically to be in much risk of actual famine, but for Ireland and large parts of the Continent famine was a normal risk. War and its effects had only accentuated, not created, that risk. Imports might reduce it, but could not avert it, because Western Europe tends to have approximately the same harvest conditions throughout, and it was impossible to draw really large supplementary supplies from anywhere else. So unimportant were overseas supplies that the Continent suffered very much more from the harvest failure of 1816, in time of peace, than from the eight years' English blockade in time of war. If overseas supplies could be got they were hard to distribute, owing to defective transport facilities. Thanks to the work of the nineteenth century, the most terrific of all wars was required to bring Western Europe face to face with what had been both a war-time and a peace-time risk a century earlier.

But the old Europe, if it had the defects, had also the elasticity of a rather primitive economic organism. Given a couple of good harvests, and a land of peasants soon recovers from war. Serbia had a good harvest last year (1919), and was at once in a state of comparative comfort, in spite of her years of suffering. A second good harvest this year, for which fortunately the prospects are favourable, would almost restore her. So it was with France and, to a less extent, Germany in 1816-18. In France acute distress in 1816-17 had been confined to the towns and to those country districts where the harvest failure was worst. The harvest of '17 put an end to it. One gets the impression that in Germany distress among the peasants themselves had been more widespread. Worse communications and the absence of a strong central Government seem to have been the chief causes of this, though perhaps the harvest failure was more complete. In France, as we have seen, the central Government took such action as was possible in the interests of the whole country. A parallel might be drawn between the German situation in 1815-17 and that of the States which have arisen from the break-up of the old Austro-Hungarian Empire since 1918. Freed from French domination, and then from the urgent necessity of co-operating against a common enemy, the German States relapsed into their ancient jealousies and conflicting economic policies, just as the new States, which were once subject to the Hapsburgs, have been forbidding exports of food and fuel and disputing with one another.

An excellent harvest in 1817 averted the risk of famine in Germany also; but anything that could be called prosperity was long delayed, whereas France was indisputably prosperous, judged by the standards of the day, and far more contented than England, by 1818-20. Germany had been so exhausted by the wars and incessant territorial changes of the Napoleonic age, and was politically so divided, that her economic life remained stagnant and her poverty great until at least 1830. It was all that the various Governments could do to find money for the most essential of all economic measures—the repair and construction of roads—whereas France had her splendid main roads in order again and had resumed work on her canals before 1820. But France had

cut her losses nearly twenty years before, and had enjoyed continuous freedom from war on her own territory between 1794 and 1814, as we have seen. She had been well, if autocratically, governed, and her war indemnity was but a trifling burden. Her peasants were free and, as a class, vigorous and hopeful. She was united and conscious of her leadership in Europe, even through her ultimate defeats.

If the experience of Europe after Waterloo is, on the whole, of good augury for agricultural States, and especially for agricultural States with a competent central Government, for the industrialised modern world that experience is less encouraging. Great Britain alone was partially industrialised in 1815-20, and Great Britain, though victorious, suffered acutely. Mismanagement was largely responsible for her sufferings—mismanagement of, or rather, complete indifference to, problems of demobilisation; mismanagement of taxes (the income tax was abandoned at the clamour of interested parties, and the interest on the huge debt paid mainly from indirect taxes, which bore heavily on the poor); mismanagement of food supplies, by the imposition of the Corn Law; and so on. But suffering due to international economic dislocation following war could not have been avoided by management, however good. The situation was unique. England alone of the European Powers had developed her manufactures to some extent on what we call modern lines. During the wars she had accumulated also great stores of colonial and American produce, which could only get into Europe with difficulty—by way of smuggling. In 1813, before Napoleon's first fall, her manufacturers and merchants were eagerly awaiting peace. In 1814 manufactures and colonial produce were rushed over, only to find that, much as Europe desired them, it could not pay the price. It had not enough to give in exchange; and England, being rigidly protectionist, was not always prepared to buy even what Europe had to give. There was no machinery for international buying credits. Merchants shipped at their own risks, usually as a venture, not against a firm order as to-day, and they had to bear their own losses—often up to 50 per cent. Continental economic historians have hardly yet forgiven us for this 'dumping,' which both drained away the precious metals to England—as there was not much else to pay with—and did a great deal of harm to the struggling young factory industries which had begun to grow up under the protection of Napoleon's anti-English commercial policy.

British exporters were so badly bitten in 1814 that, when peace finally came next year, after Waterloo, they were nervous of giving orders at home—which was very bad for the manufacturing industries and for the men who sought employment in them. There was the curious situation in 1816 that, while the price of wheat was rushing up, most other prices were falling, the bottom of the market being often reached at the end of the year, when the confidence of buyers and shippers began to revive. Raw cotton, for instance, which had touched 2s. 6d. a lb. in 1813-14, fell to a minimum of 1s. 2d. in 1816—although Europe was open and cotton badly needed.

It is as yet too early to work out a parallel between this post-war commercial and industrial slump and the slump that followed the Great

War of 1914-18, for we have not yet had it. But it is coming. More certainly, I am inclined to believe, in the United States than in England; but pretty certainly here also. I say more certainly in the United States because her position bears most resemblance to that of England in 1815-17. Consider that position. What before the war was, on the balance, a debtor country has become a creditor country. That creditor is equipped to export both raw materials and manufactures—iron and steel goods particularly—on a huge scale. It is true she is a heavy importer of some foods, such as sugar, coffee, and tea, and of certain raw materials, such as rubber, timber, and wool. But, owing to her tariff system and her general policy, she is reluctant to take many things which her debtors have to offer. Her recent ' dry ' policy, for example, has shut her markets to one of France's most valuable exports, an export with which France has always been in the habit of paying her creditors. Already, I notice, American business men are beginning to point out what English business men stated clearly in a famous document, the Petition of the London Merchants, a century ago—that the country which will not buy, neither shall it sell. This was the most solid of all free-trade arguments in the early nineteenth century, and it has lost none of its force. No doubt America is, and will be, glad to take part payment in gold, just as England was in 1814-16. But that is not a permanent solution. If she remains a creditor nation—and there is no present reason to think that she will not—she must in time arrange to take more goods from outside. Her political processes, however, are slow; and it seems unlikely that she will have adjusted her policy before the post-war slump is upon her.

The United Kingdom, which, on the whole, still takes freely what its customers have to offer it, is in a better position, provided its customers can go on offering. This may prove an important proviso. Customers who have been little hurt or even helped by the war—Spain, perhaps, or Egypt, or India, or New Zealand—should continue good buyers. But the uncertainty gives cause for anxious thought in the case of the war-damaged nations, allied and ex-enemy. Modern financial and commercial organisation has postponed the critical moment in a way that was impossible a century ago. When Europe was hungry in 1816 there were not food surpluses available anywhere on the earth, nor shipping enough on the seas, nor means of transport good enough on land, to relieve her need. If, *per impossibile*, there had been all these things, there would have been no country or group of business men anywhere ready to give her the necessary credit on a large scale. The Rothschilds, a young firm in those days, did something. They advanced money to a few German princes to buy corn for their people at the Baltic ports, for there was some corn to spare from Poland and Russia. But the huge food-financing operations of 1918-20 would have been as unthinkable as the actual handling of the foodstuffs would have been impossible. Had two harvests like that of 1816 come in succession, there would have been famine and food riots everywhere, past hope of cure.

Similarly modern finance is postponing the critical moment for the

international trade in manufactures. British business men in 1919-20 have not, I believe, often sent their goods abroad in hope of finding a vent for them, and then been forced to content themselves with prices far below cost of production, as their grandfathers were in 1814-16. Every kind of financial device—long private credit, assistance from banks, credits given by Governments—has been called in, so that trade may be resumed before the war-damaged nations are in a position to pay for what they need by exporting the produce of their own labour. The more industrial the damaged nation is, the more complex is the restarting of her economic activity. Corn grows in nine months, and pigs breed fast. The start once given, countries like Denmark and Serbia, both of which are normally great exporters of pigs or bacon, could soon pay for necessary imports of machinery or fertilisers bought on long credit to restart their rural industries. The United Kingdom, the least damaged of all the combatants except America, is believed by the Chancellor of the Exchequer to be now rather more than paying its way. That may be sanguine, but at the worst our accounts are nearly balanced. What might not have happened in 1919 if modern methods for postponing payment had not been applied internationally? The other chief combatants are far from paying their way. Italy is importing abnormal quantities of food and also her necessary raw materials with the aid of American and English credits, while Germany, who can get little in the way of credit, has hardly begun even to import the raw materials to make the goods by the export of which she may eventually pay her way, not to mention her indemnities. I have in mind such materials as cotton, wool, rubber, copper, oil-seeds, and hides—all of which she imported heavily in 1913. Some materials, of course, she possesses in abundance, but the working up even of these is hampered by her coal position. I make no political pleas : I merely illustrate the complexity of the restarting of industry under present-day conditions. France has the first claim to assistance in restarting, a claim which we all recognise ; but for the comfort and peace of the world a universal restart is desirable.

The central problem is one which I can only indicate here, not discuss. Its discussion is for experts with full inside knowledge from month to month, and the answer varies for every country. It is—when will the inability of the war-damaged nations to pay for all that they want, in food and materials, in order to restart full economic activity, make itself felt by the nations who are supplying them, primarily, that is, the United States and ourselves? In 1814-16, when the problem was, of course, infinitely smaller because nations were so much more self-sufficing, the reaction came at once for lack of long organised credits. Conceivably, all other combatants might do in turn what we seem to have done—that is, adjust their trade balance within a reasonable period and so avoid renewal of special credits. In that case the post-war trade slump would come, not as an international crisis, but as a gradual decline, when the first abnormal demand for goods of all kinds to replenish stocks is over. Already this type of demand is slackening 'n certain quarters. We shall do very well if we have nothing worse than that gradual decline, which would be eased, in our case, by our

extensive connections with undamaged countries, and by our willingness to buy most things which any nation has to offer. The situation would be still further eased if countries such as Germany and Russia were to develop in turn what might be called a reconstruction demand, to take the place of the satisfied reconstruction demands of our Allies. But the fear, as I think the quite reasonable fear, expressed in some well-informed quarters, is that, in view of the complicated and dangerous currency position in many countries; in view of the difficulty which the war-damaged nations have in collecting taxes enough to meet their obligations; in view of the slowness with which some of them are raising production to the level of consumption; in view of the complete uncertainty of the political and economic future in much of Central and Eastern Europe—that in view of these things, and quite apart from possible political disturbances, we shall have to go through a genuine crisis, as distinct from a depression; a crisis beginning in the field of finance, when some international obligation cannot be met or some international credit cannot be renewed, spreading to industry and giving us a bad spell of unemployment, comparable with the unemployment of the post-war period a century ago, and more dangerous because of the high standard of living to which the people in this and some other countries is becoming accustomed.

Personally, I am less apprehensive for the industries of this country than are many whose opinions I should ordinarily be disposed to prefer to my own. A demand, an effective demand, exists for many things that we can supply in great regions outside the war area—in China, for instance, where there is said to be at this moment a keen demand for machinery which the United Kingdom is too much preoccupied with other work to supply. Nor do I fear that a crisis will originate here, as I am disposed to think that our currency and taxation position is already relatively sound. But we should be bound to feel the reactions of a crisis which might occur elsewhere; to what extent is, however, quite impossible to foresee.

One final comparison. An extraordinary feature of the great wars of a century ago was that they coincided with a steady growth of population, and were followed by a period of rapid growth. For the United Kingdom that fact is well known and not surprising. We lost relatively few men in war. But the official French figures, 27,500,000 in 1801 and 29,500,000 in 1816, are so remarkable that one is tempted to doubt the first enumeration. Though remarkable, the figures are, however, not impossible; and it must be recalled that the losses were spread over many years. British population has grown a little since 1914; in spite of separations of man and wife and our three-quarters of a million dead. A main reason has, however, been the suspension of emigration, which was proceeding at a rate of over 200,000 a year just before the war. France estimates a dead loss of over 3,000,000 (on 39,700,000) between 1913 and 1918 on her old territory. Her census is due next year. Comparatively early in the war the German *civilian* death rate was above the birth rate; so presumably she is in much the same position as France. But, owing to changes of frontier and continued unrest, it is as yet too early to estimate the total

effect of the Great War on population.   For Western and Central Europe it must, I think, have produced a considerable net loss.   For Russia one can hardly guess; but her population is so largely rural and grew so amazingly fast before 1914, that it would not surprise me very much to learn that, with all her miseries, it had been maintained.

The growth of population in Europe after 1815 coincided with the spread of the first industrial and agricultural revolution outwards from the United Kingdom.   The world was learning new ways to feed and clothe itself; and it continued to learn all through the century.   I myself do not suppose that the age of discovery is at an end, so our troubles may be eased as time goes on; and although I have not the slightest wish that population should ever again grow so fast as it grew in Europe during the nineteenth century, I see no reason why a moderate rate of growth should not be resumed, in a few years at latest.   But perhaps I have already committed prophecy, or half prophecy, more than is altogether wise for one in my position.

ADDRESS

TO THE

ENGINEERING SECTION

BY

Professor C. F. JENKIN, C.B.E., M.A.,

PRESIDENT OF THE SECTION.

THE importance of research in all branches of industry is now becoming fully recognised. It is hardly necessary to point out the great possibilities of the Board of Scientific and Industrial Research, formed just before the war, or to lay stress on the attention which has been called to the need for research by events during the war. Probably in no branch of the Services was more research work done than in the Air Service, and the advances made in all directions in connection with flying were astonishing. My own work was confined to problems connected with materials of construction, and as a result of that work I have come to the conclusion that the time has come when the fundamental data on which the engineering theories of the strength and suitability of materials are based require thorough overhauling and revision. I believe that the present is a favourable time for this work, but I think that attention needs to be drawn to it, lest research work is all diverted to the problems which attract more attention, owing to their being in the forefront of the advancing engineering knowledge, and lest the necessary drudgery is shirked in favour of the more exciting new discoveries.

It has been very remarkable how again and again in aeroplane engineering the problems to be solved have raised fundamental questions in the strength and properties of materials which had never been adequately solved. Some of these questions related to what may be termed theory, and some related to the physical properties of materials. I propose to-day to describe some of these problems, and to suggest the direction in which revision and extension of our fundamental theories and data are required and the lines on which research should be undertaken. Let us consider first one of the oldest materials of construction—timber. Timber was of prime importance in aircraft construction. The first peculiarity of this material which strikes us is that it is anisotropic. Its grain may be used to locate three principal axes—along the grain, radially across the grain, and tangentially across the grain. It is curious that there do not appear to be generally recognised terms for these three fundamental directions. A very few

tests are sufficient to show that its strength is enormously greater along the grain than across it. How, then, is an engineer to calculate the strength of a wooden member? There is no theory, in a form available for the engineer, by which the strength of members made of an anisotropic material can be calculated.

I fancy I may be told that such a theory is not required—that experience shows that the ordinary theory is quite near enough. How utterly misleading such a statement is I will try to show by a few examples. Suppose a wooden tie or strut is cut from the tree obliquely so that the grain does not lie parallel to its length. In practice it is never possible to ensure that the grain is accurately parallel to the length of the member, and often the deviation is considerable. How much is the member weakened? This comparatively simple problem has been of immense importance in aeroplane construction, and, thanks to the researches made during the war, can be answered. The solution has thrown a flood of light on many failures which before were obscure. If the tensile strengths of a piece of timber are, say, 18,000 lb./sq. in. along the grain and 800 lb./sq. in. across it (radially or tangentially) and the shear strength is 900 lb./sq. in. along the grain—these figures correspond roughly with the strengths of silver spruce—then if a tensile stress be applied at any angle to the grain the components of that stress in the principal directions must not exceed the above strengths, or failure will occur. Thus we can draw curves limiting the stress at any angle to the grain, and similar curves may be drawn for compression stresses. These theoretical curves have been checked experimentally, and the results of the tests confirm them closely, except in one particular. The strengths at small inclination to the grain fall even faster than the theoretical curves would lead us to expect. The very rapid drop in strength for quite small deviations is most striking.

Similar curves have been prepared for tensile and compressive stresses inclined in each of the three principal planes for spruce, ash, walnut, and mahogany, so that the strengths of these timbers to resist forces in any direction can now be estimated reasonably accurately.

As a second example consider the strength of plywood. Plywood is the name given to wood built up of several thicknesses glued together with the grain in alternate thicknesses running along and across the plank. The result of this crossing of the grain is that the plywood has roughly equal strength along and across the plank. Plywood is generally built up of thin veneers, which are cut from the log by slicing them off as the log revolves in a lathe.

Owing to the taper in the trunk of the tree and to other irregularities in form, the grain in the veneer rarely runs parallel to the surface, but generally runs through the sheet at a more or less oblique angle. As a consequence the strength of plywood is very variable, and tests show that it is not possible to rely on its having more than half the strength it would have if the grain in the veneers were not oblique. It is therefore obviously possible to improve the manufacture enormously by using veneers *split off*, following the grain, in place of the present sliced veneers. The superiority of split or riven wood over cut wood has been recognised for ages. I believe all ladders and ladder

rungs are riven. Hurdles, hoops, and laths are other examples. Knees in ships are chosen so that the grain follows the required outline.

Owing to the enormous difference in strength in timber along and across the grain, it is obviously important to get the grain in exactly the right direction to bear the loads it has to carry. The most perfect example I ever saw of building up a plywood structure to support all the loads on it was the frame of the German Schutte-Lanz airship, which was made entirely of wood. At the complex junctions of the various girders and ties the wood, which was built up of very thin veneers—hardly thicker than plane shavings—layers were put on most ingeniously in the direction of every stress.

During the war I have had to reject numerous types of built-up struts intended for aeroplanes, because the grain of the wood was in the wrong direction to bear the load. The example shown—a McGruer strut—is one of the most elegant designs, using the grain correctly.

Many of the tests applied to timber are wrong in theory and consequently misleading. For example, the common method of determining Young's modulus for timber is to measure the elastic deflection of a beam loaded in the middle and to calculate the modulus by the ordinary theory, neglecting the deflection due to shear, which is legitimate in isotropic materials; but in timber the shear modulus is very small—for example, in spruce it is only about one-sixtieth of Young's modulus—and consequently the shear deflection becomes quite appreciable, and the results obtained on test pieces of the common proportions lead to errors in the calculated Young's modulus of about 10 per cent.

The lantern plates show three standard tests; the first is supposed to give the shearing strength of the timber, but these test pieces fail by tension across the grain—not by shearing. Professor Robertson has shown that the true shear strength of spruce is about three times as great as the text-book figures, and has designed a test which gives fairly reliable results. The second figure represents a test intended to give the mean strength across the grain, but the concentration of stress at the grooves is so great that such test pieces fail under less than half the proper load. This fact was shown in a striking manner by narrowing a sample of this shape to half its width, when it actually bore a greater total load—*i.e.*, more than double the stress borne by the original sample. The third figure represents a test piece intended to measure the rather vague quality, ' strength to resist splitting.' The results actually depend on the tensile strength across the grain, on the elastic constants, and on the accidental position of the bottom of the groove relatively to the spring or autumn wood in the annular rings. Unless the theory is understood, rational tests cannot be devised.

There are some valuable tropical timbers whose structure is far more complex than that of our ordinary northern woods. The grain in these timbers grows in alternating spirals—an arrangement which at first sight is almost incredible. The most striking example of this type of wood I have seen is the Indian ' Poon.' The sample on the table has been split in a series of tangential planes at varying distances from the centre of the tree, and it will be seen that the grain at one depth is growing in a right-hand spiral round the trunk; a little farther out

it grows straight up the trunk; further out again it grows in a left-hand spiral, and this is repeated again and again, with a pitch of about two inches.   The timber is strong and probably well adapted for use in large pieces—it somewhat resembles plywood—but it is doubtful whether it is safe in small pieces.   No theory is yet available for estimating its strength, and very elaborate tests would be needed to determine its reliability in all positions.   I had to reject it for aeroplanes during the war for want of accurate knowledge of its properties.

These examples show how necessary it is to have a theory for the strength of anisotropic materials before we can either understand the causes of their failure or make full use of their properties or even test them rationally.

The second material we shall consider is steel, and in dealing with it I do not wish to enter into any of the dozen or so burning questions which are so familiar to all metallurgists and engineers, but to call your attention to a few more fundamental questions.   Steel is not strictly isotropic—but we may consider it to be so to-day.   The first obvious question the engineer has to answer is, ' What is its strength? ' The usual tests give the Ultimate Strength, Yield Point, Elastic Limit, the Elongation, the Reduction of Area, and perhaps the Brinell and Izod figures.   On which of these figures is the dimension of an engine part, which is being designed, to be based?   If we choose the Ultimate Strength we must divide it by a large factor of safety—a factor of ignorance.   If we choose the Yield Point we must remember that none of the higher-grade steels have any Yield Point, and the nominal Yield Point depends on the fancy of the tester.   This entirely imaginary point cannot be used for accurate calculation except in a very few special cases.   Can we base our calculation on the Elongation—the Reduction of Area—the Izod test?   If we face the question honestly we realise that there is no known connection between the test results and the stress we can safely call on the steel to bear.   The only connecting link is that cloak for our ignorance—the factor of safety.

I feel confident that the only reliable property on which to base the strength of any engine part is the suitable *Fatigue Limit*.   We have not yet reached the position of being able to specify this figure, but a considerable number of tests show that in a wide range of steels (though there are some unexplained exceptions) the Fatigue Limit for equal $\pm$ stresses is a little under half the Ultimate Strength, and is independent of the Elastic Limit and nominal Yield Point, so that the Ultimate Strength may be replaced as the most reliable guide to true strength, with a factor—no longer of ignorance, but to give the fatigue limit—of a little over 2.

If the Fatigue Limit is accepted as the only sound basis for strength calculation for engine parts, and it is difficult to find any valid objection to it, then it is obvious that there is urgent need for extensive researches in fatigue, for the available data are most meagre.   The work is laborious, for there is not one Fatigue Limit, but a continuous series, as the signs and magnitudes of the stresses change.   Many problems in connection with fatigue are of great importance and need much fuller

investigation than they have so far received—*e.g.*, the effect of speed of testing; the effect of rest and heat treatment in restoring fatigued material; the effect of previous testing at higher or lower stresses on the apparent fatigue limit of a test piece. Some observers have found indications that the material may possibly be strengthened by subjecting it to an alternating stress below its fatigue limit, so that the results of fatigue tests may depend on whether the limit is approached by increasing the stress or by decreasing it.

Improved methods of testing are also needed—particularly methods which will give the results quickly. Stromeyer's method of measuring the first rise of temperature, which indicates that the fatigue limit is passed, as the alternating load is gradually increased, is most promising; it certainly will not give the true fatigue limit in all cases, for it has been shown by Bairstow that with some ranges of stress a finite extension occurs at the beginning of a test and then ceases, under stresses lower than the fatigue limit. But the fatigue limit in that case would not be a safe guide, for finite changes of shape are not permissible in most machines, so that in that case also Stromeyer's test may be exactly what is wanted. It can probably be simplified in detail and made practicable for commercial use. Better methods of testing in torsion are also urgently needed, none of those at present used being free from serious defects. Finally, there is a fascinating field for physical research in investigating the internal mechanism of fatigue failure. Some most suggestive results have already been obtained, which extend the results obtained by Ewing.

For members of structures which are only subjected to steady loads I suggest that the safe stress might be defined by limiting the corresponding permanent set to a small amount—perhaps $\frac{1}{2}$ per cent. or $\frac{1}{4}$ per cent. This principle has been tentatively adopted in some of the aircraft material specifications by specifying a Proof Load which must be sustained without a permanent extension of more than $\frac{1}{2}$ per cent. Whether this principle is suitable for all materials and how it will answer in practice remains to be proved by experience. It is at any rate a possible rational basis for determining the useful strength of a material under steady loads.

The relation between the proof stress and the shape of the stress-strain diagram is shown in the lantern slide. The curve is the record of an actual test on a certain copper alloy. If a length A B corresponding to $\frac{1}{2}$ per cent. elongation be set off along the base line and a line B P be drawn through the point B parallel to the elastic line, to cut the curve in P, then the stress at P is the stress which will give $\frac{1}{2}$ per cent. permanent set. Though $\frac{1}{2}$ per cent. may appear rather a large permanent set to allow it will be seen from the figure that it is less than the elastic elongation would have been at the same stress, and we do not usually find elastic elongations serious.

As a commercial test the proof load is very easily applied. For this alloy the specified proof load is shown by the horizontal line so labelled. This load is to be applied and released, and the permanent extension is required by the specification to be less than $\frac{1}{2}$ per cent. This sample passes the test easily. On the figure the condition for complying with

the specification is that the curve shall fall above Q. But the test does not require the curve to be determined.

If we admit that the fatigue limit is the proper basis for engine-strength calculations, there are a number of interesting modifications required in the common theory of the strength of materials. It will no longer be possible to neglect, as has been so general in the past, the uneven distribution of stress in irregularly shaped parts of machines. It has been generally recognised that sharp corners should be avoided when possible, but no theory is available to enable the stresses at corners to be calculated or to enable their effect on the strength of the member to be estimated. If fatigue is the critical factor in failure under fluctuating stresses such theory is most necessary. Even the roughest guide would be of great value. The nature and magnitude of the concentrations of stress which occur in practice have been investigated experimentally by Professor Coker by his elegant optical method which has given most valuable results, some of which are already being used in designing offices. If the mathematical theory is too difficult, it may be possible to lay down practical rules deduced from such experimental results—but the method still has many limitations, perhaps the most serious being that it can only be used on flat models. I believe Professor Coker expects to be able to extend the method to round models.

As a simple example to show the importance of the subject let us consider the effect of a groove round a straight round bar subject to alternating tension and compression—such a groove as a screw thread. There will be a concentration of stress at the bottom of the groove. The ratio of the stress at the bottom of a groove to the mean stress in the bar has been worked out mathematically by Mr. A. A. Griffith, and his calculations have been confirmed experimentally by his elegant soap-bubble method. The ratio depends on the relation between the depth of the groove, the radius at the bottom, and slightly on the angle between the sides. For a Whitworth form of thread the ratio will be about 3. If the Fatigue Limit is exceeded at the bottom of the groove the metal will fail and a minute crack will form there; this crack will soon spread right across the bar and total failure will result. Thus we see that the safe mean stress in the bar will be reduced to one-third what a plain bar will bear. The truth of this theory regarding the importance of concentrations of stress has still to be proved experimentally; if true, it is of far-reaching importance, since it applies to all concentrations of stress in machine parts subject to fluctuating loads.

The theory does not apply to steadily loaded members; in these the local excess of stress is relieved by the stretching of the minute portion which is overloaded, and no further consequences follow.

The theory appears to apply to grooves however small, and has an important bearing on the smoothness of the finish of machine parts. The surface of any engine part finished by filing is certainly entirely covered with scratches. Emery likewise leaves the surface scratched—though the scratches are smaller. If, however, polishing be carried further the surface may ultimately be freed from scratches and left in a burnished condition. In this condition amorphous metal has been smeared over the surface—the smooth appearance is not simply due

to the scratches being too small to see. The strength—under alternating stresses—appears to depend on the form of the scratches, and if the ratio of radius at the bottom of the scratch to its depth is fairly large, very little weakening occurs. It seems probable in the ordinary engineering finish produced by emery and oil that the scratches are broad and shallow. This subject is being investigated. A considerable amount of evidence has been collected from practical experience pointing to the important effect which a smooth finish has on the strength of heavily stressed engine parts.

Fatigue is probably the cause of failure of wires in wire ropes. A good deal of valuable experimental work has been done on the life of ropes, but so far as I am aware there is no satisfactory theory of their strength. This subject also requires research, and it seems probable that valuable practical results might follow if the true explanation of the cause of the breakages of the wires was determined.

These are only examples, but they may be sufficient to show how much work both experimental and theoretical requires to be done to give the engineer a really sound basis for the simplest strength calculations on any moving machinery. But there are more fundamental questions still which must be tackled before the simplest questions of all which meet the engineer can be answered scientifically. The two most urgent and most important questions which I met with during the war in connection with aircraft were always the same—Why did some part break? and, What is the best material to use for that part? It was most disconcerting to find how inadequate one's knowledge was to answer these two simple questions. The common answers are: To the first: ' It broke because it was too weak, make it stronger,' and to the second: ' General practice indicates such a material as the best—better not try any other or you may have trouble.' In aircraft weight is paramount, and to make a part stronger—i.e., heavier—had to be the last resort, and when used was almost a confession of failure. ' General practice ' was no guide in aeroplane engines, which are built of the strangest materials. The origins of fractures were traced to many causes, often lying far away from the site of the breakage; but with these I am not concerned to-day. I wish to confine our consideration to the actual fracture and to ask, ' What stress caused the fracture? ' and ' What property of the metal was absent which would have enabled it to withstand that stress? ' And again, ' What other material possesses suitable properties to withstand the stresses better? ' These are the fundamental questions which I have referred to—and which urgently need answers.

As an example I will take a broken propeller shaft. It has broken in a beautiful spiral fracture. What stress causes that? I have failed to explain it by any of the facts I know about the steel it is made of. It is, of course, a fatigue fracture—i.e., it spread gradually. The questions to be answered are, Did it fail under tension, bending or torsion? and, Why was a spiral direction followed by the failure as it spread?

It may be objected that the question is unimportant. I think not. For example, till we can determine the nature of the stress we cannot

indicate the nature of the load—thus I cannot say if it broke under a torsional load (possibly torsional vibration) or under a bending load (possibly due to some periodic variation of thrust on one of the propeller blades as it passed an obstruction). Until the nature of the load which caused the failure is known, it is very difficult to take steps to guard against similar accidents. For the most urgent reasons, therefore, we require to be able to understand the fracture, as in nearly all aircraft problems men's lives hang on the answer.

Turning now to the question of the most suitable material, I will take as an example the material for the crankshaft of an aeroplane engine. A few months before the Armistice there were difficulties in getting sufficient supplies of the high-grade nickel-chrome steel forgings then in general use for shafts, and proposals were made to use a plain carbon steel. Such a steel would be about 30 per cent. weaker, according to the ordinary tests. A conference of leading metallurgists and engineers was held to discuss the suggestion. No one present ventured to predict whether the weaker steel would answer or not, or whether the dimensions would have to be increased or not. It was pointed out that a French engine was now using 50-ton steel with better results than when using the 100-ton steel for which it was designed, no changes in dimensions having been made. Such a reduction of strength might be understood in ordinary engineering where there are large margins of safety, but in an aeroplane engine, in which every ounce of metal is cut off which can be spared, they show how completely ignorant engineers are of what the suitability of material depends on.

As another example, Why are oxygen cylinders annealed—repeatedly? Annealing reduces the steel to its weakest condition. I believe the fondness for annealing is due to our ignorance of the properties we require. Perhaps the quality of steel which an engineer fears most is brittleness. He believes that annealing will soften it and reduce the brittleness; so he anneals, blindly. The fact is that we do not know what brittleness is—we cannot define it—we cannot measure it—though there are endless empirical tests to detect it. Till we know what it means and can measure it we are in a miserable position. During the war I was consulted on what could be done to reduce the enormous weight of oxygen cylinders, and I advised that experiments should be made on the high-quality alloyed steel tubes we were using in aircraft construction. The department dealing with these tubes took the matter up, and alloyed steel cylinders, properly heat-treated, were made. These were, I believe, a success, and only weighed a small fraction of the old-fashioned cylinders. But my suggestion was little more than a guess, and no means was known of accurately testing the suitability of the material, so they were only accepted after passing any number of empirical tests, consisting of various kinds of rough usage, to see if they would crack or burst. Surely an engineer should be able to say whether a cylinder is safe without dropping it from the roof or rolling it down the front-door steps to see if it breaks.

These examples refer only to different grades of the same material—steel—but how far worse off we are when the problem is whether some other alloy would be suitable to replace steel. Proposals have been

made, for example, to replace the very hard steel used at present for connecting-rods by duralumin or some other forged aluminium alloy. It seems worth trying; but who, in our present state of ignorance of the real properties of metals, will say if the experiment will be a success?

How difficult it is to prophesy may be illustrated by the results of two empirical tests on duralumin and steel sheets of the same thicknesses. The ultimate strengths and elongations of the steel and the duralumin were roughly equal. The lantern slides show that under reverse-bend tests they both follow the same law, the steel being the better. But under the cupping test they follow opposite laws.

The suitability of different materials presumably depends on their fundamental physical properties. These may be many, but some physicists think that they are probably really very few, and that, knowing these few, it may be possible to deduce all the complex properties required by the engineer and to state with certainty how materials will behave under any conditions of service. This is the most fundamental problem which needs solution to enable the knowledge of the strength of materials to be put on a sound foundation. It will need the co-operation of able physicists, metallurgists, and engineers to solve it.

While urging the importance of research in the fundamental theories of stress and fundamental properties of materials, I wish to lay special stress on the nature of the researches required. Engineers are intensely practical men, and their practice has generally been ahead of their theory. The difficulties they have met have been dealt with, often with the greatest ingenuity and skill, as special problems. They have seldom had time or opportunity to solve the general problems, and as a result they are used to making their experiments and trials as close a copy— usually on a smaller scale—of the real thing as possible. The results obtained in this way, while they are applicable to the particular problem, are of little general use. They depend on many factors. The researches I am now advocating must be of a diametrically opposite description. They must be absolutely general, and the results must depend on one factor only at a time, so that general laws may be established which will be applicable to all special problems.

There are many other similar gaps in our knowledge to which I have not time to refer to to-day. I have tried to show that we need most of all a real knowledge of the fundamental properties of materials, from which we shall be able to deduce their behaviour in any condition of service, so that we may be able to compare the relative merits of diverse materials for any particular purpose.

Secondly, that we need a practical method of calculating the stresses in parts of any form, so that concentrations of stress may be avoided or that their magnitudes may be known and allowed for.

Thirdly, that we need a rational connecting link between the tests made on materials and the stresses they will bear in service, to replace the factor of safety. I have suggested two tests, the Proof Load and the Fatigue Limit, which might be used directly in estimating the allowable working stress.

Fourthly, that we need a mathematical theory for the strength of anisotropic materials, of which timber is an extreme and important example.

When the notes for this address were first drafted I ended by an appeal to the Board of Scientific and Industrial Research to undertake the necessary research work. Since then the Aeronautical Research Committee has been constituted, and a sub-committee has been appointed to deal with ' Materials.' I have great hopes that the committee will tackle many of these problems. I will therefore conclude by appealing to all who can help to assist that committee in their endeavour to solve these most important and fascinating, but most difficult, problems.

ADDRESS

TO THE

ANTHROPOLOGICAL SECTION

BY

Prof. KARL PEARSON, M.A., LL.D., F.R.S.,

PRESIDENT OF THE SECTION.

Anthropology—the Understanding of Man—should be, if Pierre Charron were correct, the true science and the true study of mankind.[1] We might anticipate that in our days—in this era of science—anthropology in its broadest sense would occupy the same exalted position that theology occupied in the Middle Ages. We should hail it 'Queen of the Sciences,' the crowning study of the academic curriculum. Why is it that we are Section H and not Section A? If the answer be given that such is the result of historic evolution, can we still be satisfied with the position that anthropology at present takes up in our British Universities and in our learned societies? Have our universities, one and all, anthropological institutes well filled with enthusiastic students, and are there brilliant professors and lecturers teaching them not only to understand man's past, but to use that knowledge to forward his future? Have we men trained during a long life of study and research to represent our science in the arena, or do we largely trust to dilettanti—to retired civil servants, to untrained travellers or colonial medical men for our knowledge, and to the anatomist, the surgeon, or the archæologist for our teaching? Needless to say, that for the study of man we require the better part of many sciences, we must draw for contributions on medicine, on zoology, on anatomy, on archæology, on folk-lore and travel-lore, nay, on history, psychology, geology, and many other branches of knowledge. But a hotch-potch of the facts of these sciences does not create anthropology. The true anthropologist is not the man who has merely a wide knowledge of the conclusions of other sciences, he is the man who grasps their bearing on mankind and throws light on the past and present factors of human evolution from that knowledge.

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[1] " La vraye science et le vray estude de l'homme c'est l'Homme." Pierre Charron, *De la Sagesse*, Préface du Premier Livre, 1601. Pope, with his " The proper study of mankind is Man," 1733, was, as we might anticipate, only a plagiarist.

I am afraid I am a scientific heretic—an outcast from the true ortho-dox faith—I do not believe in science for its own sake. I believe only in science for man's sake. You will hear on every side the argument that it is not the aim of science to be utile, that you must pursue scientific studies for their own sake and not for the utility of the resulting dis-coveries. I think that there is a great deal of obscurity about this attitude, I will not say nonsense. I find the strongest supporters of 'science for its own sake' use as the main argument for the pursuit of not immediately utile researches that these researches will be useful some day, that we can never be certain when they will turn out to be of advantage to mankind. Or, again, they will appeal to non-utile branches of science as providing a splendid intellectual training—as if the provision of highly trained minds was not itself a social function of the greatest utility! In other words, the argument from utility is in both cases indirectly applied to justify the study of science for its own sake. In the old days the study of hyperspace—space of higher dimensions than that of which we have physical cognisance—used to be cited as an example of a non-utile scientific research. In view of the facts: (i.) that our whole physical outlook on the universe—and with it I will add our whole philosophical and theological outlooks—are taking new aspects under the theory of Einstein; and (ii.) that study of the relative influences of Nature and Nurture in Man can be reduced to the trigonometry of polyhedra in hyperspace—we see how idle it is to fence off any field of scientific investigation as non-utile.

Yet are we to defend the past of anthropology—and, in particular, of anthropometry—as the devotion of our science to an immediate non-utile which one day is going to be utile in a glorious and epoch-making manner, like the Clifford-Einstein suggestion of the curvature of our space? I fear we can take no such flattering unction to our souls. I fear that 'the best is yet to be' cannot be said of our multitudinous observations on 'height-sitting' or on the censuses of eye or hair colours of our population. These things are dead almost from the day of their record. It is not only because the bulk of their recorders were untrained to observe and measure with scientific accuracy, it is not only because the records in nine out of ten cases omit the associated factors without which the record is valueless. It is because the progress of mankind in its present stage depends on characters wholly different from those which have so largely occupied the anthropologist's atten-tion. Seizing the superficial and easy to observe, he has let slip the more subtle and elusive qualities on which progress, on which national fitness for this or that task essentially depends. The pulse-tracing, the reaction-time, the mental age of the men under his control are far more important to the commanding officer—nay, I will add, to the employer of labour—than any record of span, of head-measurement, or pigmentation categories. The psycho-physical and psycho-physio-logical characters are of far greater weight in the struggle of nations to-day than the superficial measurements of man's body. Physique, in the fullest sense, counts something still, but it is physique as measured by health, not by stature or eye-colour. But character, strength of will, mental quickness count more, and if anthropometry

is to be useful to the State it must turn from these rusty old weapons, these measurements of stature and records of eye-colour to more certain appreciations of bodily health and mental aptitude—to what we may term ' vigorimetry ' and to psychometry.

Some of you may be inclined to ask: And how do you know that these superficial size-, shape-, and pigment-characters are not closely associated with measurements of soundness of body and soundness of mind? The answer to this question is twofold, and I must ask you to follow me for a moment into what appears a totally different sub-ject. I refer to a ' pure race.' Some biologists apparently believe they can isolate a pure race, but in the case of man, I feel sure that purity of race is a merely relative term. For a given character one race is purer than a second, if the scientific measure of variation of that character is less than it is in the second. In loose wording, for we cannot express ourselves accurately without mathematical symbols, that race is purer for which on the average the individuals are closer to type for the bulk of ascertainable characters than are the characters in a second race. But an absolutely pure race in man defies definition. The more isolated a group of men has remained, the longer it has lived under the same environment, and the more limited its habitat, the less variation from type it will exhibit, and we can legitimately speak of it as possessing greater purity. We, most of us, probably believe in a single origin of man. But as anthropologists we are inclined to speak as if at the dawn of history there were a number of pure races, each with definite physical and mental characteristics; if this were true, which I do not believe, it could only mean that up to that period there had been extreme isolation, extremely differentiated environments, and so marked differences in the direction and rate of mental and physical evolution. But what we know historically of folk-wanderings, folk-mixings, and folk-absorptions have undoubtedly been going on for hundreds of thousands of years, of which we know only a small historic fragment. Have we any real reason for suppos-ing that ' purity of race ' existed up to the beginning of history, and that we have all got badly mixed up since?

Let us, however, grant that there were purer races at the beginning of history than we find to-day. Let us suppose a Nordic race with a certain stature, a given pigmentation, a given shape of head, and a given mentality. And, again, we will suppose an Alpine race, differ-ing markedly in type from the Nordic race. What happens if we cross members of the two races and proceed to a race of hybrids? A Mendelian would tell us that these characters are sorted out like cards from a pack in all sorts of novel combinations. A Nordic mentality will be found with short stature and dark eyes. A tall but brachy-cephalic individual will combine Alpine mentality with blue eyes. Without accepting fully the Mendelian theory we can at least accept the result of mass observations, which show that the association between superficial physical measurements and mentality is of the slenderest kind. If you keep within one class, my own measurements show me that there is only the slightest relation between intelligence and the size and shape of the head. Pigmentation in this country seems

to have little relation to the incidence of disease. Size and shape of head in man have been taken as a rough measure of size and shape of brain. They cannot tell you more—perhaps not as much as brain-weight—and if brain-weight were closely associated with intelligence, then man should be at his intellectual prime in his teens.

Again, too often is this idea of close association of mentality and physique carried into the analysis of individuals within a human group, *i.e.* of men belonging to one or another of the many races which have gone to build up our population. We talk as if it was our population which was mixed, and not our germplasm. We are accustomed to speak of a typical Englishman. For example, Charles Darwin; we think of his mind as a typical English mind, working in a typical English manner, yet when we come to study his pedigree we seek in vain for 'purity of race.' He is descended in four different lines from Irish kinglets; he is descended in as many lines from Scottish and Pictish kings. He had Manx blood. He claims descent in at least three lines from Alfred the Great, and so links up with Anglo-Saxon blood, but he links up also in several lines with Charlemagne and the Carlovingians. He sprang also from the Saxon Emperors of Germany, as well as from Barbarossa and the Hohenstaufens. He had Norwegian blood and much Norman blood. He had descent from the Dukes of Bavaria, of Saxony, of Flanders, the Princes of Savoy, and the Kings of Italy. He had the blood in his veins of Franks, Alamans, Merovingians, Burgundians, and Longobards. He sprang in direct descent from the Hun rulers of Hungary and the Greek Emperors of Constantinople. If I recollect rightly, Ivan the Terrible provides a Russian link. There is probably not one of the races of Europe concerned in the folk-wanderings which has not a share in the ancestry of Charles Darwin. If it has been possible in the case of one Englishman of this kind to show in a considerable number of lines how impure is his race, can we venture to assert that if the like knowledge were possible of attainment, we could expect greater purity of blood in any of his countrymen? What we are able to show may occur by tracing an individual in historic times, have we any valid reason for supposing did not occur in prehistoric times, wherever physical barriers did not isolate a limited section of mankind? If there ever was an association of definite mentality with physical characters, it would break down as soon as race mingled freely with race, as it has done in historic Europe. Isolation or a strong feeling against free inter-breeding—as in a colour differentiation—could alone maintain a close association between physical and mental characters. Europe has never recovered from the general hybridisation of the folk-wanderings, and it is only the cessation of wars of conquest and occupation, the spread of the conception of nationality and the reviving consciousness of race, which is providing the barriers which may eventually lead through isolation to a new linking-up of physical and mental characters.

In a population which consists of non-intermarrying castes, as in India, physique and external appearance may be a measure of the type of mentality. In the highly and recently hybridised nations of Europe there are really but few fragments of 'pure races' left, and it is

hopeless to believe that anthropometric measurements of the body or records of pigmentation are going to help us to a science of the psychophysical characters of man which will be useful to the State. The modern State needs in its citizens vigour of mind and vigour of body, but these are not characters with which the anthropometry of the past has largely busied itself. In a certain sense the school medical officer and the medical officer of health are doing more State service of an anthropological character than the anthropologists themselves.

These doubts have come very forcibly to my notice during the last few years. What were the anthropologists as anthropologists doing during the war? Many of them were busy enough and doing valuable work because they were anatomists, or because they were surgeons, or perhaps even because they were mathematicians. But as anthropologists, what was their position? The whole period of the war produced the most difficult problems in folk-psychology. There were occasions innumerable when thousands of lives and most heavy expenditure of money might have been saved by a greater knowledge of what creates and what damps folk-movements in the various races of the world. India, Egypt, Ireland, even our present relations with Italy and America, show only too painfully how difficult we find it to appreciate the psychology of other nations. We shall not surmount these difficulties until anthropologists take a wider view of the material they have to record, and of the task they have before them if they wish to be utile to the State. It is not the physical measurement of native races which is a fundamental feature of anthropometry to-day; it is the psychometry and what I have termed the vigorimetry of white- as well as of dark-skinned men that must become the main subjects of our study.

Some of you may consider that I am overlooking what has been contributed both in this country and elsewhere to the science of folk-psychology. I know at least that Wilhelm Wundt's [2] great work runs to ten volumes. But I also know that in its 5452 pages there is not a single table of numerical measurements, not a single statement of the *quantitative* association between mental racial characters, nor, indeed, any attempt to show numerically the intensity of association between folk-mentality and folk customs and institutions. It is folk-psychology in the same stage of evolution as present-day sociology is in, or as individual psychology was in before the advent of experimental psychology and the correlational calculus. It is purely descriptive and verbal. I am not denying that many sciences must for a long period still remain in this condition, but at the same time I confess myself a firm disciple of Friar Roger Bacon [3] and of Leonardo da Vinci,[4] and believe that we can really know very little

[2] Its last volume also bears evidence of the non-judicial mind of the writer, who expresses strong opinions about recent events in the language of the party historian rather than the man of science.

[3] He who knows not Mathematics cannot know any other science, and what is more cannot discover his own ignorance or find its proper remedies.

[4] Nissuna humana investigatione si po dimandare vera scientia s'essa non passa per le mathematiche dimostratione.

about a phenomenon until we can actually measure it and express its relations to other phenomena in quantitative form. Now you will doubtless suggest that sections of folk-psychology like Language, Religion, Law, Art—much that forms the substance of cultural anthropology—are incapable of quantitative treatment. I am not convinced that this standpoint is correct. Take only the first of these sections—*Language*. I am by no means certain that there is not a rich harvest to be reaped by the first man who can give unbroken time and study to the statistical analysis of language. Whether he start with roots or with words to investigate the degree of resemblance in languages of the same family, he is likely, before he has done, to learn a great deal about the relative closeness and order of evolution of cognate tongues, whether those tongues be Aryan or Sudanese. And the methods applicable in the case of language will apply in the same manner to cultural habits and ideas. Strange as the notion may seem at first, there is a wide field in cultural anthropology for the use of those same methods which have revolutionised psychometric technique, to say nothing of their influence on osteometry.

The problems of cultural anthropology are subtle, but so indeed are the problems of anthropometry, and no instrument can be too fine if our analysis is to be final. The day is past when the arithmetic of the kindergarten sufficed for the physical anthropologist; the day is coming when mere verbal discussion will prove inadequate for the cultural anthropologist.

I do not say this merely in the controversial spirit. I say it because I want to find a remedy for the present state of affairs. I want to see the full recognition of anthropology as a leading science by the State. I want to see the recognition of anthropology by our manufacturers and commercial men, for it should be at least as important to them as chemistry or physics—the foundations of the Anthropological Institutes with their museums and professors in Hamburg and Frankfurt have not yet found their parallels in commercial centres here. I want to see a fuller recognition of anthropology in our great scientific societies, both in their choice of members and in the memoirs published. If their doors are being opened to psychology under its new technique, may not anthropology also seek for fuller recognition?

It appears to me that if we are to place anthropology in its true position as the queen of the sciences, we must work shoulder to shoulder and work without intermittence in the following directions: anthropologists must not cease:

(i) To insist that our recorded material shall be such that it is at present or likely in the near future to be utile to the State, using the word 'State' in its amplest sense.

(ii) To insist that there shall be institutes of anthropology, each with a full staff of qualified professors, whose whole energy and time shall be devoted to the teaching of and research in anthropology, ethnology and prehistory. At least three of our chief universities should be provided with such institutes.

(iii) To insist that our technique shall not consist in the mere statement of opinion on the facts observed, but shall follow, if possible

with greater insight, the methods which are coming into use in epidemiology and psychology.

I should like to enlarge a little further on these three insistencies, the fundamental ' planks ' of the campaign I have in view.

(i) *Insistence on the Nature of the Material to be dealt with.*

I have already tried to indicate that the problems before us to-day, the grave problems that are pressing on us with regard to the future, cannot be solved by the old material and by the old methods. We have to make anthropology a wise counsellor of the State, and this means a counsellor in political matters, in commercial matters, and in social matters.

The Governments of Europe have had military advisers, financial advisers, transport and food experts in their service, but they have not had ethnological advisers ; there have been no highly trained anthropologists at their command. You have only to study the Peace of Versailles to see that it is ethnologically unsound and cannot be permanent. It is no good asking why our well-meaning rulers did not consult our well-meaning anthropologists. I for one confess that we have not in the past dealt with actuality, or if we did deal with actuality, we have not treated it in a manner likely to impress either the executive or the public at large. There lacked far too largely the scientific attitude and the fundamental specialist training. I will not go so far as to say that, if the science of man had been developed to the extent of physical science in all European countries, and had then had its due authority recognised, there would have been no war, but I will venture to say that the war would have been of a different character, and we should not have felt that the fate of European society and of European culture hung in the balance, as at this moment they certainly do.

No one can allow individual inspiration to-day, and you may justly cry a Daniel has no right to issue judgment from the high seat of the feast. Daniel's business is that of the outsider, the stranger, the unwelcome person interpreting, probably his own, scrawling on the wall.

Well, if it be hard to learn from friends, let us at least study impassionately from our late foes. Some of my audience may have read the recent manifesto of the German anthropologists, their clarion cry for a new and stronger position of the science of man in academic studies. But the manifesto may have escaped some, and so closely does it fit the state of affairs here that I venture to quote certain portions of it. After reciting the sparsity of chairs for the study of physical and cultural anthropology in the German universities and how little academic weight has been given to such studies, it continues : ' Where these sciences have otherwise found recognition in the universities, they are not represented by specialists, so that anthropology is provided for by the anatomists, ethnology by the geographers, and prehistory by Germanists, archæologists and geologists, and this although, in the present extent of these three sciences, the real command of each one of them demands the complete working powers of an individual. This want

of teaching posts had made itself felt long before the war, so that the number of specialists and of those interested in our science has receded.' [5]

And again:

'During the war we have often experienced how in political pamphlets ethnology and ethnography—even as in the peace treaty of Brest-Litovsk—were used too often as catchwords without their users being clear about the ideas those words convey. The sad results of our foreign policy, the collapse of all our calculations as to national frames of mind, were based in no small degree on ethnographic ignorance; one has only to take for example the case of the Turks. Ethnology should not embrace only the spears and clubs of the savages, but also the psychology and demography of the white races, the European peoples. At this very moment, when the right of self-determination has become a foremost question of the day, the scientific determination of the boundaries of a people and its lands has become a task of the first importance. But our Government of the past knew nothing of the activity of the ethnologists, and the Universities were not in the condition to come to their aid, for ethnological chairs and institutes were wanting. The foundation of such must be the task of the immediate future.' [6]

And once more:

'The problems of the military fitness of our people, of the physical constitution of the various social classes, of the influence of the social and material environment upon them, the problems of the biological grounds for the fall in the birth-rate and its results, of the racial composition of our people, of the eventual racial differences and the accompanying diverse mental capacities of the individual strata, and finally the racial changes which may take place in a folk under the influences of civilisation, and the bearing of all these matters on the fate of a nation, these are problems which can alone be investigated and brought nearer to solution by anthropology. Even now after the war population-problems stand in the forefront of interest, the question of folk-increase and of the falling birth-rate is the vital question of the future.' [7]

I must ask your pardon for quoting so much, but it seems so strongly to point the moral of my tale. If you will study what Germany is feeling and thinking to-day do not hope to find it in the newspaper reports, seek it elsewhere in personal communication or in German writings. Then, I think, you will agree with me that rightly or wrongly there is a conviction spreading in Germany that the war arose and that the war was lost because a nation of professed thinkers had studied all sciences, but had omitted to study aptly the science of man. And in a certain sense that is an absolutely correct conviction, for if the science of man stood where we may hope it will stand in the dim and distant future, man would from the past and the surrounding present have some grasp of future evolution, and so have a greater chance of guiding its controllable factors.

[5] *Correspondenz Blatt*, u.s.w., Jahrg. l., S. 37.
[6] *Ibid*. S. 41.
[7] *Ibid*. S. 38.

We are far indeed from that to-day; but it befits us none the less to study what this new anthropological movement in Germany connotes. It means that the material of anthropology is going to change, or rather that its observations will be extended into a study of the mental as well as the physical characters, and these of the white races as well as of the dark. It means that anthropologists will not only study individual psychology, but folk-psychology. It means—and this is directly said—that Germany, having lost her colonies, will still maintain her trade by aid of consuls, missionaries, traders, travellers, and others trained academically to understand both savage and civilised peoples. This is a perfectly fair field, and if the game be played squarely can solely lead to increased human sympathy, and we shall only have ourselves to blame if other nations are before us in their anthropological knowledge and its practical applications. The first condition for State support is that we show our science to be utile by turning to the problems of racial efficiency, of race-psychology, and to all those tasks that Galton described as the first duty of a nation—'in short, to make every individual efficient both through Nature and by Nurture.'

Does this mean that we are to turn our backs on the past, to desert all our prehistoric studies and to make anthropology the servant of sanitation and commerce? Not in the least; if you think this is my doctrine I have indeed failed to make myself even roughly clear to-day. Such teaching is wholly opposed to my view of the function of science. I feel quite convinced that you cannot understand man of to-day, savage or civilised, his body or his mind, unless you know his past evolution, unless you have studied fully all the scanty evidence we have of the stages of his ascent. I should like to illustrate this by an incident which came recently to my notice, because it may indicate to some of those present the difficulties with which the anthropologist has to contend to avoid misunderstanding.

Looking into the ancestry of man and tracing him backward to a being who was not man and was not ape, had this prot-simio-human, in the light of our present knowledge, more resemblance to the gibbon or to the chimpanzee as we know them to-day? Some naturalists link man up to the apes by a gibbonlike form, others by a troglodyte type of ancestor. Some would make a push to do without either. But granted the alternative, which is the more probable? This is the problem of the hylobatic or the troglodyte origin of man. I had given a lecture on the subject, confining my arguments solely to characters of the thigh-bone. Now there chanced to be a statesman present, a man who has had large responsibilities in the government of many races. I have been honoured by seeing his comments on my lecture. 'I am not,' he says, 'particularly interested in the descent of man. I do not believe much in heredity, and this scientific pursuit of the dead bones of the past does not seem to me a very useful way of spending life. I am accustomed to this mode of study; learned volumes have been written in Sanscrit to explain the conjunction of the two vowels "a" and "u." It is very learned, very ingenious, but not very helpful. . . . I am not concerned with my genealogy so much as with my future. Our intellects can be more advantageously employed than in

finding our diversity from the ape . . .   There may be no spirit, no
soul : there is no proof of their existence.   If that is so, let us do
away with shams and live like animals.   If, on the other hand, there
is a soul to be looked after, let us all strain our nerves to the task;
there is no use in digging into the sands of time for the skeletons of
the past : build your man for the future.'

What is the reply of anthropology to this indictment of the states-
man?   You cannot brush it lightly aside.   It is the statement of a
good man and a strong man who is willing to spend his life in the service
of his fellows.   He sees us handling fossils and potsherds and cannot
perceive the social utility of our studies.   He does not believe any
enthusiasm for human progress can lie beneath the spade and callipers
of the scientific investigator.   He has never grasped that the man of
to-day is precisely what heredity and his genealogy, his past history
and his prehistory, have made him.   He does not recognise that it is
impossible to build your man for the future until you have studied the
origin of his physical and mental constitution.   Whence did he draw
his good and evil characteristics—are they the product of his nature or
his nurture?   Man has not a plastic mind and body which the enthu-
siastic reformer can at will mould to the model of his golden age ideals.
He has taken thousands of years to grow into what he is, and only by
like processes of evolution—intensified and speeded up, if we work
consciously and with full knowledge of the past—can we build his
future.

It *does* matter in regard to the gravest problems before mankind
to-day whether our ancestry was hylobatic or troglodyte.   For five years
the whole world has been a stage for brutality and violence.   We have
seen a large part of the youth who were best fitted mentally and
physically to be parents of future generations perish throughout Europe :
the dysgenic effect of this slaughter will show itself each twenty to
twenty-five years for centuries to come in the census returns of half
the countries of the world.   Science undertook work which national
feeling bade it do, but on which it will ever look back with a shuddering
feeling of distaste, an uneasy consciousness of having soiled its hands.
And as aftermath we see in almost every land an orgy of violent crime,
a sense of lost security, and at times we dread that our very civilisation
may perish owing to the weakening of the social ties, a deadening of
the responsibilities of class to class.   This outbreak of violence which
has so appalled the thinking world, is it the sporadic appearance of an
innate passion for the raw and brutal in mankind, or is it the outcome
of economic causes forcing the nations of the world to the combat for
limited food and material supplies?   I wish we could attribute it to
the latter source, for then we could eradicate the spirit of violence by
changing environmental conditions.   But if the spirit of violence be
innate in man, if there be times when he not only sees red but rejoices
in it—and that was the strong impression I formed when I crossed
Germany on August 1, 1914—then outbreaks of violence will not cease
till troglodyte mentality is bred out of man.   That is why the question
of troglodyte or hylobatic ancestry is not a pursuit of dead bones.   It
is a vital problem on which turns much of folk-psychology.   It is a

problem utile to the State, in that it throws light on whether nature or nurture is the more likely to build up man's future—and save him from the recurrence of such another quinquennium.

The critic to whom I have referred was not idle in his criticism. He had not been taught that evolutionary doctrine has its bearings on practical life. The biologist and the anthropologist are at fault; they have too often omitted to show that their problems have a very close relation to those of the statesman and the social reformer, and that the problems of the latter cannot be solved without a true insight into man's past, without a knowledge of the laws of heredity, and without a due appreciation of the causes which underlie great folk-movements.

(ii) *Insistence on Institutes of Anthropology.*

The anthropological problems of the present day are so numerous and so pressing that we can afford to select those of the greatest utility. Indeed, the three university institutes of anthropology I have suggested would have to specialise and then work hard to keep abreast of the problems which will crowd upon them. One might take the European races, another Asia and the Pacific, and a third Africa. America in anthropology can well look after itself. In each case we need something on the scale of the Paris Ecole d'Anthropologie, with its seventeen professors and teachers, with its museums and journals. But we want something else—a new conception of the range of problems to be dealt with and a new technique. From such schools would pass out men with academic training fit to become officials, diplomatic agents, teachers, missionaries, and traders in Europe, in Asia, or in Africa, men with intelligent appreciation of and sympathy with the races among whom they proposed to work.

But this extra-State work, important as it is, is hardly comparable in magnitude with the intra-State work which lies ready to hand for the anthropological laboratory that has the will, the staff, and the equipment to take it up efficiently. In the present condition of affairs it is only too likely that much of this work, being psychometric, will fall into the hands of the psychologist, whereas it is essentially the fitting work of the anthropologist, who should come to the task, it fitly trained, with a knowledge of comparative material and of the past history, mental and physical, of mankind, on which his present faculties so largely depend. The danger has arisen because the anthropometer has forgotten that it is as much his duty to measure the human mind as it is his duty to measure the human body, and that it is as much his duty to measure the functional activities of the human body—its dynamical characters—as its statical characters. By dynamical characters I understand such qualities as resistance to fatigue, facility in physical and mental tasks, immunity to disease, excitability under stimuli, and many kindred properties. If you tell me that we are here trenching on the field of psychology and medicine, I reply: Certainly; you do not suppose that any form of investigation which deals with man—body or mind—is to be omitted from the science of man? If you do you have failed to grasp why anthropology is the queen of the sciences. The University anthropological institute of the future will

have attached to it a psychologist, a medical officer, and a biologist. They are essential portions of its requisite staff, but this is a very different matter from lopping off large and important branches of its fitting studies, to lie neglected on the ground, or to be dragged away, as dead wood, to be hewn and shapen for other purposes by scientific colleagues in other institutes. Remember that I am emphasising that side of anthropology which studies man in the service of the State—anthropology as a utile science—and that this is the only ground on which anthropology can appeal for support and sympathy from State, from municipality, and from private donors. You will notice that I lay stress on the association of the anthropological institute with the university, and the reasons for this are manifold. In the first place, every science is stimulated by contact with the workers in allied sciences; in the second place, the institute must be a teaching as well as a researching body, and it can only do this effectively in association with an academic centre—a centre from which to draw its students and to recruit its staff. In the third place, a great university provides a wide field for anthropometric studies in its students and its staff. And the advantages are mutual. It is not of much service to hand a student a card containing his stature, his weight, his eye colour, and his head length! Most of these he can find out for himself! But it is of importance to him to know something of how his eye, heart, and respiration function; it is of importance to him to know the general character of his mental qualities, and how they are associated with the rapidity and steadiness of muscular responses. Knowledge on these points may lead him to a fit choice of a career, or at any rate save him from a thoroughly bad choice.

In the course of my life I have often received inquiries from schoolmasters of the following kind: We are setting up a school anthropometric laboratory, and we propose to measure stature, weight, height sitting, &c. Can you suggest anything else we should measure?

My invariable reply is: Don't start measuring anything at all until you have settled the problems you wish to answer, and then just measure the characters in an adequate number of your boys, which will enable you to solve those problems. Use your school as a laboratory, not as a weighhouse.

And I might add, if I were not in dread of giving offence: And most certainly do not measure anything at all if you have *no* problem to solve, for unless you have you cannot have the true spirit of the anthropologist, and you will merely increase that material up and down in the schools of the country which nobody is turning to any real use.

Which of us, who is a parent, has not felt the grave responsibility of advising a child on the choice of a profession? We have before us, perhaps, a few meagre examination results, an indefinite knowledge of the self-chosen occupations of the child, and perhaps some regard to the past experience of the family or clan. Possibly we say John is good with his hands and does not care for lessons; therefore he should be an engineer. That may be a correct judgment if we understand by engineer, the engine-driver or mechanic. It is not true if we think of the builders of Forth Bridges and Assuan Dams. Such men work

with the head and not the hand. One of the functions of the anthropological laboratory of a great university, one of the functions of a school anthropometric laboratory, should be to measure those physical and mental characters and their inter-relations upon which a man's success in a given career so much depends. Its function should be to guide youth in the choice of a calling, and in the case of a school to enable the headmaster to know something of the real nature of individual boys, so that that much-tried man does not feel compelled to hide his ignorance by cabalistic utterances when parents question him on what their son is fitted for.

Wide, however, as is the anthropometric material in our universities and public schools, it touches only a section of the population. The modern anthropologist has to go further; he has to enter the doors of the primary schools; he has to study the general population in all its castes, its craftsmen, and its sedentary workers. Anthropology has to be useful to commerce and to the State, not only in association with foreign races, but still more in the selection of the right men and women for the staff of factory, mine, office, and transport. The selection of workmen to-day by what is too often a rough trial and discharge method is one of the wasteful factors of production. Few employers even ask what trades parents and grandparents have followed, nor consider the relation of a man's physique and mentality to his proposed employment. I admit that progress in this direction will be slow, but if the work undertaken in this sense by the anthropologist be well devised, accurate, and comprehensive, the anthropometric laboratory will gradually obtain an assured position in commercial appreciation. As a beginning, the anthropologist by an attractive museum, by popular lectures and demonstrations, should endeavour to create, as Sir Francis Galton did at South Kensington, an anthropometric laboratory frequented by the general population, as well as by the academic class. Thus he will obtain a wider range of material. But the anthropologist, if he is to advance his science and emphasise its services to the State, must pass beyond the university, the school, and the factory. He must study what makes for wastage in our present loosely organised society; he must investigate the material provided by reformatory, prison, asylums for the insane and mentally defective; he must carry his researches into the inebriate home, the sanatorium, and the hospital, side by side with his medical collaborator. Here is endless work for the immediate future, and work in which we are already leagues behind our American colleagues. For them the psychometric and anthropometric laboratory attached to asylum, prison, and reformatory is no startling innovation, to be spoken of with bated breath. It is a recognised institution of the United States to-day, and from such laboratories the 'fieldworkers' pass out, finding out and reporting on the share parentage and environment have had in the production of the abnormal and the diseased, of the anti-social of all kinds. Some of this work is excellent, some indifferent, some perhaps worthless, but this will always be the case in the expansion of new branches of applied science. The training of the workers must be largely of an experimental character, the technique has to be devised as the work develops. Instructors and directors have to be

appointed, who have not been trained *ad hoc*. But this is remedying itself, and if indeed, when we start, we also do not at first limp somewhat lamely along these very paths, it will only be because we have the advantage of American experience.

There is little wonder that in America anthropology is no longer the stepchild of the State. It has demanded its heritage, and shown that it can use it for the public good.

If I have returned to my first insistence that the problems handled by the anthropologist shall be those useful to the State, it is because I have not seen that point insisted upon in this country, and it is because my first insistence, like my third, involves the second for its effectiveness—the establishment in our chief universities of anthropological institutes. As Gustav Schwalbe said of anthropology in 1907 —and he was a man who thought before he spoke, and whose death during the war is a loss to anthropologists the whole world over—' a lasting improvement can only arise if the State recognises that anthropology is a science pre-eminently of value to the State, a science which not only deserves but can demand that chairs shall be officially established for it in every university. . . Only this spread of officially authorised anthropology in all German universities can enable it to fulfil its task, that of training men who, well armed with the weapon of anthropological knowledge, will be able to place their skill at the service of the State, which will ever have need of them in increasing numbers.' [8]

Our universities are not, as in Germany, Government-controlled institutions, although such control is yearly increasing. Here we have first to show that we are supporting the State before the State somewhat grudgingly will give its support to us. Hence the immediate aim of the anthropologist should be—not to suggest that the State should *a priori* assist work not yet undertaken, but to do what he can with the limited resources in his power, and when he has shown that what he has achieved is, notwithstanding his limitations, of value to the State, then he is in a position to claim effective support for his science.

I have left myself little time to place fairly before you my third insistence.

(iii) *Insistence on the Adoption of a New Technique.*

What is it that a young man seeks when he enters the university— if we put aside for a moment any social advantages, such as the formation of lifelong friendships associated therewith? He seeks, or ought to seek, training for the mind. He seeks, or ought to seek, an open doorway to a calling which will be of use to himself, and wherein he will take his part, a useful part, in the social organisation of which he finds himself a member. Much as we may all desire it, in the pressure of modern life, it is very difficult for the young man of moderate means to look upon the university training as something apart from his professional training. Men more and more select their academic studies with a view to their professional value. We can no longer combine the senior wranglership with the pursuit of a judgeship; we cannot

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[8] *Correspondenz Blatt*, Jahrg. xxxviii., S. 68.

pass out in the classical tripos and aim at settling down in life as a Harley Street consultant; we cannot take a D.Sc. in chemistry as a preliminary to a journalistic career. It is the faculties which provide professional training that are crowded, and men study nowadays physics or chemistry because they wish to be physicists or chemists, or seek by their knowledge of these sciences to reach commercial posts. Even the very Faculty of Arts runs the danger of becoming a professional school for elementary school teachers. I do not approve this state of affairs; I would merely note its existence. But granted it, what does anthropology offer to the young man who for a moment considers it as a possible academic study? There are no professional posts at present open to him, and few academic posts.[9] There is little to attract the young man to anthropology as a career. Is its position as a training of mind any stronger? The student knows if he studies physics or chemistry or engineering that he will obtain a knowledge of the principles of observation, of measurement, and of the interpretation of data, which will serve him in good stead whenever he has to deal with phenomena of any kind. But, alas! in anthropology, while he finds many things of surpassing interest, he discovers no generally accepted methods of attacking new problems, *quot homines, tot sententiæ*. The type of man we want in anthropology is precisely the man who now turns to mathematics, to physics, and to astronomy—the man with an exact mind who will not take statements on authority and who believes in testing all things. To such a man anthropometry—in all its branches, craniometry, psychometry, and the wide field in which body and mind are tested together under dynamic conditions—forms a splendid training, *provided* his data and observations are treated as seriously as those of the physicist or astronomer by adequate mathematical analysis. Such a type of man is at once repelled from our science if he finds in its text-books and journals nothing but what has been fitly termed ' kindergarten arithmetic.' Why, the other day I saw in a paper by a distinguished anthropologist an attempt to analyse how many individual bones he ought to measure. He adopted the simple process of comparing the results he obtained when he took 10, 20, 30 individuals. He was not really wiser at the end of his analysis than at the beginning, though he thought he was. And this, notwithstanding that the whole matter had been thrashed out scientifically by John Bernoulli two centuries ago, and that its solution is a commonplace of physicist and astronomer !

How can we expect the scientific world to take us seriously and to treat anthropology as the equal of other sciences while this state of affairs is possible? What discipline in logical exactness are we offering to academic youth which will compare with that of the older sciences? What claim have we to advise the State until we have introduced a sounder technique and ceased to believe that anthropometry is a science that any man can follow, with or without training? As I have hinted, the problems of anthropology seem to me as subtle as

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[9] In London, for example, there is a reader in physical anthropology who is a teacher in anatomy, and a professorship in ethnology, which for some mysterious reason is included in the faculty of economics and is, I believe, not a full-time appointment.

those of physical astronomy, and we are not going to solve them with rusty weapons, nor solve them at all unless we can persuade the ' brainy boys ' of our universities that they are worthy of keen minds. Hence it seems to me that the most fertile training for academic purposes in anthropology is that which starts from anthropometry in its broadest sense, which begins to differentiate caste and class and race, bodily and mental health and disease, by measurement and by the analysis of measurement. Once this sound grounding has been reached the trained mind may advance to ethnology and sociology, to prehistory and the evolution of man. And I shall be surprised if equal accuracy of statement and equal logic of deduction be not then demanded in these fields, and I am more than half convinced, nay, I am certain, that the technique the student will apply in anthropometry can be equally well applied in the wider fields into which he will advance in his later studies. Give anthropology a technique as accurate as that of physics, and it will forge ahead as physics have done, and then anthropologists will take their due place in the world of science and in the service of the State.

Francis Galton has a claim upon the attention of anthropologists which I have not. He has been President of your Institute, and he spoke just thirty-five years ago from the chair I now occupy, pressing on you for the first time the claims of new anthropological methods. In Galton's words : ' Until the phenomena of any branch of knowledge have been submitted to measurement and number it cannot assume the status and dignity of a Science.' Have we not rather forgotten those warning words, and do they not to some extent explain why our universities and learned societies, why the State and statesmen, have turned the cold shoulder on anthropology?

This condition of affairs must not continue; it is good neither for anthropology, nor for the universities, nor for the State if this fundamental science, the science of man, remains in neglect. It will not continue if anthropologists pull together and insist that their problems shall not fail in utility, that their scientific technique shall be up to date, and that anthropological training shall be a reality in our universities—that these shall be fully equipped with museums, with material, with teachers and students.

It is almost as difficult to reform a science as it is to reform a religion ; in both cases the would-be reformer will offend the sacrosanct upholders of tradition, who find it hard to discard the faith in which they have been reared. But it seems to me that the difficulties of our time plead loudly for a broadening of the purpose and a sharpening of the weapons of anthropology. If we elect to stand where we have done a new science will respond to the needs of State and Society ; it will spring from medicine and psychology, it will be the poorer in that it knows little of man's development, little of his history or prehistory. But it will devote itself to the urgent problems of the day. The future lies with the nation that most truly plans for the future, that studies most accurately the factors which will improve the racial qualities of future generations either physically or mentally. Is anthropology to lie outside this essential function of the science of

man? If I understand the recent manifesto of the German anthropologists, they are determined it shall not be so. The war is at an end, but the critical time will be with us again, I sadly fear, in twenty to thirty years. How will the States of Europe stand then? It depends to no little extent on how each of them may have cultivated the science of man and applied its teaching to the improvement of national physique and mentality. Let us take care that our nation is not the last in this legitimate rivalry. The organisation of existing human society with a view to its future welfare is the crowning task of the science of man; it needs the keenest-minded investigators, the most stringent technique, and the utmost sympathy from all classes of society itself. Have we, as anthropologists, the courage to face this greatest of all tasks in the light of our knowledge of the past and with our understanding of the folk of to-day? Or shall we assert that anthropology is after all only a small part of the science of man, and retreat to our study of bones and potsherds on the ground that science is to be studied for its own sake and not for the sake of mankind? I do not know what answer you will give to that question, yet I am convinced what the judgment of the future on your answer is certain to be. It will be similar to Wang Yang Ming's reproof of the complacency of the Chinese cultured classes of his day: 'Thought and Learning are of little value, if they be not translated into Action.'

ADDRESS

TO THE

PHYSIOLOGICAL SECTION

BY

J. BARCROFT, C.B.E., M.A., F.R.S.,

PRESIDENT OF THE SECTION.

PROMINENT among the pathological conditions which claimed attention during the war was that of insufficient oxygen supply to the tissues, or anoxæmia. For this there were several reasons; on the one hand, anoxæmia clearly was a factor to be considered in the elucidation of such conditions as are induced by gas-poisoning, shock, &c. On the other hand, knowledge had just reached the point at which it was possible to discuss anoxæmia on a new level. It is not my object in the present address to give any account of war-physiology—the war has passed, and I, for one, have no wish to revive its memories, but anoxæmia remains, and, as it is a factor scarcely less important in peace than in war pathology, I think I shall not do wrong in devoting an hour to its consideration.

The object of my address, therefore, will be to inquire, and, if possible to state, where we stand; to sift, if I can, the knowledge from the half-knowledge; to separate what is ascertained as the result of unimpeachable experiment from what is but guessed on the most likely hypothesis. In war it was often necessary to act on defective information, because action was necessary and defective information was the best that was to be had. In this, as in many other fields of knowledge, the whole subject should be reviewed, the hypotheses tested experimentally, and the gaps filled in. The sentence which lives in my mind as embodying the problem of anoxæmia comes from the pen of one who has given more concentrated thought to the subject than perhaps any other worker—Dr. J. S. Haldane.[1] It runs, ‘Anoxæmia not only stops the machine but wrecks the machinery.’ This phrase puts the matter so clearly that I shall commence by an inquiry as to the limits within which it is true.

Anything like complete anoxæmia stops the machine with almost incredible rapidity. It is true that the breath can be held for a considerable time, but it must be borne in mind that the lungs have a volume of about three litres at any moment, that they normally contain about half a litre of oxygen, and that this will suffice for the body at rest for upwards of two minutes. But get rid of the residual oxygen from the lungs only to the very imperfect extent which is

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[1] See References, page 168.

possible by the breathing of some neutral gas, such as nitrogen, and you will find that only with difficulty will you endure half a minute. Yet even such a test gives no real picture of the impotence of the machine —which is the brain—to ' carry on ' in the absence of oxygen. For, on the one hand, nearly a quarter of a minute elapses before the reduced blood gets to the capillary in the brain, so that the machine has only carried on for the remaining quarter of a minute; on the other hand, the arterial blood under such circumstances is far from being completely reduced—in fact, it has very much the composition of ordinary venous blood, which means that it contains about half its usual quotum of oxygen. It seems doubtful whether complete absence of oxygen would not bring the brain to an instantaneous standstill. So convincing are the experimental facts to anyone who has tested them for himself that I will not further labour the power of anoxæmia to stop the machine. I will, however, say a word about the assumption which I have made that the machine in this connection is the brain.

It cannot be stated too clearly that anoxæmia in the last resort must affect every organ of the body directly. Stoppage of the oxygen supply is known, for instance, to bring the perfused heart to a standstill, to cause a cessation of the flow of urine, to produce muscular fatigue, and at last immobility, but from our present standpoint these effects of anoxæmia seem to me to be out of the picture, because the brain is so much the most sensitive to oxygen want. Therefore, if the problem is the stoppage of the machine due to an insufficient general supply of oxygen, I have little doubt that the machine stops because the brain stops, and here at once I am faced with the question how far is this assumption and how far is it proven fact? I freely answer that research in this field is urgent; at present there is too great an element of assumption, but there is also a certain amount of fact.

To what extent does acute anoxæmia in a healthy subject wreck the machinery as well as stop the machine? By acute anoxæmia I mean complete or almost complete deprivation of oxygen which, in the matter of time, is too short to prove fatal. It is not easy to obtain quite clear-cut experiments in answer to the above question. No doubt many data might be quoted of men who have recovered from drowning, &c. Such data are complicated by the fact that anoxæmia has only been a factor in their condition; other factors, such as accumulation of carbonic acid, may also have contributed to it. The remarkable fact about most of them, however, is the slightness of the injury which the machine has suffered. These data, therefore, have a value in so far as they show that a very great degree of anoxæmia, if acute and of short duration, may be experienced with but little wreckage to the machine. They have but little value in showing that such wreckage is due to the anoxæmia, because the anoxæmia has not been the sole disturbance. Of such cases I will quote two.

The first is that of a pupil of my own who received a gunshot wound in the head, to the prejudice of his cerebral circulation. I can give you the most perfect assurance that neither intellectually nor physically has the catastrophe which befell him caused any permanent injury. For the notes of the case I am indebted to

Colonel Sir William Hale White. My pupil fell wounded at 6.50 A.M. on 20th of November 1917. As far as is known, he lay insensible for about two hours. Picked up five hours after the wound, he could not move either upper or lower extremity. Thirty-six hours after the wound he underwent an operation which showed that the superior sagittal sinus was blocked, happily not by a thrombus, but by being torn and having pieces of the inner table of the skull thrust into it, so that for this period of time the motor areas on both sides down to the face area were asphyxiated, the right being much more affected than the left. Six days after the wound the cerebration was still slow, with typically vacant look.

In the left upper extremity the muscular power was much improved; he could raise his arm to his mouth, but he preferred to be fed.

In the right upper extremity there were no movements in the shoulder, elbow, or wrist, but he could flex and extend the fingers weakly.

In the left lower extremity there were fair movements of hip and knee, no movements of ankle and toes.

In the right lower extremity there were no movements of hip, knee, ankle, or toes.

Six weeks after the wound he first walked, although with difficulty; absolutely the last place in which the paresis remained was in the muscles of the right toes. Four months after the injury these toes were spread out and could not be brought together voluntarily. Gradually this has become less, but even now, two and a-half years later, all these muscles are weaker than those on the other side.

Such, then, is the wreckage of the machine caused by thirty-six hours' blockage of the blood-flow through the motor areas of the brain; wreckage enough but not irreparable.

I pass to the second case. It is that of the child of a well-known Professor of Physiology and a first authority on the subject of respiration. I am indebted to him for the following notes:

In this child, a male twin, born about twelve weeks too early, it was noticed about twenty-four hours after birth that the breathing during sleep was irregular and of a very pronounced Cheyne Stokes type, six to ten breaths being followed by a pause during which no respiratory movement occurred. Usually the pauses were of fifteen to twenty seconds' duration, but occasionally (two to ten times a day) the breathing remained suspended for a prolonged period, extending in some cases to ten and fifteen minutes, and in one accurately observed case even to twenty-five minutes, interrupted by one single breath and a cry about the middle of the period. The breathing invariably started again before the heart-beats ceased.

Cases in which the anoxæmia has been uncomplicated are to be found among those who have been exposed to low atmospheric pressures; for instance, balloonists and aviators. Of these quite a considerable number have suffered from oxygen want to the extent of being unconscious for short intervals of time.

No scientific observer has pushed a general condition of anoxæmia either on himself or on his fellows to the extent of complete unconscious-

ness. The most severe experiments of this nature are those carried out by Haldane and his colleagues. One experiment in particular demands attention. Dr. Haldane and Dr. Kellas [2] together spent an hour in a chamber in which the air was reduced to between 320 and 295 mm. It is difficult to say how far they were conscious. Clearly each believed the other to be complete master of his own faculties, but it is evident that Dr. Haldane was not so. I gather that he has no recollection of what took place, that whenever he was consulted about the pressure he gave a stereotyped answer which was the same for all questions, that, even with a little more oxygen present, he was sufficiently himself to wish to investigate the colour of his lips in the glass, but insufficiently himself to be conscious that he was looking into the back and not the front of the mirror. Dr. Kellas, who could make observations, never discovered Dr. Haldane's mental condition, though boxed up with him for an hour, and went on consulting him automatically. A somewhat similar experiment was performed on the other two observers, with results differing only in degree.

Yet the after-effects are summed up in the following sentence: ' All four observers suffered somewhat from headache for several hours after these experiments, but there was no nausea or loss of appetite.'

Of real importance in this connection are the results of carbon monoxide poisoning. Of these a large number might be cited. Those interested will find some very instructive cases described in a volume entitled ' The Investigation of Mine Air,' by the late Sir C. Le Neve Foster and Dr. Haldane. [3] The cases in question were those of a number of officials who went to investigate the mine disaster on Snaefell, in the Isle of Man, in May 1897. Of the five cases cited all suffered some after-effects, by which I mean that by the time the blood was restored sufficiently to its normal condition for the tissues to get the amount of oxygen which they required, the effects of the asphyxia had not passed off and to this extent the machine suffered, *e.g.*

Mr. W. H. Kitto says : ' On reaching *terra firma* I felt very ill and wanted to vomit . . . through the night. I had severe palpitation of the heart, a thing I have never felt in my life before or since. On the day following, Thursday, the pain in my knees was so great that I could not stand properly, and for fully a week I had great pain when walking, and still (a month later) feel slight effects of the poisoning.'

Of the five whose experiences were given, the one who received the most permanent damage was Sir Clement Foster himself.

A few days after I got back from the island the first time, about the 21st or 22nd of May, I noticed my heart; it could scarcely be called palpitation, as I understand palpitations to be, for there did not seem to be any increased rapidity of the action, but I was conscious of its beating; as a rule, I am not. This passed off, and then on the 1st and 2nd of June I noticed it very decidedly again, so much so that I went to my doctor. He sounded me, and said the heart was all right, though there was one sound which was not very distinct. This consciousness of having a heart still returns from time to time, though only to a slight extent. On the 19th May I suffered much from headache, not regularly, but intermittently. The headache lasted for several days, and the feeling in the legs was very apparent; it was an aching in the legs from the knees to the ankles. A coldness from the knees to the soles of the feet was also noticeable; it came on occasionally for a considerable time. The

headaches continued at intervals for some time, and lasted certainly for some months after the accident; indeed, I cannot say that they have disappeared altogether.

To sum up, then, what may be said of the permanent damage caused by acute anoxæmia, it seems to me to be as follows: No degree of anoxæmia which produces a less effect than that of complete unconsciousness leaves anything more than the most transient effects; if the anoxæmia be pushed to the point at which the subject is within a measurable distance of death, the results may take days or weeks to get over, but only in the case of elderly or unsound persons is the machine wrecked beyond repair.

Chronic Anoxæmia.

And now to pass to the consideration of what I may call chronic anoxæmia; that is to say, oxygen want which perhaps is not very great in amount—and I shall have to say something later about the measurement of anoxæmia in units—but which is continued over a long time: weeks, months, or perhaps years. We have to ask ourselves, how far does chronic anoxæmia stop the machinery; how far does it wreck the machine? Here we are faced with a much more difficult problem, for the distinction between stopping the machinery and wrecking the machine tends to disappear. In fact so indistinct does it become that you may ask yourself, with some degree of justice, does chronic anoxæmia stop the machine in any other way than by wrecking it?

The most obvious instances of men subject to chronic anoxæmia are the dwellers at high altitudes. Now, it is quite clear that in many instances of mountain-dwellers the anoxæmia does not wreck the machine. On what I may call the average healthy man anoxæmia begins to tell at about 18,000 feet. At lower altitudes no doubt he will have some passing trouble, but it seems to me from my own experience that this altitude is a very critical one. Yet there are mining camps at such heights in South America at which the work of life is carried on. Under such circumstances the machine is kept going by a process of compensation. This is in part carried out by a modification in the chemical properties of the blood. It would appear that both the carbonic acid in the blood and the alkali diminish. The result, according to my interpretation of my own observations on the Peak of Teneriffe, which appeared to be confirmed by the experiments in a chamber in Copenhagen [4] from which the air was partially exhausted, was this: The hydrogen ion concentration of the blood increased slightly, the respiratory centre worked more actively, and the lung became better ventilated with oxygen, with the natural result that the blood became more oxygenated than it would otherwise have been.

The difference which this degree of acclimatisation made was very great. Take as an example one of my colleagues, Dr. Roberts. On Monte Rosa in his case, as in that of the rest of the party, 15 mm. of oxygen pressure were gained in the lungs. To put the matter another way, the amount of oxygen in our lungs at the summit was what it would otherwise have been 5,000 or 6,000 feet lower down. No actual analyses of the oxygen in Roberts' arterial blood were made, but from what we now know it seems probable that his blood was about 82 per

cent. saturated; that is to say, that for every hundred grams of hæmoglobin in the blood 82 were oxyhæmoglobin and 18 reduced hæmoglobin; had this degree of acclimatisation not taken place his blood would have contained as many as 38 parts of unoxidised hæmoglobin out of every hundred, and this would probably have made all the difference between the machine stopping and going on.

The body, then, had fought the anoxæmia and reduced it very much in degree, but at the same time the anoxæmia had, in a subtle way, done much to stop the powers of the body, for this very acclimatisation is effected at the expense of the ultimate reserve which the body has at its disposal for the purpose of carrying out muscular or other work. The oxygen in the lungs was obtained essentially by breathing at rest as you would normally do when taking some exercise. Clearly, then, if you are partly out of breath before you commence exercise you cannot undertake so much as you otherwise would do. As a friend of mine, who has, I believe, camped at a higher altitude than any other man, put it to me, ' So great was the effort that we thought twice before we turned over in bed.'

One of the interesting problems with regard to chronic anoxæmia is its effect upon the mind. Working from the more acute type of anoxæmia to the more chronic, the following quotation will give an account of the condition of a person in the acute stage. It is Sir Clement Le Neve Foster's account of himself during CO poisoning, and shows loss of memory, some degree of intelligence, and a tendency to repeat what is said:

How soon I realised that we were in what is commonly called 'a tight place' I cannot say, but eventually, from long force of habit, I presume, I took out my note-book. At what o'clock I first began to write I do not know, for the few words written on the first page have no hour put to them. They were simply a few words of good-bye to my family, badly scribbled. The next page is headed ' 2 P.M.,' and I perfectly well recollect taking out my watch from time to time. As a rule I do not take a watch underground, but I carried it on this occasion in order to be sure that I left the rat long enough when testing with it. In fact, my note on the day of our misadventure was ' 5th ladder. Rat two minutes at man,' meaning by the side of the corpse. My notes at 2 P.M. were as follows : ' 2 P.M., good-bye, we are all dying, your Clement, I feel we are dying, good-bye, all my darlings all, no help coming, good-bye we are dying, good-bye, good-bye we are dying, no help comes, good-bye, good-bye.' Then later, partly scribbled over some ' good-byes' I find, ' We saw a body at 1.30 and then all became affected by the bad air, we have got to the 115 and can get no further, the box does not come in spite of our ringing for help. It does not come, does not come—I wish the box would come. Captain R. is shouting, my legs are bad, and I feel very [1], my knees are [1].' The so-called ' ringing ' was signalling to the surface by striking the air-pipe with a hammer or bar of iron. We had agreed upon signals before we went down. There is writing over other writing, as if I did not see exactly where I placed my pencil, and then : ' I feel as if I were dreaming, no real pain, good-bye, good-bye, I feel as if I were sleeping.' ' 2.15 we are all done. No [1]. or scarcely any, we are done, we are done, godo-bye my darlings.' Here it is rather interesting to note the ' godo ' instead of ' good.' Before very long the fresh men who had climbed down to rescue us seemed to have arrived, and explained that the ' box ' was caught in the shaft. Judging by my notes I did not realise thoroughly that we should be rescued. Among them occur the words ' No pain, it is merely like a dream, no pain.' I frequently wrote the same sentence over and over again. My last note on reaching the surface tells of

[1] in the above quotation indicates an illegible word in the notes.

that resistance to authority which likewise appears to be a symptom of the poisoning.

These notes afford ample confirmation of the effect produced by carbonic oxide poisoning of causing reiteration. I wrote the same words over and over again unnecessarily. The condition I was in was rather curious. I had absorbed enough of the poison to paralyse me to a certain extent and dull my feelings, but at the same time my reason had not left me.

The whole train of symptoms strongly suggests some form of intoxication, and is not dissimilar to that produced by alcoholic excess. Here it may be noted that, as far as isolated nerves are concerned, there is pretty good evidence that alcohol and want of oxygen produce exactly the same effects, *i.e.* they cause a decrement in the conducting power of the nerve. And herein lies a part of its interest, for pharmacologists of one school at all events tell me that the corresponding effects of alcohol are really due to an inhibition of the higher centres of the mind; you can therefore conceive of the mental mechanism of self-control being knocked out either because it has not oxygen enough with which to ' carry on,' or because it is drugged by some poison as a secondary result of the anoxæmia.

And now to pass to the results of more chronic anoxæmia. If I were to try to summarise them in a sentence I should say that, just as acute anoxæmia simulates drunkenness, chronic anoxæmia simulates fatigue.

The following slide shows you a photograph taken from a page in my note-book written at the Alta Vista Hut, at an altitude of 12,000 feet. You will see that the page commences with a scrawl which is crossed out, then ' 6 Sept.,' the word ' Sept.' is crossed out and ' March ' is inserted, ' March ' shares the same fate as ' Sept.,' and ' April,' the correct month, is substituted, and so on, more crossings out and corrections. All this you might say with justice is the action of a tired man. The other pages written at lower altitudes do not, however, bear out the idea that I was out of health at the time, and there was no reason for tiredness on that particular day. Another symptom frequently associated with mental fatigue is irritability. Anyone who has experience of high altitudes knows to his cost that life does not run smoothly at 10,000 feet. If the trouble is not with one's own temper, it is with those of one's colleagues : and so it was in many cases of gas-poisoning and in the case of aviators. In these subjects the apparent fatigue sometimes passed into a definitely neurasthenic condition. At this point an issue appeared to arise between the partisans of two theories. One camp said that the symptoms were definitely those of anoxæmia, the other that they were due to nerve strain. As I have indicated later on, it is not clear that these two views are mutually exclusive. It takes two substances to make an oxidation, the oxygen and the oxidised material. If the oxidation does not take place, the cause may lie in the absence of either or of both, in each case with a similar effect. The subject really is not ripe for controversy, but it is amply ripe for research, research in which both the degree of anoxæmia and the symptoms of fatigue are clearly defined.

So much, then, for the injury to the machine wrought by chronic anoxæmia.

Types of Anoxæmia.

And now to pass to the consideration of the various types of anoxæmia.

Anoxæmia is by derivation want of oxygen in the blood.

Suppose you allow your mind to pass to some much more homely substance than oxygen—such, for instance, as milk—and consider the causes which may conspire to deprive your family of milk, three obvious sources of milk deficiency will occur to you at once:

(1) There is not enough milk at the dairy;

(2) The milk is watered or otherwise adulterated so that the fluid on sale is not really all milk; and

(3) The milkman from that particular dairy does not come down your road.

These three sources of milk deficiency are typical of the types of oxygen deficiency.

The first is insufficient oxygen dispensed to the blood by the lungs. An example of this type of anoxæmia is mountain-sickness. The characteristic of it is insufficient *pressure* of oxygen in the blood. In mountain-sickness the insufficiency of pressure in the blood is due to insufficient pressure in the air, for, according to our view at all events, the pressure in the blood will always be less than that in air. But this type of anoxæmia may be due to other causes. The sufferer may be in a normal atmosphere, yet for one reason or another the air may not have access to his whole lung. In such cases, either caused by obstruction, by shallow respiration, or by the presence of fluid in the alveoli, the blood leaving the affected areas will contain considerable quantities of reduced hæmoglobin. This will mix with blood from unaffected areas which is about 95 per cent. saturated. The oxygen will then be shared round equally among the corpuscles of the mixed blood, and if the resultant is only 85-90 per cent. saturated the pressure of oxygen will only be about half the normal, and, as I said, deficiency of oxygen pressure is the characteristic of this type of anoxæmia.

The second type involves no want of oxygen pressure in the arterial blood; it is comparable to the watered milk: the deficiency is really in the quality of the blood and not in the quantity of oxygen to which the blood has access. The most obvious example is anæmia, in which from one cause or other the blood contains too low a percentage of hæmoglobin, and because there is too little hæmoglobin to carry the oxygen too little oxygen is carried. Anæmia is, however, only one example which might be given of this type of anoxæmia. There may be sufficient hæmoglobin in the blood, but the hæmoglobin may be useless for the purpose of oxygen transport; it may be turned in part into methæmoglobin, as in several diseases, *e.g.* among workers in the manufacture of some chemicals, and in some forms of dysentery contracted in tropical climates, or it may be monopolised by carbon monoxide, as in mine-air.

Thirdly, the blood may have access to sufficient oxygen and may contain sufficient functional hæmoglobin, but owing to transport trouble it may not be circulated in sufficient quantities to the tissues. The quantity of oxygen which reaches the tissue in unit time is too small.

Literally, according to the strict derivation of the word 'anoxæmia,' the third type should perhaps be excluded from the category of conditions covered by that word, but, as the result is oxygen starvation in the tissues, it will be convenient to include it. Indeed, it would be an act of pedantry not to do so, for no form of anoxæmia has any significance apart from the fact that it prevents the tissues from obtaining the supply of oxygen requisite for their metabolic processes.

The obvious types of anoxæmia may therefore be classified in some such scheme as the following, and, as it is difficult to continue the discussion of them without some sort of nomenclature, I am giving a name to each type:

<div align="center">ANOXÆMIA.</div>

1. *Anoxic Type.*	2. *Anæmic Type.*	3. *Stagnant Type.*
The pressure of oxygen in the blood is too low. The hæmoglobin is not saturated to the normal extent. The blood is dark. Examples: 1. Rare atmospheres. 2. Areas of lung partially unventilated. 3. Fluid or fibrin on surface of cells.	The quantity of functional hæmoglobin is too small. The oxygen pressure is normal. The blood is normal in colour. Examples: 1. Too little hæmoglobin. 2. CO hæmoglobin. 3. Methæmoglobin.	The blood is normal, but is supplied to the tissues in insufficient quantities. Examples: 1. Secondary result of histamine shock. 2. Hæmorrhage. 3. Back pressure.

Anoxic anoxæmia is essentially a general as opposed to a local condition. Not only is the pressure of oxygen in the blood too low, but the lowness of the pressure and not the deficiency in the quantity is the cause of the symptoms observed.

Proof of the above statement is to be found in the researches of most workers who have carried out investigations at low oxygen pressures, and it can now be brought forward in a much more convincing way than formerly that oxygen secretion is, for the time at all events, not a factor to be counted with.

The workers on Pike's Peak, for instance, emphasised the fact that the increase of red blood corpuscles during their residence at 14,000 feet was due to deficient oxygen pressure. No doubt they were right, but the point was rather taken from their argument by their assertion in another part of the paper that the oxygen pressure in their arterial blood was anything up to about 100 mm. of mercury. Let me therefore take my own case, in which the alveolar pressures are known to be an index of the oxygen pressures in the arterial blood. I will compare my condition on two occasions, the point being that on these two occasions the quantities of oxygen united with the hæmoglobin were as nearly as may be the same, whilst the pressures were widely different.

As I sit here the hæmoglobin value of my blood is 96-97, which corresponds to an oxygen capacity of ·178 c.c. of O_2 per c.c. of blood. In the oxygen chamber on the last day of my experiment, to which I refer later,[5] the oxygen capacity of my blood was ·201 c.c. Assuming the blood to be 95 per cent. saturated now and 84 per cent. saturated

then, the actual quantity of oxygen in the blood on the two occasions would be:

Oxygen Capacity.	Percentage Saturation.	Oxygen Content.
·178	95	·169
·201	84	·169

Here, I am in my usual health. In the chamber, I vomited; my pulse was 86—it is now 56; my head ached in a most distressing fashion, it was with the utmost difficulty that I could carry out routine gas analyses, and when doing so the only objects which I saw distinctly were those on which my attention was concentrated.

In the anoxic type of anoxæmia there may then be quite a sufficient quantity of oxygen in the blood, but a sufficient quantity does not avail in the face of an insufficient pressure. Indeed, as I shall show presently, the anoxic type of anoxæmia is the most serious. We are therefore confronted with something of a paradox in that the most severe type of anoxæmia is one in which there is not necessarily an insufficient quantity of oxygen in the blood at all.

And here let me justify the statement that the anoxic type is the most to be feared. I can justify it on either or both of two grounds. Firstly, of the three types it places the tissues at the greatest disadvantage as regards oxygen supply, and secondly, it is of the three the least easy type for the organism to circumvent.

Let me dwell for a moment upon the efficiency of the blood as a medium for the supply of oxygen to the tissue in the three types. The goal of respiration is to produce and maintain as high an oxygen pressure in the tissue fluids as possible. For the velocity of any particular oxidation in the tissues must depend upon the products of the concentrations (active masses) of the material to be oxidised and of the oxygen. Now the concentration of oxygen in the tissue is proportional to its partial pressure, and the highest partial pressure in the tissue must, other things being equal, be the result of that type of anoxæmia in which there is the highest partial pressure in the blood plasma.

It is interesting and not uninstructive to try to calculate the degree to which the tissues are prejudiced by being subjected to various types of anoxæmia. Let us suppose that we have a piece of tissue, muscle for instance, which normally is under the following conditions:

(a) One cubic centimetre of blood per minute runs through it.
(b) The total oxygen capacity of this blood is ·188 c.c. of oxygen per c.c. of blood.
(c) The percentage saturation is 97.
(d) The oxygen pressure is 100 mm.
(e) The oxygen used is ·059 c.c.
(f) The oxygen pressure in the tissues is half of that in the veins, in this case 19 mm.

Compare with this a severe case of anoxic anoxæmia, one in which the blood-flow is the same as above, and also the oxygen capacity value of the blood, but in which the oxygen pressure is only 31 mm. and the percentage saturation of the arterial blood 66 per cent. Let us further retain the assumption that the oxygen pressure in the tissues is half

that in the veins. It is possible to calculate, as indeed has been done by my colleague, Mr. Roughton, what the amount of oxygen leaving the capillaries is. The answer is not ·059, as in the case of the normal, but ·026—less than half the normal. So, other things being equal, cutting down the oxygen pressure in the arterial blood to 31 and the percentage saturation to 66 would deprive the tissues of half their oxygen. With this compare an example of the anæmic type. The arterial blood shall have the same total quantity of oxygen as in the anoxic case, but instead of being 66 per cent. saturated it shall contain 66 per cent. of the total hæmoglobin, which shall be normally saturated. The amount of oxygen which would pass to the tissues under these conditions is ·041 c.c.—more than half as much again as from the anoxic blood laden with the same quantity of oxygen.

And thirdly, let us take for comparison a case of stagnant anoxæmia in which the same quantity of oxygen goes to the tissue in the cubic centimetre of blood as in the anoxic and anæmic types. On the assumption which we have made the quantity of oxygen which would leave the blood would be ·045 c.c.

In round numbers therefore the prejudice to the tissues may be expressed by the following comparison. In this case both of the oxygen going to the blood and going into the tissues I have called the normal 100. This does not, of course, mean that the two amounts are the same. The former in absolute units is about three times the latter. The figure 100 at the top of each column is merely a standard with which to compare the figures beneath it.

		Oxygen in blood going to vessels of tissue. Per cent.	Oxygen leaving the blood to supply the tissues. Per cent.
Normal		100	100
Anoxæmia { Anoxic		66	42
Anæmic		66	66
Stagnant		66	75

Measurement of Anoxæmia.

In the study of all physical processes there comes a point, and that very early, when it becomes necessary to compare them one with another, to establish some sort of numerical standard and have some sort of quantitative measurements. The study of anoxæmia has reached that point. By what scale are we to measure oxygen want?

Let us take the anoxic type first. There are two scales which might be applied to it, both concern the arterial blood; the one is the oxygen pressure in it, the other is the actual percentage of the hæmoglobin which is oxyhæmoglobin. A third possibility suggests itself, namely, the actual amount of oxygen present, but this would be influenced as much by the anæmic as by the anoxic conditions. Of the two possibilities—that of measuring the pressure, and that of measuring the saturation of the blood with oxygen—the latter is the one which is likely to come into vogue, because it is susceptible of direct measurement.

Two converging lines of work have within the last few years brought us nearer to being able to state the degree of anoxæmia in man in terms of the percentage saturation of oxygen in his blood. The first, intro-

duced by the American researcher Stadie,[6] is the method of arterial puncture. It had long been the wish of physiologists to make direct examinations of the gases in human arterial blood, yet, as far as I know, this had only once been accomplished, namely, by Dr. Arthur Cooke and myself in a case in which the radial artery was opened for the purpose of transfusion. But the matter now seems to be relatively simple. The needle of a hypodermic syringe can be put right into the radial artery and arterial blood withdrawn. I am not sure that the operation is less painful than that of dissecting out the radial artery and opening it—and in this matter I speak with experience—but it is less alarming, and it has the great merit that it does not injure the artery.

Another method of determining the percentage saturation of arterial blood has invited the attention of researchers, appearing like a will-o'-the-wisp, at one time within grasp, at another far off. That method is to deduce the percentage saturation from the composition of the alveolar air. Into the merits of the rival methods for the determination of the oxygen in alveolar air I will not go: the method of Haldane and Priestley will suffice for persons at rest. Granted, then, that a subject has a partial pressure of 50 mm. of oxygen in his alveolar air, what can we infer as regards his arterial blood? A long controversy has raged about whether or no any assumption could be made about the condition of the arterial blood from that of the alveolar air, for it was an article of faith with the school of physiologists which was led by Haldane that when the oxygen pressure in the alveolar air sank, the oxygen in the arterial blood did not suffer a corresponding reduction. The experimental evidence at present points in the opposite direction, and unless some further facts are brought to light it may be assumed that the oxygen pressure in the arterial blood of a normal person at rest is some five millimetres below that in his alveolar air. And having obtained a figure for the pressure of oxygen in the arterial blood, where do we stand as regards the percentage of saturation? The relation between the one and the other is known as the oxygen dissociation curve. It differs but slightly in normal individuals, and at different times in the same individual. To infer the percentage saturation from the oxygen pressure, no doubt the actual dissociation curve should be determined, but in practice it is doubtful whether as a first approximation this is necessary, for a curve determined as the result of a few observations is unlikely to be much nearer the mark than a standard curve on which twenty or thirty points have been determined. Therefore an approximation can be made for the percentage saturation as follows: In a normal individual take the oxygen in the alveolar air, subtract five millimetres, and lay the result off on the mean dissociation curve for man.

Whether measured directly or indirectly, the answer is a statement of the relative quantities of oxyhæmoglobin and of reduced hæmoglobin in the arterial blood. The important thing is that there should be as little reduced hæmoglobin as possible. The more reduced hæmoglobin there is present the less saturated is the blood, or, as the American authors say, the more *un*saturated is the blood. They emphasise

the fact that it is the quantity of reduced hæmoglobin that is the index of the anoxic condition. They speak not of the percentage saturation, but of the percentage of unsaturation. A blood which would ordinarily be called 85 per cent. saturated they speak of as 15 per cent. unsaturated.

Anoxic anoxæmia, in many cases of lung affection, should be measured by the direct method of arterial puncture, for the simple reason that the relation between the alveolar air and the arterial blood is quite unknown. Such, for instance, are cases of many lung lesions of pneumonia in which the lung may be functioning only in parts, of pneumothorax, of pleural effusions, of emphysema, of multiple pulmonary embolism, in phases of which the arterial blood has been found experimentally to be unsaturated. In addition to these definite lung lesions, there is another type of case on which great stress has been laid by Haldane, Meakins, and Priestley, namely, cases of shallow respiration.[7] A thorough investigation of the arterial blood in such cases is urgently necessary. Indeed, in all cases in which it is practicable, the method of arterial puncture is desirable. But in the cases of many normal persons—as, for instance, those airmen at different altitudes—alveolar-air determinations would give a useful index.

The anæmic type of anoxæmia is gauged by the quantity of oxyhæmoglobin in the blood. In the case of simple anæmias this is measured by the scale in which the normal man counts as 100 and the hæmoglobin in the anæmic individual is expressed as a percentage of this. This method has been standardised carefully by Haldane, and we now know that the man who shows 100 on the scale has an oxygen capacity of ·185 c.c. of oxygen for every c.c. of blood. We can therefore, in cases of carboxyhæmoglobin, or methæmoglobin poisoning, express the absolute amount of oxyhæmoglobin pressure either by stating the oxygen capacity and so getting an absolute measurement, or in relative units by dividing one hundred times the oxygen capacity by ·185, and thus getting a figure on the ordinary hæmoglobin metre scale.

The Mechanism of Anoxæmia.

Perhaps the most difficult phase of the discussion is that of how anoxæmia produces its baneful results. In approaching this part of the subject I should like to warn my readers of one general principle the neglect of which seems to be responsible for a vast dissipation of energy. Before you discuss whether a certain effect is due to cause A or cause B, be clear in your own mind that A and B are mutually exclusive.

Let me take an example and suppose

(1) That the energy of muscular contraction in the long run depends in some way on the oxidation of sugar;

(2) That in the absence of an adequate supply of oxygen the reaction

$$C_6H_{12}O_6 + 6\,O_2 = 6\,CO_2 + 6\,H_2O$$

cannot take place in its entirety;

(3) That under such circumstances some lactic acid is formed as well as carbonic acid;

(4) That the hydrogen ion concentration of the blood rises and the total ventilation increases. On what lines are you to discuss whether the increased ventilation is due to ' acidosis,' by which is meant in this connection the increased hydrogen ion concentration of the blood, or to ' anoxæmia '? Clearly not on the lines that it must be due to one or other.

In the above instance anoxæmia and acidosis are to some extent dependant variables. I have chosen the above case because measurements have been made throughout which make the various assumptions fairly certain, and tell us pretty clearly in what sort of chain to string up the events, what is cause and what is effect. Clearly it would be ridiculous to start a discussion as to whether the breathlessness was due to ' acidosis ' or ' anoxæmia.' Each has its place in the chain of events. But I have heard discussions of whether other phenomena of a more obscure nature were due to oxygen want or to acidosis. Such discussions tend to no useful end.

Nor is this the only problem with regard to oxygen want concerning which my warning is needed. Oxygen want may act immediately in at least two ways:

(1) In virtue of absence of oxygen some oxidation which otherwise might take place does not do so, and therefore something which might otherwise happen may not happen. For instance, it may be conceived that the respiratory centre can only go through the rhythmic changes of its activity as the result of the oxidation of its own substance.

(2) A deficient supply of oxygen may produce, not the negation of a chemical action, but an altered chemical action which in its turn produces toxic products that have a secondary effect on such an organism as the respiratory centre.

Now these effects are not mutually exclusive. In the same category are many arguments about whether accumulations of carbonic acid act specifically as such or merely produce an effect in virtue of their effect on the hydrogen ion concentration.. Here again the two points of view are not, strictly speaking, alternatives, and, in some cases at all events, both actions seem to go on at the same time.

It will be evident that in any balanced action in which CO_2 is produced its accumulation will tend to slow the reaction; but, on the other hand, the same accumulation may very likely raise the hydrogen ion concentration, and in that way produce an effect.

The relation of oxygen to hæmoglobin seems to furnish a case in point. Carbonic acid is known to reduce the affinity of hæmoglobin for oxygen, and other acids do the same. On analogy, therefore, it might have, and has, been plausibly argued that CO_2 acts in virtue of the change in reaction which it produces. Put into mathematical language, the relation of the percentage saturation of oxygen to the oxygen pressure of the gas dissolved in the hæmoglobin solution is expressed by the equation

$$\frac{y}{100} = \frac{Kx^n}{1 + Kx^n} = \frac{x^n}{L + x^n}, \text{ where } L = \frac{1}{K},$$

where y is the percentage saturation and x the oxygen pressure. The value of K is the measure of the affinity of oxygen for hæmoglobin: the less the value of K the less readily do the two substances unite.

Now $\dfrac{1}{K}$ has been shown by Laurence J. Henderson,[8] and independently by Adair, to vary directly with the concentration of CO_2. The value of this constant is, according to Henderson, too great to be a direct effect of the CO_2 on the hæmoglobin, and involves as well the assumption that the hæmoglobin in blood is in four forms—an acid and a salt of reduced hæmoglobin and an acid and a salt of oxyhæmoglobin. The presence of CO_2 alters the balance of these four substances.

It is rather fashionable at present to say that 'the whole question of acidosis and anoxæmia is in a hopeless muddle.' To this I answer that, if it is in a muddle, I believe the reason to be largely because schools of thought have rallied round words and have taken sides under the impression that they have no common ground. The 'muddle,' in so far as it exists, is not, I think, by any means hopeless; but I grant freely enough that we are rather at the commencement than at the end of the subject, that much thought and much research must be given, firstly, in getting accurate data, and, secondly, on relating cause and effect, before the whole subject will seem simple. No effort should be spared to replace indirect by direct measurements. My own inference with regard to changes of the reaction of the blood, based on interpretations of the dissociation curve, should be checked by actual hydrogen ion measurements, as has been done by Hasselbach and is being done by Donegan and Parsons.[9] Meakins also is, I think, doing great work by actually testing the assumptions made by Haldane and himself as regards the oxygen in arterial blood.

The Compensations for Anoxæmia.

For the anoxic type of anoxæmia two forms of compensation at once suggest themselves. The one is increased hæmoglobin in the blood; the other is increased blood-flow through the tissues. Let us, along the lines of the calculations already made, endeavour to ascertain how far these two types of compensation will really help. To go back to the extreme anoxic case already cited, in which the hæmoglobin was 66 per cent. saturated, let us, firstly, see what can be accomplished by an increase of the hæmoglobin value of the blood. Such an increase takes place, of course, at high altitudes. Let us suppose that the increase is on the same grand scale as the anoxæmia, and that it is sufficient to restore the actual quantity of oxygen in one c.c. of blood to the normal. This, of course, means a rise in the hæmoglobin value of the blood from 100 to 150 on the Gowers' scale. Yet even so great an increase in the hæmoglobin will only increase the oxygen taken up in the capillary from each c.c. of blood from ·031 to ·036 c.c., and will therefore leave it far short of the ·06 c.c. which every cubic centimetre of normal blood was giving to the tissue. So much, then, for increased hæmoglobin. It gives a little, but only a little, respite. Let us turn, therefore, to increased blood-flow.

In the stagnant type of anoxæmia the principal change which is seen to take place is an increase in the quantity of hæmoglobin per cubic millimetre of blood.

This increase is secondary to a loss of water in the tissues, the result in some cases, as appears from the work of Dale, Richards, and Laidlaw,[10] of a formation of histamine in the tissues. Whether this increase of hæmoglobin is to be regarded as merely an accidental occurrence or as a compensation is difficult to decide at present. Roughton's calculations rather surprised us by indicating that increased hæmoglobin acted less efficiently as a compensatory mechanism than we had expected. This conclusion may have been due to the inaccuracy of our assumptions. I must therefore remind you that much experimental evidence is required before the assumptions which are made above are anything but assumptions. But, so far as the evidence available at the present time can teach any lesson, that lesson is this: The only way of dealing satisfactorily with the anoxic type of anoxæmia is to abolish it by in some way supplying the blood with oxygen at a pressure sufficient to saturate it to the normal level.

It has been maintained strenuously by the Oxford school of physiologists that Nature actually did this; that when the partial pressure in the air-cells of the lung was low the cellular covering of that organ could clutch at the oxygen and force it into the blood at an unnatural pressure, creating a sort of forced draught. This theory, as a theory, has much to recommend it. I am sorry to say, however, that I cannot agree with it on the present evidence. I will only make a passing allusion to the experiment which I performed in order to test the theory, living for six days in a glass respiration-chamber in which the partial pressure of oxygen was gradually reduced until it was at its lowest—about 45 mm. Such a pressure, if the lung was incapable of creating what I have termed a forced draught, would mean an oxygen pressure of 38-40 mm. of mercury in the blood, a change sufficient to make the arterial blood quite dark in colour, whereas, did any considerable forced draught exist, the blood in the arteries would be quite bright in colour. Could we but see the blood in the arteries, its appearance alone would almost give the answer as to whether or no oxygen was forced, or, in technical language, secreted, through the lung wall. And, of course, we could see the blood in the arteries by the simple process of cutting one of them open and shedding a little into a closed glass tube. To the surgeon this is not a difficult matter, and it was, of course, done. The event showed that the blood was dark, and the most careful analyses failed to discover any evidence that the body can force oxygen into the blood in order to compensate for a deficiency of that gas in the air.

Yet the body is not quite powerless. It can, by breathing more deeply, by increasing the ventilation of the lungs, bring the pressure of oxygen in the air-cells closer to that in the atmosphere breathed than would otherwise be the case. I said just now that the oxygen in my lungs dropped to a minimal pressure of 45 mm.; but it did not remain at that level. When I bestirred myself a little it rose, as the result of increased ventilation of the lung, to 56 mm., and at one time, when I was breathing through valves, it reached 68 mm. Nature will

do something, but what Nature does not do should be done by artifice. Exploration of the condition of the arterial blood is only in its infancy, yet many cases have been recorded in which in illness the arterial blood has lacked oxygen as much as or more than my own did in the respiration chamber when I was lying on the last day, with occasional vomiting, racked with headache, and at times able to see clearly only as an effort of concentration. A sick man, if his blood is as anoxic as mine was, cannot be expected to fare better as the result, and so he may be expected to have all my troubles in addition to the graver ones which are, perhaps, attributable to some toxic cause. Can he be spared the anoxæmia? The result of our calculations, so far, points to the fact that the efficient way of combating the anoxic condition is to give oxygen. During the war it was given with success in the field in cases of gas-poisoning, and also special wards were formed on a small scale in this country in which the level of oxygen in the atmosphere was kept up to about 40 per cent., with great benefit to a large percentage of the cases. The practice then inaugurated is being tested at Guy's Hospital by Dr. Hunt, who administered the treatment during the war.

Nor are the advantages of oxygen respiration confined to pathological cases. One of the most direct victims of anoxic anoxæmia is the airman who flies at great heights. Everything in this paper tends to show that to counteract the loss of oxygen which he sustains at high altitudes there is but one policy, namely, to provide him with an oxygen equipment which is at once as light and as efficient as possible— a consummation for which Haldane has striven unremittingly. And here I come to the personal note on which I should like to conclude. In the pages which I have read views have been expressed which differ from those which he holds in matters of detail—perhaps in matters of important detail. But Haldane's teaching transcends mere detail. He has always taught that the physiology of to-day is the medicine of to-morrow. The more gladly, therefore, do I take this opportunity of saying how much I owe, and how much I think medicine owes and will owe, to the inspiration of Haldane's teaching.

References.

1. HALDANE.
2. HALDANE, KELLAS, and KENNAWAY. Journal of Physiology, liii.
3. FOSTER and HALDANE. The Investigation of Mine Air. Griffin & Co. 1905.
4. KROGH and LINDHARD, quoted by Bainbridge.
5. BARCROFT, COOKE, HARTRIDGE, PARSONS and PARSONS. Journal of Physiology, liii. p. 451, 1920.
6. STADIE. Journal of Experimental Medicine, xxx. p. 215. 1919.
7. HALDANE, MEAKINS, and PRIESTLEY. Journal of Physiology, lii. p. 420. 1918-19.
8. L. J. HENDERSON. The Journal of Biological Chemistry, vol. xli. p. 401. 1920.
9. DONEGAN and PARSONS. Journal of Physiology, lii. p. 315. 1919.
10. DALE and RICHARDS. Journal of Physiology, lii. p. 110. 1919. DALE and LAIDLAW. Ibid. p. 355.

ADDRESS

TO THE

BOTANICAL SECTION

BY

Miss E. R. SAUNDERS, F.L.S.,

PRESIDENT OF THE SECTION.

YEAR by year we see the meetings of the Association recur, pursuing a course which neither geographer nor astronomer would venture to predict and leaving traced out behind them a figure unknown to the mathematician. Nevertheless the path of its journeyings is ever returning upon itself. As this recurrence is brought afresh to one's mind, there is a natural impulse to reflect upon the progress which has been made in the intervening period in the science which one here finds oneself called upon to represent. Not quite thirty years have elapsed since the last occasion on which the Association was welcomed to Cardiff. Curiosity to learn whether the matter of the discourse delivered by my predecessor on that occasion had a connection, close or remote, with the particular subject with which I proposed to deal in this Address led me to refer to the Annual Report of the Association for 1891. I thus became aware how recent was the occurrence of the mutation—or should I rather say of the dichotomy?—which led to the appearance of a Botanical Section, for twenty-nine years ago Section K had not yet come into existence. At that period the problems relating to living organisms, whether concerned with plant or animal, whether of a morphological or physiological nature, were all embraced within the wide field of Section D, the Section of Biology. Though in succeeding years discovery at an ever-increasing rate and in many new fields of investigation has made inevitable the separation first of Physiology, and then of Botany from their common parent, we may with advantage follow the precedent set by the Association as a whole, and, as a Section, return from time to time upon our course of evolution. I shall therefore invite your attention to a subject which lies within the wide province of Biology and makes its appeal alike to the botanist, zoologist and physiologist—the subject of Heredity.

By the term Inheritance we are accustomed to signify the obvious fact of the resemblance displayed by all living organisms between offspring and parents, as the direct outcome of the contributions received from the two sides of the pedigree at fertilisation: to indicate, in fact,

owing to lack of knowledge of the workings of the hereditary process, merely the *visible* consequence—the final result of a chain of events. Now, however, that we have made a beginning in our analysis of the stages which culminate in the appearance of any character, a certain looseness becomes apparent in our ordinary use of the word Heredity, covering as it does the two concomitant essentials, genetic potentiality and somatic expression—a looseness which may lead us into the paradoxical statement that inheritance is wanting in a case in which nevertheless the evidence shows that the genetic constitution of the children is precisely like that of the parents. When we say that a character is inherited no ambiguity is involved, because the appearance of the character entails the inheritance of the genetic potentiality. But when a character is stated not to be inherited it is not thereby indicated whether this result is due to environmental conditions, to genetic constitution, or to both causes combined. That we are now able in some measure to analyse the genetic potentialities of the individual is due to one of those far-reaching discoveries which change our whole outlook, and bring immediately in their train a rapidly increasing array of new facts, falling at once into line with our new conceptions, or by some orderly and constant discrepancy pointing a fresh direction for attack. An historical survey of the steps by which we have advanced to the present state of our knowledge of Heredity has so frequently been given during the last twenty years that the briefest reference to this part of my subject will suffice.

The earliest attempts to frame some general law which would co-ordinate and explain the observed facts of inheritance were those of Galton and Pearson. Galton's observations led him to formulate two principles which he believed to be capable of general application—the Law of Ancestral Heredity and the Law of Regression. The Law of Ancestral Heredity was intended to furnish a general expression for the sum of the heritage handed on in any generation to the succeeding offspring. Superposed upon the working of this law were the effects of the Law of Regression, in which the average deviation from the mean of a whole population of any fraternal group within that population was expressed in terms of the average deviation of the parents. These expressions represent statements of averages which, in so far as they apply, hold only when large numbers are totalled together. They afford no means of certain prediction in the individual case. These and all similar statistical statements of the effects of inheritance take no account of the essentially physiological nature of this as of all other processes in the living organism. They leave us unenlightened on the fundamental question of the nature of the means by which the results we witness came to pass. We obtain from them, as from the melting-pot, various new products whose properties are of interest from other viewpoints, but, corresponding to no biological reality, they have failed to bring us nearer to our goal—a fuller comprehension of the workings of the hereditary mechanism. Progress in this direction has resulted from the opposite method of inquiry—the study of a single character in a single line of descent, the method which deals with the unit in place of the mass. The revelation came with the opening of the present

century, for in 1900 was announced the rediscovery of Mendel's work, actually given to the world thirty-five years earlier, but at the time leaving no impress upon scientific thought. The story of the Austrian monk and the details of his experiments carried out in the monastery garden upon races of the edible pea are now familiar history, and I need not recount them here. Having formed the idea that in order to arrive at a clearer understanding of the relation of organisms to their progeny the problem must be studied in its simplest form, Mendel came to see that a scheme of analysis must deal not with mass populations but with a smaller unit—the family, and that each character of the individual must be separately investigated.

Selecting for his experiments races which showed themselves to be pure-breeding and mating together those exhibiting characters of such opposite nature as to constitute a pair—*e.g.*, tall with short, yellow-seeded with green-seeded—he obtained results which could be accounted for if it were supposed that these opposite, or as we should now term them allelomorphic, characters were distributed *unaltered and in equal proportion* to the reproductive cells of the cross-bred organism. It is this conception of the pure nature of the germ-cells, irrespective of whether the organism forming them be of pure-bred or cross-bred descent, which revolutionised our conceptions of Heredity and laid the foundations upon which we build to-day. For the intervening years have seen the instances in which the Mendelian theory is found to hold mount steadily from day to day, furnishing a weight of evidence in its support which is incontrovertible.

It chanced that in each pair of characters selected by Mendel for experiment the opposites are related to each other in the following simple manner: An individual which had received both allelomorphs, one from either parent, exhibited one of the two characteristics, hence called the dominant, to the exclusion of the other. Among the offspring of such an individual both characteristics appeared, the dominant in some, its opposite, the recessive, in others, in the proportion approximately of three to one. This is the result which might be expected from random pairing in fertilisation of two opposites, where the manifestation in the zygote of the one completely masks the presence of the other. As workers along Mendelian lines increased and the field of inquiry widened, it soon, however, became apparent that the dominant-recessive relationship is not of universal occurrence. It likewise became clear that the simple ratios which obtained in Mendel's experiments are not characteristic of every case. Mendel's own results were all, as it happened, explicable on the supposition that the two alternative forms of each character were dependent on a *single* element or factor. By a fortunate accident none of the complex factorial interrelations which have since been brought to light in other cases obscured the expression in its simplest form of the results of germ purity. It is our task, in the light of this guiding principle, to attempt to elucidate these more complicated types of inheritance.

We now know, for example, that many characters are not controlled by one single factor, but by two or more. One of the most familiar instances of the two-factor character is the appearance of the

colouring matter anthocyanin in the petals of plants such as the Stock and Sweet Pea. Our proof that two factors (at least) are here involved is obtained when we find that two true breeding forms devoid of colour yield coloured offspring when mated together. In this case the two complementary factors are carried, one by each of the two crossed forms. When both factors meet in the one individual, colour is developed. We have in such cases the solution of the familiar, but previously unexplained, phenomenon of Reversion. Confirmatory evidence is afforded when among the offspring of such cross-bred individuals we find the simple 3 to 1 ratio of the one-factor difference replaced by a ratio of 9 to 7. Similarly we deduce from a ratio of 27 to 37 that three factors are concerned, from a ratio of 81 to 175 four factors, and so on. The occurrence of these higher ratios proves that the hereditary process follows the same course whatever the number of factors controlling the character in question.

And here I may pause to dwell for a moment upon a point of which it is well that we should remind ourselves from time to time, since, though tacitly recognised, it finds no explicit expression in our ordinary representation of genetic relations. The method of factorial analysis based on the results of inter-breeding enables us to ascertain the least possible number of genetic factors concerned in controlling a particular somatic character, but what the total of such factors actually is we cannot tell, since our only criterion is the number by which the forms we employ are found to differ. How many may be common to these forms remains unknown. In illustration I may take the case of surface character in the genera *Lychnis* and *Matthiola*. In *L. vespertina* the type form is hairy; in the variety *glabra*, recessive to the type, hairs are entirely lacking. Here all glabrous individuals have so far proved to be similar in constitution, and when bred with the type give a 3 to 1 ratio in F_2.[1] We speak of hairiness in this case, therefore, as being a one-factor character. In the case of *Matthiola incana v. glabra*, of which many strains are in cultivation, it so happened that the commercial material originally employed in these investigations contained all the factors since identified as present in the type and essential to the manifestation of hairiness except one. Hence it appeared at first that here also hairiness must be controlled, as in *Lychnis*, by a single factor. But further experiment revealed the fact that though the total number of factors contained in these glabrous forms was the same, the respective factorial combinations were not identical. By inter-breeding these and other strains obtained later, hairy F_1 cross-breds were produced giving ratios in F_2 which proved that at least four distinct factors are concerned.[2] Whereas, then, the glabrous appearance in *Lychnis* always indicates the loss (if for convenience we may so represent the nature of the recessive condition) *of one and the same factor*, analysis in the Stock shows that the glabrous condition results if *any factor out of a group of four* is represented by its recessive allelomorph. Hence we describe hairiness in the latter case as a four-factor character.

[1] *Report to the Evolution Committee, Royal Society*, i., 1902.
[2] *Proc. Roy. Soc.* B, vol. 85, 1912.

It will be apparent from the cases cited that we cannot infer from the genetic analysis of one type that the factorial relations involved are the same for the corresponding character in another. That this should be so in wholly unrelated plants is not perhaps surprising, but we find it to be true also where the nature of the characteristic and the relationship of the types might have led us to expect uniformity. This is well seen in the case of a morphological feature distinctive of the N.O. Gramineæ. The leaf is normally ligulate, but individuals are occasionally met with in which the ligule is wanting. In these plants, as a consequence, the leaf blade stands nearly erect instead of spreading out horizontally. Nilsson-Ehle [3] discovered that in Oats there are at least four and possibly five distinct factors determining ligule formation, all with equal potentialities in this direction. Hence, only when the complete series is lacking is the ligule wanting. In mixed families the proportion of ligulate to non-ligulate individuals depends upon the number of these ligule-producing factors contained in the dominant parent. Emerson [4] found, on the other hand, that in Maize mixed families showed constantly a 3 to 1 ratio, indicating the existence of only one controlling factor.

From time to time the objection has been raised that the Mendelian type of inheritance is not exhibited in the case of specific characters. That no such sharp line of distinction can be drawn between the behaviour of varietal and specific features has been repeatedly demonstrated. As a case in point and one of the earliest in which clear proof of Mendelian segregation was obtained, we may instance *Datura*. The two forms, *D. Stramonium* and *D. Tatula*, are ranked by all systematists as distinct species. Among other specific differences is the flower colour. The one form has purple flowers, the other pure white. In the case of both species a variety *inermis* is known in which the prickles characteristic of the fruit in the type are wanting. It has been found that in whatever way the two pairs of opposite characters are combined in a cross between the species, the F_2 generation is mixed, comprising the four possible combinations in the proportions which we should expect in the case of two independently inherited pairs of characters, when each pair of opposites shows the dominant-recessive relation. Segregation is as sharp and clean in the specific character flower colour as in the varietal character of the fruit. Among the latest additions to the list of specific hybrids showing Mendelian inheritance, those occurring in the genus *Salix* are of special interest, since heretofore the data available had been interpreted as conflicting with the Mendelian conception. The recent observations of Heribert-Nilsson [5] show that those characters which are regarded by systematists as constituting the most distinctive marks of the species are referable to an extremely simple factorial system, and that the factors mendelise in the ordinary way. Furthermore, these specific-character factors

[3] *Kreuzungsuntersuchungen an Hafer und Weizen*, Lund, 1909.
[4] Annual Report of the Agricultural Experiment Station of the University of Nebraska, 1912.
[5] *Experimentelle Studien über Variabilität, Spaltung, Artbildung und Evolution in der Gattung Salix*, 1918.

control not only the large constant *morphological features*, but *fundamental reactions* such as those determining the condition of physiological equilibrium and vitality in general. In so far as any distinction can be drawn between the behaviour of factors determining the varietal as opposed to the specific characters of the systematist, Heribert-Nilsson concludes that the former are more localised in their action, while the latter produce more diffuse results, which may affect almost all the organs and functions of the individual, and thus bring about striking alterations in the general appearance. *S. caprea*, for example, is regarded as the reaction product of two distinct factors which together control the leaf-breadth character, but which also affect, each separately and in a different way, leaf form, leaf colour, height, and the periodicity of certain phases. We cannot, however, draw a hard-and-fast line between the two categories. The factor controlling a varietal characteristic often produces effects in different parts of the plant. For example, the factors which lead to the production of a coloured flower no doubt also in certain cases cause the tinging seen in the vegetative organs, and affect the colour of the seed. Heribert-Nilsson suggests that fertility between species is a matter of close similarity in genotypic (factorial) constitution rather than of outward morphological resemblance. Forms sundered by the systematist on the ground of gross differences in certain anatomical features may prove to be more akin, more compatible in constitution, than others held to be more nearly related because the differentiating factors happen to control less conspicuous features.

Turning to the consideration of the more complex types of inheritance already referred to, we find numerous instances where a somatic character shows a certain degree of coupling or linkage with another perhaps wholly unrelated character. This phenomenon becomes still further complicated when, as is now known to occur fairly frequently, somatic characters are linked also with the sex character. The results of such linkages appear in the altered proportions in which the various combinations of the several characters appear on cross-breeding. Linkage of somatic characters can be readily demonstrated in the garden Stock. Some strains have flowers with deeply coloured sap, *e.g.*, full red or purple; others are of a pale shade such as a light purple or flesh-colour. In most commercial strains the 'eye' of the flower is white owing to absence of colour in the plastids, but in some the plastids are cream-coloured, causing the sap colour to appear of a much richer hue and giving a cream 'eye.' Cream plastid colour is recessive to white and the deep sap colours are recessive to the pale. When a cream-eyed strain lacking the pale factor is bred with a white-eyed plant of some pale shade, the four possible combinations appear in F_2 but not, as we should expect in the case of two independently inherited one-factor characters, in the proportions $9 : 3 : 3 : 1$, with the double recessive as the least abundant of the four forms. We find instead that the double dominant and the double recessive are both in excess of expectation, the latter being more abundant than either of the combinations of one dominant character with one recessive. The two forms which preponderate are those which exhibit the

combinations seen in the parents, the two smaller categories are those representing the new combinations of one paternal with one maternal characteristic. In the Sweet Pea several characters are linked in this manner, viz.: flower colour with pollen shape, flower colour with form of standard, pollen shape with form of standard, colour of leaf axil with functioning capacity of the anthers. If in these cases the cross happens to be made in such a way that the two dominant characters are brought in one from each side of the pedigree instead of both being contributed by one parent, we get again a result in which the two parental combinations occur more frequently, the two recombinations or 'crossovers' less often than we should expect. In the first case the two characters appear to hang together in descent to a certain extent but not completely, in the latter similarly to repel each other. This type of relationship has been found to be of very general occurrence. The linked characters do not otherwise appear to be connected in any way that we can trace, and we therefore conclude that the explanation must be sought in the mechanism of distribution. Two main theories having this fundamental principle as their basis but otherwise distinct have been put forward, and are usually referred to as the *reduplication* and the *chromosome* view respectively. The reduplication view, proposed by Bateson and Punnett,[6] rests on the idea that segregation of factors need not necessarily occur simultaneously at a particular cell division. The number of divisions following the segregation of some factors being assumed to be greater than those occurring in the case of others, there would naturally result a larger number of gametes carrying some factorial combinations and fewer carrying others. If this differential process is conceived as occurring in an orderly manner it would enable us to account for the facts observed. We could imagine how it came about that gametic ratios such as $3:1:1:3, 7:1:1:7, 15:1:1:15$, and so on arose giving the series of linkages observed. It has, however, to be said that we cannot say *why* segregation should be successive nor at what moments, on this view, it must be presumed to occur. On the other hand, the conceptions embodied in the chromosome hypothesis as formulated by Morgan and his fellow-workers[7] are, in these respects, quite precise. They are built around one cardinal event in the life cycle of animals and plants (some of the lowest forms excepted), viz.: the peculiar type of cell division at which the number of chromosomes is reduced to half that to be found during the period of the life cycle extending backwards from this moment to the previous act of fertilisation. In the large number of cases already investigated the chromosome number has been found as a rule to be the same at each division of the somatic cells. We can, in fact, take it as established that it is ordinarily constant for the species. These observations lend strong support to the view that the chromosomes are persistent structures; that is to say, that the chromatin tangle of the resting nucleus, whether actually composed of one continuous thread or not, becomes resolved into

[6] *Proc. Roy. Soc.*, 1911.
[7] *The Mechanism of Mendelian Heredity* (Morgan, Sturtevant, Muller, Bridges), 1915.

separate chromosomes at corresponding loci at each successive mitosis. The reduction from the diploid to the haploid number, according to the more generally accepted interpretation of the appearances during the meiotic phase, is due to the adhering together in pairs of homologous chromosomes, each member of the set originally received from one parent lying alongside and in close contact with its mate received from the other. Later these bivalent chromosomes are resolved into their components so that the two groups destined one for either pole consist of whole dissimilar chromosomes, which then proceed to divide again longitudinally to furnish equivalent half chromosomes to each of the daughter nuclei. According to the view of Farmer the homologous chromosomes do not lie alongside, but become joined end to end. The longitudinal split seen in the bivalent structure is interpreted as a separation not of whole chromosomes but of half chromosomes already formed in anticipation of the second division of the meiotic phase. As however on either interpretation the same result is ultimately secured, viz.: the distribution of whole paternal and maternal chromosomes to different nuclei which now contain the haploid number, it is not essential to our present purpose to discuss the cytological evidence in support of these opposing views in further detail. Nor, indeed, would it be practicable within the limits of this Address. The obvious close parallel between the behaviour of the chromosomes—their pairing and separation—and that of Mendelian allelomorphs which similarly show pairing and segregation, first led to the suggestion that the factors controlling somatic characters are located in these structures. The ingenious extension of this view which has been elaborated by Morgan and his co-workers presumes the arrangement of the factors in linear series after the manner of the visible chromomeres—the bead-like elements which can be seen in many organisms to compose the chromatin structure—each factor and its opposite occupying corresponding loci in homologous chromosomes. From this conception follows the important corollary of the segregation of the factors during the process of formation and subsequent resolution of the bivalent chromosomes formed at the reduction division. We should suppose, according to Morgan, in the case of characters showing *independent* inheritance and giving identical Mendelian ratios whichever way the mating is made, and however the factorial combination is brought about, that the factors controlling the several characters are located in *different* chromosomes. Thus, in the case of *Datura* already mentioned, the two factors affecting sap colour and prickliness respectively would be presumed to be located so far apart in the resting chromatin thread that when separation into chromosomes takes place they become distributed to different members. Where unrelated characters show a *linked* inheritance the factors concerned are held on the other hand to lie so near together that they are always located in one and the same chromosome. Furthermore, and here we come to the most debatable of the assumptions in Morgan's theory, when the bivalent chromosome composed of a maternal and a paternal component gives rise at the reduction division to two single dissimilar chromosomes, these new chromosomes do not always represent the original intact maternal and paternal

components. It has been observed in many forms that the bivalent structure has the appearance of a twisted double thread. Already in 1909 cytological study of the salamander had led Janssen[8] to conclude that fusion might take place at the crossing points, so that when the twin members ultimately draw apart each is composed of alternate portions of the original pair. Morgan explains the breeding results obtained with *Drosophila* by a somewhat similar hypothesis. He also conceives that in the process of separation of the twin lengths of chromatin cleavage between the two is not always clean, portions of the one becoming interchanged with corresponding segments of the other, so that both daughter chromosomes are made up of complementary sections of the maternal and paternal members of the duplex chromosome. To picture this let us imagine that two bars of that delectable substance, Turkish Delight, one pink and one white, are laid alongside and are then given a half twist round each other and pressed together. If, with a knife inserted between the two pieces at one end, the double bar is now sliced longitudinally down the middle neither of the two halves will be wholly pink or wholly white. Each half will be particoloured, the pink portion in one and the portion which is white in the other representing corresponding regions of the original bars. If the complete twist is made, or if the number of turns is still further increased before the slicing, the number of alternately coloured portions will naturally be increased correspondingly. Though the precise manner in which the postulated chromosomal interchange is brought about in Janssen's 'chiasmatype' and Morgan's 'crossing-over' scheme is different, the resulting gametic output would be the same. A critical examination by Wilson and Morgan,[9] from different aspects, of Janssen's interpretation of the cytological evidence including discussion of his latest suggestion that in the case of compound ring chromosomes cleavage in one plane would result in the separation of homologous elements in one ring but not in another has just appeared. These authors are not disposed to accept Janssen's conclusions,[10] but reserve their final statement pending the appearance of his promised further contribution. Should Janssen's view of the evolutions of these complex chromosome structures be upheld, the process of segregation might in such cases become extended over more than one mitosis, as on the reduplication theory is conceived to be the case at some point, though evidence in this direction has hitherto been lacking. Bisection of a bivalent chromosome in this fashion might, moreover, yield the class of results to explain which Morgan has found it necessary to have recourse to hypothetical lethal factors. On the main issue, however, both schemes are in accord. A physical basis for the phenomenon of linkage is found in the presumed nature and behaviour of the chromosomes, viz.: their colloidal consistency, their adhesion after pairing at the points of contact, when in the twisted condition, and their consequent failure to separate cleanly before undergoing the succeeding division.

[8] *La Cellule*, xxv.
[9] *Am. Nat.*, vol. 54, 1920.
[10] See *Comptes Rendus Soc. Belg. Biol.*, 1919

According to Morgan the frequency of separation of linked characters is a measure of the distance apart in the chromosome of the loci for the factors concerned, and it becomes possible to map their position in the chromosome relatively to one another. In this attempt to find in cytological happenings a basis for the observed facts of inheritance our conception of the material unit in the sorting-out process has been pushed beyond the germ cell and even the entire chromosome to the component sections and particles of the latter structure.

To substantiate the ' chromosome ' view the primary requisite was to obtain proof that a particular character is associated with a particular chromosome. With this object in view it was sought to discover a type in which individual chromosomes could be identified. Several observers working on different animals found that a particular chromosome differing in form from the rest could be traced at the maturation division, and that this chromosome was always associated with the sex character in the following manner. The female possessed an even number of chromosomes so that each egg received an identical number, including this particular sex-chromosome. The male contained an uneven number, having one fewer than the female, with the result that half the sperms received the same number as the egg including the sex-chromosome, and half were deficient in this particular chromosome. Eggs fertilised with sperms containing the full number of chromosomes developed into females, while those fertilised with sperms lacking this distinctive chromosome produced males. Morgan made the further discovery in the fruit fly *Drosophila ampelophila* that certain factors controlling various somatic characters were located in the sex-chromosome. The inheritance of these characters and of sex evidently went together. A male exhibiting the dominant condition of such a sex-linked character bred to a recessive female gave daughters all dominant and sons all recessive (fig. 2), but in the reciprocal cross both sons *and* daughters proved to be all dominants (fig. 1). Since the mother with the dominant factor contributed it to all her children (fig. 1), whereas, where the father bore it, it descended only to his daughters (fig. 2), it was apparent that the female was homozygous and the male heterozygous for the *somatic* character. Further, although no distinction is observable in this species between the sperms, the occurrence of this sex-linked form of inheritance indicated that here, as in the other cases mentioned, it is the female which behaves as a homozygote for the *sex* character and the male as a heterozygote, the sex-chromosomes of some sperms differing presumably in character, though not in appearance, from those of others. The sperms of *Drosophila* are therefore conceived as of two kinds, one containing the same sex-chromosome as the eggs, the so-called X chromosome, and the other a mate of a different nature, the Y chromosome, which appears to be inert and unable to carry the dominant allelomorphs. If, now, we suppose the factor for the sex-linked somatic character to be located in the X chromosomes we understand why the dominant female, which is XX, and therefore furnishes an X chromosome to every egg, should contribute the dominant character to all her

offspring. And conversely, why the dominant male, which is XY, when bred to a recessive female, produces offspring which are either female and dominant or male and recessive.

Tracing the chromosomes into the next (F_2) generation we see also the reason for the different result obtained from the reciprocal matings if the F_1 individuals are inbred. When the *female* parent has the dominant sex-linked character half the eggs of the daughters and half the sperms of the sons receive this character. As the sperms receive it along with the X chromosome fertilisation of either kind of egg by these X sperms will cause the character to descend to each grand-daughter. The grandsons, on the other hand, since they arise from fertilisation by the sperms lacking the dominant character—*i.e.*, by the Y sperms—will be dominant or recessive according as these sperms unite with the one type of egg or with the other. Thus we get the Mendelian F_2 ratio 3D to 1R (fig. 1), but so linked with sex that the dominant class comprises half the males and all the females, while the remaining half of the males make up the recessive class. Where it is the *male* parent that carries the dominant, and where therefore the dominant character passes along with the X chromosome *only* to the daughters in F_1, their eggs, as in the reciprocal cross, are of two kinds, but the sons' sperms all carry the recessive allelomorphs. Both kinds of eggs being fertilised with both X sperms and Y sperms, the dominant and recessive characters will occur *equally in both sexes* among the grandchildren, and we get the Mendelian ratio of 1D to 1R (fig. 2). Muller [11] puts the number of factors already located in the X chromosome of *Drosophila* at not less than 500, and in those that have so far been investigated this form of inheritance has been found to hold.

Instances of sex-linked inheritance are now known in many animals, some of which are strictly comparable with *Drosophila*, others follow the same general principle, but have the relations of the sexes reversed, as exemplified by the moth *Abraxas*, which has been worked out by Doncaster, [12] whose sudden death we have so recently to deplore. Here the female is the heterozygous sex, and contains the dummy mate of the sex-chromosome.

The behaviour of the sex-chromosomes as here outlined suffices to account for the occurrence of sex-linked inheritance, but the relations found to hold between one sex-linked character and another need further explanation. If a cross is made involving two sex-linked characters, the F_1 females when tested by a double recessive male are found to produce the expected four classes of gametes, but not in equal proportions, nor in the same proportions in the case of different pairs of sex-linked characters. Partial linkage (coupling) occurs of the kind which has already been described for the Stock and the Sweet Pea. The parental combinations predominate, the recombinations (' cross-overs ') comprise the smaller categories. The strength of the linkage varies, however, for different characters, but is found to be constant for any given pair. Since the sex-linked factors are by hypothesis

[11] *Am. Nat.*, vol. liv., 1920.
[12] *Rep. Evolution Committee*, iv., 1908.

Fig.1
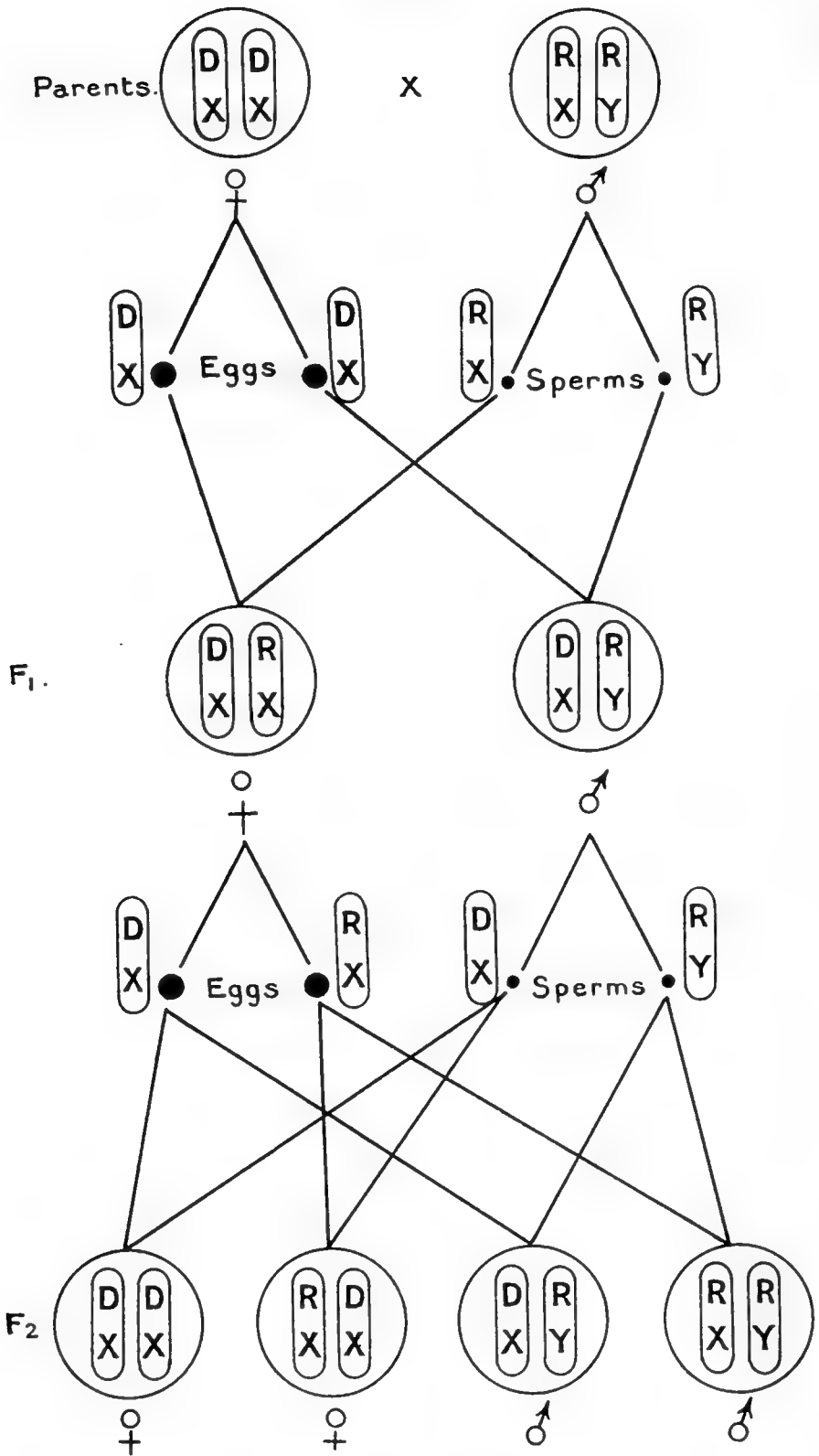
3 D : 1 R

Fig. 2.
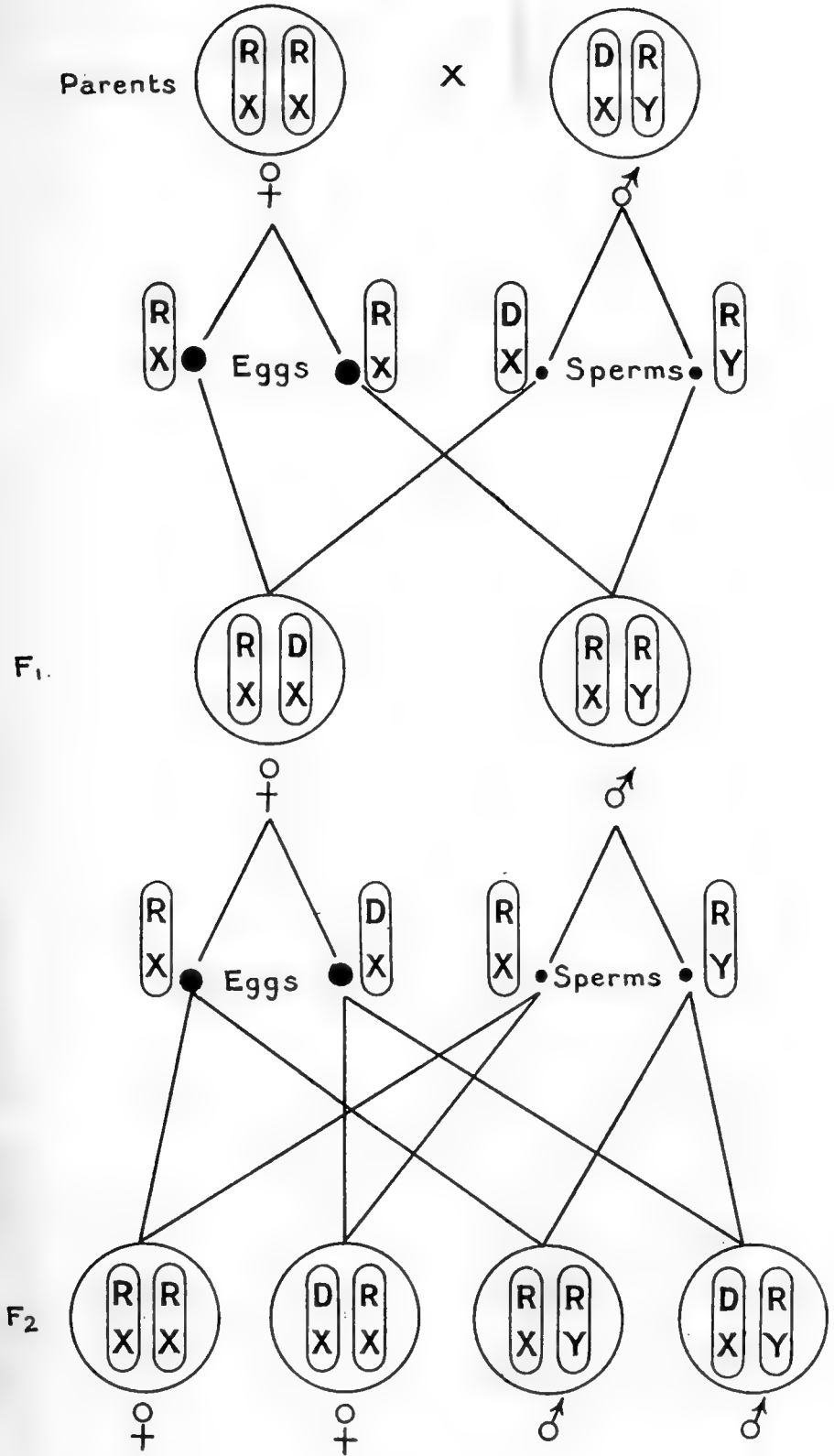
carried in the sex-chromosomes, a clean separation of homologous members at meiosis should result in the characters which were associated in the parents remaining strictly in the same combination in each succeeding generation. The fact that this is not the case has led Morgan to conclude that an interchange of chromosome material must take place at this phase among a proportion of the gametes, and that the percentage of these 'cross-overs' will depend on the distance apart of the loci of the factors concerned. This phenomenon of linkage may also be exhibited by pairs of characters which show *no* sex-linkage in their inheritance. The factors involved in these latter cases must presumably, therefore, be disposed in one of the chromosomes which is not the sex-chromosome.

To this brief sketch of the main points of Morgan's chromosome theory must be added mention of the extremely interesting relation which lends strong support to his view, and the significance of which seems scarcely to admit of question, viz.: that in *Drosophila ampelophila* there are four pairs of chromosomes, and that the linkage relations of the hundred and more characters investigated indicate that they form four distinct groups. It is hardly possible to suppose that the one fact is not directly connected with the other. The interesting discovery of Bridges [13] that the appearance of certain unexpected categories among *Drosophila* offspring, where females of a particular strain were used, coincided with the presence in these females of an additional chromosome adds another link in the chain of evidence. On examination it was found that in these females the X chromosome pair occasionally failed to separate at the reduction division, and consequently that the two XX chromosomes sometimes both remained in the egg, and sometimes both passed out into the polar body. Hence there arose from fertilisation of the XX eggs some individuals containing *three* sex-chromosomes, with the resulting upset of the expectation in regard to sex-limitation of characters which was observed.

It, however, remains a curious anomaly that in the cross-bred *Drosophila* male no corresponding crossing-over of linked characters, whether associated with the sex character or not, has yet been observed. His gametes carry only the same factorial combinations which he received from his parents. For this contrast in the behaviour between the sexes there is at present no explanation. The reverse condition has been described by Tanaka [14] in the silkworm. Here interchange takes place in the male but not in the female.

It must then be acknowledged that Morgan's interpretation of the cytological evidence has much in its favour. The striking parallel between the behaviour of the chromosomes and the distributional relations of Mendelian allelomorphs is obvious. The existence in *Drosophila ampelophila* of four pairs of chromosomes and of four sets of linked characters can hardly be mere coincidence. The employment of the smaller physical unit in accounting for the reshuffling of characters in their transmission commends itself in principle. The necessity for postulating the occurrence of some orderly irregularity in the hereditary

[13] *J. Exp. Zool.*, xv., 1913.
[14] *J. Coll. Agr.*, Sapporo, Japan, 1913-14.

process in order to explain the phenomenon of partial linkage is, it will be seen, inherent alike in both theories. When, however, we come to examine the general applicability of Morgan's theory we are confronted with a considerable body of facts among plants which we find difficult to reconcile with the requirement that factorial segregation is accomplished by means of the reduction division. An instance in which this is particularly clearly indicated is that of the sulphur-white Stock. I have chosen this example because here we have to do with two characters which are distinguished with the utmost sharpness, viz.: plastid colour and flower form. The peculiar behaviour of this strain is due to the fact that not only are the two factors for flower form (singleness and doubleness) differently distributed to the male and female sides of the individual, as in all double-throwing Stocks, but the factor controlling plastid colour likewise shows linkage with the sex nature of the germ cells. As a result every individual, even though self-fertilised, yields a mixed offspring, consisting chiefly of single whites and double creams, but including a small percentage of double whites. So far as the ovules are concerned, the mode of inheritance can be accounted for on either theory. According to the reduplication hypothesis the factors X Y [15] producing singleness and W giving white plastids are partially coupled so as to give the gametic ratio on the female side 7 WXY : 1 WXy : 1 wxY : 7 wxy.[16] On the chromosome scheme the factorial group WXY must be assumed to be disposed in one member of the bivalent chromosome formed at meiosis, the corresponding recessive allelomorphs wxy in the other. If the three factors be supposed to be arranged in the chromosome in alphabetical order, and if, on separation, a break takes place between the loci of the two factors for flower form (as shown), so as to give 'cross-overs' of Y

Ovules   Pollen.
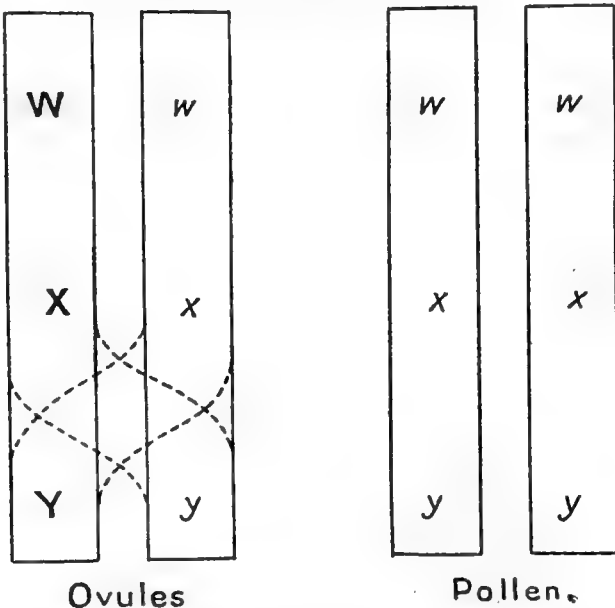
[15] The letters X and Y are used here to denote particular factors, not, as in Morgan's scheme, the entire sex-chromosomes.
[16] Or possibly 15 : 1 : 1 : 15.

and y in about 12 per cent. of the gametes, the occurrence of such 'cross-overs' would fulfil the required conditions. But the case of the pollen presents a distinct difficulty on this latter view. This Stock is distinguished both from the *Drosophila* and the *Abraxas* type by the fact that none of the male germs carry either of the dominant characters. In place of the XX—XY form of sex-linked inheritance in the former type and the WZ—ZZ in the latter, we should need to regard this form as constituting a new class, which we might represent as DR—RR, thus indicating that both members of the bivalent chromosome on the male side appear to be inert and able to carry only the recessive characters, and hence are represented as RR, in contrast with the DR pair of the female side. By this formula we can indicate the behaviour of the several double-throwing strains. It is, besides, becoming clear, I think, from recent results that there is no 'crossing over' of these factors on the male side in the F_1 cross-breds. But the real difficulty is to explain why these factors are confined to the female side in the ever-sporting individual. This may result from aberrant behaviour or loss of chromosomes at some point in pollen development. On this point I hope that evidence will shortly be available. Failing such evidence the presumption is that the elimination of XY (and in one strain of W) must have taken place *prior to,* and not at, the moment of the maturation division. Morgan's proposal to fit the pollen into his scheme for *Drosophila* by having recourse to hypothetical lethal factors does not appeal to the observer, who finds the pollen all uniformly good and every ovule set. Zygotic lethals are clearly not in question under these circumstances. The supposition of gametic lethals confined to the pollen appears far-fetched, seeing that of the missing combinations two, viz.: single white, the double dominant, and double white a dominant-recessive, occur in the ovules, and the third, the single cream, the other dominant-recessive, exists as a pure strain, so that the homozygous condition is evidently not in itself a cause of non-development. Other examples suggesting premeiotic segregation can be quoted, notably cases among variegated plants and plants showing bud sports, where somatic segregation appears to be of regular occurrence. Among the Musciniae the present evidence appears to show that the sex potentiality segregates in some forms at the division of the spore mother cells, so that already the spores possess a sex character; while in other species this separation takes place later, during the development of the gametophyte, the spores being then all alike and undifferentiated in this respect. In *Funaria hygrometrica,* an example of the latter class, an attempt has been made by E. J. Collins[17] to ascertain the stage at which sex segregation takes place by inducing the growth of new individuals from isolated portions of the vegetative tissues of the gametophyte. No doubt when the evidence is derived from experiments in which a portion of the plant has been severed from the rest, it is possible to urge that the result obtained is not necessarily indicative of the potentiality in the intact organism. Phenotypic appearance is the product of a reaction system, in which the internal as well as the external environment plays its part. We

[17] *Journal of Genetics,* vol. viii., 1919.

have, for example, evidence that the manifestation of a character may be dependent upon the variation of internal conditions with age; in other words, a time relation may be involved.[18]   Or, again, upon the state of general internal equilibrium resulting from the relation of one morphological member or region to another.   Thus removal of the lamina of the leaf, so as to leave only the midrib, may cause the mutilated individual to develop hairs on the stems and petioles in the same environment in which the intact individual remains hairless. Injury from attack by insects in a glabrous form may in like manner lead to the production of hairs which, by their resemblance to those of an allied species, show that the pathological condition set up has caused genetic potentiality to become actual.   But even if we exclude the class of evidence to which objection on these grounds might be made, there still remain various cases of normal types, where, unless the behaviour of the chromosomes should point to a different explanation, it seems most natural to assume that segregation takes place before the reduction division.

It has been argued from time to time that any scheme representing the mechanism of Heredity which leaves out of account the cytoplasm must prove inadequate.   This general statement has been expressed in more definite form by Loeb,[19] who holds that the egg cytoplasm is to be looked upon as determining the broad outlines, in fact as standing for the embryo ' in the rough,' upon which are impressed in the course of development the characteristics controlled by the factors segregated in the chromosomes.   The arguments in favour of the view that the cytoplasm, apart from its general functions in connection with growth and nutrition, is the seat of a particular hereditary process are mainly derived from observation upon embryonic characters in certain animals, chiefly Echinoderms, where the inheritance appears to be purely maternal.   It has been shown, however, that such female prepotency is no indication that inheritance of the determining factors takes place through the cytoplasm.   Other causes may lead to this result. It has been observed, for example, that hybrid sea-urchin larvae, which at one season of the year were maternal in type, at another were all paternal in character, showing that the result was due to some effect of the environment. Again, where the hybrid plutei showed purely maternal characters it was discovered by Baltzer [20] that in the earliest mitoses of the cross-fertilised eggs a certain number of chromosomes fail to reach the poles, and are consequently left out of the daughter nuclei. The chromosomes thus lost probably represent those contributed by the male gamete, for in both parents certain individual chromosomes can be identified owing to differences in shape and size.   After this process of elimination those characteristic of the male parent could not be traced, whereas the one pair distinctive of the female parent was still recognisable.   In the reciprocal cross where the first mitosis

[18] As in the case of characters which exhibit a regular change of phase, *e.g.*, the colour of white and cream Stocks is indistinguishable in the bud, and a yellow-seeded Pea is green at an earlier stage.

[19] *The Organism as a Whole*, 1916.

[20] *Archiv für Zellforschung*, v., 1910.

follows a normal course the embryos are intermediate in regard to character of the skeleton, thus affording proof of the influence of the male parent. Another type of case is found in the silkworm. Here a certain rate character determining the time of hatching out of the eggs has been shown to exhibit normal Mendelian inheritance, the appearance that it is transmissible by the female through the cytoplasm alone being delusive. The eggs are always laid in the spring. According as they hatch out immediately so that a second brood is obtained in the year, or do not hatch out for twelve months, the female parent laying the eggs is described as bivoltin or univoltin. Now the length of interval before hatching is obviously an egg character, and therefore maternal in origin. Consequently when a cross is made between a univoltin female and a bivoltin male the eggs laid are not cross-bred in respect of this character, any more than the seed formed as a result of a cross is cross-bred in respect of its seed coat, which is a maternal structure. The silkworm mother being univoltin, the eggs will not hatch out until the following spring. The F_1 mother will in turn lay eggs which again take twelve months to hatch, since the long-period factor is the dominant. It is not until the eggs of the F_2 generation are laid that we see the expression of the character introduced by the bivoltin father. For some of the egg batches hatch at once, others not for twelve months, showing that of the F_2 females some were uni- and some bi-voltin, and hence that the egg character in any generation depends upon both the maternal and the paternal antecedents of the female producing the eggs. Consequently, in the case of an egg character the effects of inheritance must be looked for in the generation succeeding that in which the somatic characteristics of the zygote become revealed. We find in fact that in almost all instances where the evidence is suggestive of purely cytoplasmic inheritance, fuller investigation has shown that the explanation is to be found in one of the causes here indicated. The case of some plants where it has been established that reciprocal hybrids are dissimilar still, however, remains to be cleared up. Among such may be cited certain Digitalis hybrids. Differences in the reciprocal hybrids of *D. grandiflora* and *D. lutea* were described by Gaertner, and in the earlier literature dealing with Digitalis species hybrids other cases are to be found. In more recent years J. H. Wilson [21] has repeated the crossing of *D. purpurea* and *D. lutea*, and states that the reciprocals are indistinguishable during the vegetative period, but that they differ in size and colouring of the flowers, the resemblance being the greater in each case to the seed parent. A detailed comparison of the differential characters of the reciprocal hybrids of *D. purpurea* and *D. grandiflora* has been set out by Neilson Jones,[22] who similarly finds in both matings a greater resemblance to the mother species. We know nothing as yet of the cytology of these cases, and it is not improbable that the interpretation may be found in some aberrant behaviour of the chromosomes. An instance in a plant type where a definite connection appears traceable between chromosome behaviour and somatic appearance has been

[21] *Rep. Third International Congress on Genetics*, R.H.S. 1906.
[22] *J. of Genetics*, vol. ii., 1912.

recently emphasised by Gates,[23] who attributes the peculiarity of the *lata* mutation in Œnothera (which has arisen as a modification at different times from each of three distinct species) to an irregularity in meiosis in the germ mother cells whereby one daughter cell receives an extra duplicate chromosome while the sister cell lacks this chromosome. The cell with the extra chromosome fertilised by a normal germ produces a *lata* individual. On the chromosome view every normal fertilised egg contains a double set of chromosomes, each carrying a complete set of the factor elements. Hence, if some of the one set become eliminated we can still imagine that a normal though undersized individual might develop. The converse relation where increased size goes with multiplication of chromosomes was discovered by Gregory,[24] in a Primula, and occurs also in *Œnothera gigas*, a mutant derived from *Œ. Lamarckiana*. Gregory found in his cultures giant individuals which behaved as though four instead of two sets of factors were present, and upon examination these individuals were found to contain twice the normal number of chromosomes. It is interesting in this connection to recall the results obtained by Nemec[25] as the result of subjecting the root tips of various plants to the narcotising action of chloral hydrate. Under this treatment cells undergoing division at the time were able to form the daughter nuclei, but the production of a new cell wall was inhibited. The cells thus became binucleate. If on recovery these cells were to fuse before proceeding to divide afresh a genuine tetraploid condition would result. So few cases of natural tetraploidy have so far been observed that we have as yet no clue to the cause which leads to this condition.

The conclusions to which we are led by the considerations which have here been put forward are, in the main, that we have no warrant in the evidence so far available for attributing special hereditary processes to the cytoplasm as distinct from the nucleus. On the other hand, there is a very large body of facts pointing to a direct connection between phenotypic appearance and chromosomal behaviour. In animals the evidence that the chromosomes constitute the distributional mechanism may be looked upon as almost tantamount to proof; in plants the observations on *Drosera, Primula, Œnothera, Sphærocarpus* are in harmony with this view. When we come, however, to the question of linkage and general applicability of the conception of ' crossing over ' as adopted by Morgan and his school we are on less certain ground. In *Drosophila* itself, the case which the scheme was framed to fit, the entire absence of ' crossing over ' in the male remains unaccounted for, while the evidence from certain plant types appears to be definitely at variance with one of its fundamental premises. If segregation at the recognised reduction division is definitely established for animal types, then we must conclude that the sorting-out process may follow a different course in the plant.

The question as to what is the precise nature of the differences for

[23] *New Phytologist*, vol. xix., 1920.
[24] *Proc. Roy. Soc.*, vol. lxxxvii. b, 1914.
[25] *Jahrb. f. wiss. Bot.*, xxxix., 1904, ' Das Problem der Begruchtungsvorgänge,' 1910.

which the Mendelian factors stand is constantly before the mind of the breeder, but we are only now on the threshold of investigation in this direction, and it is doubtful whether we can as yet give a certain answer in any single instance. Still less are we able to say what the actual elements or units which undergo segregation may be. In the case of such allelomorphic pairs as purple and red sap colour or white or cream plastid colour it may be that the difference is *wholly qualitative*, consisting merely in the formation or non-formation of some one chemical substance. But the majority of characteristics are not of this hard-and-fast type. Between some the distinction appears to be one of *range*—to be quantitative rather than, or as well as, qualitative in nature, and range must mean, presumably, either cumulative effect or a force or rate difference. It may well be, for example, that with some change in physiological equilibrium accompanying growth and development, factorial action may be enhanced or accelerated, or, on the other hand, retarded or even inhibited altogether, and a regional grading result in consequence. Range in a character is not confined to, though a common characteristic of, individuals of cross-bred origin. It may be a specific feature, both constant and definite in nature. For example, a change as development proceeds from a glabrous or nearly glabrous to a hairy condition is not of unusual occurrence in plants. In the Stock such a gradational assumption of hairiness is apparent no less in the homozygous form containing a certain weak allelomorph controlling surface character, when present with the factors for sap colour, than in those heterozygous for this or some other essential component. We see a similar transition in several members of the *Scrophulariaceæ*—*e.g.*, in various species of *Digitalis*, in *Antirrhinum majus*, *Antirrhinum Orontium*, *Anarrhinum pedatum*, *Pentstemon*, and *Nemesia*. In perennials an annual recurrence of this change of phase may be seen, as in various species of *Viola* and in *Spiræa Ulmaria*. It is somewhat curious that the transition may be in the same direction —from smoothness to hairiness—in forms in which the dominant-recessive relation of the two conditions is opposite in nature, as in *Matthiola* on the one hand and *Digitalis purpurea nudicaulis* on the other. Manifestation of the dominant characteristic gradually declines in the Foxglove, while it becomes more pronounced in the Stock. In some, perhaps all, of these cases the allelomorphs may stand for certain states of physiological equilibrium, or such states may be an accompanying feature of factorial action. A change of phase may mean an altered balance, a difference of rhythm in interdependent physiological processes. In the case, for instance, of a certain sub-glabrous strain of Stock in which the presence of a single characteristically branched hair or hair-tuft over the water-gland terminating the midrib in a leaf otherwise glabrous is an hereditary character, it is hardly conceivable that there is a localisation in this region of a special hair-forming substance. It seems more probable that some physiological condition intimately connected with the condition of water-content at some critical period is a causal factor in hair production, and that this condition is set up over the whole leaf in the type, but in the particular strain in question is maintained only at the point which receives the

largest and most direct supply. In this same strain a leaf may now and again be found lacking this hydathode trichome in an otherwise continuous hair-forming series, an occurrence which may well result from a slight fluctuation in physiological equilibrium such as is inherent in all vital processes—a fluctuation which, when the genetic indicator is set so near to the zero point, may well send it off the scale altogether. If, as is not improbable in this and similar cases, we are concerned with a complex chain of physiological processes, investigation of the nature of the differences for which the allelomorphs stand may present a more difficult problem than where the production of a particular chemical compound appears to be involved. In such a physiological conception we have probably the explanation of the non-appearance of the recessive character in certain dominant cross-breds.

Up to this point we have treated of the organism from the aspect of its being a wholly self-controlled, independent system. As regards some characteristics, this may be regarded as substantially the case. That is to say, the soma reflects under all observed conditions the genetic constitution expressed in the Mendelian formula. Correspondence is precise between genotypic potentiality and phenotypic reality, and we have so far solved our problem that we can predict certainly and accurately the appearance of offspring, knowing the constitution of the parents. In such cases we may say that the efficiency of the genetic machine works out at 100 per cent., the influence of external environment at 0. Our equation somatic appearance=factorial constitution requires no correction for effect of conditions of temperature, humidity, illumination, and the like. But most somatic characters show some degree of variability. Phenotypic appearance is the outcome primarily of genotypic constitution, but upon this are superposed fluctuations, slight or more pronounced, arising as the result of reaction to environmental conditions. In the extreme case the genetic machinery may, so to speak, be put out of action; genotypic potentiality no longer becomes actual. We say that the character is not inherited. We meet with such an example in *Ranunculus aquatilis*. According to Mer,[26] the terrestrial form of this plant has no hairs on the ends of the leaf segments, but in the aquatic individual the segments end in needle-shaped hairs. That is to say, hairs of a definite form are produced in a definite region. Again, Massart[27] finds that in *Polygonum amphibium* the shoot produces characteristic multicellular hairs when exposed to the air, but if submerged it ceases to form them on the new growth. Every individual, however bred, behaves in the same manner, and must therefore have the same genetic constitution. In an atmospheric environment genotypic expression is achieved, in water it becomes physiologically impossible. A limitation to genotypic expression may in like manner be brought about by the *internal* environment, for the relation of the soma to the germ elements may be looked upon in this light. Thus in the case of a long-pollened and round-pollened Sweet Pea Bateson and Punnett[28] found that the

[26] *Bull. Soc. Bot. de France*, i. 27, **1880**.
[27] *Bull. Jard. Bot. Bruxelles*, i. 2, **1902**.
[28] *Report to the Evolution Committee*, Roy. Soc., ii., **1905**.

F_1 pollen grains are all long, yet half of them carry the factor for roundness. If we take the chromosome view, and if it be presumed that the factor for roundness is not segregated until the reduction division, the cytoplasm of the pollen mother cells may be supposed to act as a foreign medium owing to a mixture of qualities having been impressed upon it through the presence of the two opposite allelomorphs before the moment of segregation. We should consequently infer that the round pollen shape is only produced when the round-factor-bearing chromosome is surrounded by the cytoplasm of an individual which does not contain the long factor. If, further, we regard the result in this case as indicative of the normal inter-relation of nucleus and cytoplasm in the hereditary process, we shall be led to the view that whatever the earlier condition of mutual equilibrium or interchange between these two essential cell constituents may be, an ultimate stage is reached in which the rôle of determining agent must be assigned to the nucleus. To pursue this theme farther, however, in the present state of our knowledge would serve no useful purpose.

Before bringing this Address to a conclusion I may be permitted to add one word of explanation and appeal. In my remarks I have deliberately left on one side all reference to the immense practical value of breeding experiments on Mendelian lines. To have done so adequately would have absorbed the whole time at my disposal. It is unnecessary to-day to point out the enormous social and economic gain following from the application of Mendelian methods of investigation and of the discoveries which have resulted therefrom during the last twenty years, whether we have in mind the advance in our knowledge of the inheritance of ordinary somatic characters and of certain pathological conditions in man, of immunity from disease in races of some of our most important food plants, or of egg-production in our domestic breeds of fowls.

My appeal is for more organised co-operation in the experimental study of Genetics. It is a not uncommon attitude to look upon the subject of Genetics as a science apart. But the complex nature of the problems confronting us requires that the attacking force should be a composite one, representing all arms. Only the outworks of the fortress can fall to the vanguard of breeders. Their part done, they wait ready to hand over to the cytologists with whom it lies to consolidate the position and render our foothold secure. This accomplished, the way is cleared for the main assault. To push this home we urgently need reinforcements. It is to the physiologists and to the chemists that we look to crown the victory. By their co-operation alone can we hope to win inside the citadel and fathom the meaning of those activities which take shape daily before our eyes as we stand without and observe, but the secret of which is withheld from our gaze.

ADDRESS

TO THE

EDUCATIONAL SCIENCE SECTION

BY

SIR ROBERT BLAIR, LL.D.,

PRESIDENT OF THE SECTION.

Introduction.

THE requirements of the Act of 1918 and the endeavour to frame scales of salaries for teachers on a national basis are, at present, absorbing so much of the energy of those engaged in educational administration that I have thought it advisable to turn our attention from the immediate needs of the day to two of the wider aspects of our educational activities, which belong to the spirit rather than to the form of our educational system.

It is natural that in this meeting of the British Association for the Advancement of Science I should take first the Science of Education.

I.

The value to education of science and the scientific method has hitherto been for the most part indirect and incidental. It has consisted very largely in deductions from another branch of study, namely, psychology, and has resulted for the most part from the invasion into education of those who were not themselves educationists. A moment has now been reached when education itself should be made the subject of a distinct department of science, when teachers themselves should become scientists.

There is in this respect a close analogy between education and medicine. Training the mind implies a knowledge of the mind, just as healing the body implies a knowledge of the body. Thus, logically, education is based upon psychology, as medicine is based on anatomy and physiology. And there the text-books of educational method are usually content to leave it. But medicine is much more than applied physiology. It constitutes an independent system of facts, gathered and analysed, not by physiologists in the laboratory, but by physicians working in the hospital or by the bedside. In the same way, then, education as a science should be something more than mere applied psychology. It must be built up not out of the speculations of theorists, or from the deductions of psychologists, but by direct, definite, *ad hoc* inquiries concentrated upon the problems of the class-

room by teachers themselves. When by their own researches teachers have demonstrated that their art is, in fact, a science, then, and not till then, will the public allow them the moral, social, and economic status which it already accords to other professions. The engineer and the doctor are duly recognised as scientific experts. The educationist should see to it that his science also becomes recognised, no longer as a general topic upon which any cultured layman may dogmatise, but as a technical branch of science, in which the educationist alone, in virtue of his special knowledge, his special training, his special experience, is the acknowledged expert.

Educational science has hitherto followed two main lines of investigation: first, the evaluation and improvement of teachers' methods; secondly, the diagnosis and treatment of children's individual capacities.

I. *The Psychology of the Individual Child.*

It is upon the latter problem, or group of problems, that experimental work has in the past been chiefly directed, and in the immediate future is likely to be concentrated with the most fruitful results. The recent advances in ' individual psychology '—the youngest branch of that infant science—have greatly emphasised the need, and assisted the development, of individual teaching. The keynote of successful instruction is to adapt that instruction to the individual child. But before instruction can be so adapted, the needs and the capacities of the individual child must first be discovered.

A. *Diagnosis.*

Such discovery (as in all sciences) may proceed by two methods, by observation and by experiment.

(1) The former method is in education the older. At one time, in the hands of Stanley Hall and his followers—the pioneers of the Child-Study movement—observation yielded fruitful results. And it is perhaps to be regretted that of late simple observation and description have been neglected for the more ambitious method of experimental tests. There is much that a vigilant teacher can do without using any special apparatus and without conducting any special experiment. Conscientious records of the behaviour and responses of individual children, accurately described without any admixture of inference or hypothesis, would lay broad foundations upon which subsequent investigators could build. The study of children's temperament and character, for example—factors which have not yet been accorded their due weight in education—must for the present proceed upon these simpler lines.

(2) With experimental tests the progress made during the last decade has been enormous. The intelligence scale devised by Binet for the diagnosis of mental deficiency, the mental tests employed by the American army, the vocational tests now coming into use for the selection of employees—these have done much to familiarise, not school teachers and school doctors only, but also the general public, with the aims and possibilities of psychological measurement. More recently an endeavour has been made to assess directly the results of school

instruction, and to record in quantitative terms the course of progress from year to year, by means of standardised tests for educational attainments. In this country research committees of the British Association and of the Child-Study Society have already commenced the standardisation of normal performances in such subjects as reading and arithmetic. In America attempts have been made to standardise even more elusive subjects, such as drawing, handwork, English composition, and the subjects of the curriculum of the secondary school.

B. *Treatment.*

This work of diagnosis has done much to foster individual and differential teaching—the adaptation of education to individual children, or at least to special groups and types. It has not only assisted the machinery of segregation—of selecting the mentally deficient child at one end of the scale and the scholarship child at the other end; but it has also provided a method for assessing the results of different teaching methods as applied to these segregated groups. Progress has been most pronounced in the case of the sub-normal. The mentally defective are now taught in special schools, and receive an instruction of a specially adapted type. Some advance has more recently been made in differentiating the various grades and kinds of so-called deficiency, and in discriminating between the deficient and the merely backward and dull. With regard to the morally defective and delinquent little scientific work has been attempted in this country, with the sole exception of the new experiment initiated by the Birmingham justices. In the United States some twenty centres or clinics have been established for the psychological examination of exceptional children; and in England school medical officers and others have urged the need for ' intermediate ' classes or schools not only to accommodate backward and borderline cases and cases of limited or special defect (*e.g.*, ' number-defect ' and so-called ' word-blindness ') but also to act as clearing-houses.

In Germany and elsewhere special interest has been aroused in super-normal children. The few investigations already made show clearly that additional attention, expenditure, study, and provision will yield for the community a far richer return in the case of the super-normal than in the sub-normal.

At Harvard and elsewhere psychologists have for some time been elaborating psychological tests to select those who are best fitted for different types of vocation. The investigation is still only in its initial stages. But it is clear that if vocational guidance were based, in part at least, upon observations and records made at school, instead of being based upon the limited interests and knowledge of the child and his parents, then not only employers, but also employees, their work, and the community as a whole, would profit. A large proportion of the vast wastage involved in the current system of indiscriminate engagement on probation would be saved.

The influence of sex, social status, and race upon individual differences in educational abilities has been studied upon a small scale. The differences are marked: and differences in sex and social status, when better understood, might well be taken into account both in

diagnosing mental deficiency and in awarding scholarships. As a rule, however, those due to sex and race are smaller than is popularly supposed. How far these differences, and those associated with social status, are inborn and ineradicable, and how far they are due to differences in training and in tradition, can hardly be determined without a vast array of data.

II. *Teaching Methods.*

The subjects taught and the methods of teaching have considerably changed during recent years. In the more progressive types of schools several broad tendencies may be discerned. All owe their acceptance in part to the results of scientific investigators.

(1) Far less emphasis is now laid upon the *disciplinary value of subjects*, and upon subjects whose value is almost solely disciplinary. Following in the steps of a series of American investigators, Winch and Sleight in this country have shown very clearly that practice in one kind of activity produces improvements in other kinds of activities, only under very limited and special conditions. The whole conception of transfer of training is thus changed, or (some maintain) destroyed; and the earlier notion of education as the strengthening, through exercise, of certain general faculties has consequently been revolutionised. There is a tendency to select subjects and methods of teaching rather for their material than their general value.

(2) Far less emphasis is now laid upon an advance according to strict *logical sequence* in teaching a given subject of the curriculum to children of successive ages. The steps and methods are being adapted rather to the natural capacities and interests of the child of each age. This genetic standpoint has received great help and encouragement from experimental psychology. Binet's own scale of intelligence was intended largely as a study in the mental development of the normal child. The developmental phases of particular characteristics (*e.g.*, children's ideals) and special characteristics of particular developmental phases (*e.g.*, adolescence) have been elaborately studied by Stanley Hall and his followers. Psychology, indeed, has done much to emphasise the importance of the post-pubertal period—the school-leaving age, and the years that follow. Such studies have an obvious bearing upon the curriculum and methods for our new continuation schools. But it is, perhaps, in the revolutionary changes in the teaching methods of the infants' schools, changes that are already profoundly influencing the methods of the senior department, that the influence of scientific study has been most strongly at work.

(3) Increasing emphasis is now being laid upon *mental and motor activities*. Early educational practice, like early psychology, was excessively intellectualistic. Recent child-study, however, has emphasised the importance of the motor and of the emotional aspects of the child's mental life. As a consequence, the theory and practice of education have assumed more of the pragmatic character which has characterised contemporary philosophy.

The progressive introduction of manual and practical subjects, both in and for themselves, and as aspects of other subjects, forms the most

notable instance of this tendency. The educational process is assumed to start, not from the child's sensations (as nineteenth-century theory was so apt to maintain), but rather from his motor reactions to certain perceptual objects—objects of vital importance to him and to his species under primitive conditions, and therefore appealing to certain instinctive impulses. Further, the child's activities in the school should be, not indeed identical with, but continuous with, the activities of his subsequent profession or trade. Upon these grounds handicraft should now find a place in every school curriculum. It will be inserted both for its own sake, and for the sake of its connections with other subjects, whether they be subjects of school life, of after life, or of human life generally.

(4) As a result of recent psychological work, more attention is now being paid to the *emotional, moral, and æsthetic* activities. This is a second instance of the same reaction from excessive intellectualism. Education in this country has ever claimed to form character as well as to impart knowledge. Formerly, this aim characterised the Public Schools rather than the public elementary schools. Recently, however, much has been done to infuse into the latter something of the spirit of the Public Schools. The principle of self-government, for example, has been applied with success not only in certain elementary schools, but also in several colonies for juvenile delinquents. And, in the latter case, its success has been attributed by the initiators directly to the fact that it is the corollary of sound child-psychology.

Bearing closely upon the subject of moral and emotional training is the work of the psycho-analysts. Freud has shown that many forms of mental inefficiency in later life—both major (such as hysteria, neurosis, certain kinds of ' shell-shock,' &c.) and minor (such as lapses of memory, of action, slips of tongue and pen)—are traceable to the repression of emotional experiences in earlier life. The principles themselves may, perhaps, still be regarded as, in part, a matter of controversy. But the discoveries upon which they are based vividly illustrate the enormous importance of the natural instincts, interests, and activities inherited by the child as part of his biological equipment; and, together with the work done by English psychologists such as Shand and McDougall upon the emotional basis of character, have already had a considerable influence upon educational theory in this country.

(5) Increasing emphasis is now being laid upon *freedom* for individual effort and initiative. Here, again, the corollaries drawn from the psycho-analytic doctrines as to the dangers of repression are most suggestive. Already a better understanding of child-nature has led to the substitution of ' internal ' for ' external ' discipline; and the pre-determined routine demanded of entire classes is giving way to the growing recognition of the educational value of spontaneous efforts initiated by the individual, alone or in social co-operation with his fellows.

In appealing for greater freedom still, the new psychology is in line with the more advanced educational experiments, such as the work done by Madame Montessori and the founders of the Little Commonwealth.

(6) The *hygiene and technique of mental work* is itself being based

upon scientific investigation. Of the numerous problems in the conditions and character of mental work generally, two deserve especial mention—fatigue, and the economy and technique of learning.

But of all the results of educational psychology, perhaps the most valuable is the slow but progressive inculcation of the whole teaching profession with a scientific spirit in their work, and a scientific attitude towards their pupils and their problems. Matter taught and teaching methods are no longer exclusively determined by mere tradition or mere opinion. They are being based more and more upon impartial observation, careful records, and statistical analysis—often assisted by laboratory technique—of the actual behaviour of individual children.

II.

I turn now to the second aspect.

So much of our educational system is voluntary that it is often called a dual system. But in speaking of a dual system only the primary stage is, as a rule, in our minds. Yet to foreign students some parts of our higher education, e.g., the Public Schools, appear as that which is most definitely English in character. The Public Schools, however, form no part of the system of public (i.e. of State and municipal) education and are not directly associated with it.

The reasons are fairly obvious. Many of the Public Schools are centuries old; our public system began but fifty years ago. The Act of 1870 gave us only public elementary schools. More than thirty years elapsed before we had the beginnings of a system of secondary schools. Even to-day, with the comprehensive Act of 1918, whose primary object is to establish a national system of education, the Public Schools, owing largely to the fact that the Act is administered by 318 Local Education Authorities, retain a ' non-local ' character.

The Public Schools of England have no parallel. They have their defects and their critics; but they have had a paramount influence on the intellectual and social life of the country. They are admired less for the intellectual severity of the class-room than for their traditions, their form of self-government, and as training places of a generous spirit. In the past the Public Schools in the education of the aristocracy achieved a national purpose. They were the nurseries of English thought and action. Now that the predominant power in the State has passed to the nation as a whole, it would only be in keeping with their long-cherished traditions if the Public Schools were to seek a share in the education of democracy. Moreover, the problems of Local Education Authorities are of such absorbing interest that the professional spirit of the Public Schoolmaster must be longing to assist in their solution.

The two older Universities have had a history, and have borne a part in the national life, analogous to, but on a much larger scale than, the Public Schools. They also are ' non-local ': they serve the Empire. The newer Universities are much more local in character. Yet as a whole it can hardly be said that they exercise an important influence on the work of the Local Education Authorities. I am not overlooking the fact that the Universities, like the Public Schools, play their own

part in providing the most advanced education; nor that they place their best at the disposal of the Local Education Authorities' scholars and contribute a part of the teaching staff. I am, however, to-day suggesting a closer association with Local Education Authorities, and of bringing to bear more immediately on local and public education the wealth of their long experience and the riches of their accumulated knowledge.

There is a third group of institutions which have had a large share in English education. I refer to the endowed Grammar Schools. Partly of choice, partly through stress of circumstances, many of these schools have joined forces with the Local Education Authorities. With the recent rapid growth in the cost of maintenance and with inadequacy of other sources of income they have received ' aid ' from the Authority. Some have become municipal schools : others have undertaken to bear their share in local work, but have retained their individuality of character and independence of Government, to both of which they are passionately attached. All have contributed much to the general storehouse of ideas, and the local system has been enriched by the co-operation of forces of different origin, methods, and historical significance.

All three groups of institutions were founded by the few whose spirit in so far as it sought the spread of education has now passed to the multitude. They are all national institutions, but, with the exceptions to which I have referred, they form no part of the national system administered by Local Education Authorities and supervised by the Board of Education. I do not, of course, suggest control. That is obviously impossible in the case of two of the groups. Nor am I to-day thinking of making constructive proposals as to the forms of associations. Such proposals will, I hope, be put forward later in the week. For the moment it will be sufficient to add that the association desired is direct and close rather than indirect and remote, and in teaching rather than in administration.

There is one further group which I cannot pass in silence: the private schools. Each Local Education Authority must, under Section 1 of the Act of 1918, submit a scheme for the progressive and comprehensive organisation of education within its area. Presumably, each Local Education Authority will include the local ' places ' in efficient private schools as part of the accommodation already provided in the area. All such efficient private schools, whether run for private profit or not, reduce the provision to be made by the Authority. To the extent to which they relieve the burden on the Authority they are therefore contributing to the public service. In return the Authority, while it cannot financially assist schools conducted for private profit, can confer advantages through close association with its organisation. All private schools doing local work, at all events all which claim to be efficient, would therefore serve their own interests and render public service by entering into communication with the Authority and getting the lines of local co-operation satisfactorily adjusted.

It would not be possible to exhaust the possibilities of co-operation of voluntary endeavour with the public system, even if my whole paper had been devoted to this subject. I am anxious, however, to carry my suggestions one step further. It is of the essence of voluntary effort that it is constantly evolving new forms. In most large towns within the last ten years Care Committees have been established, some merely to assist the Authorities in carrying out the more social powers and duties conferred on them by the Act; others with the higher ambition of 'building up the homes.' Such Care Committees have rendered a great service to their areas not only in work actually done under the direction of the Authority, but in the fact that they have frequently introduced new and opposite points of view from those of the administration. The Act of 1918 offers wider opportunities, and many social workers are beginning to realise it. During the last twelve months, in connection with the establishment of Day Continuation Schools, I have met in consultation, or addressed meetings, of social workers, trades-union representatives, club leaders, employers, clergy of various denominations, and parents; together and separately. I have met with opposition and criticism and divergent points of view, but what has gratified me most has been the general and eager desire for an increase in educational facilities and an improvement in social conditions. No subject for discussion has been so well received as that of training our young workers to use their leisure wisely. There has been the fullest recognition that all must join up in the common task; that the greatest opportunity of our time for joint endeavour in a wider educational effort has come; to miss it would be something in the nature of a betrayal of our several functions. If our continuation schools are to become national, not only in the sense of being universal and comprehensive, but in the generous nature of the spirit which inspires them, all that is best in our trade, social, and sports organisations must be brought to bear on their external and internal activities. On this ground alone I feel sure that there was general satisfaction that the guidance of the Juvenile Organisation Committees, and all that they stand for, was transferred from the Home Office, which has the great credit of having consolidated them, to the Board of Education, which is the official foster-mother of our educational system. In the London area the Juvenile Organisation Committees have gradually become representative in the widest sense of all social organisations, and it is anticipated that before long lines of co-operation with the Authority will be established. The task in all areas is so large that there is ample room for all; it is so complex that there is need for all and it is of such importance to the future that it would be a national misfortune not to welcome the service of all.

It is difficult for this generation to estimate with true insight the after-effects of the war. But it would seem as if there had rarely been a time when the minds of men were so much loosened from great principles. Such a condition is no doubt partly a reaction from a period of tense anxiety in which suppression of the individual and sacrifice for the community were the demands of a struggle for existence. But the general mental attitude may also be a reaction, accentuated by the

war, against the interpretation of the great principles which has hitherto directed us, to continue to deserve universal adherence. The outlook is yet clouded. Will the present individualistic point of view continue, or are we but being carried through a transition phase until the coming of a new rallying cry which will restate the brotherhood of man in some new and captivating form? However that may be, our course seems clear: it is to develop the intelligence and the spirit of social service in our whole population in complete confidence that the solidity of the English character fortified with such weapons will maintain and expand that civilisation which has brought us so far, and which we owe it to posterity to hand on not only unimpaired, but broadened and deepened by new streams of thought and action. It is in this sense that the spread of educational advantages is the hope of all, and that I have made this appeal for all educational and social forces to concentrate in one national effort. In the words of one of our greatest poets:

> Give all thou canst—high Heaven rejects the lore
> Of nicely calculated less or more.

ADDRESS

TO THE

AGRICULTURAL SECTION

BY

PROFESSOR FREDERICK KEEBLE, C.B.E., Sc.D., F.R.S.,

PRESIDENT OF THE SECTION.

Intensive Cultivation.

THERE is, so far as I can discover, no reason—save one—why I should have been called upon to assume the presidency of the Agricultural Section of the British Association, or why I should have been temerarious enough to accept so high an honour and such a heavy load of responsibility. For upon the theme of Agriculture as commonly understood I could speak, were I to speak at all, but as a scribe and not as one in authority. The one reason, however, which must have directed the makers of presidents in their present choice is, I believe, so cogent that despite my otherwise unworthiness I dared not refuse the invitation. It is that, in appointing me, agriculturists desired to indicate the brotherhood which they feel with intensive cultivators. As properly proud sisters of an improved tale they have themselves issued an invitation to the Horticultural Cinderella to attend their party, and in conformity with present custom, which requires each lady to bring her partner, I am here as her friend.

Nor could any invitation give me greater pleasure: for my devotion to Horticulture is profound and my affection that of a lover. My only fear is lest I should weary my hosts with her praises, for in conformity with this interpretation I propose to devote my Address entirely to Horticulture—to speak of its performance during the war and of its immediate prospects.

Although that which intensive cultivators accomplished during the war is small in comparison with the great work performed by British agriculturists, yet nevertheless it is in itself by no means inconsiderable, and is, moreover, significant and deserves a brief record. That work may have turned and probably did turn the scale between scarcity and sufficiency; for, as I am informed, a difference of 10 per cent. in food supplies is enough to convert plenty into dearth. Seen from this standpoint the war-work accomplished by the professional horticulturist—the nurseryman, the florist, the glass-house cultivator, the fruit-grower and market gardener, and by the professional and amateur gardener and allotment holder assumes a real importance, albeit that the sum-total of the acres they cultivated is but a fraction of the land which agriculturists put under the plough.

As a set-off against the relative smallness of the acreage brought during the war under intensive cultivation for food purposes, it is to be remembered that the yields per acre obtained by intensive cultivators are remarkably high. For example, skilled onion-growers compute their average yield at something less than 5 tons to the acre. A chrysanthemum-grower who turned his resources from the production of those flowers to that of onions obtained over an area of several acres a yield of 17 tons per acre. The average yield of potatos under farm conditions in England and Wales is a little over 6 tons to the acre, whereas the army gardeners in France produced, from Scotch seed of Arran Chief which was sent to them, crops of 14 tons to the acre. Needless to say, such a rate of yield as this is not remarkable when compared with that obtained by potato-growers in the Lothians or in Lincolnshire, but it is nevertheless noteworthy as an indication of what I think may be accepted as a fact, that the average yields from intensive cultivation are about double those achieved by extensive methods.

The reduction of the acreage under soft fruits—strawberries, raspberries, currants, and gooseberries—which took place during the war gives some measure of the sacrifices—partly voluntary, partly involuntary—made by fruit-growers to the cause of war-food production. The total area under soft fruits was 55,560 acres in 1913, by 1918 it had become 42,415, a decrease of 13,145 acres, or about 24 per cent. As would be expected, the reduction was greatest in the case of strawberries, the acreage of which fell from 21,692 in 1913 to 13,143 in 1918, a decrease of 8,549 acres, or about 40 per cent. It is unfortunate that bad causes often have best propagandas, for were the public made aware of such facts as these they would realise that the present high prices of soft fruits are of the nature of deferred premiums on war-risk insurances with respect to which the public claims were paid in advance and in full.

I should add that the large reduction of the strawberry acreage is a measure no less of the short-sightedness of officials than of the public spirit of fruit-growers; for in the earlier years of the war many counties issued compulsory orders requiring the grubbing up and restriction of planting of fruit, and I well remember that one of my first tasks as Controller of Horticulture was to intervene with the object of convincing the enthusiasts of corn production that, in war, some peace-time luxuries become necessaries and that, to a sea-girt island beset by submarines, home-grown fruit most certainly falls into this category.

Those who were in positions of responsibility at that time will not readily forget the shifts to which they were put to secure and preserve supplies of any sorts of fruit which could be turned into jam—the collection of blackberries, the installation of pulping factories which Mr. Martin and I initiated, and the rushing of supplies of scarcely set jam to great towns, the populace of which, full of a steadfast fortitude in the face of military misfortune, was ominously losing its sweetness of disposition owing to the absence of jam and the dubiousness of the supply and quality of margarine.

But though the public lost in one direction it gained in another, and the reduction of the soft-fruit acreage meant—reckoned in terms of potatos—an augmentation of supplies to the extent of over 100,000 tons. Equally notable was the contribution to food production made by the florists and nurserymen in response to our appeals. An indication of their effort is supplied by figures which, as president of the British Florists' Federation, Mr. George Munro—whose invaluable work for food production deserves public recognition—caused to be collected. They relate to the amount of food production undertaken by 100 leading florists and nurserymen. These men put 1,075 acres, out of a total of 1,775 acres used previously for flower-growing, to the purpose of food production, and they put 142 acres of glass out of a total of 218 acres to like use. I compute that their contribution amounted to considerably more than 12,000 tons of potatos and 5,000 tons of tomatos.

The market growers of Evesham and other districts famous for intensive cultivation also did their share by substituting for luxury crops, such as celery, those of greater food value, and even responded to our appeals to increase the acreage under that most chancy of crops—the onion, by laying down an additional 4,000 acres and thereby doubling a crop which more than any other supplies accessory food substances to the generality of the people.

In this connection the yields of potatos secured by Germany and this country during the war period are worthy of scrutiny.

The pre-war averages were: Germany 42,450,000 tons, United Kingdom 6,950,000 tons; and the figures for 1914 were: Germany 41,850,000 tons, United Kingdom 7,476,000 tons.

Germany's supreme effort was made in 1915 with a yield of 49,570,000 tons, or about 17 per cent. above average. In that year our improvement was only half as good as that of Germany: our crop of 7,540,000 tons bettering our average by only 8 per cent. In 1916 weather played havoc with the crops in both countries, but Germany suffered most. The yield fell to 20,550,000 tons, a decrease of more than 50 per cent., whilst our yield was down to 5,469,000 tons, a falling off of only 20 per cent. In the following year Germany could produce no more than 39,500,000 tons, or a 90 per cent. crop, whereas the United Kingdom raised 8,604,000 tons, or about 24 per cent. better than the average. Finally, whereas with respect to the 1918 crop in Germany no figures are available, those for the United Kingdom indicate that the 1917 crop actually exceeded that of 1918.

There is much food for thought in these figures, but my immediate purpose in citing them is to claim that of the million and three-quarter tons increase in 1917 and 1918 a goodly proportion must be put to the credit of the intensive cultivator.

I regret that no statistics are available to illustrate the war-time food production by professional and amateur gardeners. That it was great I know, but how great I am unable to say. This, however, I can state, that from the day before the outbreak of hostilities, when, with the late Secretary of the Royal Horticultural Society, I started the intensive food-production campaign by urging publicly the autumn sowing of vegetables—a practice both then and now insufficiently followed—the

amateur and professional gardeners addressed themselves to the work of producing food with remarkable energy and success. No less remarkable and successful was the work of the old and new allotment holders, so much so indeed that at the time of the Armistice there were nearly a million and a-half allotment holders cultivating upwards of 125,000 acres of land: an allotment for every five households in England and Wales. It is a pathetic commentary on the Peace that Vienna should find itself obliged to do now what was done here during the war— namely, convert its parks and open spaces into allotments in order to supplement a meagre food supply.

This brief review of war-time intensive cultivation would be incomplete were it to contain no reference to intensive cultivation by the armies at home and abroad. From small beginnings, fostered by the distribution by the Royal Horticultural Society of supplies of vegetable seeds and plants to the troops in France, army cultivation assumed under the direction of Lord Harcourt's Army Agricultural Committee extraordinarily large dimensions: a bare summary must suffice here, but a full account may be found in the report presented by the Committee to the Houses of Parliament and published as a Parliamentary Paper.

In 1918 the armies at home cultivated 5,869 acres of vegetables. In the summer of that year the camp and other gardens of our armies in France were producing 100 tons of vegetables a day. These gardens yielded, in 1918, 14,000 tons of vegetables, worth, according to my estimate, a quarter of a million pounds sterling, but worth infinitely more if measured in terms of benefit to the health of the troops.

As the result of General Maude's initiative, the forces in Mesopotamia became great gardeners, and in 1918 produced 800 tons of vegetables, apart altogether from the large cultivations carried out by His Majesty's Forces in that wonderfully fertile land. In the same year the forces at Salonika had about 7,000 acres under agricultural and horticultural crops, and raised produce which effected a saving of over 50,000 shipping tons.

Even from this brief record it will, I believe, be conceded that intensive cultivation played a useful and significant part in the war: what, it may be asked, is the part which it is destined to play in the future? So far as I am able to learn, there exist in this country two schools of thought or opinion on the subject of the prospects of intensive cultivation, the optimistic and the pessimistic school. The former sees visions of large communities of small cultivators colonising the countryside of England, increasing and multiplying both production and themselves, a numerous, prosperous and happy people and a sure shield in time of war against the menace of submarines and starvation. Those on the other hand who take the pessimistic view, point to the many examples of smallholders who ' plough with pain their native lea and reap the labour of their hands ' with remarkably small profit to themselves or to the community—smallholders like those in parts of Warwickshire, who can just manage by extremely hard labour to maintain themselves, or like those in certain districts of Norfolk, who have let their holdings tumble down into corn and who

produce no more and indeed less to the acre than do the large farmers who are their neighbours.

Before making any attempt to estimate the worth of these rival opinions it may be observed that the war has brought a large reinforcement of strength to the rank of the optimists. A contrast of personal experiences illustrates this fact. When in the early days of the war I felt it my duty to consult certain important county officials with the object of securing their support for schemes of intensive food production, I carried away from the conference one conclusion only: that the counties of England were of two kinds, those which were already doing much and were unable therefore to do more, and those which were doing little because there was no more to be done. In spite of this close application of the doctrine of Candide—that all is for the best in the best of all possible worlds—I was able to set up some sort of county horticultural organisation, scrappy, amateurish, but enthusiastic, and the work done by that organisation was on the average good; so much so indeed that when after the Armistice I sought to build up a permanent county horticultural organisation I was met by a changed temper. The schemes which the staff of the Horticultural Division had elaborated as the result of experience during the war were received and adopted with a cordiality which I like to think was evoked no less by the excellence of the schemes themselves than by the promise of liberal financial assistance in their execution. Thus it came about that when the time arrived for me to hand over the controllership of Horticulture to my successor, almost every county had established a strong County Horticultural Committee, and the chief counties from the point of view of intensive cultivation had provided themselves with a staff competent to demonstrate not only to cottagers and allotment holders, but also to smallholders and commercial growers, the best methods of intensive cultivation. In the most important counties horticultural superintendents with knowledge of commercial fruit-growing were being appointed, and demonstration fruit and market-garden plots, designed on lines laid down by Captain Wellington and his expert assistants, were in course of establishment. The detailed plans for these links in a national chain of demonstration and trial plots have been published, and anyone who will study them will, I believe, recognise that they point the way to the successful development of a national system of intensive cultivation.

By means of these county stations the local cultivator may learn how to plant and maintain his fruit plantation and how to crop his vegetable quarters, what stock to run and what varieties to grow.

Farm stations—with the Research stations established previously by the Ministry; Long Ashton and East Malling for fruit investigations; the Lea Valley Growers' Association and Rothamstead for investigation of soil problems and pathology; the Imperial College of Science for research in plant physiology, together with a couple of stations, contemplated before the war, for local investigation of vegetable cultivation; an alliance with the Royal Horticultural Society's Research Station at Wisley, and with the John Innes Horticultural Institute for research in genetics; the Ormskirk Potato Trial Station; a Poultry

Institute; and, most important of all from the point of view of education, the establishment at Cambridge of a School of Horticulture—constitute a horticultural organisation which, if properly co-ordinated and—dare I say it?—directed, should prove of supreme value to all classes of intensive cultivators. To achieve that result, however, something more than a permissive attitude on the part of the Ministry is required, and in completing the design of it I had hoped also to remain a part of that organisation long enough to assist in securing its functioning as a living, plastic, resourceful, directive force—a horticultural cerebrum. Thus developed, it is my conviction that this instrument is capable of bringing Horticulture to a pitch of perfection undreamed of at the present time either in this country or elsewhere.

In my view Horticulture has suffered in the past because the fostering of it was only incidental to the work of the Ministry. In spite of the fact that it had not a little to be grateful for—as for example the research stations to which I have referred—Horticulture had been regarded rather as an agricultural side-show than as a thing in itself. My intention, in which I was encouraged by Lord Ernle, Lord Lee, and Sir Daniel Hall, was to peg out on behalf of Horticulture a large and valid claim and to work that claim. The conception of Horticulture which I entertained was that comprised in the ' petite culture ' of the French. It included crops and stock, fruit and vegetables, flower and bulb and seed crops, potatos, pigs and poultry and bees. I held the view, and still hold it, that the small man's interests cannot be fostered by the big man's care; that Horticulture is a thing in itself and requires constant consideration by horticulturists and not occasional help from agriculturally minded people, however distinguished and capable.

I had to include the pig and poultry, for the smallholder and commercial grower will have to keep the one and may with profit keep both, and he will have to modify his system of cultivation accordingly. The adoption of this conception of the scope of intensive cultivation opens up an array of new problems which require investigation, and it was my intention to endeavour to secure the experimental solution of these many problems at the Research Stations and elsewhere. Beside these problems—of green manuring, cropping, horticultural rotations—horticultural surveys would be made, ' primeur ' lands demarked for colonisation, and existing orchard lands ascertained and classified, as indeed we had begun to do in the West of England. But, above all, with this measure of independence for Horticulture we, having the good will and support of the fraternity of horticulturists, aimed at putting to the test the certain belief which I hold that education —sympathetic and systematic—is an instrument the power of which, for our purpose, scarcely yet tried, is in fact of almost infinite potency. I believe with Mirabeau that, ' after bread, education is the first need of the people,' and I know that the people themselves are ready to receive it.

Contrast this horticultural prospect with the fact that a group of smallholders in an outlying district informed one of my inspectors that

his was the first visit that they had received for many years, or with the fact that remediable diseases are still rife in hundreds of gardens, or that few small growers understand the principles which should guide them in deciding whether or not to spray their potatos, or that West Country orchardists exist who let dessert fruit tumble to the ground and sell it in ignorance of its true value, or that unthrifty fruit-trees may be top-grafted but are not, or that it is often ignored that arsenate of lead as a spray fluid for fruit pays over and over again for its use, or even that growers in plenty still do not know that Scotch or Irish or once-grown Lincolnshire seed potatos are generally more profitable than is home-grown or local seed. The truth is that great skill and sure knowledge exist among small cultivators side by side with much ignorance and moderate practical ability. Herein lies the opportunity of the kind of education which I have in mind. But for any such intensive system of education to prevail the isolation both of cultivators and of Government Departments must be abolished. Out of that isolation hostility arises, in which medium no seed of education will germinate. It is troublesome, but not difficult, to abolish hostility. It vanishes when direct relations are established and maintained between a Department and those whose affairs it administers. The paternal method will not do it. The official life, lived ' remote, unfriendly, alone,' with only underlings as missionaries to the heathen public, will not do it.

There is only one way to prepare the ground for the intensive cultivation of education, and that is to secure the full co-operation of officials and cultivators. If this be not done the official must continue to bear with resignation the unconcealed hostility of those he wishes to assist. That a state of confidence and co-operation may be established is proved by the record of the Horticultural Advisory Committee which was set up by Lord Ernle during my controllership. The Committee consisted of representatives of all the many branches of Horticulture—fruit-growers, nurserymen, market gardeners, growers under glass, salesmen, researchers, and so forth. That Committee became, as it were, the Deputy-Controller of Horticulture. To it all large questions of policy were referred, and to its disinterested service Horticulture owes a great debt. That its existence has been rendered permanent by Lord Lee is of good augury for the future of intensive cultivation. As an instance of the judicial temper in which this Committee attended to its business I may mention that when an Order—the Silver Leaf Order—was under discussion the only objection to its terms on the part of the fruit-growers on the Committee was that the restrictive measures which it contemplated were not drastic enough : a noteworthy example of assent to a self-denying ordinance.

It may be asked What are the subjects in which growers require education? To answer that question fully would require an Address in itself. Among those subjects, however, mention may be made of a few : the extermination or top-grafting of unthrifty fruit, the proper spacing and pruning of fruit-trees, the use of suitable stocks, systematic orchard-spraying, the use of thrifty varieties of bush fruit and the proper manuring thereof, the choice of varieties suitable to given

soils and districts and for early cropping, the better grading and packing of fruit.

Of all methods of instruction in this last subject the best is that provided by Fruit Exhibitions. Those interested in the promotion of British fruit-growing will well remember the object-lesson in good and bad packing provided by the first Eastern Counties Fruit Show, held at Cambridge in 1919. That exhibition, organised by the East Anglian fruit-growers with the assistance of the Horticultural Division of the Ministry of Agriculture, demonstrated three things: first, that fruit of the finest quality is being grown in East Anglia; second, that this district may perhaps become the largest fruit-growing region in England; and, third, that among many growers profound ignorance exists with respect to the preparation of fruit for market.

The opinions which I have endeavoured to express on the organisation of intensive cultivation may be summarised thus:—

1. The object of the organisation is to improve local and general cultivation, the former by demonstration, the latter by research.

2. The method of organisation must provide for co-operation between the horticultural officers of the State and the persons engaged in the industry. This co-operation must be real and complete. Dummy Committees are silly devices adopted merely by second-rate men and merely clever administrators. The co-operation must embrace the policy as well as the practice of administration. Nevertheless the horticultural officers of the State must be leaders. They can, however, lead only by the power of knowledge. Wherefore an administrator who lacks practical knowledge and scientific training is not qualified to act as the executive head of a horticultural administration. The head must of course possess administrative capacity, but this form of ability is by no means uncommon among Britons, although it is a custom to represent it as something akin to inspiration and the attribute of the otherwise incompetent. The directing head must possess a wide practical knowledge of Horticulture; that alone can fire the train of his imagination to useful and great issues. His right-hand man, however, must be one versed in departmental and interdepartmental intricacies—the best type of administrator—of sober and cool judgment and keen intelligence, unused perhaps to enthusiasm, but not intolerant of nor immune from it. Similarly in each sub-department for cultivation, disease-prevention, small stock, &c., the head must be a trained practical man with an administrator as his chief assistant. The outdoor officers, the intelligence officers of the organisation, must also be men of sound and wide practical knowledge and must know that their reports will be read by someone who understands the subjects whereof they speak.

It was on these lines that the Horticultural Division was organised under Lord Ernle, Lord Lee, and Sir Daniel Hall. The work accomplished justified the innovation.

This is the contribution which I feel it my duty to make on the vexed question of the relation between expert and administrator in Departments of State which deal with technical and vital problems.

I believe that no administrator, save the rare genius, can direct the

expert, whereas the expert with trained scientific mind and possessed of a fair measure of administrative ability can direct any but a genius for administration. If the work of a Government office is to be and remain purely administrative no creative capacity is required, and it may be left in the sure and safe and able hands of the trained administrator; but if the work is to be creative it must be under the direction of minds turned as only research can turn them—in the direction of creativeness. To the technically initiated initiation is easy and attractive, to the uninitiated it is difficult and repugnant.

The useful work that such a staff as I have indicated would find to do is well-nigh endless. It would become a bureau of information in national horticulture. and the knowledge which it acquired would be of no less use to investigators than to the industry. Diseases ravage our orchards and gardens, some are known to be remediable and yet persist, others require immediate and vigorous team-wise investigation and yet continue to be investigated by solitary workers or single research institutions.

Certain new varieties of some soft fruits are known to be better than the older varieties, and yet the latter continue to be widely cultivated. The transport and distribution of perishable fruit is often inadequate—'making a famine where abundance lies.' The information gathered in during the constant survey of the progress of Horticulture would serve not only to direct educational effort into useful channels. but to stimulate and assist research. For the headquarters staff of trained men learns in the course of its administrative work many things, which, albeit unknown to the researcher, are of first importance to him who is bent on advancing horticultural knowledge.

For example, it is known that the trade of raisers of seed potatos for export to Jersey or Spain is in some places menaced by the presence of a plot of land a mile or two away in which wart disease has appeared. It may be that the outbreak occurred on only a single plant, yet nevertheless the seed-potato grower may be inhibited from exporting the seed grown by him on clean land. The prohibition is just, but the man who refuses to issue a licence to export, if he be a trained horticulturist in touch with research, will know that there is research work to hand and that immediately, and will bring the problem to the urgent notice of the researchers. Thus the scientifically trained administrator becomes, although not himself witty in research, the cause of wit in others. To ask the researcher, who must inevitably be to some extent like Prospero ' wrapt in secret studies and to the State grown stranger,' to discover problems which arise out of administrative embarrassments is unreasonable; on the other hand. the scientifically trained administrator acts naturally as liaison officer between the laboratory and the land, passing on the problems which arise out of administrative necessities or expedients.

In this connection it is interesting to recall the fact that the importance of the existence of varieties of potatos immune from wart disease was observed years ago by an officer of the Ministry, Mr. Gough, who is also a man possessed of a scientific training, and I believe also that I am right in saying that either this officer or another suggested

long ago that the clue to the spread of wart disease in England was to be sought in the potato fields of Scotland. Mr. Taylor will, I hope, give us the latest and most interesting chapter in the story of wart disease, and I will not therefore spoil his story by anticipation of its conclusions.

The tacit assumption which has so far underlain my Address is that an extension of intensive cultivation in this country is desirable. I have indicated that areas are to be discovered where soil and climate are favourable to this form of husbandry, and that by the establishment of a proper form of research—administrative—and educational organisation the already high standard reached by intensive cultivators may be surpassed. It remains to inquire whether any large increase in the area under intensive cultivation is in fact either desirable or probable.

The dispassionate inquirer will find his task by no means easy. He should, as a preliminary, endeavour to discern in the present welter of cosmic disturbance what are likely to be the economic conditions of the politician's promised land—the new world which was to be created from the travail of war. In the first place, and no matter how academic he may be, he cannot fail to recognise the fact that costs of production, including labour, are at least twice and probably $2\frac{1}{2}$ times those of pre-war days, and he must assume that the increase is permanent and not unlikely to augment. What this means to the different forms of cultivation may be judged from the following estimates of capital costs of cultivation of different kinds:—

Labour and Capital for Farming and Intensive Cultivation.

—	Labour per 100 Acres	Capital per Acre	
		Pre-War	Present
	Men	£	£
Mixed Farming	3–5	10	20–25
Fruit and Vegetable growing	20–30	50	100–125
Intensive Cultivation in the open (French Gardening)	200	750	1,500–1,875
Cultivation under glass	200–300	2,000	4,000–5,000

In the second place the inquirer is bound to assume that the intensive cultivator of the future, like his predecessor in the past, will have to be prepared to face the competition of the world. He may, I believe, look for no artificial restriction of imports, and therefore he must be prepared to find that higher costs of production will not necessarily be accompanied by increased receipts for intensively cultivated commodities.

But, on the other hand, he may find some comfort in the fact that both immediately before and, still more, subsequently to the war, the standard of living both in this country and throughout the world was, and is still, rising. Hence he may perhaps expect a less severe competition from foreign growers and also a better market at home.

He may also derive comfort from the reflection that the increased cost of production which he must bear must also, perhaps in no less

measure, be borne by his foreign competitors. Even before the war the cost of production of one of the chief horticultural crops—apples— was no higher in this country than in that of our main competitors. There are also certain other apparently minor but really important reasons for optimism with regard to the prospects of intensive cultivation. Among these is the increasing use of road in lieu of rail transport for the marketing of horticultural produce. The advantages of motor over rail transport for the carriage of perishable produce for relatively short distances—say up to 75 miles from market—lie in its greater punctuality, economy of handling, and elasticity. Only a poet native of a land of orchards could have written the lines: ' When I consider everything that grows holds in perfection but a single moment.' Fruit crops ripen rapidly and more or less simultaneously throughout a given district. They must be put on the market forthwith or are useless. A train service, no matter how well organised, does not seem able to cope with gluts, and hence it arises that a season of abundance in the country rarely means a like plenty to the consumer. I am aware that the problem of gluts is by no means simple and that the railways are sometimes blamed unjustly for failing to cope with them, but nevertheless I believe that, as Kent has discovered, the motor-lorry will be more and more called in to redress the balance between the home growers and the foreign producers in favour of the former; for by its use the goods can be delivered with certainty in time to catch the market and thus give the home producer the advantage due to propinquity which should be his. Increasing knowledge of food values, together with the general rise in the standard of living, also present features of good augury to the intensive cultivator. Jam and tomatos and primeurs may be taken as texts.

In 1914 the consumption of jam in the United Kingdom amounted to about a spoonful a day per person. The more exact figures are 2 oz. per week, or 126,000 tons per annum.

It is difficult to estimate the area under jam fruit—plums, strawberry, raspberry, currants, &c.—required to produce this tonnage, but it may be put at between 10,000 and 20,000 acres.

By 1918, thanks to the wisdom of the Army authorities in insisting on a large ration of jam for the troops, and thanks also to the scarcity and quality of margarine, the consumption of jam had more than doubled. From 126,000 tons of 1914 it reached 340,000 tons in 1918. To supply this ration would require the produce of from 25,000 to 50,000 acres of orchard, which in turn would directly employ the labour of say from 5,000 to 10,000 men. Yet even the tonnage consumed in 1918 only allows a meagre ration of little more than a couple of spoonfuls a day. It may therefore be anticipated that if, as is probable, albeit only because of the immanence of margarine, the new-found public taste for jam endures, fruit-growers in this country will find a considerable and profitable extension in supplying this demand.

The remarkable increase in consumption which the tomato has achieved would seem to support this conclusion. Fifty years ago, as Mr. Robbins has mentioned in his paper on ' Intensive Cultivation ' (Journal of Board of Agriculture, xxv. No. 12, March 1919), this

fruit was all but unused as a food. To-day one district alone, the Lea Valley, produces 30,000 tons per annum. The total production in this country amounts to upwards of 45,000 tons. Yet the demand for tomatos has increased so rapidly—the appetite growing by what it feeds upon—that the imports in 1913 from the Channel Islands, Holland, France, Portugal, Spain, Canary Islands, and Italy amounted to nearly double the home crop, viz. 80,000 tons, making the total annual consumption not less than $1\frac{1}{4}$ tons or about 2 pounds per week per head of population. Is it too fanciful to discern in this rapidly growing increase in the consumption of such accessory foodstuffs as jam and tomatos, not merely an indication of a general rise in the standard of living and a desire on the part of the community as a whole to share in the luxuries of the rich, but also a sign that in a practical, instinctive, unconscious way the public has discovered simultaneously with the physiologists that a monotonous diet means malnutrition, and that even in a dietetic sense man cannot live by bread alone? As lending support to this fancy and as indicating that the value of vitamines was discovered by people before vitamines were discovered by physiologists, I may mention the curious fact that the general public has always shown a wise greediness for an accessory food which, though relatively poor in calories is rich in vitamines—namely the onion. Even in pre-war times the annual value of imported onions amounted to well over one million pounds sterling; and, when the poverty of the winter diet of the people of England and Wales is considered, it must be admitted that this expenditure represented a sound investment on the part of the British public. It is a curious fact also that the genius of Nelson led him to a like conclusion. He took care, during the long years when his blockading fleet kept the seas, to provide his sailors with plenty of exercise and onions.

If, as I think, the increasing consumption of the accessory foods which intensive cultivation provides represents not merely a craving for luxuries, but an instinctive demand for the so-called accessory food bodies which are essential to health, then it may be expected that, as has been illustrated in the case of jam and tomatos, consumption will continue to increase. If this be so, the demand both for fresh fruit and also for ' primeurs '—early vegetables—should grow and should be supplied at least in part by the intensive cultivators of this country.

If the home producer can place his wares on the market at a price that can compete with imported produce—and it is not improbable that he will be able to do so—he need not, even with increased production, apprehend more loss from lack of demand than he has had to face in the past. Seasonal and other occasional gluts he must, of course, expect.

Even when judged by pre-war values, his market, as indicated by imports, is a capacious one. Thus in 1913 the imports into the United Kingdom of soil products from smallholdings were of the value of about 50 million pounds sterling. To-day it is safe to compute them at over 100 millions. To that sum—of 50 millions—imported vegetables contributed $5\frac{1}{2}$ million pounds sterling, apples $2\frac{1}{4}$ millions, other fruits nearly 3 millions, eggs and poultry over 10 millions, rabbits and rabbit-

skins a million and a half, and bacon and pork over 22 millions. No one whose enthusiasm did not altogether outrun both his discretion and knowledge would suggest that the home producer could supply the whole or even the greater part of these commodities. But, on the other hand, few of those who have knowledge of the skill and resources of our intensive cultivators, and of the suitability of favoured parts of this country for intensive cultivation, will doubt but that a modest proportion, say, for example, one fifth, might be made at home. This on a post-war basis would amount in value to over 20 million pounds, would require the use of several hundred thousand acres of land and provide employment for something like 100,000 men. The fact that Kent has found it profitable to bring one-fifth of its total arable land under fruit and other forms of intensive cultivation is significant and a further indication that intensive cultivation offers real prospects to the skilful and industrious husbandman. The present reduced acreage under fruit, due partly to war conditions, but mainly to the grubbing of old orchards, enhances the prospects of success.

The estimated acreage under fruit in England and Wales is:—

	Acres
Apples	170,000
Pears	10,000
Plums	17,000
Cherries	10,000
Strawberries	13,000
Raspberries	6,000
Currants and Gooseberries	22,000
	248,000

exclusive of mixed orchards and plantations.

These figures are, however, well-nigh useless as indicating the areas devoted to the intensive cultivation of fruit for direct consumption. Of the 170,000 acres of apples, cider fruit probably occupies not less than 100,000, and of this area much ground is cumbered with old and neglected trees. Of the 10,000 acres in pears some 8,000 are devoted to perry production, and hence lie outside our immediate preoccupation. Having regard, however, to the reduction of acreage under fruit, to the increasing consumption of fruit and jam, and to the success which has attended intelligent planting in the past, it may be concluded that a good many thousand acres of fruit might be planted in this country with good prospects of success.

Lastly, it remains to consider what results are likely to occur if intensive cultivation comes to be more generally practised in this country. I am indebted to one of our leading growers for an example of the results which have attended the conversion of an ordinary farm into an intensively cultivated holding.

The farm—of 150 acres and nearly all arable—was taken over in 1881. At that date it found regular employment for three men and a boy—with the usual extra help at harvest. The rate of wages paid to the farm hand was 15s. a week.

In 1883, two years after the farm had been taken over and converted

to the uses of a horticultural holding, from 20 to 25 men and 80 to 100 women, according to season, were at work on it, and the minimum wage for men was 20s. per week. The holding was increased gradually to 310 acres, and at the present time gives employment on an average to 90 men and 50 women during the winter months and 110 men and 200 women during the summer months. In 1913 the wages bill was 7,981l., and in 1918 10,000l. per annum, that is, over 34l. per acre.

Another concrete example of the effect of intensity of cultivation on density of population is provided by the comparison of two not far distant districts—Rutland and the Isle of Ely. The rich soil and industrious temperament of the inhabitants of the Isle have justly brought it prosperity and fame. The Isle of Ely comprises 236,961 acres, of which number 170,395 are arable; Rutland 97,087 acres with 35,000 arable. The land of Rutland is occupied by 475 persons, that of the Isle by 2,002; the average acreage per occupier in Rutland is 206, in the Isle 118. The total number of agricultural workers in Rutland is 2,146, and in the Isle 13,382. The density of agricultural population in terms of total acreage is in Rutland 2.5 per 100 acres, and in the Isle 5.6, or 20 more cultivators to the square mile in the Isle of Ely than in Rutland; from which the curious may estimate the possibility of home colonisation by introducing as a supplement to extensive agriculture such an amount of intensive cultivation as may be practised in districts similar in climate and soil to the Isle.

The immediate object of the comparison is to show, however, that the difference between the closeness of colonisation of the two lands is accurately presented by the difference between the acreages amenable to intensive cultivation which by reason of soil must, however, always remain relatively larger in the Isle than in Rutland. Thus in Rutland the area under fruit is 204 acres, and in the Isle 7,126. If these areas and the workers thereon be deducted from the total arable in the two districts, the respective agricultural populations in terms of 100 acres of arable become almost identical, viz. 6.7 for Rutland and 6.9 for the Isle. The difference of agricultural populations is measured by the area under intensive cultivation. The agricultural workers engaged on the 7,126 acres of fruit in the Isle of Ely are almost as numerous as those engaged in doing all the agricultural work of Rutland—say, about 2,000 as compared with 2,416.

It may of course be true that a chance word, a common soldier, a girl at the door of an inn, have changed, or almost changed, the fate of nations, but it is probable that the genius of peoples and the pressure of economic and social forces are more potent. Is there then, it may be asked, any indication that the people of this country will seek in intensive cultivation a means of colonising their own land rather than continue to export their surplus man-power? The problem is too complex and too subtle for me to solve, but I will conclude by citing a curious fact which may have real significance in indicating that if a nation so wills it may retain its surplus population on the land by adjusting the intensity of its cultivation to the density of its population. If a diagram be made combining the intensity of pro-

duction of a given crop, *e.g.*, the potato, as grown in the chief industrial countries of the world, it will be found that the curve of production coincides closely with that of density of population.

Density of Population and Intensity of Production. Potatos.

	Density of Population Square Mile.	Percentage of Population.	Percentage of Yield.	Yield in Tons per acre less seed. Average 1911–13.
United States . .	31	10	33	1·3
France . . .	193	62	56	2·2
Germany . . .	311	100	100	3·9
U.K.	374	120	110	4·3
England and Wales .	550	177	128	5
Belgium . . .	658	212	155	6·04

From these facts we may take comfort, for they indicate that as a population increases so does the intensity of its cultivation: the tide which flows into the towns may be made to ebb again into the country. The rate of return, however, must depend on many factors : the proclivities of peoples, the relative attractiveness of urban and rural life and of life at home and abroad, but ultimately the settlement or non-settlement of the countryside must be determined by the degree of success of the average intensive cultivator. The abler man can command success; whether the man of average ability and industry can achieve it, will, I believe, depend ultimately on education. He can look for no assistance in the form of restricted imports. He must be prepared to face open competition. Wherefore he should receive all the help which the State can render; and the measure of success which he, and hence the State, achieves will be determined ultimately by the quality and kind of education which he is able to obtain.

REPORTS ON THE STATE OF SCIENCE, ETC.

Seismological Investigations.—Twenty-fifth Report of the Committee, consisting of Professor H. H. TURNER (*Chairman*), Mr. J. J. SHAW (*Secretary*), Mr. C. VERNON BOYS, Dr. J. E. CROMBIE, Sir HORACE DARWIN, Dr. C. DAVISON, Sir F. W. DYSON, Sir R. T. GLAZEBROOK, Professors C. G. KNOTT *and* H. LAMB, Sir J. LARMOR, Professors A. E. H. LOVE, H. M. MACDONALD, J. PERRY, *and* H. C. PLUMMER, Mr. W. E. PLUMMER, Professor R. A. SAMPSON, Sir A. SCHUSTER, Sir NAPIER SHAW, Dr. G. T. WALKER, *and* Mr. G. W. WALKER.

General.

THE transference of the Milne books and apparatus from Shide to the University Observatory at Oxford was completed in September last. Mrs. Milne sailed for Japan, after some shipping delays, on September 27, and news of her safe arrival on November 13 has been received. The greater part of the books, records, cards, and the two globes for preliminary calculations are conveniently housed in a room in the Students' Observatory, apart from the main building : the remainder of the material is for the present stored in an outbuilding. But by a timely benefaction of 400*l.* from Dr. Crombie, a small house has been acquired near the Observatory, of which it is hoped to get occupation in September, and this will easily hold all that is required, and serve at the same time as a dwelling for the seismological assistant. These arrangements have been made in accordance with the spirit of Professor Schuster's resolution (quoted in the last report), offering to establish a Central Bureau at Oxford, which could not be exactly carried into effect at the moment owing to circumstances there mentioned. Further, in pursuance of this plan, the Cambridge Committee entrusted with the appeal for a Geophysical Institute which should include Seismology, finding their appeal unsuccessful, passed the following resolution on March 10, 1920 :—

> It was agreed that Professor Turner should be informed that no objection could be taken by the Committee to a seismological station and establishment at Oxford.

This resolution, with a letter from the Chairman of the Committee and a summary of other information, was next reported to the University of Oxford through the Board of Visitors in May last, and approved. Finally, these facts were reported to this Committee (B.A. Seismology) at its meeting on July 2, and the plan of locating the work at Oxford approved. It remains to obtain the funds necessary for the salary of a full-time director and for replacing the grants temporarily made by the British Association and the Royal Society. A Royal Commission is at present reviewing the finances of the Universities of Oxford and Cambridge, and a note has been addressed to this Commission on the subject of Seismology, in the first instance by the Board of Faculty of Natural Science, supplemented by a more particular note from Professor Turner.

Instrumental.

The Milne-Shaw seismograph erected in the basement of the Clarendon Laboratory has worked well through the year. Professor Lindemann has given formal sanction to the arrangement, and included the basement in his general

installation of electric light in the laboratory. This has much facilitated the operations of changing films, comparing clocks, &c., but the gas-jet is retained for the photography. The room has further been cleaned and whitewashed, and an outer door has been added shutting it off from draughts. It is now a very convenient laboratory, and is large enough for the erection of at least one more machine, when one is available.

The Milne-Shaw machine formerly erected at Eskdalemuir for direct comparison with Galitzin records has been now transferred (on loan) to the Royal Observatory, Edinburgh, and readings have been received from July 4, 1919. The situation seems peculiarly liable to microseismic disturbance, obviously connected with wind.

The instrument mounted in the 'dug-out' near West Bromwich has given some interesting results as regards these microseisms on which Mr. Shaw writes a special note at the end of this report.

Various other instruments are being constructed as rapidly as present difficulties permit.

Milne-Shaw machines have recently been dispatched to Cape Town, Montreal, Honolulu, and Aberdeen. Others are being made for India, China, Egypt, New Zealand, Canada, and Ireland.

Bulletins and Tables.

'The Large Earthquakes of 1916' have been collated and published as a single pamphlet of 116 pages, but there are great difficulties in obtaining satisfactory determinations of epicentres for the later war years, which have delayed further publication.

The corrections to adopted tables have not yet been completed.

Earthquake Periodicity.

The study of long periods in the 'Chinese Earthquakes' directed attention to a period near 260 years. This was in the first instance identified as 240 years ('Mon. Not. R.A.S.,' lxxix., p. 531) as mentioned in the last report, and Mr. De Lury pointed out that this value also suited tree-records (Pub. Amer. Ast. Soc. 1919). But an investigation on the secular acceleration of the Moon by Dr. Fotheringham recalled attention to a value nearer 260 years, which was also found to suit the tree-records ('Mon. Not. R.A.S.,' lxxx., p. 578) over the same period. Ultimately a much longer series of tree-records was obtained (Mr. A. E. Douglass's compilation from 1180 B.C.) and a full analysis of these, now in the press ('Mon. Not. R.A.S.,' 1920 Supp. No.), suggests a double periodicity, with components of approximate lengths 284 and 303 years. Long as it is, the series of tree-records is not long enough to separate these components themselves : the evidence for separation is provided by the harmonics, especially the third harmonic, which shows components of 101 years and 94·4 years clearly separated, the former and longer being the stronger, whereas in the main terms the shorter period is the stronger. The second harmonic of the longer period, i.e., half 303, or, say, 152 years, is quite possibly the 156-year period referred to in the last report.

These results have been obtained so recently that their full relation to the earthquake records have not yet been worked out. But a welcome confirmation may be mentioned. In the 'Bull. Seism. Soc. of America,' vol. ii., No. 1, Miss Bellamy found a later list of 'Chinese Earthquakes' compiled by N. F. Drake. It is not entirely independent of the catalogue already studied (compiled by Shinobu Hirota in 1908 and mentioned by Drake as having been received too late for inclusion or comparison), but it differs from it in one important respect, being copious in the later centuries where Hirota's catalogue is scanty. Further, it is confined to 'destructive or nearly destructive' earthquakes, so that the records are probably more precisely comparable inter se, although they still show a large increase about A.D. 1300, which must be attributed to greater

completeness of the later records, or rather imperfection in the earlier years. The following table gives the analysis in periods of 284 years :—

TABLE I.

Numbers of Chinese Destructive Earthquakes (Drake).

Initial Year	2081 B.C. to 94 B.C.	93 B.C. to A.D. 190	191 to 474	475 to 758	759 to 1042	1043 to 1326	1327 to 1610	1611 to 1894	Total	Revised
0	1	2	1	2	2	3	28	27	66	48
24	2	2	1	5	2	7	21	12	52	58
48	0	3	2	0	3	2	1	11	22	25
71	1	2	4	0	2	5	0	2	16	28
95	0	0	5	1	3	0	7	8	24	27
119	0	2	10	1	2	1	9	5	30	39
142	2	0	0	3	0	4	26	7	42	37
166	3	3	4	1	1	7	30	4	53	53
190	4	5	0	0	1	0	23	9	42	42
213	0	7	2	0	2	4	22	12	49	39
237	1	7	1	1	9	9	23	15	66	68
261	0	3	1	4	3	18	22	14	65	63
Total	14	36	31	18	30	60	212	126	527	527

The totals in the last column but one are governed chiefly by the later cycles. To minimise this effect the eight columns were all reduced to the same total 66, using one place of decimals until the sums were formed. The results are given under the heading 'Revised,' and it will be seen that they give substantially the same curve, with pronounced minimum extending from the 48th year to the 118th, and a pronounced maximum at the end. The 48th year of the present cycle will be 1942, so that we are approaching the time of minimum quakes and have passed the maximum. But it is not yet clear whether these figures for China apply unmodified to the whole earth. It may be possible to observe this decline in the near future, but up to the present the records are affected by so many uncertainties, owing partly to the novelty of the science, partly to the war, and to other causes, that it is very difficult to compare one year with another. Thus the Eskdalemuir records show the following *total* numbers of earthquakes :—

1911	1912	1913	1914	1915	1916	1917	1918
236	393	287	278	184	163	166	192

which at first sight might be interpreted as a notable falling-off in earthquake activity, but is probably chiefly due to a change of method in 1915. The point will, however, be further examined. Analysing the last two columns of Table I. harmonically we get from the simple totals

$$20 \cos (\theta - 301°) + 11 \cos (2\theta - 348°) + 1 \cos (3\theta - 304°) + 5 \cos (4\theta - 98°)$$

from the revised

$$14 \cos (\theta - 304°) + 8 \cos (2\theta - 340°) + 3 \cos (3\theta - 211°) + 8 \cos (4\theta - 138°).$$

The third harmonic is small—smaller than the fourth, for instance. But on analysing the results in 101 years a larger term is obtained. The totals are (for twelve groups to the cycle, which gives nearly the same mean as above)

48 41 45 41 31 53 38 37 33 56 43 54

which gives a term $5 \cos (\theta - 331°)$.

This is in accordance with the results found from trees—that the 101-year term should exceed the 94 year.

Microseisms.   By J. J. Shaw.

Microseisms appear to have been a much neglected study. A few observers have counted them, measured their frequency and amplitude, and noted their seasonal character, but beyond this little seems to have been done. This is all the more remarkable in view of the fact that microseisms, unlike earthquakes, are always more or less available for investigation.

In 1911 the International Seismological Congress in Manchester allotted 500*l.* for their investigation, and as a result the Central Bureau at Strasbourg tabulated a number of observations, and, but for the European War, would probably have reported at Petrograd in 1914. If any conclusions were arrived at they do not appear to have been published.

In the 1917 report of this Committee attention was drawn to the readiness with which a microseismic wave could be identified at two adjacent stations (in that case, in separate buildings 60 feet apart).

The two machines, arranged with precisely similar constants, produced identical records of the microseisms; but an interesting feature was observed, that, when keeping the nominal magnifications of the two machines the same, and at the same time varying the relative sensitivity to tilt of one machine to as much as four times the other, the amplitude shown on the film remained the same on each machine. This seems to indicate that a microseismic wave is purely horizontal and compressional rather than of an undulating gravitational character.

In the same report it was suggested that, by gradually increasing the distance between the recording stations (but only so long as it was possible to identify the individual waves), it might be possible to trace the origin and cause of these movements.

With this object in view two suitable stations were secured. The one was the writer's household cellar at West Bromwich, the other a 'dug-out' in a pit bank at Millpool Colliery situated two miles away, and kindly placed at our disposal by T. Davis, Esq., of the Patent Shaft and Axletree Co., of Wednesbury.

The dug-out was a tunnel 60 feet into the mound and 15 feet below the surface. It lay 17° west of north of the 'home' station.

The first observations were made in March and April 1919, when for a few weeks two Milne-Shaw machines were available.

It was at once seen that at stations two miles apart the records of the microseismic waves were almost identical.

The clock in use at the dug-out was not of a sufficiently high standard to obtain the precise difference in time of arrival at the respective stations.

Several seismograms were obtained during this time and were seen to be similar in every detail.

In March and April of the present year a first-class timing clock was substituted, and two more machines installed with the intention of timing the microseismic wave over this two-mile base line.

The usual means of synchronising were not available, therefore the clocks were adjusted as follows :—

A watch with an excellent hourly rate was chosen and carried per motor-cycle between the stations. Two observations, with 30-minute intervals, were made on the home clock, two on the dug-out clock, and two more on the home clock. It was estimated that on favourable occasions the two clocks were set alike within one-tenth of a second. The clocks were checked once per day, and the waves timed by measuring on the film from a minute eclipse to the nearest apex at the extreme of an excursion.

This first method was continued from January 31 to February 15. As differences of 1½ to 2 seconds were shown—being probably erroneous—an effort was made during March to secure a closer comparison.

Firstly, the clocks were checked twice per day. Secondly, as, on a closer scrutiny, small fluctuations in the peripheral speed of the recording drums could be detected, it was seen to be inadvisable to measure any intermediate point during a minute, but to rely only upon the moment when the eclipsing shutter opened or closed.

The duration of the eclipse was 4·7 seconds in each case, so that opening or closing were equally serviceable as datum points. Therefore a new method of comparing the films was devised as follows :—

The eclipsing shutter was provided with a narrow slit through which a small percentage of light could pass when the shutter was closed. This feeble beam produced a ghost-like trace during the interval of each eclipse.

In making comparisons instances were chosen where the amplitude was not only large but also where the shutter had opened or closed near the middle or zero position of the wave.

The change of intensity of the trace was sharp and easily measured, whilst the extremity of the excursion could be seen in the ghost.

The period of the wave and its phase at the datum point having been determined, it was then possible to resolve the harmonic motion, and so obtain the difference in time to one-tenth of a second.

It is interesting to note that by either method the average difference was 0·8 second, but the second method gave much more consistent readings.

A further object was to note to what extent the direction of propagation, the amplitude, or the period were affected by meteorological conditions, particularly the direction and force of the wind.

We were indebted to A. J. Kelly, Esq., Director of the Birmingham and Midland Institute Observatory (four miles distant), for his help in this matter.

The force of the wind and the amplitude did appear to be co-related, inasmuch that the microseisms were small during calm spells and *vice versa*, but there was a notable exception on March 10. During March 9 and 10 the air movement had been small, 178 and 272 miles in each 24 hours respectively, yet on the evening of the 10th nearly the largest waves of the series were recorded.

Within a period of 24 hours, March 12 to 13, the velocity of the wind ranged from 37 to 12 and back to 37 miles per hour in three nearly equal periods, but there was no corresponding fluctuation in the amplitude of the microseisms. Similar fluctuations on other dates were equally ineffective to produce sudden change in the ground movement.

There was little variation in period. It was usually 6 to 7 seconds. On a few occasions it fell to 4·5 seconds, but never exceeded 8 seconds. It will be observed that the period appears to increase with the amplitude.

The outstanding, and we venture to think important, discovery was that the microseismic waves always arrived from the same direction. On every film they were seen to arrive at the 'dug-out' or northerly station first.

During the period of observation the wind blew from all points, except north to east, but no quarter seemed to affect the regularity with which the waves arrived from the north.

Column two in the following table gives the time in seconds by which the waves arrived at the dug-out first :—

By First Method.

Date	Difference, Sec.	Wind Direction	Daily Horl. Motion of the Wind, Miles	Amplitude μ	Wave Period, Sec.
1920					
Jan. 31	0·0	S—WSW	427	5·8	7·3
Feb. 2	0·0	SW—S	587	4·0	7·5
,, 6	1·5	SSE	295	2·8	6·7
,, 9	1·0	WSW	491	3·2	6·3
,, 10	1·0	SW	670	9·5	8·0
,, 12	0·0	WNW—S	354	3·6	6·2
,, 13	2·0	SSW—W	528	6·4	6·8
,, 15	1·5	S	423	5·0	6·2
Average	·87		472	5·0	6·9

By Second Method.

Date	Difference, Sec.	Wind Direction	Daily Horl. Motion of the Wind, Miles	Amplitude μ	Wave Period, Sec.
1920					
March 4	1·0	WSW—S	260	4·9	7·5
,, 5	1·0	S	285	1·6	6·7
,, 6	0·75	S	476	4·5	6·0
,, 8	1·1	NW	407	2·4	6·7
,, 9	—	W	178	—	—
,, 10	0·7	SSW	272	7·0	7·0
,, 11	1·1	NW	257	4·9	6·5
,, 12	0·5	W	541	5·7	6·0
,, 13	1·0	S	377	4·5	6·2
,, 18	0·8	W	500	4·0	6·7
,, 19	—	W	228	2·0	6·0
,, 20	—	W	131	0·8	5·5
,, 24	0·8	S	348	4·0	7·3
,, 26	0·7	S	613	5·3	7·3
,, 28	0·8	S	498	3·2	5·7
Average	·83		371	3·9	6·5

It will be observed that the time in column two is generally about one second, which is the approximate time required for a surface wave to travel two miles, thus indicating that the direction of propagation was more or less constant and approximately from north to south.

On the other hand there are differences ranging between 0·7 sec. and 1·1 sec. Remembering the method of synchronising the clocks it is possible many of the irregularities are due to personal and instrumental error. To what extent they indicate that the azimuth wanders round the northern semicircle it is difficult to determine, but from the fact that the southern half was never indicated, it would seem feasible to presume that the waves came generally from the north.

More precise information is very desirable, and can only be obtained from not less than three stations with preferably a longer base of operation, and with better timing facilities.

It is hoped, at some future date, when three machines are simultaneously available and suitable quarters and observers found, to make the experiment on a ten-mile triangle.

An attempt was made to identify the microseisms recorded at Oxford with those of West Bromwich (80 miles apart), but unfortunately the booms are oriented 90° from each other. From some measures made by Professor Turner there was a suggestion of agreement, but nothing really tangible has at present been detected.

A fruitful investigation for observatories would be to determine whether this unidirectional character of microseisms is general, and whether the azimuth depends upon the contour or physical features of a country.

From the foregoing it is clear that microseisms are real travelling waves of the same character as those propagated by earthquake shocks, and if a seismograph fails to perceive them then it is not recording all that is passing.

Two stations where Milne-Shaw instruments are installed, viz., Bidston and Edinburgh, seem to be very liable to microseisms. Both stations are near the sea, and both stand upon the crest of a hill.

Shide was within six miles of the open sea, but did not stand upon a hill. This station did not find the microseisms more prevalent than an average station.

Oxford and West Bromwich are well removed from the sea. They record microseisms as freely as Shide. It has yet to be determined whether the seaboard is more liable to these movements : the evidence points to that conclusion.

The P phase of a seismogram sometimes, but not often, begins with a sharp kick—denoted i P; but sensitive machines show that much more frequently this sharp kick is preceded by two or three waves of smaller amplitude and higher frequency. When the frequency is distinctly quicker than that of the prevailing microseisms, and the amplitude of the latter is not too great, it is easy to detect the true P as a superimposed wave, but if the period of these small precursors approximate to that of the microseisms, then it is difficult to determine the true inception of the earthquake record.

Machines which do not record the microseisms will not record these minute waves. With such machines probably more uniformity, by reading the bigger kick, will result, but misguided uniformity will not be conducive to obtaining the true rate of propagation of the P phase.

It is to sensitive machines and careful scrutiny of the record that we must look for data for the perfecting of seismological tables.
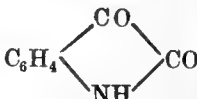
Absorption Spectra of Organic Compounds.—Report of Committee
(Sir J. J. Dobbie, *Chairman*; Professor E. E. C. Baly, *Secretary*;
and Dr. A. W. Stewart). *Drawn up by the Secretary.*

Various theories have been advanced from time to time to explain the absorption
bands exhibited by organic compounds, and it would seem advisable at this time
to deal with these and to state the position that has been reached in this branch
of scientific investigation. There is no doubt that the pioneer in this field of
work was the late Sir Walter Noel Hartley. He was the first to undertake a
detailed investigation on scientific lines of the absorption exerted by organic
compounds in the visible and ultra-violet regions of the spectrum. He was the
first to recognise the fact that isolated measurements of the absorption spectrum
of a substance in solution are valueless, and he devised the method whereby com-
plete records of the absorption could be obtained. Hartley's method consisted
in measuring the oscillation frequencies of the light for which complete absorption
is shown by definite thicknesses of a solution of known strength of the sub-
stance. The observations were repeated with the same thicknesses of more and
more dilute solutions until no measurable absorption was observed. By plotting
the oscillation frequencies against the thicknesses expressed as equivalent thick-
nesses of some selected concentration an absorption curve was obtained, called by
Hartley a molecular curve of absorption.

At the present time this method of observation has been displaced by the
quantitative measurement of the light absorbed. The absorptive power exhibited
by a given substance for light of a given frequency is expressed in terms of the
molecular extinction coefficient, log $Io/I \div dc$, where Io/I is the ratio of the
intensities of the incident and emergent light as observed with a layer d cms
thick of a solution containing c gram molecules of the absorbing substance
dissolved in a litre of some diactinic solvent.

Reference may be made to the use of a solution of the substance under
examination. In general it may be said that the absorptive power exerted
by compounds is large, with the result that it is necessary to use very thin layers
for purposes of observation. This is impossible of realisation with solid sub-
stances, and indeed with many liquids the thickness required is so small that
without very accurate and expensive apparatus the necessary thin layers cannot
be obtained. By common consent, therefore, solutions of known strength in
diactinic solvents are employed. It must be remembered, however, that the
influence of a solvent on the absorptive power of a compound is often very
marked, and due allowance must be made for this effect. The question of the
influence of a solvent will be discussed later.

The region of the spectrum dealt with by Hartley extended from the red
end to the limit of the ultra-violet as set by a quartz spectrograph working in
air, that is to say, between the limits of wave-length 6000 and 2100 Ångströms.
He showed in the first place that substances can in general be divided into two
classes, namely, those which exhibit selective absorption, *i.e.*, absorption bands
between the above spectral limits, and those which exhibit only general absorp-
tion. It is not necessary here to detail the whole of Hartley's work, but one
important fact was established, namely, that, providing no disturbing factor
intervenes, the absorption curves shown by compounds of similar constitution are
themselves similar. This fact was made use of in determining the constitution
of a few substances with reference to which the chemical arguments at the time
were at fault. It was shown for instance that phloroglucinol is a true trihydroxy-
benzene and not ketonic since its absorption curve is very similar to that of its

trimethyl ether.[1]   Similarly   the   constitution   of   isatin C_6H_4 $\begin{matrix} CO \\ CO \end{matrix}$ NH

[1] See references, p. 243.

carbostyril[2] C_6H_4 < $\begin{array}{c} CH=CH \\ | \\ NH-CO \end{array}$, and o-oxycarbanil[3] C_6H_4 < $\begin{array}{c} O \\ | \\ NH \end{array}$ >CO, was determined
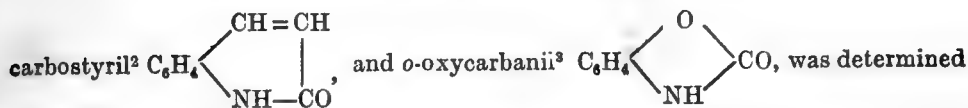
by comparison of their absorption curves with those of their nitrogen and oxygen methyl derivatives.

It may readily be understood that high hopes were engendered that this method might prove to be of immense value to the chemist as independent evidence in the determination of the constitution of compounds, but it may be said at once that these high hopes have not been realised. A very brief account may be given of the various attempts that have been made to co-ordinate constitution and absorption of light, because all of these attempts have some importance in relation to more recent developments. Following on Hartley's successful work an attempt was made to determine the constitution of ethyl acetoacetate and its metallic derivatives by comparison with its two ethyl derivatives, ethyl β-ethoxycrotonate and ethyl ethylacetoacetate.[4] It was found, however, that the parent ester and its metallic derivatives differ in absorptive power very materially from the two isomeric ethyl derivatives. The two latter do not show selective absorption, whilst the metallic derivatives show well-marked absorption bands. The deduction was made from this that the origin of the absorption bands is to be found not in any specific structure but in a tautomeric equilibrium between the two forms, that is to say, the selective absorption of light is due to

the change of linking involved in the process $-\overset{\overset{O}{\|}}{C}-CHM- \underset{\longleftarrow}{\overset{\longrightarrow}{}} -\overset{\overset{OM}{|}}{C}=CH-$, where

M stands for hydrogen or a metal.

This theory was extended to aromatic compounds where the selective absorption was considered to be due to the oscillation of linking supposed to be present in the benzene ring. The absence of selective absorption observed with some benzenoid compounds was considered to be due to the restraint on the oscillation exercised by certain strongly electro-negative substituent groups such as NO_2, &c.[5]
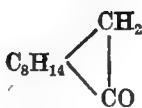
Without question one of the most important theories connoting absorption and structure is that known as the quinonoid theory which connected visible colour with a structure analogous to that of either para- or ortho-benzoquinone. This theory has found great favour on account of the undoubted fact that when a quinonoid structure is possible the substance in the majority of cases is visibly coloured, whilst in the case of an isomeric substance in which a quinonoid structure is not possible the colour is in general less intense or indeed very slight. It was a simple matter to apply the oscillation theory in explaining the visible colour of the quinonoid compounds. The oscillation was suggested as that between the two forms
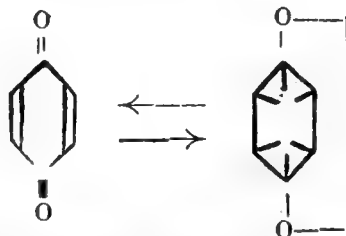
Similarly the visible colour of the α-diketones was explained by the oscillation

between the two forms $-\overset{\overset{O}{\|}}{C}-\overset{\overset{O}{\|}}{C}- \underset{\longleftarrow}{\overset{\longrightarrow}{}} -\overset{\overset{O}{|}}{C}=\overset{\overset{O}{|}}{C}-$, which after all is only a slight variation of the quinonoid conception. This particular type of oscillating linking was named isorropesis.[6]

It was soon pointed out, however, that this theory was open to serious objection because certain compounds in which no oscillation seemed possible

exhibit strong selective absorption.   For example camphor [7] $C_8H_{14}\diagup\diagdown\begin{smallmatrix}CH_2\\|\\CO\end{smallmatrix}$ shows

a marked band, as also does the disubstituted compound [8]

$$C_8H_{14}\diagup\diagdown\begin{smallmatrix}/CH_3\\C\diagdown Br\\|\\CO\end{smallmatrix}$$
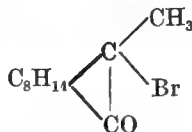
in which no tautomeric equilibrium seems possible.   Again, azo-*iso*-butyronitrile shows marked selective absorption.

$$\begin{smallmatrix}CH_3\diagdown\\CH_3\diagup\end{smallmatrix}C-N=N-C\begin{smallmatrix}\diagup CH_3\\\diagdown CH_3\end{smallmatrix}$$
$$\quad\quad\;| \quad\quad\quad\;|$$
$$\quad\quad CN \quad\quad\; NC$$
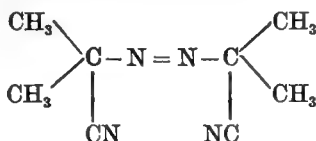
The most interesting example of a compound which exhibits an absorption band is chloropicrin, CCl_3NO_2, which does not contain any hydrogen atoms at all.   It may be noted that Hantzsch has taken up the position that there is a definite correlation between constitution and absorption, and he has published very many papers in support of his theory.   The starting-point of the theory is the derivatives of ethyl acetoacetate which have already been referred to.   He showed that ethyl dimethylacetoacetate, which is an absolutely definite ketonic compound, exhibits only slight general absorption.   The enolic derivative ethyl β-ethoxycrotonate at equal molecular concentration exhibits more strongly marked general absorption.   Hantzsch assumes [9] that the absorption curves are truly characteristic of the ketonic and enolic forms respectively.   He then assumes that the absorption band shown by the metallic derivatives of ethyl acetoacetate is due to the constitution where M stands for a monovalent metal.   The novelty

$$\begin{matrix}CH_3\\|\\C\\ \diagup\quad\diagdown\\H_2C\quad\quad O\\|\quad\quad\;|\\C\quad\quad M\\EtO\diagup\;\diagdown\quad\diagup\\O\end{matrix}$$
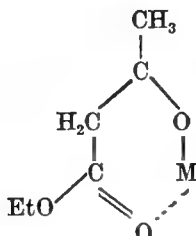
of the conception lies in the mutual influence of the secondary valencies or residual affinities of the metal and oxygen atoms, this influence being denoted by the dotted line in the formula.   It will be seen that this explanation of selective absorption does not involve any liable atoms but attributes the phenomenon to secondary valencies.   Starting from this original assumption Hantzsch has built up a complete theory of a direct correlation between absorption and constitution which states that if a substance exhibits different absorption curves under different conditions of solvent, &c., this is due to a definite change in constitution.   It is not worth while to describe in detail the conclusions which Hantzsch arrives at as regards the specific compounds examined by him,[10] such, for instance, as the variety of absorption bands shown by compounds of an acid type when dissolved in different basic solvents, each different absorption band being attributed to a different structure of the compound.   It is perhaps worthy of mention that Hantzsch finds it necessary to confess that in some cases the variations in absorption shown by certain compounds are more numerous than can be accounted for by changes in constitution.

It may be stated at once that there are several very grave objections to

Hantzsch's theory, and indeed these are so fundamental that it becomes impossible to accept the theory as it stands. In the first place, as was pointed out above, the cardinal assumption on which the whole theory rests is that the absorption band shown by the metallic derivatives of ethyl acetoacetate is due to the secondary valencies of the metallic atom and the carbonyl oxygen of the carboxyl group. There are many cases of compounds in which secondary valencies must be postulated in order to explain their very existence, and these compounds do not generally show absorption bands in the visible and ultra-violet. Some peculiar merit must therefore be attributed to the six-membered 'ring' of Hantzsch's formula, and it is difficult to accept this since the selective absorption of such compounds as the alkaline nitrates and chloropicrin obviously cannot have any relation to a six-membered ring.

More important still are two facts which appear to have escaped the notice of Hantzsch. First, ethyl dimethylacetoacetate in the presence of alkali shows an absorption band very similar to that shown by ethyl acetoacetate in the presence of alkali. Second, ethyl β-ethoxycrotonate shows an incipient absorption band in the presence of acid. It is obvious that these two observations are in direct opposition to the Hantzsch formula as the correct explanation of the selective absorption shown by the metallic derivatives of ethyl acetoacetate.

Still more cogent arguments against the theory of correlation between structure and absorption in the visible and ultra-violet are to be found in such cases as pyridine and piperidine. Pyridine in the homogeneous state and in solution in various solvents exhibits an absorption band with centre at $1/\lambda = 3910$, but in the vapour state it shows an entirely different band with centre at $1/\lambda = 3587$.[11] Piperidine vapour shows a well-marked absorption band, but in solution and in the homogeneous state it is completely diactinic. Analogous dissimilarities between the molecular absorptive powers of liquid and vapour have been observed with other compounds, and clearly on the structure-absorption theory the structure of the molecules in the liquid and vapour phases must be different. This would seem to be impossible at any rate in the case of symmetrical molecules such as pyridine and piperidine.

The evidence against the direct structure-absorption correlation theory as developed by Hantzsch is overwhelmingly great, and this is equally true of the quinonoid explanation of visible colour. The evidence of numerous colourless compounds which cannot be quinonoid in structure is sufficient to condemn this theory, even were there no other evidence against it. One of the most often quoted instances in which the quinonoid theory is invoked is the well-known case of aminoazobenzene. This compound gives with hydrochloric acid (one equivalent) a salt which is more highly coloured than it is itself. This is universally accepted as being due to the salt having the structure

$$\text{ClH}_2\text{N} = \bigodot = \text{N} - \text{NH} - \bigcirc$$
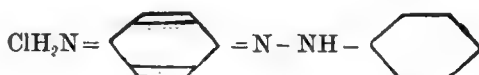
because the colour and absorption spectrum is entirely different from that of benzeneazophenyltrimethylammonium iodide.

$$\underset{\text{Cl}}{\overset{(\text{CH}_3)_3}{>}}\text{N} - \bigcirc - \text{N} = \text{H} - \bigcirc$$
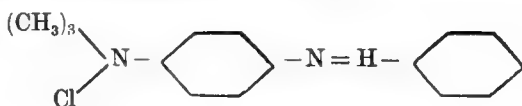
which of course corresponds to the normal form of the hydrochloride.

$$\underset{\text{Cl}}{\overset{\text{H}_3}{>}}\text{N} - \bigcirc - \text{N} = \text{N} - \bigcirc$$
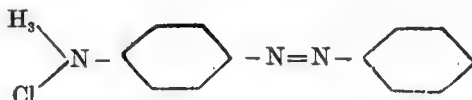
On the other hand, the trimethylammonium compound also gives a salt which is more highly coloured than it is itself, and obviously this cannot be due to a quinonoid structure. It is clearly unjustifiable to explain the one case of colour

change by the quinonoid configuration when the other case of exactly analogous colour change cannot be so explained.

Another well-known application of the quinonoid hypothesis is to the alkali metal salts of the nitrophenols which are highly coloured. It is stated, for example, that the sodium salt of p-nitrophenol has the constitution
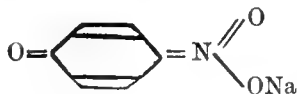
If that is so, what is the constitution of the nitrophenol when in solution in concentrated sulphuric acid, for it is equally coloured under these conditions? A similar coloured solution is obtained when p-nitroanisole is dissolved in sulphuric acid. Many other instances could be quoted, and there is no doubt that the evidence against a direct structure-absorption correlation is overwhelmingly great.

There are two general objectives to any of the theories that have been referred to. In the first place, no theory can be sound which is limited to a very minute section of the spectrum such as the visible and ultra-violet, and in the second place, no theory can hold good unless it rests on a quantitative physical basis. There is also another aspect of the phenomenon of absorption, namely, its undoubted connection with the phenomena of fluorescence and phosphorescence. Just as the selective absorption of light must be due to specific properties of molecules, so also must the emission of light by molecules be due to similar properties. It is evident that any theory must take cognisance of both phenomena. It is true that many theories were advanced to explain the fluorescence of organic compounds, but none of these can be said to hold the field. Devised to explain visible fluorescence they fail entirely to offer any explanation of the ultra-violet fluorescence shown by many compounds.

In general it may be said that the most recent work on the absorption by organic compounds has increasingly shown that there is some relation between the absorption bands shown by a substance and its reactivity. Perhaps the first observations which supported this view were those of certain amino-aldehydes and -ketones of the aromatic series and their salts with hydrogen chloride.[12] It was found that alcoholic solutions of these compounds exhibit well-marked absorption bands. On the addition of small quantities (0.1 to 0.5 eq.) of hydrochloric acid to these solutions a new absorption band, situated nearer to the red, is developed in each case. On the addition of more acid this band disappears and gives place to the absorption characteristic of the hydrochloride of the original base. This shows that the base as it exists in alcohol solution does not react with the acid to give the salt, but that it is first converted into an intermediate or reactive phase which then reacts with more acid to give the salt.

These observations were extended to many substances, notably certain phenolic compounds including the nitrophenols.[13] The compounds in alcoholic solution exhibit well-marked absorption bands which are not appreciably changed when sulphuric acid is added. When dissolved in concentrated sulphuric acid they develop visible colour due to absorption bands in the visible region. The compounds in sulphuric acid solution, on being allowed to remain, slowly undergo sulphonation to give colourless sulphonic acids. Clearly, therefore, these phenols in the condition in which they exist in alcoholic solution do not react with sulphuric acid. When dissolved in strong sulphuric acid they are changed into a reactive phase which slowly reacts with the sulphuric acid to give the sulphonic acid. They are therefore exactly analogous to the amino-aldehydes and -ketones already mentioned.

It might easily be said that the coloured reactive modifications have undergone a change in structure, but further evidence shows that no change of structure has taken place. The majority of these compounds in alcoholic solution exhibit fluorescence when exposed to light of frequency equal to that of their absorption bands. The frequency of this fluorescent emission has been accurately measured, and it has been found in every case of the above-mentioned substances that the frequency of the fluorescence of the compound in alcoholic

solution is equal to that of the absorption band shown by that compound when in the reactive phase. The same frequency therefore is characteristic of a given substance in two solvents, in one of which it is exhibited as emission and in the other as absorption. It is evident, therefore, that the constitution of each compound is the same in the two cases.

Very important conclusions may be drawn from these observations, namely, that a given compound can exist in at any rate two phases which differ in their reactivity and which are characterised by different absorption bands. Also the absorption bands shown by the reactive phases are nearer to the red end of the spectrum. It is therefore an obvious deduction that a definite absorption band is associated with a definite type of reactivity.

The next question to consider is whether an explanation of these facts can be found. In the theories of absorption spectra given above no reference is made to the ultimate destination of the light which is being absorbed. It is perfectly obvious that, unless the absorbing compound undergoes a photochemical change, the total amount of energy absorbed must again be radiated. It is equally evident that just as the light energy is absorbed at frequencies which are characteristic of the absorbing substance, so also must this energy be radiated at frequencies characteristic of the substance. Careful experiments have proved that, provided the absorbing substance or its solution is free from dust, there is no evidence of radiation at the frequencies which lie within the absorption band. Clearly, therefore, the phenomenon of absorption is not one of optical resonance, that is to say, the light energy absorbed by a substance is radiated at frequencies which are not the same as those at which it has been absorbed. Except in those cases where fluorescence or phosphorescence is observed, the whole of the absorbed energy is radiated at frequencies which lie in the infra-red region of the spectrum, and we have therefore—

Energy absorbed (visible or ultra-violet) = energy radiated (infra-red).

This necessarily establishes a relationship between the various frequencies exhibited by a substance in the infra-red, visible, and ultra-violet regions, and, indeed, invites investigation of this relationship.

It will be remembered that Planck formulated the theory that absorption and radiation of energy are not continuous processes, but are discontinuous in the sense that the energy is absorbed or emitted in a series of fixed amounts. To these fixed amounts he gave the name of energy quanta, and he showed that the size of the quantum is given by the product of the frequency into a universal constant, the most recent value of which is $6 \cdot 56 \times 10^{-27}$. According to this theory, therefore, if a substance is absorbing light with a frequency of, say, 9×10^{14}, the process is not continuous, but each molecule absorbs a series of quanta, each of which is $9 \times 10^{14} \times 6 \cdot 56 \times 10^{-27}$, or $5 \cdot 904 \times 10^{-12}$ ergs. Without discussion of the fundamental basis of this quantus theory it may be applied to the problem of the absorption and radiation of energy by a molecule when, as already explained, the total quantity of energy absorbed is radiated at another and smaller frequency. Let a molecule absorb one quantum of light energy at its absorbing frequency. This energy is then radiated at another and smaller frequency, but it must be radiated as a whole number of quanta at that frequency. It follows, therefore, that when a molecule is absorbing at one frequency and radiating at another and smaller frequency, one quantum of energy at the larger frequency must be equal to a whole number of quanta at the smaller frequency. Finally, since the quantum is the product of the frequency into the universal constant, the conclusion is reached that the absorbing frequency must be an exact multiple of the radiating frequency. In other words, the frequencies of each absorption band shown by a substance in the visible and ultra-violet must, on the basis of Planck's theory, be an exact multiple of a frequency characteristic of that substance in the infra-red. It was not difficult to test the validity of this deduction since the existence of characteristic frequencies in the infra-red possessed by a substance can be proved by the method of absorption spectra observations in that region, and indeed a great number of substances had already been investigated in this manner.

It may be stated at once that the relation has been found to be true in the

case of every substance examined.[14] Further than this, it is well known that certain substances exhibit more than one absorption band in the visible or ultra-violet, and it has been found that the frequencies of each of these absorption bands are exact multiples of one and the same frequency characteristic of that substance in the infra-red. It follows, therefore, that when a substance shows more than two absorption bands in the visible or ultra-violet there must exist a constant difference between the frequencies of consecutive bands, and this difference must equal the fundamental infra-red frequency. This has also been proved to be true.

The application of the Planck theory has led to the discovery of relationships between the frequencies of the absorption bands shown by a substance, relationships which are of considerable importance because they form a quantitative basis of molecular frequencies. It is not possible here to give the mathematical development of Planck's theory, and the theory is only mentioned because it led to the discovery of the relation between the frequencies.

It is advisable at this point to discuss in some detail what is meant by the frequency of an absorption band and also the influence of a solvent upon that frequency. It is common knowledge that in many instances under high resolving power an absorption band is found to possess a structure. The most common phenomenon is when an absorption band consists of a series of sub-groups. In this case one sub-group always exhibits a maximum absorptive power, and those on either side exhibit decreasing absorptive power the farther they are situated from the principal sub-group. Then, again, it is generally found by the examination of the vapour of the substance that each of the sub-groups is resolved into fine absorption lines, and that the arrangement of these lines as regards their intensity is analogous to that of the sub-groups themselves. There is always in each sub-group one line of maximum intensity, and the other lines are arranged in series of decreasing intensity with regard to this central line.

Now when a substance is cooled to low temperatures it is found that its absorption bands become narrower, this being due to the suppression of the outermost sub-groups. With further fall of temperature more and more sub-groups disappear, and finally there is left only the principal line of the principal sub-group. This absorption line persists even at the lowest temperatures yet reached. It is perfectly evident therefore that this single frequency is truly characteristic of the molecules, and that the other frequencies which make up the breadth of the band are due to some cause connected with the temperature of the molecules. There is, of course, no necessity to cool a substance to low temperatures in order to recognise the true molecular frequency, because this frequency is always that one for which the absorptive power is the greatest in the absorption band. In the quantitative relationships given above it is this true molecular frequency which is referred to.

It is perhaps not out of place to refer to the confusion that has arisen from time to time from carelessness in nomenclature in dealing with absorption spectra observations. The term 'band' is applied to the whole region covered by one set of associated groups or sub-groups. In the literature the word band has been used when a sub-group of a band is meant, and thus considerable confusion has been caused.

The next point to be dealt with is the variation in absorption caused by a solvent, a fact that is of material importance in connection with the quantitative relations between the molecular frequencies exhibited by a compound. Hartley was the first to observe the difference in frequency of a particular absorption band according to whether a substance is examined in the vapour state or in solution in a solvent, and he noted that there is always a small shift towards the red in passing from vapour to solution. There are, in fact, two different effects of a solvent upon the absorption spectrum of a substance as observed in the vapour state. One of these has already been mentioned, namely, the appearance of an entirely different absorption band when the substance is dissolved. In this case the vapour exhibits a molecular frequency which is one multiple of the infra-red frequency, whilst the solution exhibits a molecular frequency which is another multiple of that infra-red frequency. In the case of some compounds it has been shown that by the use of different solvents a number of different multiples of the infra-red fundamental are called into play.

The second effect of a solvent is when the same molecular frequency is common to vapour and solution, but the measurements of this frequency with vapour and solution do not give exactly the same values. It is this particular effect which requires discussion, because unless the phenomenon is understood the relationships between the infra-red fundamental frequency and the visible and ultra-violet frequencies will apparently not hold good. Without going fully into the quantitative measurements it may be stated that the change in the value of the molecular frequency in passing from vapour to solution depends on the nature of the solvent and on the concentration in that solvent.[15] As regards the effect of concentration, the difference between values of the molecular frequency as observed with vapour and solution is greatest with concentrated solutions. As the solution is diluted the value more and more nearly approaches the value for the vapour until at very great dilution the value for the solution equals that for the vapour. This change in the molecular frequency in passing from vapour to solution is not due to the fact that the quantitative relation between visible or ultra-violet bands and the infra-red fundamental does not hold, but to the fact that the infra-red fundamental itself varies slightly in position with the nature of the solvent and the concentration in that solvent. Another important fact to be recorded is that a compound in the liquid state does not show exactly the same molecular frequency as it does in the state of vapour. This, again, is due to a small difference in the infra-red fundamental frequency in the two states. It is obvious, therefore, that in making measurements of molecular frequencies the true values are those obtained with the vapour. If, as frequently happens, measurements cannot be made with the vapour, then very dilute solutions must be used. Above all, in comparing together the various molecular frequencies shown by a given substance it is necessary that all the measurements be made with the substance under the same conditions.

In connection with the effect of solvents on the absorption exerted by a substance, a brief reference may be made to the variation in the absorptive power with concentration. Measurements have as yet only been made for frequencies in the ultra-violet region. At first sight it might be expected that Beer's law would hold good, namely, that the molecular absorptive power would be independent of the concentration. It is, however, rarely the case that Beer's law holds good, and in the great majority of cases the absorptive power increases with dilution up to a constant maximum. It has been found that if K is the maximum absorptive power shown by a substance at very great dilution in a given solvent, and k is the absorptive power at a definite concentration $k/K = 1 - e^{-aV}$, where V is the volume in litres containing one gram molecule of the absorbing substance and a is a constant. A more convenient form of the above is $\log (K/K-k) = aV$.

The quantitative relationships between the various frequencies shown by a molecule may now be further considered. It has already been stated that the principal frequencies of all the absorption bands shown by a compound in the visible and ultra-violet are always exact multiples of the principal frequency of an important absorption band shown by that substance in the infra-red. This is true of all the absorption bands which are shown by a substance in different solvents, and which Hantzsch attempted to explain by assigning a different formula for each band. Other quantitative relationships have also been discovered, and these may briefly be described, because it has been found possible from a knowledge of them to formulate a quantitative theory which would seem capable of explaining all the observations that have been made on absorption spectra.

In the first place it may be noted that the examination of the absorption exerted by a compound in the infra-red reveals the existence of many more bands than the important one which has been called the infra-red fundamental, and which determines the frequencies of the visible and ultra-violet bands. Further, in every case yet examined the infra-red fundamental lines were in the short wave infra-red region, i.e., between the wave-lengths limits of 8μ and 3μ. If the principal frequencies of all the infra-red bands are examined additional interesting relationships are found. Thus the fundamental infra-red frequency either is the least common multiple of certain of the long wave infra-red

frequencies or is a multiple of that least common multiple, and indeed this relationship gives the key to the whole of the system of frequencies exhibited by a molecule. Again, the whole of the principal frequencies in the infra-red are derived from certain constants, and these constants are characteristic of the elementary atoms of which the absorbing molecules are composed. These constants or elementary atomic frequencies lie in the very long wave infra-red region, and the corresponding wave-lengths are of the order of 1000μ.

The whole of the principal frequencies shown by a molecule are determined as follows : The fundamental infra-red frequency either is the least common multiple of all the elementary atomic frequencies which are active in the molecule or is an exact multiple of that least common multiple. The principal frequencies of all the visible or ultra-violet absorption bands shown by that molecule under various conditions are exact multiples of that fundamental infra-red frequency, and therefore are characteristic of that molecule. In addition to all these frequencies which are true molecular frequencies, there also exist frequencies which are the least common multiples of some (not all) of the elementary atomic frequencies, and these are due to specific groups of atoms in the molecule, and are called intra-molecular frequencies.

The question might be asked as to how these relationships have been proved within a very high degree of accuracy in view of the fact that measurements of absorption in the infra-red have not reached a high level of accuracy. It has been found that if a molecule exhibit a principal frequency F in the infra-red, visible, or ultra-violet, there will be associated with that frequency subsidiary frequencies $F + A$, where A stands for either the intra-molecular frequencies or the elementary atomic frequencies. Indeed, it is to this cause that the breadth of the absorption bands is due. As the result of this it is possible to arrive at highly accurate determinations of the intra-molecular and elementary atomic frequencies by analysis of the absorption bands, especially those in the ultra-violet where the accuracy of measurement is very high.

The most usual arrangement of the subsidiary frequencies within an absorption band is as follows : The band consists of a series of sub-groups symmetrically arranged with respect to the principal sub-group with the greatest absorptive power. These sub-groups each possess a principal line for which the absorptive power is a maximum, and all these principal lines form a series of constant frequency difference. This frequency difference is an intra-molecular frequency and is characteristic of a specific group of atoms within the molecule.

Then, again, each sub-group is exactly similar in structure and consists of two or more series of lines, each with constant frequency difference and symmetrically arranged with respect to the principal line. These constant frequency differences are the elementary atomic frequencies characteristic of the atoms composing the specific group within the molecule, and the least common multiple of these is the intra-molecular frequency characteristic of that group of atoms.

Two instances may be given which exemplify very fully these relationships. The complete absorption system of sulphur dioxide has been found to be based on three elementary atomic frequencies.[16] Of these, two, $8 \cdot 19 \times 10^{11}$ and $1 \cdot 296 \times 10^{12}$, are characteristic of the sulphur atom because they also form the basis of the infra-red frequencies of hydrogen sulphide, and the third, $2 \cdot 4531 \times 10^{11}$, is characteristic of the oxygen atom. From direct measurement the two possible intra-molecular frequencies of the water molecule have been found to be $7 \cdot 5 \times 10^{11}$ and $1 \cdot 7301 \times 10^{12}$. Obviously if $2 \cdot 4531 \times 10^{11}$ is characteristic of the oxygen atom it should form one of the fundamental constants of the water molecule. From these three values alone it has been found possible [17] to calculate the whole of the structure of the infra-red bands of water, and the values obtained agree absolutely with those observed.[18]

Again, in one of the ultra-violet bands of naphthalene there exists a constant frequency difference of $1 \cdot 4136 \times 10^{13}$ between the sub-groups, which is therefore an intra-molecular frequency, and thus must be characteristic of a definite group of atoms within the naphthalene molecule. The two most obvious groups of atoms are the phenyl group and the olefine group, and therefore the frequency $1 \cdot 4136 \times 10^{13}$ should be the true molecular frequency of either benzene or one of the olefines, the olefines being very similar in their characteristic frequencies.

This was found to be true for the olefines since ethylene shows a series of bands in the short wave infra-red, the principal frequencies of which are exact multiples of 1.4136×10^{13}.

In formulating a theory of absorption spectra the following relationships which have been established must be considered.[19]

1. Every elementary atom possesses one or more frequencies which are characteristic of the element.

2. When atoms of different elements enter into combination the resulting molecule is endowed with a new frequency which is the least common multiple of the frequencies of the atoms it contains. This is called the true molecular frequency.

3. The central frequencies of all absorption bands, that is, those frequencies for which the absorptive power is greatest, are molecular frequencies characteristic of the molecules, since these alone persist when the substance is cooled to low temperatures.

4. The molecular frequencies in the visible and ultra-violet regions are exact multiples of a molecular frequency in the short wave infra-red, which is called the infra-red fundamental frequency.

5. The infra-red fundamental frequency either is the true molecular frequency or is an exact multiple of the true molecular frequency.

6. The breadth of an absorption band as observed at ordinary temperatures is due to the combination of the molecular central frequency with subsidiary frequencies.

The first question which arises is the meaning of the characteristic atomic frequencies which are the fundamental constants from which the whole system of frequencies shown by a molecule is derived. Presumably they are connected with the shift of an electron from one stationary orbit to another, a change which must require a definite amount of energy depending upon the electro-magnetic force field of the atom. Indeed, it would seem that, if a possibility be allowed of the shift of an electron from one stationary orbit to another, it becomes necessary at once to accept the conclusion that a definite and fixed amount of energy is involved in the change. It is proposed, therefore, to start from this assumption, that in any elementary atom it is possible to shift an electron from one stationary orbit to another, that a definite amount of energy is required to effect the change, and that this fixed quantity of energy is connected with the frequency by the relation—

$$\frac{\text{Fixed Quantity of Energy}}{\text{Constant}} = \text{Frequency.}$$

This is readily to be understood if the constant involves a function of the time taken in the actual operation, which is the same for every atom and is a universal constant.

This elementary quantum of energy involved in the electron shift is without doubt the basis of the whole energy quantum hypothesis as applied to absorption and radiation, for it can be shown that the whole can be built up from the original assumption of the elementary quantum as a specific property of the atom. For the sake of convenience only it will be necessary to make use of a value for the constant, and the most recent value for this, based on Planck's theory, is 6.56×10^{-27}. Using this value, the elementary quanta already calculated, namely, those of hydrogen, oxygen, and sulphur, lie between 5.25×10^{-16} and 1.65×10^{-15} erg, corresponding with frequencies between 8.19×10^{10} and 2.54×10^{11}.

The difference between this conception and Planck's theory may be emphasised. Whereas according to the latter the frequency is accepted as a characteristic of the atom and the quantum is the result of discontinuous absorption or emission at that frequency, the present theory assumes the quantum of energy as being due to a specific process taking place in the atom and hence a fundamental characteristic of the atom, and that the frequency exhibited by the atom is established and determined by that process. The present theory, therefore, gives a simple physical basis to the energy quantum.

The first fact to be dealt with is that when two or more atoms unite together the resulting molecule becomes endowed with a new frequency which is the least common multiple of the frequencies characteristic of the atoms. Leaving on one side the cause of the chemical combination, the energy lost in the process may be considered. The simplest possible assumption to make is that in the synthesis of any one molecule each of the component atoms contributes an equal amount of the total energy lost. An elementary atom *ex hypothesi* can only gain or lose energy in elementary quanta, and, further, can only enter into chemical combination if it already contains energy that can be evolved. Let the case be considered of two elementary atoms, the characteristic frequencies of which are 9×10^{10} and $1\cdot5 \times 10^{11}$, or in wave numbers $(1/\lambda)$ 3 and 5. The smallest equal amounts of energy that the two atoms can lose are five elementary quanta at the frequency 9×10^{10} in the one case, and three elementary quanta at the frequency $1\cdot5 \times 10^{11}$ in the other. These two amounts are each equal to one quantum measured at the frequency $4\cdot5 \times 10^{11}$, which is the least common multiple of the two atomic frequencies. In this is doubtless to be found the key to the first problem—namely, that the true molecular frequency is the least common multiple of the frequencies of the atoms in the molecule.

Further, the gain or loss of energy by a molecule as a whole must be equally shared in by the component atoms. When a molecule absorbs or loses energy as a whole, it must do so by means of the elementary quanta characteristic of its atoms. In the case of the molecule specified above, the smallest amount of energy it can gain or lose as a whole is the sum of five quanta at the frequency 9×10^{10} and three quanta at the frequency $1\cdot5 \times 10^{11}$. This minimum amount of molecular energy is two quanta at the true molecular frequency, and in this again is to be found an explanation of the fact that the true molecular frequency is the least common multiple of the atomic frequencies.

It is evident, therefore, that starting from the conception of the elementary energy quantum required to shift one electron and making the simple assumption that the combining atoms share equally in the energy loss on combination and in the future energy changes of the resulting molecule, we arrive at the conception of molecular quanta, and hence molecular frequency, the latter being the least common multiple of the atomic frequencies.

It can be shown that, when molecules under normal conditions are dealt with, one of the most important frequencies they possess is the infra-red fundamental frequency, which is an exact multiple of the true molecular frequency. In the case of sulphur dioxide the infra-red fundamental is fourteen times the true molecular frequency, and in the case of water it is eight times the true molecular frequency. It was stated above that the smallest possible equal amounts of energy which two or more atoms can evolve when combining together are equal to one quantum measured at the frequency which is the least common multiple of their atomic frequencies. It does not follow, of course, that the reacting atoms only evolve this smallest possible amount of energy. They may evolve an amount of energy which is 2, 3, 4, &c., times this smallest quantity, with the result that the smallest frequency truly characteristic of the molecule may be a multiple of the true molecular frequency. Indeed, it would seem that the infra-red fundamental is the frequency which is truly characteristic of the freshly synthesised molecule.

At the commencement the simplest possible case was considered of the combination of two atoms, each characterised by a single elementary quantum. There is no necessity to restrict the conditions in this way, and it is to be expected that, at any rate in the atoms of some elements, there will exist more than one possibility of shift of the electrons, and that there will be elementary quanta of different sizes associated with such atoms. It has already been found that two different elementary quanta are associated with the atom of oxygen in the water molecule and with the atom of sulphur in the molecule of sulphur dioxide.

Whilst the establishment of molecular quanta, and hence of molecular frequency, is a simple deduction from the conception of elementary atomic quanta, it cannot be denied that the molecule may also exhibit those frequencies which are characteristic of its component atoms. Although these atoms have united together to form the molecule, there is no reason to expect that they have

thereby lost their individuality as far as their powers of absorbing or radiating energy are concerned. The conception of the molecular quantum is based on the assumption that the component atoms can gain or lose elementary quanta when in combination. In addition to this, there is definite evidence that the molecule exhibits the specific frequencies of its atoms, since, although these atomic frequencies have not yet been observed in the long-wave infra-red, they are found in combination with the molecular frequencies as subsidiary frequencies within the absorption band groups in the infra-red, visible, and ultra-violet regions. The question then arises as to the course of events when a molecule is exposed to radiation of a frequency that is the same as one of its characteristic atomic frequencies which may be active in the extreme infra-red. Let it be supposed that the molecule formed by the combination of two elementary atoms having the characteristic frequencies 9×10^{10} and 1.5×10^{11} is exposed to monochromatic radiation of the frequency 9×10^{10}. The atom having this frequency will absorb this energy in elementary quanta of $9 \times 6.56 \times 10^{-17}$ erg : and further, let it be supposed that this atom absorb five such quanta. The total quantity of energy now absorbed is equal to the minimum quantity of energy which that atom evolves when combining with the atom with characteristic frequency 1.5×10^{11}, and is equal to one molecular quantum at the true molecular frequency. If the postulate made at the beginning as to the combination of atoms be accepted, then it would seem to follow as a natural consequence that the total energy absorbed by the atom can be transferred to or taken over by the whole molecule as exactly one true molecular quantum. In fact the molecule can obtain one true molecular quantum by the absorption of a whole number of elementary quanta by its atoms, the whole number being of course determined by the frequencies of the other atoms in the molecule and the least common multiple of all the atomic frequencies. Further, there is no reason against this process being continuous in the sense that a molecule will be able to gain more true molecular quanta than the single one by absorption of the specified number of elementary quanta by its atoms.

Again, this process will be reversible : that is to say, a molecule will be able to radiate one or more true molecular quanta in the form of the specified number of elementary quanta characteristic of one of its atoms.

It will be seen that this leads to the conception of critical amounts of energy associated with elementary atoms in combination, the critical amount of energy of an atom being a whole number of elementary quanta characteristic of that atom which in their sum equal one true molecular quantum characteristic of the molecule of which that atom forms a part. When an atom is exposed to radiation of a frequency equal to its own frequency, it can absorb its elementary quanta until its critical quantity is reached, when this critical quantity becomes merged into the molecular energy content as one true molecular quantum.

Amongst the quantitative relationships detailed above was mentioned the fact that the central frequencies of all absorption bands, that is to say, all molecular frequencies exhibited by a molecule in the visible and ultra-violet, are exact multiples of the infra-red fundamental. It is therefore evident that one molecular quantum absorbed at one of the molecular frequencies in the visible or ultra-violet is equal to an exact number of quanta at the infra-red fundamental. If a molecule absorbs one quantum at one of these higher frequencies, this amount of energy can be radiated again as a whole number of quanta at the infra-red fundamental, or partly as quanta at this frequency and partly as elementary atomic quanta. This is the process underlying the phenomena of phosphorescence and fluorescence, and in this particular case the phosphorescence will be in the form of infra-red quanta. Further, it is obvious that the fluorescence emission need not of necessity be evolved as a whole number of molecular quanta at the infra-red fundamental, but may be radiated as one molecular quantum at a molecular frequency which is a multiple of the infra-red fundamental, the remainder being radiated as molecular quanta at the infra-red fundamental or as elementary atomic quanta. For example, if the molecule absorbs one molecular quantum at the frequency which is ten times the infra-red fundamental, this energy may be evolved as one quantum at the frequency which is nine times the infra-red fundamental and one quantum at the infra-red fundamental itself. In such a case the fluorescence will be in the visible or ultra-

violet region of the spectrum. The factors governing these various alternatives are determined by the conditions under which the molecules exist. It will be seen from this that a molecule can acquire one or more molecular quanta at the infra-red fundamental in three different ways : by exposure to radiation equal to its atomic frequencies, by exposure to radiation of frequency equal to the infra-red fundamental, or by exposure to radiation of a frequency which is an exact multiple of the infra-red fundamental.

The next point to be considered is the structure of the absorption bands, that is to say, the system of subsidiary frequencies which are always found associated with the true molecular frequency when the absorbing or radiating power of molecules is examined at ordinary temperatures. These subsidiary frequencies have been attributed by Bjerrum [20] to the rotation of the molecules and by Krüger [21] to their precessional motions. Without discussion in detail it may be pointed out that both these theories break down. In the first place neither theory takes account of the fact that the subsidiary frequencies are due to the atomic frequencies, and in the second place it is necessary for the purpose of these theories to postulate impossibly large variations in the values of the molecular rotation or molecular precession.

On the other hand, the conception now put forward of elementary atomic quanta of energy, whereby definite atomic frequencies are established, would seem capable of affording a very simple and straightforward explanation. Moreover, this conception leads to the establishment of exact frequencies without any possibility of variation. The case may again be considered of the molecule formed by the combination of the two elementary atoms for which the elementary quanta are $9 \times 6 \cdot 56 \times 10^{-17}$ and $1 \cdot 5 \times 6 \cdot 56 \times 10^{-16}$ erg, and which therefore exhibit the characteristic frequencies 9×10^{10} and $1 \cdot 5 \times 10^{11}$ respectively. *Ex hypothesi* the elementary quantum is associated with the shift of one electron from one stationary orbit to another, and, of course, there is no reason to assume that only one electron can be so shifted. There may be many such electrons which can be so shifted, the amount of energy being the same for each ; and consequently it will be possible for one atom to absorb 1, 2, 3, &c., elementary quanta in the same unit of time. The atom will therefore exhibit frequencies which are 1, 2, 3, &c., times its fundamental frequency. The two atoms specified above will in the free state exhibit frequencies of $n \times 9 \times 10^{10}$ and $n \times 1 \cdot 5 \times 10^{11}$ respectively, where $n = 1, 2, 3$, &c. The molecule formed by the combination of these two atoms can also exhibit these frequencies, but now the upper limit of n will be fixed by the critical quantity previously defined. Since the least common multiple of the two atomic frequencies is $4 \cdot 5 \times 10^{11}$, the upper limits of n for the two atomic frequency series shown by the molecule will be 4 and 2 respectively, since when $n = 5$ and 3, the two atomic frequency series will converge in the true molecular frequency. Perhaps, therefore, the true molecular frequency will be better understood as the convergence frequency of the atomic frequency series than as the least common multiple of the atomic frequencies.

We may now consider one of the true molecular frequencies. Since the molecule can absorb as a whole one quantum at that frequency, and since also each atom within the molecule can absorb one or more elementary quanta, there is no reason why the two processes should not be simultaneous. The molecule will then absorb in one unit of time an amount of energy equal to the sum of one true molecular quantum and one or more elementary quanta. This will result in the establishment of the subsidiary frequencies $M + nA$, where M is the true molecular frequency, A is the atomic frequency, and $n = 1, 2, 3$, &c., the upper limit of n being fixed by the critical value as already explained.

Similarly there will be established the subsidiary frequencies $M - nA$, for the following reason. Let the molecule which is in radiant equilibrium with its surroundings absorb one quantum of energy at one of its atomic frequencies. In order for it to gain a molecular quantum at one of its true molecular frequencies it will now only be necessary for it to absorb the molecular quantum, less the atomic quantum already absorbed. It has already been shown how on the present conception summation of atomic quanta can take place to form molecular quanta ; so it would follow that, after the absorption of a given number of elementary quanta beyond that associated with the radiant equilibrium, the molecule will be able to absorb the balance necessary to form one molecular

quantum. In other words, the molecule will be endowed with the frequencies $M - nA$.

Emphasis may be laid on the fact that, under normal conditions, when the molecule is in radiant equilibrium with its surroundings the subsidiary frequencies $M \pm nA$ are actually observed; and further, that in these series of subsidiary frequencies the maximum observed value of n is one less than the critical value; that is to say, the subsidiary frequencies associated with two consecutive values of the molecular frequency do not overlap. Obviously, if the molecule is screened from all external radiation with frequency equal to its atomic frequencies—that is to say, it is cooled to low temperatures—the whole of the above deductions as to subsidiary frequencies fail, and the subsidiary frequencies must therefore vanish. This has been observed, since at very low temperatures only the central molecular frequencies remain.

In the foregoing the simplest case only was dealt with of a binary molecule formed by the combination of atoms of two different elements. Exactly the same conditions will, of course, obtain in more complex molecules, but added to these will be new conditions resulting from the existence of groups of atoms within the molecule. For instance, even in the apparently simple case of the water molecule the conditions will be more complex, owing to the undoubted fact that in this molecule the hydroxyl group exists as an integral portion of the molecule. Whilst, of course, the true molecular frequency will be the convergence frequency of all the atomic frequencies, it is the subsidiary frequencies that will exhibit a greater complexity. This complexity, however, is only one of degree, and its explanation follows exactly the same principles as were laid down for the simplest possible binary molecules. The specific case of the water molecule may be discussed in which there are three atomic frequencies, $1 \cdot 0635 \times 10^{11}$, $2 \cdot 1159 \times 10^{11}$, and $2 \cdot 4531 \times 10^{11}$. Whilst the true molecular frequency of the water molecule is the convergence frequency of these three, $6 \cdot 1326 \times 10^{12}$, we have also to take into account the intra-molecular frequency of the OH group. Now in the molecule H—O—H there are two frequencies active for oxygen and one for hydrogen, and thus there are two possible intra-molecular frequencies for the OH group, depending on which oxygen frequency is concerned. In addition, therefore, to the three atomic frequency series the molecule will also show intra-molecular or OH series. Each of these intra-molecular frequencies is the convergence frequency of two atomic series, and will be associated with subsidiary frequencies to form a band group. If I be the intra-molecular frequency, the only subsidiary frequencies associated with I will be given by $I \pm nA_1$ and $I \pm nA_2$, where A_1 and A_2 are the two atomic frequency series converging at I, and $n = 1, 2, 3$, &c., with an upper limit defined by the critical value. There will also exist two series of frequencies, I_1 $2I_1$ $3I_1$, &c., and I_2, $2I_2$, $3I_2$, &c., each associated with its subsidiary frequencies. These intra-molecular frequencies will converge at the true molecular frequency.

In the case of the water molecule there are two intra-molecular frequency series, namely $7 \cdot 5 \times 10^{11}$, which is the convergence frequency of the atomic frequencies, $1 \cdot 0635 \times 10^{11}$ and $2 \cdot 1159 \times 10^{11}$, and $1 \cdot 7301 \times 10^{12}$, which is the convergence frequency of the atomic frequencies $2 \cdot 1159 \times 10^{11}$ and $2 \cdot 4531 \times 10^{11}$.

When the subsidiary frequencies associated with the given true molecular frequency are considered, instead of only the subsidiary frequencies $M \pm nA$, there will exist as subsidiary frequencies $M \pm nI \pm mA$, where n and $m = 0, 1, 2$, &c., each having its own critical limit, I is one or other of the intra-molecular frequencies, and A stands for the two atomic frequencies which have I as their convergence frequency. This will obviously result in the whole group of subsidiary frequencies associated with the given molecular frequency being divided into sub-groups. The central sub-group will be given by $n = 0$, and the central lines of the sub-groups will be given by $m = 0$. This is exactly the structure that has been observed in the case of water and sulphur dioxide, both of which molecules have three atomic frequencies. Perhaps the most striking experimental confirmation is to be found in the fact that in any one sub-group the subsidiary frequencies are formed from only those atomic frequencies which have the intra-molecular frequency as their convergence frequency. None of the previous theories are able to account for this selective association of the atomic frequencies.

With still more complex molecules it becomes necessary to accept the exist-ence of small atomic groupings within the principal groupings. Without going into the resulting system in detail it may be stated that this will result in the sub-division of the sub-groups into smaller sub-groups. It is of considerable interest to note that the phosphorescence and absorption bands shown by certain uranyl compounds exhibit this type of structure.[22]

Before entering further into the quantitative relationships one point arising from the foregoing discussion of energy quanta may be mentioned. It has been shown that in the water molecule the oxygen atom exhibits two characteristic frequencies and the hydrogen atom one, whilst in sulphur dioxide the oxygen atoms exhibit one and the sulphur atom two characteristic frequencies. It is difficult to avoid the conclusion that the characteristic atomic frequency is the basis of the valency of that atom. Thus a univalent atom may be one for which there is only one possible shift of its electrons, with a bivalent atom there may be two possible shifts, and so on. From this it would also follow that the numerical size of the elementary quantum associated with the atoms of an element determines the position of that element in the series of electro-positivity. Obviously the larger the elementary quantum associated with an atom the greater will be the energy given out when that atom enters into combination. Further, when a multivalent atom enters into successive com-bination with atoms of a given univalent element, its largest elementary quantum will be concerned when it combines with the first atom. This will be followed by the next largest, and so on. This will mean that the 'strength' of its different valencies will be different, and the individual bonds with the various atoms of the univalent element will require different amounts of energy to resolve them.

There now remains to be considered the origin of chemical reaction. The relationships between the frequencies shown by a molecule and its component atoms have been discussed, but nothing has been said as to why atoms combine together and why certain specific properties are associated with the molecules produced. It would seem that the key to this problem is to be found in the electromagnetic force fields of the atoms. It is evident that, according to the modern view of atomic structure, a central positive nucleus with negative electrons in rotation round it, each atom must form the centre of an electro-magnetic field of force. These force fields were first dealt with by Humphreys,[23] who showed that they are capable of giving a quantitative explanation of the Zeeman effect and also of the pressure-shift of spectrum lines. He deduced the fact that two atoms will attract one another when they approach in such a way that the direction of their electronic motions is the same, and will repel one another when their electronic motions are in opposite directions. Each atom therefore possesses two faces, and when one pair of faces comes together they repel one another, and when the other pair comes together they attract one another. In other words, an atom forms the centre of an electromagnetic field of force, the opposite poles of which are localised in two opposite faces of the atom.

Let it be supposed that two atoms of different elements are brought together in such a way that their mutually attracting faces come together. They will at once tend to form an addition complex which can lose energy in the manner already described. The two atoms radiate equal amounts of energy as a whole number of elementary quanta whereby the resulting molecule becomes endowed with the frequency based on the least common multiple of the atomic frequencies. This molecule is now rendered a stable entity, and can only be resolved into its atoms by absorbing an amount of energy equal to that lost in its formation. This quantity of energy consists of a definite number of true molecular quanta.

As will be noticed, however, in this suggestion, that the reactivity of atoms for one another is due to the attraction of their respective force fields, and that their combination consists in their joint loss of equal amounts of energy, no account has been taken of the other faces of these combining atoms. Whereas the combination of the atoms produces a molecule characterised by a specific energy quantum, it is not possible to consider that the force fields due to the external atomic faces can exist without influence on one another. These external force lines must condense to form an external molecular force field, and

in this process energy must be evolved. It was not possible previously to determine the amount of energy lost by each molecule in this process, but the theory of elementary and molecular quanta put forward now enables this to be done with accuracy. It was shown above that a freshly synthesised molecule is characterised by a definite molecular quantum, and hence by a specified frequency in the short wave infra-red, which has been called the infra-red fundamental frequency. When a freshly synthesised molecule loses energy as a whole it must do so in quanta at the infra-red fundamental, and thus it would follow that, when the external force fields of the component atoms of a freshly synthesised molecule condense together to form the molecular force field, the system loses energy in quanta at the infra-red fundamental of that molecule. Clearly, the molecule itself will not suffer any loss of individuality as far as its characteristic frequencies are concerned. None of the deductions from the conception of elementary and molecular quanta made above will be contradicted, and the only change accompanying the formation of the molecular force field will be the endowment of the system with an additional molecular frequency which is an exact multiple of the infra-red fundamental. Let it be supposed that in the formation of its molecular force field a given molecule loses one molecular quantum at the infra-red fundamental. If the freshly synthesised molecule were allowed to absorb one quantum at the infra-red fundamental it would become endowed with certain properties. If now it is required to bring the molecule with its molecular force field established by the loss of one quantum into this physical state it will be necessary to supply it with energy equal to two energy quanta at the infra-red fundamental. There can be no reason against the molecule and its force field absorbing both these quanta simultaneously, and therefore it may be concluded that the system of molecule and force field becomes endowed with a new and additional frequency which is exactly twice the infra-red fundamental. Similarly, it follows that, if the force-field condensation proceeds to the extent defined by the loss of two molecular quanta at the infra-red fundamental, the molecule and its force field will be endowed with a new and additional frequency which is exactly three times the infra-red fundamental. Generally, if the infra-red fundamental of a freshly synthesised molecule be denoted by M, and if in the formation of the force field x quanta are evolved at that frequency, the system will be characterised by two molecular frequencies, namely M and $M(x+1)$. Since the external atomic fields are bound to undergo a certain amount of condensation, it is evident that the molecule must exist in one of a number of possible phases, each molecular phase being defined by the number of molecular quanta lost in the force-field condensation and characterised by a specific frequency which is an exact multiple of the infra-red fundamental.

The initial assumption was made that the chemical reactivity of atoms is due to the attraction exerted by their electromagnetic fields. As the result of this attraction the atoms form an addition complex which constitutes the first stage in the reaction between them, the second stage being the joint loss of equal amounts of energy by all the atoms whereby the freshly synthesised molecule is formed with its infra-red fundamental. Similarly the reactivity of molecules will be a function of their force fields, and the first stage of any reaction between two or more molecules will be the formation of the addition complex due to the attraction between their respective force fields. It follows, therefore, that the reactivity of a molecule will depend on the molecular phase in which it exists, and, further, the greater the extent to which the condensation in the molecular force field has taken place the smaller will be the reactivity. The phase in which a molecule exists is governed by the nature of the external force fields of its atoms. The more equally balanced these are the greater will be the condensation that takes place between them. The particular phase assumed by a molecule will depend on the external conditions, such as temperature, nature of solvent, &c.

The experimental evidence in favour of the existence of these molecular phases is exceedingly strong. It is not possible to give here a detailed account of this evidence, but two or three of the most striking observations may be mentioned. For instance, it is common knowledge that substances which possess very small reactivity are characterised by molecular frequencies which are

large multiples of their infra-red fundamentals and lie in the extreme ultra-violet. The converse of this is also true that substances with measurable reactivity are characterised by frequencies which relatively are smaller multiples of the infra-red fundamental. Again, it is possible by changing the external conditions of temperature or solvent to change the molecular frequency exhibited by a given substance, and in some cases as many as six different molecular frequencies have been brought into play, each of which is an exact multiple of the infra-red fundamental of that substance. This means that six different molecular phases of the same compound have been observed. Then, again, it has been proved that a particular frequency is associated with a specific chemical reactivity, or, in other words, a particular molecular phase is endowed with its own reactivity.

An interesting point arises at once when the force fields of free elementary atoms are considered. It has been assumed that in a molecular force field the force lines due to the external faces of its atoms undergo condensation to form a condensed molecular force field. It is manifest if an atom consist of a central positive nucleus with a single plane ring of electrons that the force lines at the two faces of that atom will be exactly equal and opposite, that condensation must occur to form an atomic field of force, and that this condensation will be very great with the evolution of a large number of atomic quanta. Such an atom will under ordinary circumstances possess little or no power of attracting other atoms, and hence will have no measurable chemical reactivity. It is possible that the atoms of the inactive gases, helium, neon, &c., are of this type. On the other hand, if there exist more than one plane orbit of electrons, a condition of asymmetry will be set up in the atomic force field, with the result that the complete condensation to form a non-reactive atomic field is no longer possible. It does not seem improbable that in the various types of asymmetry likely to exist the explanation is to be found of the various properties of elementary molecules which are familiar to the chemist. The extreme conditions resulting from this asymmetry would be (1) the non-reactive diatomic molecule such as H_2, N_n, &c.; (2) the highly reactive mon-atomic molecule such as Na, K, &c.; (3) the highly reactive diatomic molecule such as F_2; (4) the non-reactive polyatomic molecule such as those of carbon. Apart from this possibility, which need not now be discussed, it is necessary to take into account the fact that at any rate in the case of elementary molecules containing more than two atoms the different molecular phases may be capable of separate existence. Smits has put forward the theory that the different allotropic modifications of an element are equilibrium mixtures of different molecular species of that element. Thus the various allotropic modifications of sulphur are equilibrium mixtures of some or all of four molecular species of sulphur known as S_λ, S_μ, S_π, and S_φ. There seems little doubt that what Smits calls molecular species are in reality four different molecular phases of sulphur, which differ in their energy content by a definite number of quanta at the infra-red fundamental of sulphur. It is of considerable interest to note that each of the four varieties of the sulphur molecule exhibits a different molecular frequency in the visible or ultra-violet region, and that they therefore conform to the definition of molecular phases.

The molecular phase hypothesis throws a considerable light on the mechanism of chemical reaction, and enables accurate calculations to be made of the complete energy changes which are involved in any reaction. In the first place, the calculation may be made of the total energy which is evolved during the combination of elementary atoms to form molecules which are in radiant equilibrium with their surroundings.

Let the case be considered of the combination of atoms of different elements, and further let the characteristic frequencies of these atoms be 9×10^{10}, 1.2×10^{11}, 1.5×10^{11}, and 2.1×10^{11} respectively. The least common multiple of these four frequencies is 1.26×10^{13}, and this therefore will be the true molecular frequency of the resulting molecule. On the assumption made in the preceding paper that an equal amount of energy is contributed for each atomic frequency, the smallest equal amount evolved for each atomic frequency is $1.26 \times 6.56 \times 10^{-14}$ or 8.2656×10^{-14} ergs. The total quantity of energy evolved therefore in the actual formation of each molecule will be $4 \times 8.2656 \times 10^{-14}$ or 3.30624×10^{-13} ergs,

which will result in the establishment of the infra-red fundamental 5.04×10^{13}. Since one quantum at this frequency equals the sum total of energy evolved, the absorption of one energy quantum at this frequency will result in the molecule just being resolved back again into its atoms.

The next stage in the process will be the formation of the molecular force field, and let this be accompanied by the loss of 20 quanta at the infra-red fundamental 5.04×10^{13}. As shown above, the molecular system will now be endowed with an additional characteristic frequency, $5.04 \times 21 \times 10^{13}$ or 1.0584×10^{15}, which lies in the ultra-violet region of the spectrum. The energy lost by each molecule during the condensation of its force field will be $5.04 \times 20 \times 6.56 \times 10^{-14}$ or 6.61248×10^{-12} ergs. The total energy therefore evolved in the two processes is the sum of 3.30624×10^{-13} ergs evolved in the combination of the atoms and 6.61248×10^{-12} ergs evolved during the condensation to form the molecular force field, which amounts to 6.943104×10^{-12} ergs. This amount of energy, however, is equal to one quantum at the frequency 1.0584×10^{15}, which is characteristic of the molecular phase. As this is obviously true whatever may have been the number of quanta at the infra-red fundamental lost during the formation of the molecular force field, the general conclusion is reached that one energy quantum measured at the largest frequency characteristic of the molecule is just sufficient to resolve that molecule into its atoms. This is a general conclusion which includes Einstein's photochemical law.

The values taken above of atomic frequencies,* infra-red fundamental, and molecular phase frequency closely approximate to those observed with many compounds. It will be seen that the amount of energy evolved in the complete process may be very large, and for a gram-molecule amounts in the above instance to about 102,320 calories. It must, of course, be remembered that in any reaction the observed heat evolved is less than the total amount evolved in the formation of the molecular systems of the products by the amount necessary to resolve the initial substance or substances into atoms.

An important deduction from this molecular phase theory may be made as regards the energy changes involved in chemical reaction. It is obvious that in any reaction in which the first stage is the resolution of the molecule into its atoms the energy necessary for this first stage can at once be found from the frequency of the phase in which that molecule exists. Unfortunately, there does not seem to be known at present a single instance of a simple reaction in which the molecular phase frequencies have been accurately measured, both for the original substance and the products, and consequently it is not possible at the present time accurately to calculate the net change of energy observed in any reaction. On the other hand, in the vast majority of chemical reactions the reacting molecules are not resolved into their atoms in the first stage of the process. It has been shown in a number of cases that it is only necessary to bring the molecules into a particular phase in order to enable them to enter into the desired reaction. A very typical example of the difference in reactivity shown by the different molecular phases of the same molecule is afforded by benzaldehyde. In alcoholic solution this substance exhibits two molecular frequencies in the ultra-violet, and therefore two molecular phases co-exist. It is well known that in alcoholic solution benzaldehyde is readily oxidised by gaseous oxygen to benzoic acid, and that it is not converted to benzaldehydesulphonic acid when sulphuric acid is added to the

* In the example given simple numbers have been used for the atomic frequencies in order to avoid complexity in calculation. It is perhaps worth while to point out here that there are certain indications that the fundamental frequencies of the atoms of different elements are possibly connected by simple arithmetical relations. A sufficient number of these atomic frequencies has not yet been computed, owing to the dearth of accurate measurements of the subsidiary frequencies of simple molecules, to justify any conclusions being made. It is of some interest, however, to note that in sulphur dioxide the oxygen frequency 2.4531×10^{11} is almost exactly three times the sulphur frequency 8.19×10^{10}, and that in the case of the water molecule the atomic frequency 2.1159×10^{11} is very nearly twice the atomic frequency 1.0635×10^{11}.

solution. The reaction with oxygen, therefore, is characteristic of one or both of the two molecular phases present in alcoholic solution. If benzaldehyde is dissolved in concentrated sulphuric acid it exhibits two new molecular frequencies, one in the visible and the other in the ultra-violet region. Two further molecular phases, therefore, exist in solution in sulphuric acid. In this case the benzaldehyde is no longer oxidised by oxygen, but is readily converted to the sulphonic acid.

Now the question arises as to the amount of energy necessary to convert one molecular phase into another and the mechanism whereby this energy is supplied. The amount of energy required per molecule is readily calculated, and is equal to one or more quanta measured at the infra-red fundamental of that molecule. If the frequency characteristic of the first phase is x times the infra-red fundamental and the required phase is characterised by a frequency which is y times the infra-red fundamental, then the energy required for each molecule is $x-y$ quanta at the infra-red fundamental. Obviously the molecular system can absorb this energy when exposed to radiation of a frequency equal to its infra-red fundamental, or, as explained above, it may absorb it at any of the frequencies characteristic of its component atoms. Lastly, the molecule may absorb one quantum at its characteristic phase frequency, and under ordinary circumstances this energy will again be entirely radiated as quanta at a lower phase frequency, the infra-red fundamental, or the atomic frequencies. If there is present a substance capable of reacting with a less condensed phase, then the molecule is converted into that phase and reacts, the balance of energy being evolved as infra-red radiation. The essential point is that the necessary amount of energy to change the molecular phase is $x-y$ quanta at the infra-red fundamental, and that when one quantum is absorbed at the phase frequency the excess energy over and above that required is radiated. The change of molecules from one phase to another under the influence of light is readily enough shown experimentally, but it is necessary to stabilise the second phase in some way, since otherwise it returns instantaneously to the first phase. An interesting example is furnished by trinitrobenzene, an alcoholic solution of which contains a molecular phase characterised by a frequency in the ultra-violet. A piperidine solution contains a molecular phase of trinitrobenzene which is characterised by a frequency in the blue and the solution is deep red in colour. This second phase, therefore, is favoured by piperidine. If to an alcoholic solution of trinitrobenzene a small quantity of piperidine is added, not more than one molecule of piperidine to 10 molecules of trinitrobenzene, the solution remains perfectly colourless. On exposure to light of the frequency characteristic of the phase in alcohol the solution turns red, owing to the formation of the second molecular phase, and the solution slowly becomes colourless again when placed in the dark.

There is no need to enter into a discussion of the application in detail of this theory to the quantitative relations involved in the energy changes of chemical reaction. It is obvious that the theory renders possible the calculation of the complete energy changes, and this aspect of the phenomena may be left on one side. From the point of view of absorption spectra the essential fact is that the theory leads to the conclusion that a molecule must exist in one of a number of possible phases, each of which is characterised by its own absorption band in the visible or ultra-violet region of the spectrum. It has been proved that a molecule can be brought from one phase to another by the gain of a whole number of fundamental infra-red quanta and that this can be brought about by exposure to radiant energy at a frequency characteristic of the molecule. Reference has already been made to the fact that it is possible to change the phase in which a molecule exists by the use of a suitable solvent, and indeed it is to this effect of a solvent that the variation in the absorption spectra of many compounds is due.

In order to understand this effect of a solvent it is necessary to consider the condensation of the molecular force fields a little more in detail. From what has already been said it is clear that this condensation will proceed to the farthest possible extent. In the case of a molecule in which the external force fields of the atoms are well balanced the condensation will proceed far with the establishment of a highly condensed field characterised by an absorption band in the extreme ultra-violet. On the other hand, if the external force fields

are not balanced the condensation will not be so great, and a balance of force lines of one type will remain uncompensated. If this balance be removed in some way then there will be nothing to prevent the condensation from proceeding further with the escape of more fundamental infra-red quanta and the formation of a more highly condensed phase. It may be noted in passing that an uncompensated balance of force lines remaining after the condensation of the force field has taken place is in all probability the origin of what is known to chemists as residual affinity. Let the case be considered of a molecule which possesses residual affinity of an acid type, and let this molecule be brought into the neighbourhood of another molecule which possesses a force field basic in type. The two will together form a complex, and since the residual affinity of the first is now compensated there is no reason why its force field should not undergo further condensation with the evolution of one or more fundamental infra-red quanta. Provided that the fundamental infra-red frequencies of the two molecules are similar, these quanta may be absorbed by the second molecule, which is thereby converted into a less condensed phase. The similarity of the infra-red fundamental frequencies necessary for this transference of energy quanta is very probable, because, in the first place, observation shows that the fundamental infra-red frequencies of at any rate organic compounds are very near together. In the second place, it has been found that when two substances with not very different fundamental infra-red frequencies form a complex, this complex becomes endowed with a new fundamental infra-red frequency of its own which lies between those of its components. This is of material importance, not only because it shows that the complex is a definite entity, but also because the mechanism for transference of fundamental infra-red quanta from one component to the other is perfect. It would seem that in this process is to be found the explanation of the change of phase which frequently takes place when organic compounds pass into solution.

It is not possible to avoid mentioning the bearing of this upon the whole problem of catalysis. It has already been stated that each phase of a given molecule is endowed with its own reactivity, and that in order to cause a molecule to enter into a specific reaction it is necessary to bring it into the proper phase. This change of phase may be produced by the action of light, in which case the reaction is called a photochemical one. On the other hand, the change in phase may be produced by a material substance which is called a catalyst. The substance is a catalyst because it increases the velocity of the particular reaction, owing to the fact that it brings more molecules into the reactive phase than would otherwise exist in that phase. Not the least interesting application of the present theory is to the phenomenon of catalysis, a phenomenon which has not hitherto found a completely satisfactory explanation.

After what has been stated of the existence of molecular phases, each with its own characteristic frequency in the visible or ultra-violet, a frequency which is an exact multiple of the infra-red fundamental, it is perhaps scarcely necessary to discuss many of the observations of the absorption spectra of organic compounds, since the application of the theory is obvious. In order to illustrate this application, however, some of the observations recorded in the earlier pages of this report may be considered, and the case of ethyl acetoacetate and its derivatives may be selected first. It was shown quite clearly that neither the original theory of tautomeric equilibrium nor the Hantzsch six-membered 'ring' formula can explain the absorption band shown by the sodium salt. The absorption band is due to the fact that the substance in the presence of a basic solvent is changed into a phase the characteristic absorption band of which lies in the ultra-violet region. This alteration of phase is characteristic of the ketonic form, since the disubstituted compound, ethyl dimethylacetoacetate, shows the same band when dissolved in a basic solvent. It is noteworthy that exactly the same bands are shown when these compounds are dissolved in piperidine.

The reason why the two derivatives, ethyl β-ethoxycrotonate and ethyl dimethylacetoacetate, show only general absorption in alcoholic solution is because they exist in a phase the characteristic band of which lies in the extreme

ultra-violet region beyond that reached with a quartz spectrograph working in air. Lastly, the incipient or very shallow absorption band shown by ethyl β-ethoxycrotonate in the presence of acid is due to the fact that relatively few molecules are brought into a less condensed phase by the action of the acid.

It must be clearly understood that the statement that a compound only shows general absorption is very misleading, because it only means that no absorption band is exhibited by that compound between the spectral limits of 7000 and 2100 Angströms. Such substances will certainly be found to exhibit selective absorption when investigations are made in the very extreme ultra-violet. There is a very fertile field of research in this direction by the use of a vacuum spectrograph with a fluorite prism or a grating, in order to obviate the absorption due to air and quartz. Some preliminary investigations have already been made by Stark, who found evidences of selective absorption in this region by some of the so-called diactinic substances.

Again, the explanation of the results recorded in the examination of the aromatic aminoaldehydes and aminoketones is very simple. These compounds show one absorption band in alcoholic solution, a second in the presence of a trace of acid, and a third in the presence of a great mass of acid, the frequency of the second being the smallest and that of the third being the greatest. The molecules exist in three different phases under the three conditions. Similarly the variety of absorption bands which Hantzsch found certain substances to exhibit in different solvents is due to a variety of phases of the same molecule. Thus diphenylvioluric acid can be brought into several different phases by alkali according to the chemical strength of LiOH, NaOH, KOH, RbOH, CsOH. Further, a considerable variety of phases of trinitrobenzene, picric acid and its ether trinitroanisole, can be produced by the use of solvents of different basicity, such as water, alcohol, pyridine, piperidine, dimethylaniline, and alcoholic sodium ethoxide.[24] The case of trinitrobenzene and also of trinitro-toluene is interesting, for it is possible with these two compounds not only to obtain them in highly coloured molecular phases by solution in basic solvents, but also to prepare these phases in the pure state. The coloured liquid phase of trinitrotoluene is well known to those engaged in the manu facture of this compound. The corresponding phase of trinitrobenzene can be obtained by dissolving the compound in piperidine. On pouring this solution into excess of hydrochloric acid the trinitrobenzene is precipitated as a red solid, and after drying the colourless form may be dissolved in ether or benzene, leaving the red form. A solution of this in alcohol shows the same absorption band as does the piperidine solution of trinitrobenzene.

There indeed is little doubt that the existence of a compound in two or more forms, as is frequently the case in organic chemistry, means the isolation of two or more different molecular phases of the same compound. One of the most interesting cases of the preparation of a molecular phase less condensed than the ordinary phase is the so-called *aci*-ethers of the nitrophenols.[25] These compounds on the basis of the quinonoid theory were considered to have the quinonoid formula typified by

$$O= \langle\!\!\langle\ \rangle\!\!\rangle =N\!\!<^{OCH_3}_{O}$$
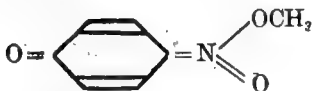
but the only evidence on which this formula was based was the instability of the compounds and their visible colour. They are extraordinarily unstable and change at once under the influence of certain solvents into the normal forms. In the light of present-day knowledge there is not the slightest doubt that they are simply less condensed phases of the nitrophenol ethers, having the usually accepted formulæ.

Reference was made previously to the quinonoid explanation of the highly coloured hydrochloride of dimethylaminoazobenzene. This again is another example of the conversion of a weak base into a less condensed phase by the addition of acid such as occurs with the aminoaldehydes and aminoketones. It was pointed out above that the quinonoid explanation fails because a similar

change in absorption takes place with benzeneazophenyltrimethylammonium iodide in the presence of acid, although the change is less obvious to the eye.[26] The most serious criticism of the quinonoid explanation is to be found in the fact that in concentrated acid the colour is not so intense as in dilute acid, for it hardly seems scientific to state that a particular configuration is favoured by acid and then to have to agree to a change from that configuration to another when more acid is added. Here again as with the aminoaldehydes three molecular phases exist, one in alcohol, one in dilute acid, and one in strong acid, the primary structure of the molecule being the same in all three. A very analogous case is pararosaniline, which with one equivalent of acid gives a very marked colour, but in the presence of excess of acid the colour and absorption are different. Three phases again are formed, one in alcohol, one in dilute acid, and one in concentrated acid.

In all probability the above instances are sufficient to indicate the application of the theory of molecular phases to absorption spectra. In conclusion it may be claimed for the theory that it attempts to co-ordinate on a definite physical basis all absorption spectra observations over the whole spectrum between the extreme limits of wave-length 1000μ and $0\cdot1\mu$, and that these attempts seem to meet with considerable success.

References.

1. Hartley, Dobbie, and Lauder, ' Trans.,' **81**, 929 (1902).
2. Hartley and Dobbie, ' Trans.,' **75**, 640 (1899).
3. Hartley, Dobbie, and Paliatseas, ' Trans.,' **77**, 839 (1900).
4. Baly and Desch, ' Trans.,' **85**, 1029 (1904) ; **87**, 766 (1905).
5. Baly and Collie, ' Trans.,' **87**, 1332 (1905).
6. Baly and Stewart, ' Trans.,' **89**, 502 (1906).
7. Baly, Marsden, and Stewart, ' Trans.,' **89**, 966 (1906).
8. Lowry and Desch, ' Trans.,' **95**, 807 (1909).
9. Hantzsch, ' Ber.,' **43**, 3049 (1910) ; **44**, 1771 (1911) ; **45**, 559 (1912) ; **48**, 772 (1915).
10. Hantzsch and Colleagues, ' Ann.,' **384**, 135 (1911) ; Ber., **41**, 1204 (1908) ; **42**, 68, 889, 1216, 2119, 2129 (1909) ; **43**, 45, 68, 106, 1651, 1662, 1685, 2129, 2512, 3049 (1910) ; **44**, 1771, 1783 (1911) ; **45**, 85, 553, 559, 3011 (1912) ; **46**, 1537, 3570 (1913) ; **48**, 158, 167, 772, 797, 1407 (1915) ; **49**, 213, 226, 511 (1916) ; **50**, 1413, 1422 (1917) ; **52**, 493, 509, 1535, 1544 (1919) ; ' Zeit. phys. Chem., **84**, 321 ; **86**, 624 (1913).
11. Purvis, ' Trans.,' **97**, 692 (1910) ; Baly and Tryhorn, 'Phil. Mag.,' **31**, 417 (1916).
12. Baly and Marsden, ' Trans.,' **93**, 2108 (1908).
13. Baly and Rice, ' Trans.,' **101**, 1475 (1912).
14. Baly, ' Phil. Mag.,' **2**, 632 (1914) ; ' Astrophys. J.,' **742**, 4 (1915).
15. Baly and Tryhorn, ' Phil. Mag.,' **31**, 417 (1916).
16. Garrett, ' Phil. Mag.,' **31**, 505 (1916) ; Baly and Garrett, ibid., **31**, 512 (1916).
17. Baly, ' Phil. Mag.,' **39**, 565 (1920).
18. Sleator, ' Astrophys. J.,' **48**, 125 (1918).
19. Baly, ' Phil. Mag.,' **40**, 1, 15 (1920).
20. Bjerrum, ' Nernst Festschrift,' page 90 (1912).
21. Krüger, ' Ann. der Phys.,' **50**, 346 ; **51**, 450 (1916).
22. Nichols and Merritt, ' Phys. Rev.,' **6**, 630 (1915) ; **9**, 113 (1917).
23. Humphreys, ' Astrophys. J.,' **33**, 233 (1906).
24. Baly and Rice, ' Trans.,' **103**, 2085 (1913).
25. Hantzsch and Gorke, ' Ber.,' **39**, 1073 (1906).
26. Baly and Hampson, ' Trans.,' **107**, 248 (1915).

Appendix.

LIST OF SUBSTANCES OF WHICH THE ABSORPTION SPECTRA HAVE BEEN EXAMINED IN THE ULTRA-VIOLET AND VISIBLE REGIONS SINCE THE PUBLICATION OF THE LAST REPORT IN 1916.

A

Acetic acid and salts.   Hantzsch.   ' Ber.,' **50**, 1422 (1917).
Acetone.   Lifschitz.   ' Zeit. wiss. Phot.,' **16**, 140 (1916).
Acetylenedicarboxylic acid.   Macbeth and Stewart.   ' Trans.,' **111**, 829 (1917).
Alizarin.   Meek.   ' Trans.,' **111**, 969 (1917).
Alizarin-blue.   Meek.   ' Trans.,' **111**, 969 (1917).
Alizarin-Bordeaux.   Meek.   'Trans.,' **111**, 969 (1917).
Alizarin-cyanine.   Meek.   ' Trans.,' **111**, 969 (1917).
Aminoazobenzene.   Ghosh and Watson.   ' Trans.,' **111**, 815 (1917).
Anthragallol.   Meek.   ' Trans.,' **111**, 969 (1917).
Aurin.   Ghosh and Watson.   ' Trans.,' **111**, 815 (1917).

B

Behenolic acid.   Macbeth and Stewart.   ' Trans.,' **111**, 829 (1917).
Benzeneazoanthranol.   Sircar.   ' Trans.,' **109**, 757 (1916).
Benzene-1-azo-4-anthrol.   Sircar.   ' Trans.,' **109**, 757 (1916).
Benzeneazocatechol.   Ghosh and Watson.   ' Trans.,' **111**, 815 (1917).
Benzeneazo-1.5-dihydroxynaphthalene.   Ghosh and Watson. ' Trans.,'   **111**,   815 (1917).
Benzene-1-azo-4-naphthol.   Sircar.   ' Trans.,' **109**, 757 (1916).
Benzeneazo-α-naphthol.   Ghosh and Watson.   ' Trans.,' **111**, 815 (1917).
Benzeneazo-β-naphthol.   Ghosh and Watson.   ' Trans.,' **111**, 815 (1917).
Benzeneazo-β-naphthylamine.   Ghosh and Watson.   ' Trans.,' **111**, 815 (1917).
Benzeneazophenol.   Ghosh and Watson.   ' Trans.,' **111**, 815 (1917).
        ,,   Sircar.   ' Trans.,' **109**, 757 (1916).
Benzeneazopyrogallol.   Ghosh and Watson.   ' Trans.,' **111**, 815 (1917).
Benzeneazoquinol.   Ghosh and Watson.   ' Trans.,' **111**, 815 (1917).
Benzeneazoresorcinol.   Ghosh and Watson.   ' Trans.,' **111**, 815 (1917).
Benzene-1-azo-1'.2'.3'.4'-tetrahydro-4-naphthol.   Sircar.   ' Trans.,' **109**, 757 (1916).
Benzoic acid.   Ley.   ' Zeit. wiss. Phot.,' **18**, 178 (1918).
p-Bromobenzene-1-azo-4-anthrol.   Sircar.   ' Trans.,' **109**, 757 (1916).
p-Bromobenzene-1-azo-4-naphthol.   Sircar.   ' Trans.,' **109**, 757 (1916).
p-Bromobenzeneazophenol.   Sircar.   ' Trans.,' **109**, 757 (1916).
p-Bromobenzene-1-azo-1'.2'.3'.4'-tetrahydro-4-naphthol.   Sircar.   ' Trans.,' **109**, 757 (1916).
Bromodinitrotriphenylmethane.   Hantzsch and Hein.   ' Ber.,' **52**, 493 (1919).

C

Chloranil.   Lifschitz.   ' Ber.,' **49**, 2050 (1916).
Chrysoidine.   Ghosh and Watson.   ' Trans.,' **111**, 815 (1917).
Cinnamic acid.   Ley.   ' Ber.,' **51**, 1808 (1918).
        ,,   ,,   Ley.   ' Zeit. wiss. Phot.,' **18**, 178 (1918).
        ,,   ,,   Macbeth and Stewart.   ' Trans.,' **111**, 829 (1917).
        ,,   ,,   Stobbe.   ' Ber.,' **52**, 1021 (1919).
Cinnamylideneacetic acid.   Macbeth and Stewart.   ' Trans.,' **111**, 829 (1917).
Cobalt acetate.   Ley and Ficken.   ' Ber.,' **50**, 1123 (1917).
Cobalt picolate.   Ley and Ficken.   ' Ber.,' **50**, 1123 (1917).
Crystal violet.   Hantzsch.   ' Ber.,' **52**, 509 (1919).
        ,,   ,,   Kehrmann and Sandoz.   ' Ber.,' **51**, 915 (1918).
        ,,   ,,   derivatives.   Kehrmann and Sandoz.   ' Ber.,' **51**, 915 (1918).
        ,,   ,,   nitrile of.   Lifschitz.   ' Ber.,' **52**, 1919 (1919).
Cyanoacridine and Cyanopyronin Dye-stuffs.   Kehrmann and Sandoz.   ' Ber.,' **53**, 63 (1920).

D

4.4'-Diaminoazobenzene. Ghosh and Watson. 'Trans.,' **111**, 815 (1917).
Dibenzyl. Ley. 'Zeit. wiss. Phot.,' **18**, 178 (1918).
Diethylthiazin bromide. Kehrmann. 'Ber.,' **49**, 2831 (1916).
1.4-Dihydroxyanthraquinone. Meek. 'Trans.,' **111**, 969 (1917).
3.4-Dihydroxymalachite-green. Ghosh and Watson. 'Trans.,' **111**, 815 (1917).
Diiodoacetylene. Macbeth and Stewart. 'Trans.,' **111**, 829 (1917).
Diiodoethylene. Macbeth and Stewart. 'Trans.,' **111**, 829 (1917).
p.p'-Dimethoxyfuchsonedimethylimonium chloride. Hantzsch. 'Ber.,' **52**, 509 (1916).
Dimethyl sulphide. Hantzsch. 'Ber.,' **52**, 1544 (1919).
Dimethylaminoazobenzene. Hantzsch. 'Ber.,' **52**, 509 (1919).
Dimethylaniline. Ley. 'Zeit. wiss. Phot.,' **18**, 178 (1918).
Dimethyldiacetylene. Macbeth and Stewart. 'Trans.,' **111**, 829 (1917).
Dimethylpyrone. Hantzsch. 'Ber.,' **52**, 1535 (1919).
Dimethylthiazin perchlorate. Kehrmann. 'Ber.,' **49**, 2831 (1916).
Dimethyl-o-toluidine. Ley. 'Zeit. wiss. Phot.,' **18**, 178 (1918).
4.5-Dinitro-3-acetylaminoveratrole. Gibson, Simonsen, and Rau. 'Trans.,' **111**, 69 (1917).
5.6-Dinitro-3-acetylaminoveratrole. Gibson, Simonsen, and Rau. 'Trans.,' **111**, 69 (1917).
p.'p-Dinitrodiazoaminobenzene. Hantzsch and Hein. 'Ber.,' **52**, 493 (1919).
p.p'-Dinitrodiphenylamine. Hantzsch and Hein. 'Ber.,' **52**, 493 (1919).
s-Diphenylethane. Macbeth and Stewart. 'Trans.,' **111**, 829 (1917).
Di-cyclo-pentadiene. Stobbe and Dünnhaupt. 'Ber.,' **52**, 1436 (1919).
Dithioindigo. Lifschitz and Lourié. 'Ber.,' **50**, 897 (1917).
Doebner's violet and derivatives. Kehrmann and Sandoz. 'Ber.,' **51**, 915 (1918).

E

Elaidic acid. Macbeth and Stewart. 'Trans.,' **111**, 829 (1917).
Eosine. Miethe and Stenger. 'Zeit. wiss. Phot.,' **19**, 57 (1920).
Erucic acid. Macbeth and Stewart. 'Trans.,' **111**, 829 (1917).
β-Ethoxycinnamic acid. Ley. 'Ber.,' **51**, 1808 (1918).
α-Ethoxystyrol. Ley. 'Ber.,' **51**, 1808 (1918).
β-Ethoxystyrol. Ley. 'Ber.,' **51**, 1808 (1918).
Ethyl benzoylacetate. Ley. 'Ber.,' **51**, 1808 (1918).
Ethyl dinitrophenylmalonate. Hantzsch and Hein. 'Ber.,' **52**, 493 (1919).
Ethyl nitrate. Schaefer. 'Zeit. wiss. Phot.,' **17**, 193 (1918).
Ethyl ortho-formate. Hantzsch. 'Ber.,' **50**, 1422 (1917).
     „   „   „   salts. Hantzsch. 'Ber.,' **50**, 1422 (1917).
Ethyl phenylpropiolate. Macbeth and Stewart. 'Trans.,' **111**, 829 (1917).
Ethyl trinitrophenylmalonate. Hantzsch and Hein. 'Ber.,' **52**, 493 (1919).
Ethylbenzene. Macbeth and Stewart. 'Trans.,' **111**, 829 (1917).
Ethylene iodide. Macbeth and Stewart. 'Trans.,' **111**, 829 (1917).

F

Filter yellow. Miethe and Stenger. 'Zeit. wiss. Phot.,' **19**, 57 (1920).
Fluorescein. Miethe and Stenger. 'Zeit. wiss. Phot.,' **19**, 57 (1920).
Formic acid and salts. Hantzsch. 'Ber.,' **50**, 1422 (1917).
Fuchsine. Hantzsch. 'Ber.,' **52**, 509 (1919).
Fuchsonedimethylimonium chloride. Hantzsch. 'Ber.,' **52**, 509 (1919).
Fumaric acid. Macbeth and Stewart. 'Trans.,' **111**, 829 (1917).

H

Hexamethylbenzene. Lifschitz. 'Ber.,' **49**, 2050 (1916).
Hexamethyltriaminotriphenylcarbinol. Hantzsch. 'Ber.,' **52**, 509 (1919).
Hexatriene. Macbeth and Stewart. 'Trans.,' **111**, 829 (1917).
p-Hydroxybenzeneazo-1.3-dihydroxynaphthalene. Ghosh and Watson. 'Trans.,' **111**, 815 (1917).
p-Hydroxybenzeneazo-1.5-dihydroxynaphthalene. Ghosh and Watson. 'Trans.,' **111**, 815 (1917).

p-Hydroxybenzeneazo-*α*-naphthol. Ghosh and Watson. ' Trans.,' **111**, 815 (1917).
p-Hydroxybenzeneazo-*β*-naphthol. Ghosh and Watson. ' Trans.,' **111**, 815 (1917).
p-Hydroxybenzeneazo-*β*-naphthylamine. Ghosh and Watson. ' Trans.,' **111**, 815 (1917).
4-Hydroxymalachite-green. Ghosh and Watson. ' Trans.,' **111**, 815 (1917).

I

Imidovioluric acid. Lifschitz and Kritzmann. ' Ber.,' **50**, 1719 (1917).
    ,,   ,, salts. Lifschitz and Kritzmann. ' Ber.,' **50**, 1719 (1917).
Indigo. Lifschitz and Lourié. ' Ber.,' **50**, 897 (1917).

K

Ketothiodimethylpyrone. Hantzsch. ' Ber.,' **52**, 1535 (1919).

M

Malachite-green. Ghosh and Watson. ' Trans.,' **111**, 815 (1917).
    ,,   ,, Hantzsch. ' Ber.,' **52**, 509 (1919).
    ,,   ,, Kehrmann and Sandoz. ' Ber.,' **51**, 915 (1918).
    ,,   ,, derivatives. Kehrmann and Sandoz. ' Ber.,' **51**, 915 (1918).
Maleic acid. Macbeth and Stewart. ' Trans.,' **111**, 829 (1917).
Martius yellow. Miethe and E. Stenger. ' Zeit. wiss. Phot.,' **19**, 57 (1920).
Methoxymalachite green. Hantzsch. ' Ber.,' **52**, 509 (1919).
α-Methylcinnamic acid. Ley. ' Zeit. wiss. Phot.,' **18**, 178 (1918).
β-Methylcinnamic acid. Ley. ' Zeit. wiss. Phot.,' **18**, 178 (1918).
Methyl *o*-formate. Hantzsch. ' Ber.,' **50**, 1422 (1917).
Methylphenylthiazin bromide. Kehrmann. ' Ber.,' **49**, 2831 (1916).
α-Methylstilbene. Ley. ' Zeit. wiss. Phot.,' **18**, 178 (1918).
α-Methylstyrol. Ley. ' Zeit. wiss. Phot.,' **18**, 178 (1918).
β-Methylstyrol. Ley. ' Zeit. wiss. Phot.,' **18**, 178 (1918).
Methylthiazin perchlorate. Kehrmann. ' Ber.,' **49**, 2831 (1916).
Monochloroacetic acid and salts. Hantzsch. ' Ber.,' **50**, 1422 (1917).
Monosulphuryl indigo. Lifschitz and Lourié. ' Ber.,' **50**, 897 (1917).

N

Naphthophenazoxonium derivatives. Kehrmann and Sandoz. ' Ber.,' **51**, 923 (1918).
p-Nitrobenzeneazoanthranol. Sircar. ' Trans.,' **109**, 757 (1916).
p-Nitrobenzene-1-azo-4-anthrol. Sircar. ' Trans.,' **109**, 757 (1916).
p-Nitrobenzene-1-azo-4-naphthol. Sircar. ' Trans.,' **109**, 757 (1916).
p-Nitrobenzene-1-azo-4-naphthol-3-carboxylic acid and salts. Sircar. ' Trans.,' **109**, 757 (1916).
p-Nitrobenzeneazophenol. Sircar. ' Trans.,' **109**, 757 (1916).
p-Nitrobenzeneazosalicylic acid and salts. Sircar. ' Trans.,' **109**, 757 (1916).
p-Nitrobenzene-1-azo-1'.2'.3'.4'-tetrahydro-4-naphthol. Sircar. ' Trans.,' **109**, 757 (1916).
p-Nitrodiazoaminobenzene. Hantzsch and Hein. ' Ber.,' **52**, 493 (1919).
p-Nitrodiphenylamine. Hantzsch and Hein. ' Ber.,' **52**, 493 (1919).
p-Nitronaphthalene-1-azophenol. Sircar. ' Trans.,' **109**, 757 (1916).
4-Nitronaphthalene-1-azosalicylic acid and salts. Sircar. ' Trans.,' **109**, 757 (1916).
Nitrosodimethylaniline. Miethe and Stenger. ' Zeit. wiss. Phot.,' **19**, 57 (1920).
p-Nitrotriphenylmethane. Hantzsch and Hein. ' Ber.,' **52**, 493 (1919).

P

Parafuchsin. Kehrmann and Sandoz. ' Ber.,' **51**, 915 (1918).
Pararosaniline. Lifschitz. ' Ber.,' **52**, 1919 (1919).
cyclo-Pentadiene. Stobbe and Dünnhaupt. ' Ber.,' **52**, 1436 (1919).
Phenazoxonium derivatives. Kehrmann and Sandoz. ' Ber.,' **50**, 1667 (1917).
Phenazthionium derivatives. Kehrmann and Sandoz. ' Ber.,' **50**, 1673 (1917).
Phenol. Ley. ' Zeit. wiss. Phot.,' **18**, 178 (1918).
Phenyl benzoate. Ley. ' Zeit. wiss. Phot.,' **18**, 178 (1918).
α-Phenyl cinnamate. Ley. ' Zeit. wiss. Phot.,' **18**, 178 (1918).

Phenyl salicylate. Ley. 'Zeit. wiss. Phot.,' **18**, 178 (1918).
α-Phenyl stilbene. Ley. 'Zeit. wiss. Phot.,' **18**, 178 (1918).
Phenylacetylene. Macbeth and Stewart. 'Trans.,' **111**, 829 (1917).
Phenylethylene. Macbeth and Stewart. 'Trans.,' **111**, 829 (1917).
Phenylpropiolic acid. Macbeth and Stewart. 'Trans.,' **111**, 829 (1917).
β-Phenylpropionic acid. Macbeth and Stewart. 'Trans.,' **111**, 829 (1917).
Phenylthiazin bromide. Kehrmann. 'Ber.,' **49**, 2831 (1916).
Phorone. Lifschitz. 'Zeit. wiss. Phot.,' **16**, 140 (1916).
α-Picoline. Herrmann. 'Zeit. wiss. Phot.,' **18**, 253 (1919).
β-Picoline. Herrmann. 'Zeit. wiss. Phot.,' **18**, 253 (1919).
Picolinic acid. Ley and Ficken. 'Ber.,' **50**, 1123 (1917).
Piperidine. Herrmann. 'Zeit. wiss. Phot.,' **18**, 253 (1919).
Purpurin. Meek. 'Trans.,' **111**, 969 (1917).
Pyridine. Herrmann. 'Zeit. wiss. Phot.,' **18**, 253 (1919).
Pyridonium and Pyroxonium salts. Hantzsch. 'Ber.,' **52**, 1535, 1544 (1919).

Q

Quinizarin. Meek. 'Trans.,' **111**, 969 (1917).
Quinone. Hantzsch and Hein. 'Ber.,' **52**, 493 (1919).

R

Resaurin. Ghosh and Watson. 'Trans.,' **111**, 815 (1917).

S

Salicylic acid. Ley. 'Zeit. wiss. Phot.,' **18**, 178 (1918).
Stearic acid. Macbeth and Stewart. 'Trans.,' **111**, 829 (1917).
Stearolic acid. Macbeth and Stewart. 'Trans.,' **111**, 829 (1917).
Stilbene. Macbeth and Stewart. 'Trans.,' **111**, 829 (1917).
  „ Ley. 'Zeit. wiss. Phot.,' **18**, 178 (1918).
Styrene. Macbeth and Stewart. 'Trans.,' **111**, 829 (1917).
Styrol. Ley. 'Ber.,' **51**, 1808 (1918). 'Zeit. wiss. Phot.,' **18**, 178 (1918).
Succinic acid. Macbeth and Stewart. 'Trans.,' **111**, 829 (1917).
p-Sulphobenzene-1-azo-4-anthrol. Sircar. 'Trans.,' **109**, 757 (1916).
p-Sulphobenzene-1-azo-4-naphthol. Sircar. 'Trans.,' **109**, 757 (1916).
p-Sulphobenzeneazophenol. Sircar. 'Trans.,' **109**, 757 (1916).
p-Sulphobenzene-1-azo-1'.2'.3'.4'-tetrahydro-4-naphthol. Sircar. 'Trans.,' **109**, 757 (1916).

T

Tartrazine. Miethe and Stenger. 'Zeit. wiss. Phot.,' **19**, 57 (1920).
Tetrabenzylarsonium iodide. Hantzsch. 'Ber.,' **52**, 1544 (1919).
Tetraethylphosphonium iodide. Hantzsch. 'Ber.,' **52**, 1544 (1919).
1.2.5.8-Tetrahydroxyanthroquinone. Meek. 'Trans.,' **111**, 969 (1917).
Tetramethyldiaminofuchsone. Hantzsch. 'Ber.,' **52**, 509 (1919).
Tetramethyldiaminoquinone. Hantzsch. 'Ber.,' **52**, 509 (1919).
Tetrapropylammonium iodide. Hantzsch. 'Ber.,' **52**, 1544 (1919).
Thiazin chloride. Kehrmann. 'Ber.,' **49**, 2831 (1916).
Tolane. Macbeth and Stewart. 'Trans.,' **111**, 829 (1917).
Trialkylsulphonium haloids. Hantzsch. 'Ber.,' **52**, 1544 (1919).
Trichloroacetic acid and salts. Hantzsch. 'Ber.,' **50**, 1422 (1917).
1.2.4-Trihydroxyanthraquinone. Meek. 'Trans.,' **111**, 969 (1917).
Trihydroxyaurin. Ghosh and Watson. 'Trans.,' **111**, 815 (1917).
2.3.4-Trihydroxymalachite-green. Ghosh and Watson. 'Trans.,' **111**, 815 (1917).
Trinitrobenzene. Hantzsch and Hein. 'Ber.,' **52**, 493 (1919).
p-Trinitrotriphenyl carbinol. Hantzsch. 'Ber.,' **50**, 1413 (1917).
Trinitrotriphenylmethane. Hantzsch and Hein. 'Ber.,' **52**, 493 (1919).
Triphenylcarbinol. Hantzsch. 'Ber.,' **52**, 509 (1919).
  „   Kehrmann and Sandoz. 'Ber.,' **51**, 915 (1918).
Triphenylmethylphosphonium iodide. Hantzsch. 'Ber.,' **52**, 1544 (1919).
α-Truxillic acid. Stobbe. 'Ber.,' **52**, 1021 (1919).
β-Truxillic acid. Stobbe. 'Ber.,' **52**, 1021 (1919).

Fuel Economy. Third Report of Committee (Professor W. A. BONE *
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    Professor W. W. WATTS,* Mr. W. B. WOODHOUSE, *and* Mr. C. H.
    WORDINGHAM *) appointed for the Investigation of Fuel Economy,
    the Utilisation of Coal, and Smoke Prevention.*

Introduction.

THE Committee has held altogether six meetings since its reappointment last
year, and is investigating (*inter alia*) the following matters, namely :—

(*a*) The present official methods of arriving at coal-mining statistics (*e.g.*,
outputs of coal, etc.) in this and other coal-producing countries.

(*b*) The effect of the war upon the British coal export trade.

(*c*) The chemical constitution of coal.

(*d*) The low temperature carbonisation of coal.

(*e*) The thermal efficiencies at present attainable (i) in the carbonisation and
gasification of coal by various systems, (ii) in domestic fires and heating
appliances, (iii) in metallurgical and other furnaces, (iv) in steam raising and
power production, and (v) in regard to the generation of electric power in
public stations.

(*f*) Sources of supply of liquid fuels.

Although the Committee has made satisfactory progress with its inquiries
in certain directions during the past year, both time and opportunity have been
wanting for completing them. The present Report, therefore, is of an interim
nature, but the Committee hopes to report more fully on the above matters to
the Edinburgh Meeting next year.

Coal-mining Statistics.

The attention of the Committee having been drawn by Professor Henry
Louis to the fact that, owing to considerable variations in the modes of arriving
at the official data concerning coal outputs, etc., periodically published by
Government Departments in the various coal-producing countries, it is impossible
to regard them as being properly comparable, the Committee requested him
to prepare a Memorandum on the subject. This he subsequently did, and,
having regard to the great importance of the matter, the Committee decided
to publish the Memorandum *in extenso* as Appendix I. to this Report, in
the hope that it may lead to the desired reform being effected. In particular,
the Committee endorses Professor Louis' view concerning the importance of
summoning an International Conference for determining the precise manner in
which mineral statistics of all kinds shall be collected, tabulated, and finally
issued to the public.

* Denotes a member of the Executive Committee.

Coal Outputs and Average Pithead Prices in 1919.

According to information kindly furnished to the Committee by the Statistical Department of the Board of Trade, the total output of coal in the United Kingdom during the year 1919 has been provisionally estimated at 229,668,000 tons, and the total output per person employed (below and above ground) in the mines at 197·5 tons.

Owing to abnormal circumstances during the period of coal control, it is difficult to give strictly comparable figures for the average pithead prices of coal in the years immediately preceding and following (respectively) the war. According to official estimates supplied by the Statistical Department of the Board of Trade, the pithead prices per ton of coal raised in 1913, and in July 1919, respectively, were approximately as follows :—

	Average for 1913		On July 16, 1919	
	s.	d.	s.	d.
Labour	6	4	19	5½
Timber and Stores	1	0	3	2¼
Other Costs	0	11	1	2½
Royalties	0	5½	0	6¾
Owners' Profits	1	5	1	2
Compensation	—		0	3¼
Administration, etc.	—		0	2¼
Total	10	1½	26	0½

In the Report recently made to the Prime Minister by Messrs. Alfred Tongue & Co., Chartered Accountants, of Manchester and Glasgow, and presented to Parliament by command of His Majesty (Cmd. 555), it was estimated that the average cost per ton of coal raised in British mines during the year ending March 31, 1920, was as follows :—

	s.	d.
Wages	19	7¾
Timber and Stores	3	10
Other Costs	1	6
Royalties	0	6½
Administration	0	1
Capital Adjustments under Finance Acts	0	4
Control and Contingencies	0	2
Owners' Profits	1	2
Total	27	3¼

It would thus appear that the pithead cost of coal has been nearly trebled as the result of the war.

Coal Export Statistics.

The Statistical Department of the Board of Trade has also placed at the disposal of the Committee detailed information concerning the amounts of coal exported from the principal ports of the kingdom (a) to British possessions, and (b) to foreign countries, during each of the years 1913-1919 inclusive. In view of the importance of such statistics, the Committee has decided to publish them in tabular form as Appendix II. to this Report. The Committee is also collecting information as to average prices obtained at the principal ports for the coal exported during each of the years in question. In the light of such statistics the Committee hopes next year to be able to review the question of the effect of the war upon the coal export trade.

Chemistry of Coal.

During the year considerable progress has been made with the researches on the chemistry of coal under the direction of Professor Bone at the Fuel Laboratories at the Imperial College of Science and Technology, further details

of which will shortly be published. The Committee has also followed with close attention the work recently published (a) by Drs. Marie Stopes, R. V. Wheeler, and Rudolph Lessing upon the four macroscopically distinguishable portions of banded bituminous coal and their respective behaviour on carbonisation and oxidation, (b) by Mr. S. R. Illingworth at the Treforest School of Mines, and (c) by Mr. F. S. Sinnatt and collaborators of the Lancashire and Cheshire Coal Research Association.

Future Standards of Gas Supplies.

Since it reported its views on the above subject to the Bournemouth Meeting of the Association last year, the Committee has followed up the matter, and on February 2 last a deputation, consisting of the Chairman, Sir Robert Hadfield, Messrs. W. H. Patchell and H. James Yates, waited upon the then President of the Board of Trade (the Rt. Hon. Sir Auckland C. Geddes, K.C.B.) to lay before him the views of the Committee upon the subject, with special reference to impending legislation.

In introducing the deputation, Professor Bone called the attention of the President to (a) the Report on Gas Standards which had been made by the Fuel Research Board, (b) the conclusions thereon that had been arrived at as the result of a conference between representatives of consumers, local authorities, and gas undertakings, and (c) the announcement by the President of the Board of Trade that a Bill would shortly be introduced in Parliament to give effect to the recommendations of the Fuel Research Board.[1] He explained that the Committee had looked at the question primarily from the view of the national interests as a whole, and particularly from that of domestic and industrial gas consumers. It agreed with the Fuel Research Board that the future basis of charge to the consumer should be the actual number of thermal units supplied to him in the gas which passed through his meter, but desired that the charge should be based upon the ' ascertained net calorific value ' of the gas supplied rather than its ' declared calorific value,' as proposed by the Fuel Research Board. It also endorsed the Fuel Research Board's original recommendation that the gas should be supplied at a pressure of ' not less than two inches of water at the exit of the consumer's meter,' but expressed its disagreement with the Board's subsequent view that the pressure condition might be reduced to one of ' not less than two inches of water in any main or service pipe of two inches in diameter '; because what mattered to the consumer was the adequacy of the pressure in his own pipes rather than in the gas mains outside his premises.

It was also stated that the Committee attached great importance to the pressure being maintained as constant as possible, as well as to gas undertakings being required to pay greater attention than ever to the removal of cyanogen and sulphur impurities from the gas. Finally, it was explained that the Committee, whilst agreeing generally with the proposals in regard to the new thermal basis for the sale of gas, and to the restriction of its inert constituents, considered that its chemical composition would need some statutory regulation, and that in particular no public gas supply should be allowed to contain less than 20 per cent. of methane or more than 20 per cent. of carbon monoxide.

After Sir Robert Hadfield had endorsed the views of the Committee from the point of view of industrial consumers of gas, Mr. H. James Yates outlined his views as a maker of gas fires who had for many years given much attention to the scientific investigation of domestic heating and ventilation. He laid stress upon the importance of maintaining a constant pressure of not less than two inches water-gauge on the consumer's side of the service pipes, and that the gross calorific value of the gas supplied should not be allowed to fall below 450 B.Th.U. per cubic foot, stating that if gas undertakings supplied gas of lower calorific value a large part of the existing gas appliances would become useless.

Sir Auckland Geddes, in his reply, promised to give full consideration to the facts and opinions which they had laid before him. Also, he said that he

[1] The Bill was subsequently introduced by Sir Robert Horne in the House of Commons on May 19, 1920.

had been impressed with the physiological side of the question and with the danger of cyanogen and of too high a proportion of carbonic oxide in gas.

The 'Gas Regulation Bill,' as subsequently presented to the House of Commons on May 19 last by Sir Robert Horne (the new President of the Board of Trade), contained far-reaching new proposals concerning the public sale and distribution of gas, among which the following are of especial importance to consumers :—

(a) That the Board of Trade may, on the application of any gas undertakers, by order, provide for the repeal of any enactments or other provisions requiring the undertakers to supply gas of any particular illuminating or calorific value, and for substituting power to charge for thermal units supplied in the form of gas.

(b) That where such substitution has been decided upon, the new basis for the sale of gas shall be 100,000 British Thermal Units (to be referred to in the Bill as a 'therm'). The consumer will then be charged according to the number of 'therms' supplied to him in the gas, and the standard price per therm fixed by the order shall be a price corresponding as nearly as may be to the price fixed by former provisions for each 1,000 cubic feet, but with such additions (if any) as appear to the Board to be reasonably required in order to meet unavoidable increases since June 30, 1914, in the costs and charges of and incidental to the production and supply of gas by the undertakers ; and the order may make such modifications of any provisions whereby the rate of dividend payable by the gas undertakers is dependent on the price of gas supplied as appear to the Board to be necessary.

(c) That an order under the Act shall prescribe the time when, and the manner in which, the undertakers are to give notice of the calorific value of the gas they intend to supply (i.e., 'declared calorific value'), and shall require the undertakers, before making any alteration in the declared calorific value, to take at their own expense such steps as may be necessary to alter, adjust, or replace the burners in consumers' appliances in such a manner as to secure that the gas can be burned with safety and efficiency.

(d) That the gas supplied under the Act (i) shall not contain any trace of sulphuretted hydrogen, (ii) shall not be at a pressure of less than two inches water-gauge in any main or service pipe of two inches diameter or upwards, and (iii) shall not contain more than a certain permissible proportion of incombustible constituents (namely, 20 per cent. during a period of two years after the passing of the Act, 18 per cent. during the succeeding two years, and 15 per cent. thereafter).

(e) That as soon as may be after the passing of the Act the Board shall cause an inquiry to be held into the question whether it is necessary or desirable to prescribe any limitations of the proportion of carbon monoxide which may be supplied in gas used for domestic purposes, and may, if on such inquiry it appears desirable, make a special order under the Act prescribing the permissible proportion.

(f) That Gas Referees and Examiners shall be appointed for the purpose of (i) prescribing the apparatus and method for testing the gas, and (ii) carrying out of such prescribed tests.

During the passage of the Bill through its Committee stage in the House of Commons, the important sub-section limiting the amount of incombustible constituents permissible in gas (vide (d) (iii) above) was deleted, on the understanding that, subsequent to the passing of the Act, the matter shall be made the subject of an official inquiry by the Board of Trade. The effect of this amendment is, therefore, to put the question of 'inerts' into the same category as that of carbon monoxide, and the whole matter now stands as follows :—

> The Board of Trade shall, as soon as may be after the passing of this Act, cause inquiries to be held into the question whether it is necessary or desirable to prescribe any limitations of the proportion of carbon monoxide which may be supplied in gas used for domestic purposes, and into the question whether it is necessary or desirable to prescribe any limitations of the proportion of incombustible constituents which may be supplied in gas so used, and may, if on any such inquiry it appears desirable, make one or more special orders under this Act prescribing the permissible proportion in either case, and any such

special order may have effect either generally or as regards particular classes of undertakings, and the provisions of the special order shall have effect as if they were enacted in this section.

When such official inquiries are instituted by the Board of Trade this Committee will hope to be given an opportunity of presenting again its views (as already reported) upon the matters concerned.

Alcohol from Coke-oven Gas.

During the past year a notable development has been made in connection with the technology of by-product recovery from coal as the result of Mr. E. Bury's successful experimental trials, in conjunction with Mr. O. Ollander, at the Skinningrove Iron Works, upon the absorption of ethylene from debenzolised coke-oven gas and its conversion into ethyl alcohol. These trials have demonstrated the possibility of obtaining on a large scale 1·6 gallons of absolute alcohol per ton of the particular Durham coal carbonised. Assuming a similar yield from the 15,000,000 tons (or thereabouts) of coal now annually carbonised in British by-product coke ovens, it is claimed to be possible to obtain from coke works alone a 95 per cent. industrial alcohol in quantities equivalent to about 24 million gallons per annum of the absolute spirit.

Although a full account of the investigation has already been given by Messrs. Bury and Ollander in a paper before the Cleveland Institution of Engineers in December last (*vide* also *Iron and Coal Trades Review*, December 1919), the Committee, whilst not expressing any opinion as to the commercial prospects of the process, considers that the technical importance of it is such as to warrant attention being drawn in this Report to some of its salient features (see Appendix III.).

The Committee recommends that it be reappointed to continue its investigations, with a grant of 35*l*.

APPENDIX I.

Memorandum upon Coal-mining Statistics.

The most important statistics concerning coal are the figures giving the annual production of coal, the number of workers employed in the mines, the number of fatal and of non-fatal accidents respectively. These statistics are collected and published by the Government Departments in most coal-producing countries, and upon these are based a number of comparative statements by which the progress of the industry in different countries is usually estimated, such as the production per worker employed, the accident death-rate per thousand workers, etc. For most economic and social studies, the number of workers employed is in several respects the most important of these figures, and unfortunately it would appear to be the one upon which the least dependence can be placed. Elaborate reports have been drawn up, and legislation has even been enacted, based upon the comparative results of these data; and it has been quite freely assumed that the figures given for different countries or different districts of a country are properly comparable, whilst as a matter of fact the methods of arriving at these figures vary so widely that they come to bear quite different meanings, and the assumption that similar headings always connote similar interpretations is utterly without foundation.

Production.—In this country the returns of the output of coal until recently included the stones and dirt sent up to bank with the coal and picked out on the belts or screens; since that time the weight of coal alone is supposed to be returned. The instructions at present issued by the Home Office read as follows :—

> The weight given should be the net weight after screening or sorting. . . . Where the net weight of the coal is not determined during the year in respect of which the return is being made, it will be sufficient if a deduction is made according to the average percentage of dirt extracted from the coal at the mine. In cases where the coal is sold as it leaves the pit without screening or sorting it will be proper to give the gross weight sent out of the pit as the amount of output.

It will be seen that the instructions are somewhat vague, and that they also leave considerable openings for guess-work and estimates instead of accurate facts; furthermore, the instructions would in some cases at any rate compel the inclusion of washery dirt under the heading of output, since this dirt does not always come under the heading of 'dirt extracted from the coal at the mine.' It is by no means uncommon for one company to control two collieries not far distant from each other and to erect at one of them a washery to which the small coal from the first colliery is to be sent for washings; in such a case if the instructions are literally followed, washery dirt will be included in the returns of the coal output from the first colliery and excluded from the second. Accordingly, it is natural that the practice in making up these returns varies greatly from district to district, and even from colliery to colliery. In some cases both the dirt picked out on the belts and that washed out in the washery are deducted from the pithead weight, i.e., from the tonnage on which the men are paid; in other cases no deduction at all is made for washery dirt, and in yet other cases an arbitrary percentage is deducted from the coal sent to the washery. There is also some difference as regards the practice concerning 'free coal' given to the miners and coal for colliery consumption. In most cases all this coal is returned as part of the production; in some cases the coal consumed by the pits is not included, and apparently in a few cases both the 'free coal' and coal for colliery consumption are deducted from the output. In some places it is customary to give as a return of output the landlord's tonnage, that is the amount on which royalty is paid, which is usually the output less certain deductions allowed by the terms of the lease. In view of this wide variation, it would be a distinct advantage if the Home Office were to issue specific instructions on all the above points, so as to secure uniformity of method in making returns throughout the United Kingdom. The methods used in Canada might well be adopted here.

In Canada a more definite system is adopted; the introduction to the Canadian Annual Statistics states in definite language what is intended, as follows :—

> The term 'production' in the text and tables of this report is used to represent the tonnage of coal actually sold, or used, by the producer, as distinguished from the term 'output,' which is applied to the total coal extracted from the mine, and which includes, in some cases, coal lost or unsaleable or coal carried into stock on hand at the end of the year.

Apparently throughout Canada the various Provinces issue sheets which have to be filled up every month, and which the different Provincial Governments have agreed to issue in identical form, so that returns for the Dominion can be made by the Canadian Department of Mines or by the Dominion Bureau of Statistics. The whole of the collection of statistics, and, in fact, the administration of mining law, is controlled by the respective Provincial Governments, with the exception of mining lands in certain of the Western Provinces and North-West Territories, which are controlled directly by the Dominion Government. These monthly returns show the amount of free coal or of coal sold to miners at a reduced price, the quantity used for colliery consumption, specifying any used on the colliery company's own railways, the quantity of coal used for making coke and briquettes, the quantity stocked, and the quantity on hand. The only fault that can be found with these returns is that they do not specifically ask for a return of the dirt picked out and washed out respectively. In Canada the term 'production' is restricted to marketable or economically useful coal, whilst the term 'output' is the equivalent of what we sometimes speak of in this country as 'drawings,' i.e., everything drawn out from the colliery, inclusive of any dirt that may be extracted subsequently.

In the United States the production means the total production of clean coal, that is to say, coal with the exclusion of pickings and washery dirt, and including colliery consumption. The work is done by the Mineral Resources Division of the United States Geological Survey, but there is a good deal of overlapping and difficulty owing to some of the statistics being collected by State Bureaux and others by Federal Bureaux: in this respect attention may be directed to the Conference on this subject held at Washington in 1916, the results of which are printed in a report of the Committee on the Standardisation of Mining

Statistics in 1918. At present cards in the shape of card slips are issued, to be filled up annually, and these ask for the total production, which is defined to 'include all marketable coal, excluding only refuse from washeries and slack coal wasted.' It distinguishes between the coal loaded at the mine for shipment, coal used locally, colliery consumption, and coal used for making coke at the mine. It will be seen that these instructions are fairly clear and definite.

In France the production includes the whole of the drawings, deducting only the worthless waste, *i.e.*, pickings and washery refuse.

In Belgium the same practice is followed, the production including colliery consumption and coal given or sold to employees, but definitely excluding pickings and washery waste.

It will be seen that all these producing countries are aiming at one definite meaning for the word 'production,' and in this respect there is at any rate uniformity of intention. Unfortunately the execution of the object leaves much to be desired. The Canadian practice of monthly returns has much in its favour; it no doubt throws a certain amount of additional work both upon individual collieries and upon the department collecting statistics, but, on the other hand, it enables half-yearly and quarterly statements to be issued very shortly after the conclusion of the respective periods, and in the same way annual statements can be produced much more rapidly than would be the case if the whole of the returns began to come in after the end of the year. It is quite desirable that the returns should show definitely the total weight of drawings, the weight of dirt picked and washed out, the weight given or sold to employees, the colliery consumption, and the coal used for making coke. Again, there would not be a great deal of labour involved in keeping these figures, and the information would be of the greatest value.

Number of Employees.—In this country the only information asked for is a return 'of persons ordinarily employed'; the returns specify that it must include all the persons employed on the mine premises, such as officials, storekeepers, clerks, etc., those employed on the pit sidings, on private branch railways and tramways, and in washeries adjacent to and belonging to the mine. Furthermore, the number employed underground must be kept separate from those employed above-ground, and there is also a separation according to age and sex. There is, however, no information as to what is meant by 'the number of persons ordinarily employed,' although this is evidently the crux of the whole matter. The consequence is that extremely variable methods are made use of. Some pits merely give the number of men entered on the pay sheet for the particular day in the year on which the return is made out; others take two or three days which they consider normal and average these. Some return the number of employees on the books of the company, others the number on the time roll; with the prevailing amount of absenteeism, the former number will exceed the latter by about 25 per cent., but there is no instruction as to which of the two is the figure intended to be given. Some of the more painstaking collieries average the number of men employed daily, but this is apparently exceptional. It is evident that a more definite and systematic method would have to be adopted before it is possible to attach anything like a precise meaning to returns of numbers employed in this country.

In Canada, apparently, monthly returns are made, and these are averaged for the year. The Canadian intention is to 'show the actual amount of labour in terms of days worked, rather than the actual number of individual men that may have been engaged,' and this is obviously the correct way of dealing with the subject. The returns ask for a classification under eight different heads and separate them into underground and above-ground workers; it may be noted that in Canada the number of men employed at the coke ovens and briquetting plants in connection with collieries is included in the mine employees, whilst according to the wording of the English return these should be excluded in this country, although there is no warranty for saying that the instructions for making the latter returns are in all cases strictly complied with. Furthermore, in Canada there is an interesting table showing the time lost through absenteeism, meaning thereby the fault of the men and through a series of other reasons which may be classified as the fault of the mine or of the industry. It would be a distinct advantage if such returns were available for this country.

In the United States of America the information asked for is the average number of men employed during the year, excluding coke workers and office force. In the exclusion of the latter item this return differs from the British return; in the exclusion of the former item it differs from the Canadian return. The number of hours per working day is also asked for, as well as the average number of days lost by strikes and the number of men thereby affected. The intention in America is to get the average number of men employed during the year, but apparently the methods of obtaining these are about as vague as they are in this country. In the report already referred to it is stated that 'without instruction in regard to the way these averages (average number of employees) should be computed there will be a lack of uniformity of method, and in many cases the figures submitted will not be averages, and will not represent even approximately the real average number of persons employed.' No one with any experience of the subject will doubt the accuracy of this statement, and it is certainly applicable to countries other than the United States. In the report in question the definition is put forward that 'the average number of men should be the actual number of man-hours for the year.' This obviously is a clear and intelligible definition, and it would probably be a great advantage if it were generally adopted.

In Belgium this principle is carried into effect; the number of employees returned represents the quotient of the number of days' work done in the year divided by the number of working days. This figure is thus really the mean number of workmen engaged during the working days.

In France, on the other hand, the number of employees is intended to be the number of names regularly on the colliery pay roll; a column is reserved for the number of days worked in the year. It is obvious that we are dealing here, under the same heading, with two entirely different conceptions; some countries return the number of men who normally get their living by the industry, without any regard to the amount of absenteeism or the length of time that these men may be at work, whilst others return the number of men who have put in a full year's work, meaning thereby have worked on all the days on which the mine was in operation. Obviously, these two figures differ widely from each other, and the fact that both are returned indifferently under the same heading vitiates many of the conclusions that have been drawn upon the basis of these returns.

Fatal Accidents.—It is a curious fact that whereas every coal-mining country publishes a return of fatal accidents, there appears to be in none of them any legal definition of what constitutes a fatal accident. In the absence of legal definition in this country the Home Office has for many years made a practice of classifying all mine accidents which result in death within a year and a day as fatal accidents, apparently for no better reason than that in so doing they have followed the old Coroner's Law.

In Canada the Mineral Resources Statistics Branch does not collect accident statistics, and these appear to be left to the relative departments of different Provinces. They are not asked for in the statistical returns, but are obtained from the reports to the Inspector of Mines. In the Province of Alberta a fatal accident is construed as an accident which causes death within a twelvemonth. In the other Canadian Provinces there appears to be no definition at all, and it would seem that if a man dies from the effect of a mining accident, however long the death may be after the accident, it would apparently be reported as a fatal accident for the year in which the death takes place.

In the United States mine-accident statistics are gathered by the various States and are by no means as reliable as statistics gathered by the Bureau of Mines. Mr. G. S. Rice, the chief mining engineer of the Bureau of Mines at Washington, gives me the following information : 'As to what constitutes a definition of a fatal accident, this varies in the different States. In some States it means immediate death, in others within a day or two, in still others, if the man dies from the direct cause of the accident before the report is turned in, which is in February for the preceding calendar year, which may mean from two to thirteen months after the accident.' It will be seen that these figures are obviously vague and unreliable. It is a curious fact that in the report of the Committee on the Standardisation of Mining Statistics already referred to, the terms fatal and non-fatal accidents are freely used, but there is no attempt at definition.

In France the principle followed is that the records of fatal accidents are restricted to those who are mortally injured in a mine accident, that is to say, either those killed on the spot or who die as the result of their injuries within a few hours after the accident, or at the outside within a few months without ever having been able to resume work. With regard to those whose death, occurring after a considerably longer interval, is the consequence of injuries received, they do not appear on the record, the Statistical Department not being, as a rule, informed of their death, and being, moreover, unable to determine its real cause.

In Belgium, on the other hand, a fatal accident is restricted to an accident that causes death within thirty days.

Here, again, it may be pointed out that this extremely important matter is in a chaotic condition, and that it is most urgent that an agreement be arrived at as to what precisely is meant by a fatal accident.

Non-Fatal Accidents.—Here, again, there is a wide variation to be noted in practice. In this country the return is asked for of non-fatal accidents within any given year, non-fatal accidents being defined as accidents disabling the victim for more than seven days.

In Canada the practice varies in the different Provinces. Apparently in Nova Scotia a non-fatal accident is classified as an accident by which a man must be disabled for at least seven days, but from which he recovers. In the Province of Saskatchewan accidents entailing a disability of less than six days are not recorded. In Alberta a non-fatal accident must be reported if a man is off for more than fourteen days; apparently in some cases accidents involving a disability of less than fourteen days are tabulated as slight accidents.

In the United States of America the question of what constitutes a non-fatal accident is even more unsettled than the definition of a fatal accident. In some of the States statistics are collected based on the State Compensation Acts, under which compensation is paid for an injury causing a loss of at least two weeks; in metal mines apparently an accident causing a loss of at least one shift is tabulated as a slight injury, and one involving a loss of two weeks as a serious injury.

In France injuries causing disability to work for more than twenty days are counted as non-fatal accidents.

In Belgium all non-fatal accidents are accidents that cause permanent disability, whether this be total or partial, accidents involving only temporary disability not being included in the returns.

The above can only be looked upon as an attempt to supply a portion of the information which is evidently needed before it is possible to read coal-mining statistics at all intelligently. It will be obvious, however, from what has been said, that attempts at comparisons, which have been so freely made without taking into account the striking differences in interpretation given above, must result in wholly inaccurate comparisons. I sincerely hope that the data here given may be further extended to all coal-producing countries, and I wish to urge again, as I have done in more than one International Congress, the importance of an International Conference for determining the precise manner in which mineral statistics of all kinds shall be collected and tabulated, and the precise meaning that should be attached to the various headings.

HENRY LOUIS.

APPENDIX II.

TABLE A.

Showing Tons of Coal exported from the United Kingdom in each of the years 1913 to 1919 inclusive, distinguishing the principal Countries to which consigned.

Countries to which consigned	1913	1914	1915	1916	1917	1918	1919
Russia	5,998,434	3,087,805	42,559	4,804	1,075,005	128,114	221,490
Sweden	4,563,076	4,250,255	2,659,995	1,646,502	613,573	1,051,121	1,592,324
Norway	2,298,345	2,462,200	2,643,187	2,307,753	1,059,227	1,250,867	1,330,646
Denmark (including Faroe Islands)	3,034,240	3,059,162	3,130,642	2,305,409	856,037	1,045,701	1,742,711
Germany	8,952,328	5,256,765	—	—	—	—	4,301
Netherlands	2,018,401	1,722,215	1,792,951	1,346,129	376,819	88,001	401,901
Belgium	2,031,077	1,168,554	—	—	—	4,246	143,769
France	12,775,909	12,330,545	17,601,361	17,311,877	17,512,545	16,511,005	16,204,596
Algeria	1,281,664	910,211	939,846	721,803	616,053	160,340	523,091
Portugal	1,201,722	1,048,608	916,992	794,315	248,056	147,388	544,302
Azores	22,608	33,639	43,893	46,518	101,871	40,345	99,875
Madeira	131,751	87,378	61,870	46,139	25,024	6,367	29,749
Spain	2,534,131	2,260,362	1,597,083	2,007,899	773,030	429,003	805,740
Canary Islands	1,114,629	679,786	470,680	399,865	38,641	—	262,184
Italy	9,647,161	8,625,254	5,788,460	5,710,098	4,140,960	4,053,570	4,641,046
Austria-Hungary	1,056,634	564,362	—	—	—	—	142,567
Greece	727,899	578,757	309,198	168,106	18,540	41,328	139,315
Roumania	251,925	218,218	—	—	—	—	—
Turkey	369,789	429,506	10,324	5,987	40,513	19,136	135,738
Chile	588,526	377,482	46,407	24,194	13,154	9,000	7,294
Brazil	1,886,871	1,176,780	498,340	196,973	237,516	169,982	189,205
Uruguay	723,936	550,876	332,541	179,472	199,692	162,417	184,813
Argentine Republic	3,693,572	2,883,064	1,618,603	728,412	309,216	258,110	639,089
Other Foreign Countries	1,014,372	879,707	589,023	590,654	1,270,675	1,221,121	1,057,454
Total to Foreign Countries	67,919,000	54,641,491	41,093,955	36,542,909	29,526,147	26,797,162	31,043,200

1920

TABLE A (continued).

Showing Tons of Coal exported from the United Kingdom in each of the years 1913 to 1919 inclusive, distinguishing the principal Countries to which consigned.

Countries to which consigned	1913	1914	1915	1916	1917	1918	1919
Channel Islands	167,862	162,066	131,110	115,819	100,502	93,516	114,647
Gibraltar	354,702	309,650	356,075	517,135	1,224,340	1,615,985	1,465,736
Malta and Gozo	700,111	338,673	151,153	84,784	1,729,685	1,195,883	733,045
Egypt	3,162,477	2,594,723	1,362,073	844,398	1,574,251	1,697,908	1,675,163
Sierra Leone	40,602	35,524	23,492	12,186	226,648	160,899	48,333
Anglo-Egyptian Soudan	—	38,858	26,841	49,288	18,650	403	23,186
Aden and Dependencies	181,204	139,821	132,475	29,935	22,925	—	49,479
Ceylon and Dependencies	239,657	250,173	43,487	25,726	12,541	—	13,305
Hong Kong	52,408	10,376	—	—	—	—	12,241
Falkland Islands	27,977	34,678	67,432	50,416	29,840	24,344	34,490
Other British Possessions	554,118	483,847	146,467	78,957	530,258	166,804	36,743
Total to British Possessions	5,481,118	4,398,389	2,440,605	1,808,644	5,469,640	4,955,742	4,206,368
TOTAL	73,400,118	59,039,880	43,534,560	38,351,553	34,995,787	31,752,904	35,249,568

TABLE B.

Showing Tons of Coal exported from each of the undermentioned districts of the United Kingdom during the years 1913 to 1919 inclusive.

	1913	1914	1915	1916	1917	1918	1919
Bristol Channel Ports	29,875,916	24,475,551	18,601,896	17,417,707	19,893,015	17,000,834	20,229,802
North-Western Ports	751,819	600,719	524,214	486,734	668,937	699,273	86,728
North-Eastern Ports	23,023,810	19,241,980	14,132,040	13,329,275	10,102,841	9,948,995	11,701,435
Humber Ports	8,883,353	6,049,741	3,791,392	1,804,953	1,812,724	1,617,719	530,471
Scottish Ports { East Coast	8,253,023	6,177,065	3,723,935	3,004,449	910,347	1,198,739	1,905,176
{ West Coast	2,184,174	2,201,244	2,724,005	2,297,088	1,564,291	1,212,692	745,899
TOTAL (from United Kingdom)	73,400,118	59,039,880	43,534,560	38,351,553	34,995,787	31,752,904	35,249,568

Appendix III.

Memorandum upon the Skinningrove Process for the Production of Alcohol from Coke Oven Gas.

THE following is a brief outline of the process devised by Messrs. Bury and Ollander for the removal of ethylene from debenzolised coke oven gas and its conversion into ethyl alcohol.

The average amount of olefines present in a debenzolised gas from a typical Durham coking coal is usually between 2·0 and 2·5 per cent. They consist chiefly of ethylene with small quantities of propylene and possibly other higher members of the series.

The process for their removal from the gas is based upon the well-known fact that ethylene is absorbed by concentrated sulphuric acid forming ethyl hydrogen sulphate, which may be subsequently hydrolysed by the dilution of the acid with water yielding ethyl alcohol and sulphuric acid. The sequence of the reactions concerned may be represented by the following equations :—

$$(a) \quad C_2H_4 + H_2SO_4 = \begin{matrix} C_2H_5 \\ H \end{matrix}\!\!\!\Big\rangle SO_4,$$

$$(b) \quad \begin{matrix} C_2H_5 \\ H \end{matrix}\!\!\!\Big\rangle SO_4 + H_2O = C_2H_5OH + H_2SO_4.$$
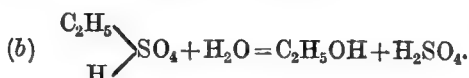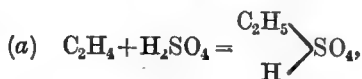
The problem presented to the investigators was not only the determination of the conditions under which 2 per cent. of ethylene in an industrial gas can be rapidly absorbed by concentrated sulphuric acid so as to produce ethyl hydrogen sulphate exclusively, but also how the much smaller quantities of higher olefines contained in the gas can be removed from it prior to the desired absorption of ethylene.

Laboratory experiments proved (i) that, although the absorption of ethylene by concentrated sulphuric acid proceeds far too slowly at ordinary temperatures, yet between 60° and 80° C., the time of contact required between the acid and coke oven gas, in order to ensure the absorption of 70 per cent. of its total ethylene content, need be no more than $2\frac{1}{2}$ minutes, and (ii) that under such conditions the only product formed is ethyl hydrogen sulphate. On the other hand, if the temperature be allowed to exceed 80° C. some decomposition occurs and ethyl ether is produced.

The successful operation of such an absorption process on a large scale pre-supposes the elimination from the crude gas of tars, ammonia, naphthalene, and benzol hydrocarbons in the order named. At the Skinningrove Works the Otto direct process is employed for this purpose.

The next step consists in the successive elimination from the cooled and debenzolised gas of (a) sulphuretted hydrogen, and (b) higher olefines than ethylene, together with most of its water vapour content. For the elimination of the sulphuretted hydrogen it is proposed to make use of the well-known reaction between sulphuretted hydrogen and sulphur dioxide gases :—

$$2H_2 + SO_2 \rightleftharpoons 2H_2O + 3S.$$

The advantage of such a procedure is that it would not only dispense with the necessity of employing iron oxide purifiers (except perhaps as a final pre-caution), but it would also enable the small amount of sulphur dioxide arising from the reduction of the hot strong sulphuric acid during the later ethylene absorption process to be utilised.

Propylene and other higher olefines are next removed by scrubbing the gas with an 80 per cent. sulphuric acid at the ordinary temperature in a tower on the counter-current principle, which also effects the removal of about 97 per cent. of its water vapour content. The resulting cooled and dried gas is then passed

through a 'heat exchanger' situated so near the ovens that its temperature can be raised to between 60° and 80° C. at the expense of some of the sensible heat in the hot crude gas leaving the ovens. The strong acid (95 per cent.) used for the absorption is also pre-heated to the same temperature. The scrubbing process for the removal of ethylene is carried out on the counter-current principle, and the time of contact between the pre-heated gas and acid is $2\frac{1}{2}$ minutes, which is sufficient to effect absorption of 70 per cent. of the total ethylene present. The acid can be used until it has absorbed up to 5 per cent. of its weight of ethylene with the formation of a corresponding quantity of ethyl hydrogen sulphate.

The strong acid from the ethylene absorption towers containing the ethyl hydrogen sulphate is next taken to a special form of distilling column where it meets a current of steam which dilutes the acid to about 75 per cent. strength and simultaneously hydrolyses the ethyl hydrogen sulphate forming ethyl alcohol and sulphuric acid. The heat produced during the dilution is sufficient to raise the temperature of the diluted acid to between 90° and 100° C., under which conditions the resulting alcohol distils over and is subsequently condensed, finally leaving the plant as a 95 per cent. alcohol.

The diluted acid is finally pumped to the top of a Gaillard concentration tower where it is concentrated to a 95 per cent. strength, which is then used over again for the absorption of ethylene. Any small quantity of sulphurous acid formed by the reducing action of the gases upon acids in the absorption tower is, during the dilution process, decomposed, and the resulting sulphur dioxide is (as aforesaid) utilised for the elimination of sulphuretted hydrogen from the debenzolised gas.

From figures given in Messrs. Bury and Ollander's paper (*loc. cit.*) the composition of the debenzolised gas from a Durham coking coal, *before* and *after* the removal of the greater parts of its ethylene content in the manner proposed, is as follows :—

	Before	After
Carbon Dioxide	2·0	2·08
Carbon Monoxide	5·4	5·61
Ethylene, &c.	2·0	0·62
Methane	25·0	25·96
Hydrogen	50·0	51·91
Nitrogen and Water Vapour, &c.	15·6	13·82
	100·0	100·0

	Before	After
Gross	467·8	458·9
Net	412·2	402·8

W. A. BONE.

*Old Red Sandstone Rocks at Rhynie, Aberdeenshire. Final Report of
Committee* (Dr. J. Horne, *Chairman*; Dr. W. Mackie, *Secretary*; Dr. J. S. Flett, Dr. W. T. Gordon, Dr. G. Hickling,
Dr. R. Kidston, Dr. B. N. Peach, Dr. D. M. S. Watson)
appointed to excavate Critical Sections therein.

Dr. W. T. Calman and Mr. D. I. Scourfield have continued their examination
of the microscopic sections of the plant-bearing cherts discovered by Dr. Mackie
in the Old Red Sandstone at Rhynie, Aberdeenshire. The following Report on
the results of their investigations has been furnished by Dr. Calman :—' A large
number of sections and chips have now been studied by Mr. Scourfield, and
drawings have been made of the more important remains. It has not been
possible to discover any regularity in the way in which the specimens are
distributed in the chert, or to correlate their presence with anything visible to
the naked eye in hand specimens. Although the remains are of the most
fragmentary kind, it has been possible to determine with fair certainty the
more important characters of the body and limbs. We are now convinced that
all the remains belong to a single species which is most nearly related to the
Anostraca, although it differs in important morphological characters from the
recent representatives of that order. It is hoped to discuss these remains
at greater length in a memoir which is now in preparation. A few fragments
of limbs and body-somites of a much larger Arthropod have been observed.
There is some evidence to suggest that these may belong to a Diplopod, but it
is not proposed to discuss them unless further material should be discovered.'

As sufficient material has been collected for further examination, the balance
of the grant from the Royal Society has been returned. The Committee may
now be discharged.

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*Experiments in Inheritance of Colour in Lepidoptera.—First Report of
Committee* (Prof. W. Bateson, *Chairman*; Hon. H. Onslow,
Secretary; Dr. F. A. Dixey).

Spilosoma mendica and var. *rustica.*—The white variety of the male is incompletely dominant. About 400 insects were raised from the following types of
mating :—DR × RR, DD × DR, and DD × RR. Eggs were obtained from a
number of pairings of DR × DR, which will emerge next year, 1921. These,
with the previous records, should be sufficient to elucidate the nature of the
inheritance. Owing to the great colour variation of the F_1 generation, readings
of the colour of each individual are being made with the 'Tintometer' to show
the colour distribution.

Boarmia consortaria and var. *consobrinaria.*—About 200 insects were reared,
which confirmed the dominance of the melanic variety. The suggestion made in
the *J. of Genetics,* Vol. IX. No. 4, March 1920, p. 339, that the intermediate
variety is dominant to the type, was also confirmed. Ova were obtained which
should show the relationship of the intermediate to the melanic variety next year.

Hemerophila abruptaria and var. *brunneata.*—Ova were obtained from several
pairs in order to see whether the melanic variety behaves as a simple Mendelian
dominant. In the published experiments of Hamling and Harris there is a large
excess of melanics in matings of the type DR × RR.

Callimorpha dominula, and the yellow variety.—About 700 insects were raised
from these crosses, which completely confirm Bateson's suggestion that the yellow
form is recessive to the red. The insects reared will serve as material for an
examination of the pigments.

Zygœna filipendulœ, and the yellow variety.—Larvæ were reared from several
pairings between red and yellow varieties and from the F_1 generation. The
larvæ have not as yet pupated.

Abraxas grossulariata and var. *varleyata.*—About 400 insects were reared
from matings between the type and the melanic variety. Together with previous

records, they fully confirm the recessiveness of the melanic variety. In the F_1 generation there appears to be a difference in the distribution of the black pigment between the two sexes. This is being investigated by means of camera lucida drawings of the F_1 generation, from which a calculation of the black areas may be obtained by means of a planimeter.

A. grossulariata var. *lacticolor* and var. *varleyata*.—The combination of these two characters in the same zygote produces a new and very beautifully marked variety, named var. *exquisita*.

If T = type, and t = *varleyata*, and if G = type, and g = *lacticolor*,

then ♂ type = TTGG, ♀ type = TTGg
♂ *varleyata* = ttGG, ♀ *varleyata* = ttGg
♂ *lacticolor* = TTgg, ♀ *lacticolor* = TTgg
♂ *exquisita* = ttgg, ♀ *exquisita* = ttgg

Therefore TtGg × TtGg should give a 9 : 3 : 3 : 1 ratio of type, *varleyata*, *lacticolor* and *exquisita*, but, owing to the spurious allelomorphism in the F_1 generation, the sex ratios will not be normal. Experiments are being made to test this. Up to the present, though the numbers are small, owing to the unfortunate incidence of disease, an excess of type and *varleyata* individuals is indicated.

It is hoped, if possible, to complete the experiments indicated above next year. It is also suggested that, in the event of material being procurable from Germany, experiments might be initiated to elucidate the inheritance of *Aglia tau* and its var. *lugens*.

Committee to co-operate with Local Committees in Excavations on Roman Sites in Britain.—(Sir W. RIDGEWAY, *Chairman*; Mr. H. J. E. PEAKE, *Secretary*; Dr. T. ASHBY, Mr. WILLOUGHBY GARDNER, Prof. J. L. MYRES).

OWING to the war the Committee has been in suspense since 1914, when all excavations in the Hill Fort in Kinmel Park, which was being explored by the Abergele Antiquarian Association and the Cambrian Archæological Association in co-operation with this Committee, came to an end.

Although the Committee had not been revived in 1919, and therefore no grant was available, Mr. Willoughby Gardner reopened his investigations in November 1919, and has supplied the Committee with a detailed report of them.

The Committee asks to be reappointed.

Abstract of Report on Further Excavations in Dinorben, the Ancient Hill-Fort in Parch-y-Meirch Wood, Kinmel Park, Abergele, N. Wales, during 1920.

By WILLOUGHBY GARDNER, *F.S.A.*

THE excavations in this native hill-fort (see Reports of the British Association, 1912, 1913, and 1915) were reopened in November 1919. A first objective was the further investigation of the huge main rampart. Attention was directed to the cutting through it on the S.W., where archæological 'floors' and constructions belonging to two occupations, A and B, had been previously discovered, and where the top course of a wall belonging to an earlier construction had been found beneath the lower Floor B. Before we could excavate this it was necessary to widen our cutting considerably, thus enabling us to examine further areas of Floors A and B. In the uppermost, A, more relics of the fourth century were found. Below it the revetting wall excavated in 1914 was

found to be built upon an earthen surface marking a fresh floor between Floors A and B. Upon this lay bones, pot-boilers, &c., but no pottery or coins, differentiating it therefore from Floor A and causing us to name it Floor A2. The core of the rampart behind this wall consisted of rubble stones in its lower half and of layers of clay above. This clay had been visibly laid on wet, and had afterwards dried hard like cement, so as to give a foundation for any stone structure, such as a parapet wall, which is indicated by surviving stones seen here and there along the crest of the rampart.

A further length of the back revetting wall of the rampart belonging to the earlier Floor B was next uncovered. The rampart consisted of rubble stones and was about 15 ft. thick, but its outer side and facing wall were ruined and missing. It was built upon a similar layer of hardened clay 4 ft. thick. Behind it Floor B, of dark soil 3 ft. thick and gradually shallowing, stretched towards the interior of the hill-fort. Upon this were found bones of domestic animals, much charcoal, broken pot-boilers, a pounding stone, sawn antlers, a deer-horn toggle, and an iron knife-blade. About 20 ft. from the wall a mass of burnt limestones mixed with baked red soil and much charred wood like burnt timbers was unearthed, also several stone-lined post-holes. Apparently this was the ruin of a stone-and-timber building destroyed in some great conflagration, such as previously observed in and near the earliest S.E. entrance.

Having removed Floor B, it was possible to investigate the wall-top found below it in 1914. This was followed downwards till a ruined dry masonry facing wall, still standing in one part 5 ft. high and backed with an 8 ft. thick core of large stones and rubble, was revealed to view. This newly discovered rampart was erected upon a floor surface of dark soil 6 in. thick, which we designated Floor C. It would appear to be a portion of the earliest defence constructed upon the site. Keen search was therefore made for relics here. The floor was followed up as far as practicable at the bottom of our deep cutting, revealing charcoal, pot-boilers, broken bones of domestic animals, and sling stones, but unfortunately nothing definitely dateable. In front of the wall the ground was piled high with the débris of the upper half of the thrown-down rampart, which was once, apparently, about 10 ft. high.

Search was next made for a ditch defending this rampart. The hill slope was followed beneath the fallen ruins and then beneath layers of clay for a horizontal distance of 23 ft. from the wall before one was found. But here a section of a rock-cut ditch 6 ft. wide and 5 ft. deep was dug out. It was filled with rubble mixed with a few wall-facing stones, with a 1 ft. layer of dark soil half-way down containing animal bones and a portion of an antler probably used as a pick. It was covered at the bottom with 6 in. of silting.

As the second and third only of the three earth-cut ditches belonging to Floor A seen along the S. side of the hill-fort were visible, though nearly filled with débris, along the S.W. side, our cutting was extended outwards to search for the first. This was found buried deep under silting and filled with stony débris from the partial demolition of the latest main rampart. It was rock-cut, 15 ft. wide and 7 ft. deep, and had but a thin layer of silting on the bottom. The second and third ditches were then fully excavated, the second being filled with stony débris and the third with soil and a few fallen stones. No relics were found.

Turning from the S.W. cutting, renewed attention was paid to the defences upon the S. side of the hill-fort. Here a cutting had already been made across the upper portion of the great main rampart and the three outer earth-cut ditches had been excavated. The first ditch had been found to be filled with clean quarried rubble stones mixed with wall-facing stones—the thrown-down débris of a parapet which once stood upon the crest of the rampart and which was approximately contemporaneous with the fourth-century Floor A. In order to excavate deeper down, this cutting also had to be widened, at a cost of much expensive labour; but by this widening, additional knowledge of the periods of occupation of the hill-fort was obtained. Here, as in the S.W. cutting, a floor and construction, A2, were found intermediate between Floors A and B. A further length of the back revetting wall of the rampart belonging to Floor B was also uncovered, built upon a similar foundation of

hardened clay. The core of this rampart was composed in places of rubble and in places of clay, probably the work of different gangs of labourers. Here there does not appear to be a floor in actual contact with the wall, but there was one four feet below it. This, Floor B, was first visible as a thin layer of soil covering the surface of a former clay rampart thrown up rather S. of the crest of the present one. It was followed up towards the N., 4 ft. below the bottom of the above-named wall, 2 ft. below Floor A2, over a low bank of made clay, till, at length, it became indistinguishable from Floor A. Along its course animal bones, charcoal, pot-boilers, and the cut shank-bone of an ox with four holes drilled in it (probably used for weaving) were found. Considerable stony remains, apparently of buildings destroyed by fire, as in the S.W. section, were also encountered. These ruins will require working out by exposure of larger areas.

Floor C was next sought for in this cutting by driving in trenches at increasing depths from the southern slope of the rampart. The floor, which was shown to be a surface of human habitation by finds of broken animal bones and charcoal, was finally discovered at a level of 27 ft. below the present crest. It was followed up for a distance of 32 ft., when the cutting became too dangerous to continue. The absence of a ditch in this floor in a similar position to that found in the S.W. cutting was noticeable. We next excavated to reach this floor from the northern side of the rampart. Digging down below Floor B, we came upon the core of a stone rampart, with several massive wall-facing stones *in situ* in front of it, erected upon a demonstrable continuation of Floor C. This rampart had been visibly thrown down almost to its foundations and its stones filled an earth-cut ditch 10 ft. wide which was subsequently discovered a few feet in front of it. We cleared out most of these stones, finding animal bones, pot-boilers, and an antler among them, and measured the V-shaped sides of the ditch as we proceeded. But the dangerous nature of the cutting unfortunately prevented our reaching the bottom, 7 ft. deep, except by probing; a cracking side obliged us to withdraw—just before a fall of earth commenced. There could be little doubt that this ruined rampart and ditch were portions of the earliest defences erected upon the hill-side. Strange to say, however, the rampart did not here rest upon the original ground. Beneath it there was a layer, 6 in. to 12 in. thick, composed of broken stones—at first sight much like the metalling of a road. But inspection showed that they were fractured by heat and were such as were usually recognised as pot-boilers. Upon and among these stones numerous bones of domestic animals were found, many broken for marrow, as well as much charcoal. Altogether this layer, which we uncovered for some four square yards, had all the appearance of a 'cooking hearth,' except for the absence of an adjacent water-supply. This 'hearth' must have existed on the hill-side before rampart C was thrown up.

While the main rampart was being excavated, about 85 square yards of the top Floor A were explored in the interior of the stronghold and many relics found which were unmistakably dated by coins found alongside. These relics, when worked out, will afford valuable material for the classification of the later Romano-British pottery, or at any rate of such common wares as were spread among native hill tribes by traders at that time. Although marked progress has been made and much has been learned as the result of this year's work, the area uncovered of the deeper Floors B and C is as yet very limited. It is most desirable that larger surfaces of both should be excavated. Then only will it be possible to join up the layers of occupation found in the ramparts with the roadways and constructions discovered in former years in the S.E. entrances. Many promising sites spread over the $5\frac{1}{2}$ acres of the hill-fort are also 'calling out' for exploration. We look forward to continuing work at Dinorben next year.

Exploration of the Palæolithic Site known as La Cotte de St. Brelade, Jersey.—Report of the Committee (Dr. R. R. MARETT, *Chairman*; Mr. G. F. B. DE GRUCHY, *Secretary*; Dr. C. ANDREWS, Professor A. KEITH, Mr. H. BALFOUR, and Colonel R. GARDNER WARTON).

DURING August and September 1919 and again in April 1920 excavation was continued, mostly by the aid of amateur labour, as funds were low. A trench was driven along the W. wall of the cave some 10 ft. below the bottom of the palæolithic floor. Nothing, however, came to light here except a curious peaty deposit in two layers, each about a foot thick, and separated by a stratum of sandy clay; nor was bedrock anywhere reached. On the other hand, just out-side the cave-entrance a sloping platform was discovered where flint-knapping must once have been energetically carried on, as was proved by the presence of nearly 1,000 flint fragments, including a few well-finished instruments. Here, among other bone, a tooth of the cave bear (*Ursus spelæus*) was found. Strangely enough, this species had not hitherto been reported from this site.

At a higher level in the ravine adjoining the cave-entrance is a rodent-bed, differing slightly in its composition from the rodent-bed within the cave. This year's collection consists, according to Mr. Hinton's analysis, of two species only, viz., *Dicrostonyx henseli* 85.3 per cent., and *Microtus anglicus* 14.7 per cent. Associated with this bed was a bone determined by Dr. Andrews as undoubtedly belonging to the Great Auk (*Alca impennis*). This also is a new record for the cave, and it is interesting to find this species so far South.

The Distribution of Bronze Age Implements.—Interim Report of the Committee (Professor J. L. MYRES, *Chairman*; Mr. HAROLD PEAKE, *Secretary*; Dr. G. A. AUDEN, Mr. H. BALFOUR, Mr. L. H. D. BUXTON, Mr. O. G. S. CRAWFORD, Sir W. BOYD DAWKINS, Professor H. J. FLEURE, Mr. G. A. GARFITT, Dr. R. R. MARETT, Sir C. H. READ, and Sir W. RIDGEWAY).

THE Committee was first appointed in 1913, and decided that before any con-clusions could be drawn as to the Distribution of Bronze Age Implements it was necessary to compile an illustrated card catalogue of all the metal objects of the Bronze Age found in the British Isles. This task was begun in July 1914, but during the war it was not possible to progress rapidly with the work.

The Committee has had throughout the assistance of Dr. H. S. Harrison, representing the Royal Anthropological Institute; Lord Abercromby, represent-ing the Society of Antiquaries of Scotland; and Mr. E. C. R. Armstrong, repre-senting the Royal Irish Academy.

The method employed is to make full-size drawings of the objects, with one or two sections, on thin paper, and to note full particulars as to the discovery and subsequent history of each object, references to published accounts, as well as its dimensions, weight, condition, and associations; these particulars were then transferred to cards, 10 in. by 7 in., by the secretary. During the years of war a number of such drawings were made by voluntary helpers, and about a thousand cards were completed.

It was decided in 1919 that if the work was to progress more rapidly the Committee would require the assistance of paid workers. The Association made a grant of 100*l.*, with permission to raise additional funds in its name. No general appeal has yet been made for funds, but the money received to date has been:

British Association	.	.	.	.	£100 0 0	
Robert Mond, Esq.	.	.	.	. £30 0 0		
G. A. Garfitt, Esq.	.	.	.	. 10 10 0		
Mrs. Hookham	.	.	.	. 5 0 0		
Royal Irish Academy	.	.	.	. 5 0 0		
					50 10 0	
					£150 10 0	

The thanks of the Committee are due to these helpers for their support.

In May 1920 a draughtsman was engaged and trained, and some work was put out at piece rates to another draughtsman, so that by the end of June the cards completed numbered 1,649, while about 1,000 additional drawings still remained to be copied.

Several Curators of Museums have kindly supplied the drawings of the objects in their custody, and many more have undertaken to do this in the near future, so that the work of the draughtsman will be mainly confined to filling in the cards and visiting small museums and collections.

On June 30 the expenditure amounted to 72*l*. 5*s*., and the balance available will pay for the draughtsman until the end of September. It is estimated that about 200*l*. a year will be required to pay for the one draughtsman regularly employed, together with his travelling expenses, and the purchase of additional material.

Electromotive Phenomena in Plants.—*Report of Committee* (Dr. A. D. WALLER, *Chairman;* Mrs. WALLER, *Secretary;* Dr. F. O'B. ELLISON, Prof. J. B. FARMER).

DURING the last year we have taken a considerable number of observations on the growth of plants in the garden and of their amputated parts in the laboratory.

We have examined into the relative physiological activity of growing and non-growing zones as shown by their electrical response to electrical excitation (blaze-currents).[1] The disposition of apparatus has been as described in previous publications and is summarised in the diagram overleaf.

Observations have for the most part been carried out upon *Iris germanica* during the months of April, May, and June. The following is a representative experiment :—

Iris in its natural habitat in a flower bed with S.W. aspect. Leaf, 25 cm. long, marked by Indian ink into 50 equal parts on April 24. The marked leaf was examined from time to time and the markings remained apparently unaltered in length, but raised *en masse* with the growth of the leaf which was measured and shown at the Royal Society on May 13 the lowest mark was raised 5 cm.—*i.e.*, the growth had been exclusively basal.

A similar leaf, about 20 cm. long, was led off to the galvanometer from its base close to the rhizome and from a point of the leaf 5 cm. higher up; it exhibited a current of rest directed in the leaf from base towards apex.

In response to a strong break induction shock, B to A (*i.e.*, ascending), a strong blaze-current was aroused in the same direction B to A (post-anodic homodrome action current indicative of predominant change at B). A similar, but less marked, response in the same direction, B to A, was aroused by a strong induction shock passed through the leaf from A to B (post-kathodic antidrome response).

Strong alternating induction currents were now passed through the A B portion of the leaf for a period of one minute in order to effect its electrocution. The blaze test by a single induction shock first in the ascending then in the descending direction failed to arouse any marked response : in each case the only visible effect was a small deflection antidrome to the exciting current—*i.e.*, in the direction of polarisation. The suppression of the blaze-current by electrocution can be definitive or temporary, according to strength.

Conclusion.—I. The basal zone of the Iris leaf, in which alone active growth is in progress, is electrically active (zincative) in relation to parts where active growth has ceased.

II. The zone of active growth is aroused to greater physiological activity (*i.e.*, is more zincable) than are parts in which growth is not proceeding.

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[1] The rationale of 'blaze-currents' as a sign of life has been set forth in several previous communications and is summarised in the following : *Lectures on the Signs of Life from their Electrical Aspect* (John Murray, London, 1903) ; *Physiology the Servant of Medicine* (University of London Press, 1910).
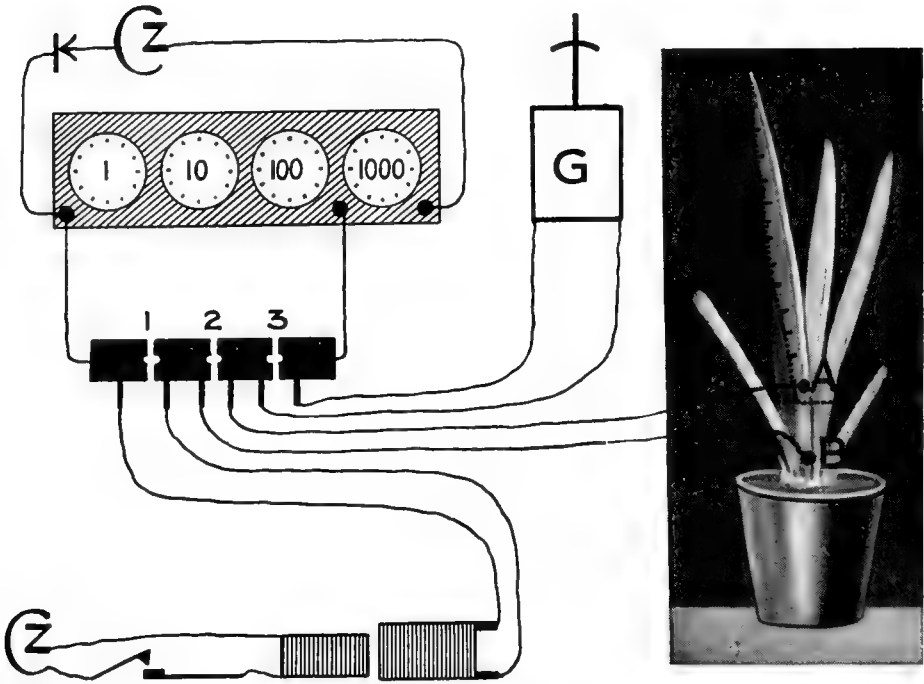
Diagram of the circuit required for the systematic observation of the blaze-currents of plants. B A are the unpolarisable electrodes by which the plant currents are led off to the galvanometer G. Any accidental current or current of injury of the plant is neutralised by the compensator. The wires from the compensator are connected with the two ends of the three-plug key. A single induction shock of given strength and direction can be short-circuited or not by a plug at the first plug-hole. The plant can be short-circuited or not at the second plug-hole. The galvanometer can be short-circuited or not at the third plug-hole.

Procedure.—Any accidental current or current of injury of the plant is neutralised (and measured) by adjustments on the dials of the compensator. The galvanometer is short-circuited at the third plug-hole. A break induction shock of given strength and direction is sent through the plant by (closing and) opening a contact-key in the primary circuit of the induction coil (during closure of this key the secondary coil is short-circuited at the first plug-hole to cut off the make shock from the plant). Immediately after the break shock has passed through the plant, the galvanometer is unplugged at the third hole; the blaze-current aroused by the previous break induction shock now causes deflection of the galvanometer. The voltage of a deflection is ascertained by comparison with the deflection given by 0.01 volt from the compensator. The resistance in circuit is ascertained by comparison with the deflection of 0.01 volt through 1,000,000 ohms.

Illustrating ' Electromotive Phenomena in Plants.'

Museums in Relation to Education.—Final Report of Committee (Professor J. A. GREEN, *Chairman*; Mr. H. BOLTON *and* Dr. J. A. CLUBB, *Secretaries*; Dr. F. A. BATHER, Rev. H. BROWNE, Mr. C. A. BUCKMASTER, Professor E. J. GARWOOD, Dr. A. C. HADDON, Dr. H. S. HARRISON, Mr. M. D. HILL, Dr. W. E. HOYLE, Sir H. MIERS, Professor P. NEWBERRY, Mr. H. R. RATHBONE, Dr. W. M. TATTERSALL, Sir RICHARD TEMPLE, Mr. H. HAMSHAW THOMAS, Professor F. E. WEISS, *and* Dr. JESSIE WHITE).

THE Committee was formed at the Birmingham Meeting of the British Association in 1913, with the following terms of reference: ' To examine, inquire into, and report on the character, work, and maintenance of Museums, with a view to their organisation and development as institutions for Education and Research; and especially to inquire into the requirements of schools.'

The work was carried on energetically for four years, and numerous subsidiary reports drawn up by sub-committees were considered. The increasing gravity of war conditions and the absence of members upon various war activities suspended further work.

Recent Legislation.	Museums and Advanced Students.
The Functions of Museums.	Museums and Classical Education and
Museums in 1914.	the Humanities.
Museums in Relation to the General	Staffing of Museums.
Public.	Overseas Museums.
Museums and Schools.	Manchester Scheme.

Recent Legislation.

The Education Act passed in 1918, and the more recent Libraries Act of 1919, have profoundly modified the position of Museums in relation to Education, and definite lines of development have been foreshadowed by the Reports of the Committee on Adult Education of the Ministry of Reconstruction, and by the issue of ' Draft Suggestions for the Arrangement of Schemes under the Education Act of 1918,' by the Board of Education.

These changes have been taken into account in the present Report, and have rendered necessary a revision of some parts of the work previously done.

1. The Education Act of 1918 made it possible for local Education Committees to seek the assistance of Museums in the furtherance of local schemes of educational development.

This was emphasised in 1919 by the ' Draft Suggestions for the Arrangement of Schemes under the Education Act of 1918,' issued by the Board of Education. These Suggestions indicate the desirability of arrangements for ' securing and developing the educational uses of Museums and Libraries,' and for ' developing the educational activities of local Literary, Historical, Archæological, Scientific, Musical, Artistic, and Dramatic Associations.'

In 1918 the Ministry of Reconstruction presented to Parliament an ' Interim Report of the Committee on Adult Education upon the Industrial and Social Conditions in Relation to Education.' The questions raised in that Report were more fully reported upon in a ' Third Interim Report ' presented to Parliament in 1919.

In a still later Report the ' nmittee on Adult Education considered the conditions and work of Museums, and advanced the suggestion that these

institutions should, together with Public Libraries, be definitely included in any scheme of education for a local area in England and Wales, and that these institutions should be taken into account in State grants allotted to the local authorities.

It was further suggested that, under the powers and duties of the Local Government Board, Libraries and Museums should be transferred forthwith to the Board of Education by an Order in Council.

The Committee on Adult Education did not seek the advice of this Committee, or of the Museums Association, and unfortunately their recommendations for the transference of Museums and Libraries to the local Education Authority proved unacceptable to both the Museums Association and the Library Association. The Public Libraries Act, which received the Royal Assent in December 1919, makes it possible for the change to be brought about locally at any time. The same Act also abrogates the Museums and Gymnasiums Act of 1891, under which a rate of $\frac{1}{2}d.$ in the £ might be levied by a local authority for the maintenance of Museums. A County or Town Council may constitute itself as the Library Authority, and bring all public Museums under its control, though it is in the discretion of this authority to appoint a separate committee for Museum management. The amount of the rate for maintenance for any year is to be decided by the Library Authority, no limit being fixed. Further, it is provided that a county which has adopted the Libraries Acts may borrow for the purpose of these Acts, as for the purpose of the Local Governments Acts, 1888; sixty years is the time period laid down for repayment of loans.

The Libraries Act, 1919, thus provides for the adequate maintenance of Museums, if local authorities choose to exercise their powers. It also enables the raising of capital sums for buildings and fittings.

Furthermore, official recognition has now been given to the Museum as an auxiliary factor in public education, but we desire at once to point out that the recommendations of the Committee on Adult Education for the transference of Museums by an Order in Council to the control of the Board of Education may, if pressed too far, seriously prejudice the functions of Museums as conservators of material and centres of research.

The Functions of Museums.

Before considering the questions specially raised in the terms of reference to the Committee, and in order to clarify the subject, it seems advantageous to state what seem to the Committee to be the proper functions of a Museum.

Museums are of many kinds. There are institutions which rank as Museums in one sense, yet have no collections; such is the Whitechapel Art Gallery, which educates through loan exhibitions. There is at least one Museum in the United States which has only a director's office, since all its possessions are always out on loan. The Circulating Department of the Victoria and Albert Museum carries out the same idea on a larger scale; but by its historical development, and in the general acceptation of the term, a Museum is a place where objects appealing by their form, not by the written word, are preserved for reference and study.

The aims and functions of this last kind of Museum are :—

1. Collection of works of Nature and of man. Collecting may be through work in the field, through purchase, and through donations. The first of these is the most valuable as assuring accurate data of provenance. Obviously, the function of collecting must precede all others.

2. Preservation of material thus collected. Much of this is the irreplaceable groundwork of human knowledge, and ought to be safeguarded at all costs. This is the necessary second function.

3. Study of the collected objects. This is the research side of Museum work, and, whether carried out by the staff (as in large measure it should and must be) or by specialists under the direction of the staff, it must be prosecuted if Museums are to fulfil their highest function, which is the advancement of Science, Art, and Industry.

4. Classification of Museum material, so that each specimen is readily accessible to future students.

Functions 3 and 4 are the necessary preliminary to those which follow.

5. Publication of the results achieved and of guides to the contents of the Museum.

    (*a*) By printed memoirs, catalogues, summary lists, and guide-books.

    (*b*) By the exhibition of specially selected series of specimens in an arrangement designed to bring out some definite information, and provided with labels written for the same end.

    (*c*) By the loan of material to other Museums, exhibition galleries, schools, and similar institutions.

    (*d*) By lectures in or outside the Museum, in the galleries or in a lecture-room, on the ordinary exhibited series, or on specimens selected *ad hoc.*

Whereas (*a*) is largely connected with the function of research (3), this in part, and sections (*b*), (*c*), (*d*) entirely, constitute the educational side of Museum work. The greater the weight attached to this function, the greater the need to realise that it must be based on those which precede.

Though implicit in the above statement, certain points require emphasis for our present purpose.

The exhibition of all material is undesirable.

Special material liable to loss or damage (*e.g.*, from light) should be withheld from exhibition.

Access to exhibition cases by scholars is undesirable; if specimens are to be handled they must form part of a special teaching series.

Loan collections for scholastic purposes should consist of easily replaceable material.

Museums in 1914.

A. *Establishment.*—A fair idea of the general character of Museums as they existed in 1914 was obtained by the issue of a lengthy questionnaire, to which the authorities of one hundred and thirty-four Museums replied. Two of these Museums were privately owned, twenty were the property of Institutions or Societies, and nineteen belonged to Universities, Colleges, and the large Public Schools. Ninety-two were municipally owned. The governing authorities were even more diverse than the ownership, and it seems probable that many Museums, though more or less public in character, will remain unaffected by recent legislation.

The replies to the questionnaire showed that Museums had arisen in various fashion; possessed widely diverse governing bodies; and were supported in a great variety of ways. Purpose was as diverse as origin and control. Those owned by municipalities had, however, gradually moved towards a common type and common standard, owing largely to the work of the Museums Association.

Governing authorities consist of elected Councils, Museum Committees, Scientific Societies, Library Committees, Members of Town and Corporation Councils, Boards of Directors, Subscribers, University Senates, &c. Sixty-four were supported by Borough and District funds or by a Library rate, thirteen had voluntary contributions, twenty-one had subscribers, eleven had invested funds and donations, ten received admission fees, and two were supported by Societies. Fourteen received funds from a University chest, a College, or a School fund.

Annual income varied from 6*l.* to 11,000*l.*, the greater number having an income of less than 2,000*l.* Notwithstanding lack of income, most Museums opened every day, and forty opened on Sundays. The value of the collections in public Museums is probably beyond calculation, and the buildings themselves represent a very large capital value. The following figures are indicative of the actual position : Three Museums have buildings of a capital value of from 200,000*l.* to 300,000*l.* each, two from 100,000*l.* to 200,000*l.*, nine from 50,000*l.* to 100,000*l.*, and twelve from 10,000*l.* to 50,000*l.* The collections of one Museum are valued at 320,000*l.*; another, 250,000*l.*; four at 100,000*l.* to 250,000*l.*

The most widely represented sections of Museum work are : Zoology, 80 Museums; Geology, 75; Archæology and Antiquities, 60; Fine Arts, 40;

Botany, 35; Painting and Engraving, 30; Engineering, Geography, Commerce, Chemistry, Physiology, Physics, and Astronomy are slightly represented in existing collections.

It may also be pointed out that many towns of considerable size are not provided with Museums. Possibly recent legislation may find its best fruits in the filling up of these gaps.

B. *Museums and Schools.*—The Committee's inquiries show that Museums collectively have, on their own initiative, anticipated in a striking way many of the requirements considered necessary for the needs of schools, a fact which illustrates their readiness to co-operate with the educational developments foreshadowed by the Education Act of 1918. It shows also that educational work on the side of Museums is possible without injury to their other functions. Given an adequate staff and an increased maintenance income, Museum curators, in conjunction with the teachers, will be able to work out suitable methods for the educational use of their collections. The experience so far gained goes far to show that the training and opportunities of teachers do not enable them to realise for themselves the possibilities of Museum collections as aids to education.

Many British Museums have for years encouraged visits from schools and classes, either under the leadership of their teachers or the guidance of a member of the Museum staff. In some cases these visits have been systematised by arrangement with the educational authorities, and the Museum collections have been studied according to a pre-arranged plan. A great development of this system was tried with success in Salford and Manchester under special conditions arising in connection with the late war. (See page 279 *et seq.*)

Other Museums have established systematic courses of lessons to school classes upon a plan jointly agreed upon by the Education and Museum authorities.

Sheffield has for years maintained circulating collections of special groups of objects for schools.

Replies to the Committee's questionnaire showed that for many years similar work of a less systematic kind had been carried on in many centres. They showed that :—

1. Instruction was given by the teachers alone in twenty-eight Museums.

2. Instruction was given mainly by teachers, the curators sometimes assisting, in twenty-four Museums.

3. Instruction was given by the Museum staff in sixteen Museums.

4. Sketching parties, classes, and individual students from Schools of Art were regular visitors in most Museums.

The experience gained by the use of Museums in education in the United States is so extensive, and has been so thoroughly tested, that it is desirable in any educational plans for the use of British Museums that this experience, the methods adopted, and the nature and extent of the work, should be carefully considered. (See page 277 *et seq.*)

C. *Museum Guides.*—From returns supplied to the Committee, only two Museums, other than National, had established official guides; one was a rate-supported Museum, the other owned by a Society.

That the need was recognised, and met as far as possible, is shown by the fact that seventy-four Museums stated that arrangements exist whereby the curator and his staff give demonstrations to special parties.

Financial stringency and inadequate staffs alone had prevented this work from becoming a well-developed branch of Museum work. It is eminently desirable that means should be available either for the employment of special officers for this duty, or for enlarged staffs, if the time of curators is not to be unduly taken up by work which is not the most important of their duties.

Museums in Relation to the General Public.

The special work of this Committee concerns the Museum as a factor in education. The words 'General Public' are used in the widest sense. We propose, therefore, to consider first the educational work the Museum may do for the general public. The term covers a wide range of needs. It represents the vast majority of visitors to the public Museums; we may safely regard them

as having little or no special knowledge, and a very large proportion of them enter the Museum without any specific purpose. They are just 'looking round.'

For such people the Museum may do great service, if it sets about it in the right way. By some means or other it should strive to put them *en rapport* with the purpose of the Museum. This purpose, for them at least, is to reveal one aspect or other of an ordered universe to people largely uninstructed. These casual visitors are easily overwhelmed by multiplicity of specimens and of words. The first essential is a definite scheme, carried out with simplicity, boldness, and clearness. Elaborate labels, completely logical series, and involved argument do not assist them.

What the scheme is to be, and its detailed carrying out, must vary with the locality and circumstances. We may, however, suggest that every Museum should illustrate fully in its exhibition cases the local fauna and flora, geology, archæology, history, industries, and art. The local natural history should be treated from an ecological point of view, so that the visitor may not only realise what is to be found locally, but will learn under what conditions it is found, the associations of which it forms a part, why certain animals and plants are found locally, and why others are not, and the relations of the fauna and flora to the local geology.

It is also important that the general public should realise the changes in the local fauna and flora which have been brought about by the growth of civilised communities and the activities of the human race. Every locality will provide instances of this fact, and every Museum should make a point of illustrating it for the locality which it serves.

Where towns or localities in which Museums are situated possess special industries, such industries should be illustrated in the local Museum, both historically, by showing the growth and development of the industry in the town, and the products of that industry at various periods; and technically, by illustrating the various processes of manufacture and technique. The Museum should possess a representative collection of present-day industrial products of the locality, and, by keeping that up to date, the industrial history of the district will always remain clear. Similarly, the Public Health work of the district should be adequately illustrated in the Museum. The general public should look to the local Museum for information on all the various activities going on around them. If they are sure of that, their powers of observation will be stimulated. They will go to the Museum to learn what ought to be seen, its why and its wherefore. If they see anything new, they will go to the Museum for information. Both Museum and public will thus be mutually benefited.

It follows that in Museums reference collections of everything local should be as complete as possible, not necessarily exhibited, but so housed as to be readily available for the use of those who desire to consult them. By this means it is possible to turn the general public into real students, which is the ultimate aim of this branch of Museum work.

The Reference Library attached to the Museum should be accessible to visitors, who should be encouraged to use it. The library might also make a special feature of local publications and all books dealing with the locality from any point of view. Reports of local societies and organisations should be made a strong feature.

A Museum which remains entirely local misses something of high educative importance. To interpret the local fauna and flora rightly means an acquaintance with a wider range of facts, and the Museum should try to provide that wider setting which will give meaning to local phenomena. In providing this wider range of specimens, the well-conducted Museum will in its total form resemble an iceberg of which only one-tenth appears above the water-line. The cases will be suggestive and directive, not complete and confusing. Everybody should know, however, that there is much more material in the recesses of the Museum than is shown in the public galleries, and that this is quite accessible to all who have an intellectual need which it can satisfy. Reference collections should be as complete as possible within the range they cover. Better a narrow range completely represented than a wide one with many gaps. Index collections are necessary in all large Museums, and smaller Museums might well arrange their cases on the principle of the Index collection.

Temporary exhibitions are a means of attracting the attention of the general public. They offer an opportunity of driving home a particular lesson, and suggest a wide field of activity for the living direction of a Museum. Similarly, the frequent changing of exhibits in the cases will add to the attraction, though aimlessness must be avoided here as everywhere in Museum work. Crowded cases of birds, which are not merely inartistic and ugly, but are also wholly ineffective, are characteristic features of many Museums. Their contents ought to be re-arranged on some definite principle. In many cases, no doubt, this overcrowding is due to the absence of store-rooms in the building.

The general public may be helped by the Museum authorities in a variety of ways :—

1. For general use a Guide, simply written and dealing with the collections in the order in which they should be viewed, is best. Handbooks to special sections are also useful, and often command a ready sale. Publications should be as cheap as possible, and, if sold at some loss, the sale is merely carrying a step further the provision of free specimens, free labels, and free lectures.

2. The part played by an official guide in a small Museum will probably differ considerably from that of his colleague in a large Museum. It may be found that the 'conducted party' is not easily got together in a small Museum, and that the services of the guide will not be greatly in request. For societies and other special parties he will be useful, and also for informal demonstrations. Schools will make use of him, but this is not always necessary, as the teachers themselves may be competent.

3. Lectures play an important part in attracting visitors, in developing interest in the control of the Museum, and in stimulating further study. A lecture-room should be provided in every Museum.

Museums and Schools.

The services of the Museum to the school must vary greatly according to local circumstances. A Museum can never take the place of field work in Nature Study, or Geography, nor can the exhibition of historical relics, models and the like be more than a pale substitute for visits to historical sites, buildings, &c., when these are accessible, though an enterprising curator may gather within walls significant fragments from the past, and by careful arrangement do much to help the young to build up pictures of the old times which will make history something more than a mere matter of words. The success of the Museum as an educative agency depends very much upon the skill with which it suggests a world of reality outside the Museum itself. The difficulty of managing this varies according to the experience of the visitor. The aimless wanderer whose curiosity may be awakened and directed by good Museum arrangement is not, however, here in question. The schoolboy is, *ex hypothesi*, under personal guidance or under the guidance of ideas and problems which the schoolmaster has inspired. How can the Museum encourage and assist him?

We have already noted activities in this direction actually in existence in 1914, and the Manchester experiment is a later development. Much may also be learned from the practice of the Overseas Museums. The Committee's inquiries have led to the following conclusions :—

There are, broadly speaking, two types of use to which the Museum lends itself.

1. Its collections may be used to illustrate a particular course of instruction and reading which is part of the school curriculum. A class which is studying Australia may visit the Museum to see specimens of its fauna and its minerals, and to study the ethnographical collection. Here the Museum renders ancillary service of the highest value. A close acquaintance with the school curricula in the locality seems desirable, and temporary exhibitions might be arranged if co-operation between school and Museum services could be secured.

2. On the other hand, the Museum collection as such may be an object of study. The work will be very different in character. It centres in the Museum which is, as it were, a textbook in material form, and the instruction aims at making this material intelligible. It is clear that for work of this kind the ordinary logical arrangement of the Museum cases will in many ways need revision. Teaching must take its start from the pupils' own minds, and select its material

from the point of view of its relation to the world they know. It is the psychological order that must be followed, and the logical arrangement will determine the end, not the beginning, of the course.

It is precisely through the difficulties which this involves that the trained teacher is more likely to be successful in this type of work than the curator or his assistant, but the average teacher has neither the knowledge of the subject nor the command of the Museum's resources which are essential. The Manchester device has much to teach us in this regard.

Whether or not a particular Museum is suited to this kind of work will depend partly upon its contents and its buildings. A lecture-room equipped with lantern, with various devices for showing specimens at the lecture table, and with tables rather than sloping desks, in order that specimens may be handled by the class, seems absolutely essential.

System and purpose must govern the use of the Museum by schools. The aimless wanderings of groups of children about the galleries is sheer dissipation, a nuisance to the staff and to the public. Enough has been said to indicate the lines which may be followed. A keen teacher will find something helpful even in a dead Museum, and a living Museum will lay itself out to seek the advice and help of teachers in the attempt to play its part in the educational service of the locality.

We may especially note the possibilities, which have so far been only slightly developed, in the provision of circulating sets of illustrative objects designed for school use. It is essential, however, that such sets should avoid the error of over-systematisation, especially for use in primary schools. The contents of the cases should be determined by the point of view of those for whom they are designed, and not by a specialist who knows his subject as a systematised body of knowledge, but has no conception of what his specimens will mean to a person who is both young and uninstructed.

Museums in Relation to the Advanced Student.

This question has to be considered from two distinct, and at times conflicting, points of view—viz., the needs of the particular class of student; and the needs of the Museum and its staff in relation to other calls. These students for the present purpose may be divided into :—

1. Research Students.
2. University Students.
3. Private Students.
4. Collectors.

The number of Museums which can render material aid to the advanced student is limited, although it must not be forgotten that even the smallest Museum usually possesses one or more objects of scientific or artistic value.

The demands made upon the Museum staff by the advanced student are serious. Not only is a great amount of time consumed in providing material, but almost invariably calls are made for enlightenment upon points which arise in the course of his inquiry; he needs frequently to discuss his conclusions with a specialist member of the staff.

But advanced students vary in type, and their needs are best considered separately.

The Research Student.—This student is frequently a man of established scientific reputation. He requires only original material, and this must be furnished with full data of provenance, evidence of its type distinction or other points of interest, and full references to literature. Probably all other specimens of similar type possessed by the Museum will be required for comparative study, and all these should be supplemented by information similar to that of the study specimen or specimens.

When the material cannot be brought together in a room suitable for study, provision for the accessibility of the series is necessary, the Index catalogue will be required, and cases and cabinets must be open. When required specimens are on exhibition, as they needs must be in many cases, it will be necessary to dismount them, or if this is impossible, work must proceed at the open case, a most undesirable method. For the satisfactory work of a researcher, a well-lighted work-room is essential.

Access to a good library is also essential, and very little good work is done if books are only available for short periods on loan.

Instruments of research will be required, such as microscope, preparatory and developing tools, photographic appliances, &c. Everything which the researcher requires for the thorough prosecution of his studies ought to be at hand.

The needs of the researcher at a distance entail less calls upon the staff, and are usually restricted to the loan of specimens, plaster casts, and full details of provenance, &c.

The University Student.—Under this head we include the students of all advanced training institutions, whether University or not. Where calls are many, and on similar lines, it may be possible to form special ' Students' Series,' from which the student can gain a practical knowledge of his subject. Either lack or excess of material beyond what is absolutely required is a disadvantage. The material will be chosen for its instructive value, not for its attractiveness. A special Student Series can be kept on exhibition, from which it is not removable, or in cabinets, so that its use throws no additional work upon the curator. Special Student Series should be available for use by the lecturers to advanced students, as well as by the students themselves. If required in the lecture-room, the collection should be kept in drawers, and not exhibited, unless special rooms, not open to the general public, are provided. Exhibited series, which have been specially selected, arranged, and labelled in the public exhibition cases, at times fall under the head of Special Students' Series. These collections should have references to the literature dealing with the subject. The arrangements should allow students to use note-books, or a small table. Most, if not all, of the specimens in the series will have to be handled by the student, and numbers corresponding to a catalogue list should be painted or otherwise fixed upon each.

University students frequently work in pairs, or small numbers, and need to have several specimens before them at a time. Suitable rooms must, therefore, be provided for their special use.

There remains the larger question of co-operation between the University and the Museum. The public Museums in most of the University towns are large and well provided with collections, in which there is a considerable store of material suitable for research. The staffs usually include one or more trained scientific men, and more such are being attracted to permanent Museum work. We believe that, with good will and intelligent co-operation between the University and the Museum, difficulties will disappear, to the great gain of both institutions. For University professors to restrict themselves to a Museum of their own is to restrict themselves and their students to a limited field of observation and research, as the public Museum is always likely to contain and receive more material in any subject than the University is normally likely to acquire. The curator, on the other hand, must conserve, suitably labelled, and keep in good order, extensive collections which are of primary value both for teaching and research. Mutual aid cannot fail to bring in good results. The professorial staff of the University can help by furnishing scientific or artistic knowledge, and by supplying dissections, prepared in the laboratories. They can assist the curator in the preparation of exhibition groups of current scientific importance, and can in turn profit by a first-hand acquaintance with material in sections of their own science, which may not fall within the range of their ordinary professorial teaching.

Such a co-operation is merely extending to independent Museums what is already done in those directly connected with a University, as at Oxford, Cambridge, Liverpool, and Manchester. It is, however, in the United States that the best examples of co-operation are seen. (See Appendix I.)

The advantages of this intercourse are many. Expense is shared instead of duplicated, and through the intermediary of the Museum the public is brought into close contact with the higher seats of learning. The Museum comes into its rightful place in the fighting-line of science, and the student comes face to face with actual problems and gets a close grip of connected facts; he in fact does work under more real conditions than in the forcing house, or laboratory. But the student, too, may fairly be asked to help by doing some curatorial work. Instead of disarranging and damaging Museum specimens, let the post-graduate prepare, determine, label, and arrange a limited

group under the joint direction of professor and curator. The Museum will profit by the improved arrangement of the objects, and the student will learn how to utilise specimens, and how to discover and use the relevant scientific, or other, literature. Such work will also give him a solid foundation of systematic knowledge too often wanting even in the best products of modern education.

The Private Student.—This student is usually either working for an examination or to perfect himself in a subject which appeals to him. If of the former type, he may find all he wants in the exhibition series, with a little guidance from the curator, or it may be necessary to give him access to the series used by the University student. The needs of the second type of student will generally be amply supplied by the explanatory labels, and occasional access to books.

The Collector Student.—This student usually visits the Museum in order to identify specimens in his own collection, and therefore requires access to systematically arranged and stored collections. As the exhibited series will not, or ought not to, contain the long series of specimens he desires to see, these students will be best served and helped if Museums used by them provide one or more well-lighted work-rooms, with large tables, adjacent to the reserve series, which should be kept in strict systematic order, and fully labelled. The rooms containing the reserve stores can frequently be used as work-rooms for the advanced students also, and with great advantage, as specimens can be returned to place at once when done with, before others are taken out.

Museums in Relation to Classical Education and the Humanities.

Much of what might be written upon this branch of Museum work has already been published in book form by a member of the Committee, and need not be repeated again here ('Our Renaissance : Essays on the Reform and Revival of Classical Studies,' by Professor Henry Browne, S.J. Longmans, Green and Co., no date of publication).

Professor Browne rightly urges that, whilst the subjects of Natural Science may claim priority, Museums which neglect the promotion of the love of culture and art among all classes will be incomplete and one-sided.

Teachers of Ancient History may reasonably expect to find, in Museums, collections having a bearing upon the subjects they teach, and in some of the large provincial Museums this is already the case. No Museum, however, need consider itself too poor or remote to be able to do something towards illustrating ancient life.

The objects required for the purpose can be so chosen as to appeal to the public as well as to the student, and success will depend upon the exhibition of such material as ought to interest persons of average intelligence and education. The bringing together of local or other evidence of the Roman occupation of England, for example, will attract every one. Much can be done by the provision of reproductions (including casts and electrotypes, as well as photographs and slides) of objects not otherwise obtainable. Many of these being replaceable, could be loaned to schools and colleges.

From a special questionnaire much information was obtained, and can be found set out in full in Professor Browne's book. It will suffice if a summary only is given here.

Practical evidence of the value to all grades of education by such collections in Museums was supplied by the authorities of the British Museum, and of the Museums of Oxford, Cambridge, Liverpool, and the Universities.

In this instance, again, American Schools of Learning and Museums are far in advance of the British Isles. The greatest development has been reached by the Museum of the University of Pennsylvania, Philadelphia, which is a University and Public Museum combined. The classical and art collections are remarkably good, and fully used by the University professors, who find that the interests of students are stimulated and encouraged, whilst their studies take on a more real character than from bookwork alone. The Boston Museum of Fine Arts is doing much useful work, and teaching is carried on upon a large scale. The collections are used by the students of Harvard, and

by Public Schools. In one year lectures were delivered to nearly 12,000 people by 'Docents' of Academic distinction. The Museums of most of the American Universities possess a strong classical section, and all are used for instruction purposes by the professors.

The Archæological Institute of America is steadily encouraging the extension of classical teaching, and specially promotes a knowledge of ancient cultures. It also gives support to, and derives material from, the Archæological Schools of Athens, Rome, Jerusalem, and Santa Fé (New Mexico), and publishes journals which have a large circulation. Forty-five local branches, arranged in four geographical sections, are in operation, and from 200 to 250 lectures are arranged for yearly. The study of classical history will probably increase in the near future by reason of the renaissance which Eastern countries will experience as the result of the recent war, and collections which illustrate, even fragmentarily, some features of their ancient history, will be of great service, but any extensive development of such collections will necessarily be restricted to the large cities and University towns.

Principles of Museum Administration, Maintenance, and Staffing.

The determination of any general principles of government and administration does not appear to have been formulated or applied to British Museums. Neither are British Museums, or their governing bodies, referable to a common standard, even when they exist mainly for the benefit of the general public. The Libraries Act, 1919, may eventually secure uniformity of government for county and town Museums, although it is optional for a county or town to place its Museum under the control of the Library Committee, or under a distinct Museum Committee. The many public, or semi-public, Museums, owned by societies and private bodies, are not recognised by the Act, and no inducement or provision is made for their transference to the constituted public authority. These will, therefore, remain under their present diverse methods of administration, and be governed more in the interests of the societies than in that of the general public.

This is to be deplored, as many of these Museums contain the nucleus of a good public Museum, and many national treasures, yet cannot develop sufficiently for want of funds, whilst they are large and important enough locally to hinder the formation of a wholly free public Museum. No fixed rate per £ can be devised applicable to all Museums, as the differential rateable values, population, and requirements of towns cannot be brought to a common standard. It is possible, however, to fix a minimum income based upon the cost of maintaining a trained curator at an adequate salary, one attendant, and cleaners. (No curator of average ability ought to receive less than 300l. per annum, and this amount, added to the wages of attendant and cleaners, will, with the fixed charges, and at least 150l. for purchases, printing, &c., entail a minimum cost of 800l. per annum.)

It is essential that each department of a Museum should have a definite sum allocated for purchases, mounting, labelling, &c., otherwise the balance of sections is likely to be destroyed by the enthusiasts of one or more sections. This is a prime fault in provincial Museums.

The principles and cost of maintenance of a Museum are questions not always capable of settlement by the local governing body, and these would profit considerably if it were possible to seek the advice of some recognised national or central authority fully conversant with the cost, maintenance, and development of the various departments suitable for a Museum in a given town or district. The help of such a body of experts would also assist local Museum Committees in securing a suitable balance of departments and an economic expenditure.

In order that the Museums may fulfil their proper mission in the community, it is obvious that the question of staff is of the first importance. The old practice of uniting the functions of librarian and Museum curator is vigorously and unanimously condemned by the Committee. Progress of Museum work depends upon the formation of an adequately paid corps of specialist workers. At present all Museums are understaffed. Even in small Museums, a single curator cannot be sufficiently well informed to arrange and label all his material.

Large Museums will require two or more members of staff, each of whom should have extended his general preparatory training by special study of those groups with which he is called upon to deal.

The National Museums have for long specialised the work of their members of staff. They require of them a high standard of attainment. A few of the provincial Museums have secured directors and curators with recognised qualifications in science or in art, but the greater number of curators are either self-trained or have acquired their special knowledge on other than systematic lines of study. Notwithstanding the lack of preliminary systematic training, most curators have acquired qualifications which fit them adequately to perform their present duties. It is evident, however, that future Museum work will be best served if Museum assistants and curators have previously passed through a systematic course of instruction in those sciences and arts likely to be needed in their subsequent work. A sound University training in letters or in science must soon be a *sine quâ non*. It is also desirable that the elements of Museum technique should be taught whenever means can be devised. A reading knowledge of French and German is almost indispensable.

It is generally admitted that Museum officials are badly paid—that is, their stipends are less than similar attainments and powers of mind would earn in other walks of life. Considerable improvement has taken place in this matter during recent years, but adequate progress in this direction 'can only, be effectively made when the curatorship of a Museum is looked upon as an honourable and desirable profession for men of high intellectual attainments' (Sir W. H. Flower).

Some General Conclusions.

Many of the recommendations of the Committee are embodied in the Report, but there are one or two general questions which may be briefly referred to here.

1. In the view of the Committee, Museums can and should be developed into centres of research. This may be done partly in co-operation with Universities. Much unworked material lies in many Museums, and a wide field of useful research lies open, if suitable facilities for the work are provided.

2. It would assist research if an official list of the principal contents of all provincial Museums could be published by the Board of Education. This list would also doubtless indirectly increase local pride in the collections and so add to the steps taken to secure safe custody.

3. All Museums suffer greatly from want of funds. If educational work and research are to be developed, grants-in-aid on a liberal scale are absolutely essential. There is some fear that Museum funds may be seriously diverted from what all authorities agree to be their first aim—viz., the advance of knowledge, for the more popular ventures in connection with the schools. Research must be regarded as the first function, at any rate, of all the greater Museums. Some principle of grading of Museums for purposes of grant might be adopted, based upon the work they are doing or planning.

4. Curatorial functions demand a high degree of special knowledge and training. The Universities and the National Museums have a duty to the nation in this respect.

5. In reference to the work Museums may do for schools, the Committee believes that the system of special circulating loan collections for schools, so highly elaborated in the United States, deserves wider extension in this country: and it recommends that, to pay for the necessary material and the special staff required, appropriations should be made from the Education grant to those Museums which are prepared to carry out the system.

APPENDIX I.

Sub-Committee's Report upon Overseas Museums.

(a) *Australia.*—The Committee's delegates visited the Australian Museums at Perth, Adelaide, Melbourne, Sydney, and Brisbane, and found that the general work of each was carried out on similar lines to that in British Museums. The fundamental purpose of Museums is well maintained, all material bearing upon

the aborigines, native plants and animals, and the mineral resources of Australia is carefully conserved. The Art Gallery attached to the Melbourne Museum takes special cognisance of examples of Australian Art. Public lectures are given at all the Museums, and schools and classes encouraged to visit them, the Museum staffs giving demonstrations and lectures whenever possible. Special student collections are being made at Perth, Adelaide, and Brisbane. The Technological Museum, Sydney, loans collections to illustrate lectures given at the Technical Colleges and Nature Study in schools. Timber, minerals, building and ornamental stones, &c., are in process of collection in order to constitute an exposition of the mineral wealth of the country. It also sends out collections of native material to distant towns and schools.

The educational work of the Queensland Museum, Brisbane, is on a more extensive scale than elsewhere. Carefully graded lectures are given by the Museum staff to classes from elementary and secondary schools, and special time is allotted to classes for definite studies. Higher education and research receive special attention, research being specially encouraged.

(b) *The United States.*—Adequately to describe the educational activities of the American Museums would require a large volume. The Committee's delegates visited those at Chicago, Pittsburgh, Washington, Philadelphia, Harvard, Boston, Brooklyn, and New York. Time did not allow of more extended visits, but information was readily obtained from all to which application was made.

The Museum of Natural History and the Metropolitan Museum of Art, New York, the Museum of the Academy of Sciences, and the Field Museum, Chicago, the Carnegie Museum, Pittsburgh, and the Museum in Philadelphia were pre-eminent amongst those visited for their extensive schemes of educational work in connection with the Public Schools. They are also actively associated with higher education and University work, as are the Museums of Washington, Harvard, and Boston. The work of the Natural History Museum, New York, may be outlined as an example of what most American Museums are doing to aid education, as it has probably done more work of this character than any other Museum in the world. Large circulation sets of Nature Study collections have been prepared, and in 1913 were sent out to 501 schools by means of special motor vans. These collections were used by one and a quarter million of pupils. The study collections number about 600, and have been much increased since. Special teaching collections are set up in the Museum, and class-rooms and lecture theatre are available for use at any time.

Members of the staff frequently lecture to the children and to the teachers, whilst a special guide service is maintained. Special provision has been made for blind students, who are permitted to handle specimens. It is said that they gain in this way quite a remarkable knowledge of the form and adaptations of animals.

A Lantern-slide Department has been organised for some years, and now possesses over 30,000 slides, which are loaned in series to schools for teaching purposes. At the time of the delegates' visit the formation of branch teaching Museums was under consideration, and the establishment of ten Lecture Centres in various parts of New York.

The needs of higher education and research have been met by an arrangement with the Columbia University; the professors lecture to their students at the Museum, and hold the position of curators in the Museum in their several subjects.

The Metropolitan Museum of Art, New York, retains expert guides to assist its members, teachers, and schools when visiting the collections. The service is free to teachers and schools. The city maintains one paid lecturer. Regular visits are paid from schools for instruction in the History of Art. The University and Museum are in close co-operation, especially on the classical and historical sides. It has been suggested that a Faculty of Arts should be established in the Museum, with lectures for special collections. The Boston Museum of Fine Arts and the Art Institute of Chicago are doing similar work; in the latter case, a School of Art is maintained in connection with the Museum.

The Field Museum of Chicago has entered upon an ambitious scheme whereby a specially prepared series of Circulation Collections will be available for schools in the city area. A quarter of a million of dollars was given for this

purpose, and has since been increased. The work of the Academy of Sciences, Chicago, and of the Children's Museum at Brooklyn, is remarkable in that the children are encouraged to take part in the Museum work, by the maintenance of aquaria, the loan of specimens to be taken home, and the preparation of costumes, &c., to illustrate the clothing of various periods and nations. Classes in the natural and physical sciences are taught in the Brooklyn Children's Museum by the staff, and many children have become expert in wireless telegraphy, blow-pipe analysis, and in the use of other scientific instruments.

The Public Museum, Milwaukee, possesses a full lecture system reaching all sections of the public from the elementary schools upwards, and has established a Science Club for High School Students. Arrangements are made for all Public-school children of certain grades to visit the Museum twice yearly.

The American Museums make free use of Museum and Art 'Docents' for the delivery of lectures and demonstrations. These ladies and gentlemen are chosen for their special knowledge, and are maintained either by the Museum or the city, or partly by both, or in some cases, as at the Brooklyn Institute of Arts and Sciences, by an Art League. The two 'Docents' of this Museum lectured to 114,000 pupils in one year.

The American Museums are in a position to undertake this valuable educational work owing to—

1. Large gifts of money from wealthy persons.
2. Large staffs of enthusiastic workers.
3. The association with every Museum of a large body of rich and cultured people, who are themselves interested in the collection and study of Museum objects; they frequently give large sums of money for general maintenance, or earmarked for special purposes, and also present private collections.

As educational work extends, the popularity and usefulness of the Museums increase, and their purpose and utility become more highly valued. The attainments of the staff are recognised, and opportunity given for the prosecution of their own line of research.

An essentially American feature in the formation of new Museums is the preparation and publication, long in advance, of full plans of intended new buildings, and their free display in journals and public places. Reduced models to scale are also prepared of the suggested buildings, and these are exhibited to the public. These people do not hesitate to embark upon schemes which will take years of work to accomplish. Definite steps forward are taken as opportunity arises, but the whole scheme is kept prominently before the public as an earnest of the future and a stimulant to gift.

APPENDIX II.

Manchester Scheme.

At the beginning of the war several of the elementary schools in Manchester were taken over for military hospitals, and the scholars, therefore, temporarily dispossessed of accommodation. The Education Authorities thereupon instituted a half-time system in certain of the remaining schools, in order that the dispossessed scholars should receive, at least, some instruction. It was decided in these half-time schools to fill up part of the remaining half of the scholars' time by visits of educational value to various Manchester institutions and other places of local interest. In this connection the Keeper of the Museum, in consultation with the Education Authorities, organised a scheme whereby the scholars attended at the Manchester Museum for courses of lessons in Natural History and Egyptology. The main points of the scheme are as follows :—

1. The classes are limited to twenty in number.
2. The classes are, as far as is possible with existing accommodation at the Museum, provided with separate class-rooms, seating accommodation, desks, &c., so that the lessons are given as nearly as possible under school conditions.
3. The teachers are trained teachers on the staff of the Manchester Education Committee, who have also a special knowledge of the subjects illustrated in the Museum. They have been specially appointed to this work by the Education Committee.

4. The classes do not come for detached lessons, but regularly once a week for organised courses of six to nine lessons on one subject.

5. The Museum authorities provide duplicate specimens for the use of the classes, and the staff of the Museum render as much assistance and help in this way as possible.

6. A lesson, broadly, consists of about forty minutes' tuition in the class-room, after which the class is taken into the Museum and shown the cases illustrating the subject of the lesson.

At first eight classes in Zoology and Geology were held daily, and from 900 to 1000 children per week shared in the lessons. The scheme proved remarkably successful, and two additional teachers for Botany were added the following year. Class-room accommodation was a difficulty, and a part of the Museum had to be shut off for the purpose. It became clear, however, that work of this character requires a Museum lecture-room and class-rooms to obtain the best results. The increased demands upon the time of the Museum staff in providing material were considerable.

By 1916-17 the scheme had so far proved its value that four teachers were specially appointed by the Education Committee to conduct Museum classes throughout the year in Geology, Zoology, and Botany. Towards the end of the session a fifth teacher was appointed, whilst the assistant in charge of the Egyptological collections also conducted classes. The attendance of scholars increased to 2000 per week. In 1919-20 the number of special teachers was increased to six, and the number of scholars attending the courses to 2500 per week.

Classes from the secondary schools also visited the Museum, and were taught by their own teachers. The development of this valuable educational work reacted upon the Museum in increased public interest, whilst a considerable addition of adult visitors was brought by the scholars in their spare time.

The total attendance for the period of four years was as follows :—

1915-16	.	.	. 45,000	1917-18	.	.	. 100,000
1916-17	.	.	. 70,000	1918-19	.	.	. 130,000

It must be borne in mind that these were not discontinuous attendances, but represent the total visits at complete courses of six to nine lessons for each scholar. Some scholars attended more than one course per year.

The Manchester scheme was adopted after a close study of the educational work conducted in other Museums in this country, and especially in America. Compared with other schemes and methods, it is claimed that the Manchester scheme shows considerable advantages in the following ways :—

1. The limitation of the classes to small numbers. It was felt that to attempt to instruct large classes resulted in 'entertainment' and not 'instruction.'

2. The atmosphere of the school class-room is approached as nearly as possible.

This is valuable from a disciplinary point of view, and also avoids physical fatigue so usually attendant on visits from school children to Museums and such places.

3. The classes are taught by trained teachers and not by the Museum staff.

The advantages of this from an educational point of view are obvious. The staff of a Museum is appointed primarily for quite other work than teaching, and Museum curators do not pretend to understand the psychology of the child mind or to be trained in the art of teaching.

4. A proper balance is maintained between this branch of Museum work and the many other functions and duties which a Museum is called upon to fulfil, and the educational work can be carried on without the absorption of an undue amount of time, to the detriment of the other work of the institution.

Training in Citizenship.—Interim Report of the Committee, Right Rev. Bishop WELLDON, D.D. (*Chairman*), Lady SHAW (*Secretary*), Lieut.-Gen. Sir ROBERT BADEN-POWELL, Mr. C. H. BLAKISTON, Mr. G. D. DUNKERLEY, Mr. W. D. EGGAR, Principal MAXWELL GARNETT, C.B.E., Sir RICHARD GREGORY, Mr. SPURLEY HEY, Miss E. P. HUGHES, LL.D., Sir THEODORE MORISON.

Introduction.

TRAINING in Citizenship consists of two parts, subjective and objective. The former may be described as character-training and is concerned with the development in the individual of those qualities which fit him to take his place in a community with full appreciation of such privileges and duties as are the birthright of every good citizen.

The second part is concerned with the education of the individual in the history of civilisation and the laws appertaining to communal life which assure to every member freedom for full personal development of mind and body.

With this two-fold purpose in view it was decided to take, as far as possible in the limited time and with the limited facilities at the disposal of a small Committee of busy persons, a survey of the educational organisation of this and of other countries from which information could be acquired for practical training of the young in citizenship, and, further, to draw up a syllabus of theoretical instruction which would be capable of expansion into an authorised text-book on civics.

A letter contributed by the Chairman appeared in *The Times* Supplement for December 25, 1919, asking for help in compiling the items of the survey. From the answers to this appeal it was evident that the pressing need was for the syllabus. It did not, however, appear that one syllabus could be prepared to meet all cases. A request came from Bootham School, York, for ' short courses on the training of citizenship as well as long courses,' ' to help in a practical way schools that uphold the idea of citizenship throughout the school career, and are unable to find the time for more than a short intensive course of lessons.' Similar requests came from other schools, but the greater number of correspondents asked for an authoritative handbook of civics, and it was decided to take up this work and to meet the other varying needs by appending a selection from the specimen syllabuses and suggestions for lessons that were sent to the Committee by schools and associations interested in the work.

The Preparation of the Syllabus.

The Chairman, at the request of the Committee, drew up and circulated a detailed syllabus of civics which after criticism by the Committee was expanded by Mr. Dunkerley from reports sent in by school teachers and from suggestions made by members of the Committee and others and from his own experience. The syllabus thus expanded was again considered by the Committee and adopted by them. It is included as Appendix I. in this Report.

The Committee learned that Mr. Blakiston had in hand a Text-book of Civics designed for use in the senior classes of the Public Boarding Schools. This book has been completed on the lines of the syllabus, and the Chairman has contributed a foreword to it.

So important is it, however, that children of both sexes in all schools, and not least in elementary schools, should be systematically taught to recognise their duty to the Nation and the Empire that the Committee feel the time is opportune for issuing an official handbook upon Civic Duty ; and, if the syllabus now printed should receive the approval of the Educational Section, they desire that a handbook on the lines of the syllabus should, if possible, be issued with the authority of the British Association. They would invite

Mr. Dunkerley, with the assistance of the Chairman, Bishop Welldon, and the Secretary, Lady Shaw, to undertake the task of drawing up such a book. The Committee entertain the strong opinion that the handbook, while supplying information upon various aspects of municipal and political life, should aim especially at inculcating the unselfish patriotic spirit which would, as they believe, go far towards preventing, or at least mitigating, the industrial controversies now threatening to undermine the basis of society.

The Selected Syllabuses.

In response to their inquiry into the work now carried on in various educational institutions, the Committee received a number of syllabuses and notes of lessons which may be of use and which will certainly interest the members of Section L.

The difficulty has been to decide which to reproduce when so many were of equal value. Those appended have been selected because they include special points not common to all.

Examples of Courses in Citizenship.

Sir Robert Baden-Powell's scheme needs no comment by the Committee. Its practical value has been thoroughly tested and proved. It is reproduced as Appendix II.

Appendix III. contains the following :—

A. The Devon County Education Committee's Report, 1911. Suggestions as to moral instruction and training in citizenship.
B. The Hammersmith syllabus supplies an example of the short intensive courses for which many schools made inquiry.
C. That from Blackley, Manchester, is admirable as showing how the schoolboy may be trained to be a good citizen and patriot in the widest sense of the word.
D. Caerau Mixed School, Bridgend, gives its syllabus of lessons, and the school self-government scheme by girls and boys working together.
E. The Roath Park Boys' School, Cardiff, sets out (1) a Citizenship scheme taken as part of history, (2) the Prefect system, which includes self-government, and (3) an experiment in Scoutcraft as a school subject.
F. The syllabus from First Derry (Ireland) Boys' School is a specimen of courses in Civics followed in certain Irish National Schools.
G. That from Stobswell School is a specimen from Scotland.

Schemes of School Management.

Appendix IV. contains the following :—

Skerton Council School, Lancashire; the statement is long, but has great value as that of a school having a 'constitution.'
Cowley School, St. Helens, Lancashire. This account is written by a boy at school.
Penarth County School, Wales. This is written by a girl at school.
Roath Park Boys' School : Section on the Prefect system.
Roath Park Boys' School : Section on Scoutcraft.
High School, Glasgow, describes a system of Prefects.

Appendix V. gives Lord Lytton's Suggestions for Organising Regional Study and Maintaining a Permanent Regional Record.

Appendix VI.—Mr. Valentine Bell : Notes of Lessons in Regional Survey.

A circular letter was sent on December 12, 1919, to the Secretaries of the Head-masters' Conference and to the Associations of Head Masters, Head Mistresses, Assistant Masters, Assistant Mistresses, Private-school Masters, Private Schools, Science Masters, and Training Colleges, asking them to bring the matter to the notice of their members. It was also sent to Newnham and Girton Colleges.

Of these the Committee of the Head-masters' Conference agreed 'to print the statement contained in the letter in their terminal Bulletin' in February

of this year. The Private Schools Association inserted a notice, supplied on the invitation of the President, in their paper 'Secondary Education' on February 1. Newnham College, Cambridge, inserted the letter in their journal, the 'Newnham College Roll.' The Association of Science Masters put the Committee in touch with the Catholic Social League.

All the Secretaries promised to bring the subject to the notice of their Committees.

THE EDUCATIONAL SURVEY.

The Committee decided not to confine their survey to the United Kingdom, but to include as far as possible notes of educational methods throughout the British Empire; and, for the sake of comparison, to ascertain the latest developments in other countries. For this last purpose they have asked the help of the Education Section of the International Council of Women, which is engaged in a similar investigation. The response to the inquiry instituted by the International Council is not due in this country before the meeting of the British Association in August; and replies to the Committee's letters addressed to Japan and China have not yet been received. It is therefore not possible to present a report of the survey to the meeting in August 1920, and this must be postponed to 1921. The Section should, however, be in possession of some account of the action taken by the Committee to ensure complete information for the final report.

In Canada in 1917 a movement was started at Winnipeg ' for a National Conference to consider the bearing of Canadian Education on Character and Citizenship.' After a considerable amount of preparatory work, money was raised and the Conference was fixed for October 1919. This Conference was attended by about 1,500 accredited delegates of public offices, the aim being to gather business men and educators in one great assembly to secure the creation of a permanent body for study of the question. The Committee have received a verbatim report of this Conference, the attendance at each session of which was never less than 2,000 persons, and on one evening rose to 5,000. The report says 'the distinctive features of the gathering were its diversified representative character, the combination of citizens as such and professional educators, and the spirit of lofty ideality in the interests of the nation that animated all.' The outcome was the creation of a National Council of fifty members, thirty-six to be elected from the various provinces by the Conference itself, and fourteen to be elected by the Council as so far constituted. This Council met in February of this year, and the report of their proceedings has been supplied to this Committee. Inquiry has now been made of four prominent men whose names were supplied by Professor Macallum, of Toronto University, asking what effect, if any, has been produced in the schools or in any way, and also asking for information as to the training given in the schools. The Committee hope some similar action may be taken in the Mother Country.

A list of voluntary agencies for dealing with Civic Education in the United Kingdom is being compiled. Meanwhile the Committee have obtained particulars of methods of work from the National Federation of Teachers, the Schools Personal Service Association, the Citizenship Studies Association, the Union of Educational Institutions, the Union of Lancashire and Cheshire Institutes, the Workers' Educational Union, the Catholic Social League, the Civic and Moral Education League, the Federation of Women's Institutes, and the Cavendish Association. A circular letter has been sent to thirty-six head-masters and sixteen head-mistresses of public schools, to two mixed schools and to two private schools. The answers received will be considered in next year's report.

Application was made some years ago by a Committee of Section L to the Board of Education for lists of private and of charitable schools, and County Directors have now been asked for lists of such schools in their respective areas, but neither the Board of Education nor the local education authorities keep such records. Further efforts will be made to obtain the necessary data to complete this part of the survey.

From the Board of Education the Committee have received copies of 'Suggestions for the consideration of Teachers and others concerned in the work of

Public Elementary Schools' and of the 'Syllabus of the Board's Final Examination of Students in Training Colleges, 1922.'

There are sixty-eight County Directors of Education in England and Wales. A questionnaire was prepared for these, and Miss E. P. Hughes undertook its circulation to the twenty-nine County Directors in Wales. In addition to the thirty-nine questionnaires distributed in England, personal letters were written to eight other Directors of Education. The response was not complete, but a mass of valuable information is in the hands of the Committee to be dealt with later.

Much time has not elapsed since the appeal was sent to Scotland and Ireland, but some returns have already been received. A considerable amount of work remains to be done before the survey can be considered to be complete.

Since this report was drafted the Committee have received from Mr. Shyam Shankar, Pandit and Secretary to H.H. the Maharajah of Jhalawar, the terms of a proposal for a Students' League or League of Empire for Native Students in India, which has practically the same objects as the schemes which are herein referred to. The draft will be considered in connection with similar proposals for other parts of the Empire in a future report.

The Committee desire to record their thanks to all who have given assistance so far, and, since 'gratitude is the sense of favours to come,' they look forward to additional help in pursuing the research and preparing a final report.

TABLE OF APPENDICES.

I. Syllabus of Instruction in Civics prepared by the Committee.

II. Analysis of the Scout Scheme of Training towards Citizenship, by Lieut.-Gen. Sir R. Baden-Powell.

III. Examples of Courses in Citizenship selected from other sources :—

    A. The Devon County Education Committee.
    B. The Ellerslie Road School, Hammersmith.
    C. Blackley School, near Manchester.
    D. Caerau Mixed School, Bridgend, Glamorgan.
    E. The Roath Park Boys' School, Cardiff.
    F. Ireland : First Derry Boys' School.
    G. Scotland : Stobswell School.

IV. Schemes of School Management :—

    Skerton Council School, Lancaster.
    Cowley School, St. Helens : Boy's Essay, ' Civic Government by Boys.'
    Penarth County School for Girls, Wales : Girl's Essay, ' Self-Government.'
    Roath Park Boys' School, Cardiff.
    The High School of Glasgow.

V. Suggestions for Organising Regional Study, by the Earl of Lytton.

VI. Notes of Lessons in Regional Survey (Lambeth), by Mr. Valentine Bell.

APPENDIX I.—SYLLABUS PREPARED BY THE COMMITTEE.

1. *The Origin of the State.*

Man a social animal.
    Impossibility of his living a solitary life.
The family the birthplace of the State.
    Plato and Aristotle upon the origin of the State.
    Augustine upon the Christian State.
Society implies interdependence; interdependence implies division of labour or specialisation.
Social unity of groups—Family—Guilds—Trade Unions.

Two objects of the State :—

> (1) To produce worthy and contented citizens. Common interests of all who are members of one Society, *e.g.* in obtaining the necessities of life, in securing the safety of person and property, easy communication, and opportunities of leisure and recreation.
> (2) To promote progress. The State can do for individual citizens something which they cannot do for themselves. It can afford them means of knowledge and culture. It can encourage education, temperance, and civic and patriotic devotion. It can offer opportunities for development and elevation. True freedom lies not in self-assertion but in subordination to the public good. Civilised man more truly free than a savage.

The State, therefore, essential to human welfare. But as every organism in its development becomes more complex, so a modern State with interests, it may be, in all parts of the world is far more complex than the ancient State, even when the ancient State had become an Empire.

2. *The History of Civilisation.*

Process of civilisation from East to West.
Influence of Greece and Rome.
Life and death of States.
Characteristics or tests of civilisation.
Man's command of Nature.
Influence of discoveries and invention, such as printing press, steam engine, aeroplane, gunpowder.
Advance of civilisation, development of,
    Science and its applications and inventions.
The great epochs of human progress marked by discoveries or inventions.
Comfort. Standard of living. Comparison of modes of living during the Roman Conquest, English Conquest, Medieval Period (the Barons, Monks, &c.), Elizabethan and Victorian Periods.
Interdependence of nations and countries—supply of wheat, wool, flax to England—coal, iron and manufactured goods from England.
Growth of corporate life—association in

> (a) The Feudal Structure.
> (b) Craft Guilds.
> (c) Trade Unions.
> (d) Co-operative Societies.
> (e) Friendly Societies.

Knowledge. Education : its opportunity and responsibility.
Growth of Humanity, as in abolition of slavery, torture, &c.
    Treatment of women and children.
The greatest happiness of the greatest number.
International relations : interdependence.
True end of civilisation :—The welfare of humanity as a whole.

3. *Citizenship.*

Citizenship begins at home.
Home life and surroundings.
    Type case in poor district, 60 houses on each side in a typical slum.
    Type family, father, mother, seven children (eldest 15).
    Type house, two bedrooms, small kitchen, parlour, only water supply a tap in a yard.
Importance of the individual; poverty no bar to success.
Importance of knowledge of individual capacity; loss of much splendid talent owing to wrong occupations being taken up.
Importance of individual joining some organisation with a definite object.
Good health a necessity for good citizenship.
Relation of the citizen to the State. Whether the individual citizen exists for the State, or the State for the individual citizen.

Civic pride—a citizen of a great community with a glorious heritage in men and books.

> Interest in local history, natural history, and local industry, regional surveys.
> Historical records; public memorials, historical pageants.

Civic ideals and duties.

> Unselfishness (good turns) and self-sacrifice.

Individual service.

Home.

School. Care of buildings, &c.

Outside. Public property (Parks).

> Proper use of conveyances and streets.
> Use of proper language.

Community service. Country—Fire brigade, special constables, accident corps.

> Humanity—Hospital service.
> Religion—Mission work.

Development of self-control. Consider gambling—smoking.

Common Prejudices to be guarded against—

> At School—ridicule of dull and physically weak boys.
> Religion—bigotry; sectarian jealousy.
> National—depreciation of members of other nations and races.

Man is essentially and before all else a member of the State and must live up to that membership.

Differences between the ancient and the modern world.

Compulsory military service; if a citizen can claim security he must be prepared to fight for it if necessary, and the State has a right to call upon him to do so.

Tendency of democracy to get as much as possible out of the State; to look upon the State as a dispenser of charities. A citizen's right—a fair wage; a citizen's duty—a fair day's work.

Universal franchise of adult men and women based upon equal interest of both sexes and of all classes in good government.

Danger of party spirit; each party only a section, and not justified in seeking its own with little or no reference to the good of the State.

The spirit of true citizenship evoked and evinced by the War.

So great the debt of the citizen to the State that he may be justly expected to make large sacrifices for the good of the State.

The daily life of a citizen.

Great citizens :—discoverers, inventors, philanthropists, writers, musicians, artists.

Desire of all classes to have a more permanent share in the Government, hence importance of all having a good conception of civic responsibilities : Franchise implies a duty as well as a right.

Citizenship inculcated by practice, dramatisation, self-government (school commonwealths, trials, debates).

4. *Monarchy and Democracy.*

Necessity for government.

Forms of government : absolute monarchy, limited monarchy, oligarchy, republic.

History proceeds as from East to West, so from the power of the few to the power of the many.

Monarchy the only possible government in primitive society.

Few good Kings and Queens.

The divine right of Kings an exploded doctrine.

Sir Robert Filmer's Patriarch.

The King the chief servant of the State.

Constitutional monarchy still useful as ensuring the unity of State and Empire.

The King to be recognised and to recognise himself as being what he really is.

Democracy the healthiest government, as resting upon the widest and strongest basis.

Democracy the only possible government in the modern world. Autocratic monarchy discredited.

Object of the Great War to make the world safe for democracy.

Drawbacks thought to be inherent in democracy :

- (1) that it may resist progress—Sir H. Maine and Mr. Lecky.
- (2) that it may fail to govern.
- (3) that mob-rule may prevail. *Cf.* Greek historians.

No form of government without possible defects, *e.g.* ' vote-catching ' policy. Failure in Ireland.

The need of strengthening democracy by constitutional safeguards, as in U.S.A.

Burke's criticism of democracy. Democracy indeed above other forms of government requires high character in its citizens.

Modern tendencies : anti-centralisation. Bolshevist theory of the State.

5. *Central Government.*

The State being one whole, a certain uniformity is necessary in its administration. Such variety of laws and customs as might prevail in the Heptarchy impossible in the United Kingdom. Thus in U.S.A. authority tends to pass from separate States to the Central Government, in such matters as the railway service, divorce, and temperance.

In general, as a State grows larger, the province of the Central Government becomes restricted.

Home Rule.

Control of such matters as properly belong to the Central Government : Army and Navy and Air Services, Customs and Excise, Post Office, Telegraphs and Telephones, Taxation, Education, Foreign Affairs, including peace and war, marriage and divorce, and the liquor trade.

Definitions of the functions attaching in the British Constitution to the Sovereign, the Prime Minister, the Cabinet, and Parliament. Lords and Commons. Election of Parliament. Exchequer. Direct and indirect taxation.

Passing of Bills into Acts.

The franchise and the ballot.

Devolution now inevitable within the Cabinet itself.

Sir Robert Peel probably the last Prime Minister who tried to keep his hand upon all departments of administration.

Danger of allowing the Government to be upset by a chance vote in the House of Commons.

Disadvantage attaching to the American system of associating offices which ought to be permanent with the fortunes of a political party.

Amount of agreement necessary among members of the same Cabinet.

Value of permanent officials in a democracy.

6. *Local Government.*

Danger of a Central Government being overburdened by a multitude of tasks.

The British Parliament a signal example of the difficulty arising from excessive centralisation.

Examples of local questions with which Parliament is obliged to deal.

Devolution possesses the advantage of an appeal to local knowledge, local interest, and local patriotism.

Scotland and Ireland respectively instances of success and failure in combining local with general sentiment.

Value of Municipal life. Unity of all large cities except London.

The Central Government to enunciate principles ; the municipalities to execute them in detail.

Good work already done by local Boards of Guardians, local Education Authorities, &c.

Difference between rates and taxes.

Money collected locally to be as far as possible expended locally ; revenue from dog licences, &c., expended in county in which licence is taken out.

Lord Mayors and Mayors.

County Councils.

Councils of County Boroughs, other Boroughs, other Urban districts, Rural districts. Parish Councils, Boards of Guardians.

Functions and duties of statutory and other Committees.
    (*a*) Municipal levies and expenditure—*e.g.* provision of Municipal baths, parks, trams, libraries, &c.
    (*b*) Education.
    (*c*) Provision for Public Health, including the care of the insane, and Housing.
    (*d*) Care for the destitute poor. Poor Law, almshouses, workhouses, casual wards.
    (*e*) Maintenance of roads, streets, buildings, and land.
    (*f*) Police and justice. Licensing.
Gas, electricity, and water supplies.
The danger that the best citizens will stand aloof from local administration.
All honour due to the men and women who often spend their lives without remuneration in the service of their cities and towns.
Municipal life as a training ground for political life.
Importance of dissociating municipal life as far as possible from political partisanship.
Use of local history.
Description of the way in which a city or borough is governed.
Tendency to extend governmental power and interference.

7. *The Administration of Justice.*

The supremacy of law one main feature in civilisation; justice said to be a reflection of the Divine Nature.
The law of a country to be (1) clearly defined; (2) popularly known; (3) equally administered.
Distinction between civil and criminal law.
The presumption of innocence in an accused person.
Jurors—how appointed; their powers and duties.
Classes of persons exempted from service on juries.
Defects of trial by jury.
Rights of individual citizens as guaranteed by laws; above all, the Habeas Corpus Act and the Bill of Rights.
Equality of all citizens before the law.
Rights of women as well as of men.
Incorruptibility of judges not established without difficulty, but now an assured fact of public life in Great Britain.
How laws are enacted and how law is gradually developed so as to become applicable to changing conditions.
Sir H. Maine on law.
Value of assizes.
Law to be made cheap and easy, but not so as to facilitate vexatious litigation.
Tendency to substitute judicial arbitration for trials by law.

8. *The Police and Public Safety.*

Civilised society differs from barbarous society by the maintenance of law and order.
All citizens entitled to perform their daily avocations in peace and safety.
Dangerous state of the roads, even so late as the beginning of the nineteenth century. Highwaymen on the outskirts of London. Numerous robberies and robberies with violence. Popular sympathy often on the side of the highwaymen as being supposed to be friends of the poor and enemies of the rich.
Inefficiency of the police down to 1829.
The police force as then instituted by Sir Robert Peel.
Difference between it and its predecessors (among these were the watchmen known as 'Charlies').
Occasions of appointing special constables.
The Riot Act. Power belonging to local authorities in grave emergency. The Peterloo massacre.
Training of the police. Their functions and powers. Women police.
Relation between the police and other citizens.

Friendly attitude of all classes except the criminal class to the police.
Juvenile offenders. Prevention better than cure.
Schools rather than prisons. The Borstal system. Reformatories.
Police-court Missions and Discharged Prisoners' Aid Societies.
Perils attaching to misuse of cinematograph shows.
The young of both sexes to be instructed in the laws which they are called to
    obey, and to be taught that the law is the only safeguard of liberty, as
    civilised men, although subject to more control, enjoy far more liberty
    than uncontrolled savages.

9. *Public Health.*

Health of the nation a chief concern of the country or city.
The Prime Minister's statement that a million more men would have been
    available for military service had the conditions of physical welfare been
    observed.
Impossibility of making an A 1 nation out of C 3 men.
Every child to have the chance of a healthy physical and moral life.
The State slowly waking up to its duty in respect of public health.
Reports of Medical Officers of Health.
Health Insurance Act.
Royal Commissions.
Provision of hospitals, clinics, and nurses.
Legislation affecting mines and factories.
Laws of Health.
Habits making for good health—
    (1) Exercise—sports—swimming—outdoor life.
    (2) Cleanliness—body and mind.
    (3) Temperance in every way.
    (4) Insistence on good ventilation.
Many diseases shown by experience to be wholly or nearly preventable, *e.g.*
    small-pox, diphtheria, and, above all, hydrophobia. Leprosy and Black
    Death long since extinct in Great Britain. Ravages of venereal disease.
    Report of Royal Commission. Immediate measures to be taken for check-
    ing and curing the disease.
Sanitation itself—a recent study. Effort and achievement of Sir E. Chadwick.
    Importance of good sanitary conditions in schools. Neglect of conditions
    even in public schools.
Statistics of infantile mortality. Need of instruction upon maternity. Peril
    of drunkenness to health and life.
Clinics. Care of crippled and defective children.
Treatment of defective eyesight.
Crusade against dangerous employment. White lead. 'Phossy jaw.'
Warm clothing as a preventive of chills and consequent maladies. Injury
    that women may do to themselves by following fashions in dress.
Provision of nurses for the poor in their homes during sickness.
Cleanliness. Free public baths.
Free medical attendance.
Hospitals at present inadequate to number of patients.
Questions of hospitals voluntarily supported as against hospitals dependent on
    the rates.
Welfare work.
A healthy and skilful body of workers, upright in character and self-reliant—
    a source of strength to the country.

10. *Life Assurance and Pensions.*

Democratic conception of government—that it is the duty of the Government
    to take at the public expense such measures as will give every citizen a
    chance of working while his strength lasts, and of living in peace when
    work is no longer possible.
In time past the poor have been haunted by the dread of old age, without the
    power of working, without resources, and without children or friends
    who might be willing and able to support them. The life of the poor to
    be set free from this anxiety.

The minimum rate of wages to be such that the wage-earner can live and bring up a family in decent comfort.

Life Assurance to be made compulsory when the workman is capable of paying a part of the premium, the State to pay the other part.

The duty of advocating and practising thrift. Savings Banks before the War.

Causes of pauperism and how to diminish it.

Importance of self-dependence and habits of prudence.

Honourable dislike of charitable relief among the poor.

Dread of the workhouse. Almshouses wholly insufficient in number and not ideal homes for old age.

Habit of casting upon Providence blame due to improvidence.

Valuable work done by Provident Societies.

Irresistible claim of mutilated soldiers and sailors.

National Insurance the affair of the Nation.

Apart from assurance, the equity of a pension payable to every man or woman who after 70 (or an earlier age) can no longer make provision for himself or herself.

Pensions give old people independence, or, if they live with their children, make them no longer unwelcome guests.

The pensionable age to be reconsidered in view of the statistics of life.

The Government to avoid ill-considered charity.

11. *Education.*

Education acknowledged to be the right of every citizen.

The Educational Highway. The State not to subvert or impair responsibility for children. 'La carrière ouverte aux talents' the true educational object.

'Entire object of true education is to make people not only do the right things but enjoy the right things.' (Ruskin.)

The value of education—influence on character—intelligence—observational power—broad-mindedness—power of self-expression—decision in action— self-reliance—capacity for responsibility.

Influence of Public Schools' games in character training—not confined to Public Schools.

Education inefficient if it ends too soon. Mr. Fisher's Act. Age of compulsory education prolonged.

Continuation Schools.

Vocational and non-vocational education.

Technical education—value to the workers.

Higher Education—Secondary Schools—The University.

Adult education—School and college only the beginning of the education of the citizen—Study Clubs, Workers' Educational Association.

The Educational curriculum not to be too wide.

Reading, Writing, Spelling, and Speaking to be taught thoroughly in primary schools.

Need of acquaintance with English History and Literature and the possessions and resources of the British Empire.

Ancient and Modern Universities.

A common educational basis necessary. Evil of premature specialisation.

A teacher's duty to discover and encourage special aptitudes in his pupils.

Mitigation in the severity of treatment of children.

Discipline—its value—obedience to just rules and orders.

'Nelson's signal.'

'Loss of the *Birkenhead.*'

Every teacher to feel that his or her success lies in the treatment of difficult pupils.

Study of writers upon education—*e.g.* Pestalozzi, Froebel, Spencer, Montessori.

Training and testing of teachers. Character of teacher even more important than advanced literary attainments. Teachers not to look for results too early.

Religious teaching. Advantage of non-sectarian teaching for children in day schools. The co-ordination of different Christian Churches.

12. *National Defence.*

The experience of the Great War.

No nation safe against unscrupulous aggression unless it is able to defend itself.

So great is the debt of every citizen to the State that every citizen may be justly called on in time of need to defend the State.

The object of statesmen to be that all citizens should defend the State not compulsorily but voluntarily.

Loyalty of Colonies in War.

Sea-power not in the future as in the past the determining factor of national life.   Submarine vessels and torpedoes.

Great Britain no longer an Island.   All its past history influenced by its isolation.

Recent and rapid development of aviation.   Command of the air even more important than command of the sea.

No nation secure so long as the nations of the world watch each other with jealous, unscrupulous eyes.

The League of Nations.   Attempt to introduce into public affairs the moral standard of private life and to bring the general sentiment of humanity into play against any one aggressive Power.

Information respecting the armed forces of the Crown.

No Monarch or Government to make war without the consent of the people.

Balance of power to yield place to the law of right, as defined by the majority (presupposing general broad-mindedness and reasoning power).

The process of general disarmament.   The nation to be strong, but solely for defensive purposes.

Problems of national defence to be regularly considered by a committee on Public Safety.

13. *The British Empire.*

The history of the Empire, its creation, the work of the Elizabethan mariners, their names and exploits.

Stages in growth of the Empire.

(*a*) Foundation—American colonies in Stuart period.   East India Company, 1600.

(*b*) The great quarrel—Loss of American colonies in eighteenth century; acquisition of Canada, India, and Australia; end of eighteenth century—acquisition of South Africa.

(*c*) Modification in relation of colonies to home country; at first valued mainly as contributing to welfare of home country, and governed from home; gradual grant of self-government (Durham report, 1840, &c.); federation of colonies (Canada, 1867, &c.).

(*d*) Twentieth-century development in *organic* connection between colonies and home country, or, in a simpler way,

(1) That of Elizabeth.

(2) That of Cromwell.

(3) That of George the Third (really Chatham's).

(4) That of Victoria.

The Crown in relation to the Empire.

History of the Indian Empire.

Present extent of the Empire.   Its varieties of peoples and national resources.

Value of travelling over the Empire.

Children in the schools to learn the dignity of the Empire by the study of the Union Jack, by the observance of Empire Day, and by the biographies of the men who founded and extended the Empire.

The British Empire is the greatest human institution under Heaven, the greatest secular organisation for good.

Principles of the Empire which must never be forgotten or abandoned : (*a*) Justice, respect of native races for British judicial integrity. (*b*) Good faith, honesty in trade; Honesty the best policy, but honesty not to be practised because it is the best policy.   The word of an Englishman.

(c) Freedom of speech, of public meeting, of political sentiment, of religious worship. (d) Progress.

Government of subject peoples to be always directed to their advancement and improvement—instances of failure, New Zealand in the thirties and South Africa subsequently.

14. *National Unity.*

Citizens in time of peace apt to make too much of divisions and dissensions. Consciousness of unity inspired by the crisis of the Great War.

Great Britain, and England itself, a witness to the possibility of fusing different elements, Anglo-Saxon and Norman characteristics. 'We are a people yet.'

How and why Scotland accepted union with England and made the most of it. Why Anglo-Irish Parliamentary Union has not been successful.

National unity involves the subordination of the party spirit to the good of the whole.

All in danger of prosecuting sectional and not national ends.

Foreign affairs. Taken, by mutual consent among parties, out of the range of party warfare.

The Crown as the centre of national unity. Benefit of a supreme authority which is independent of the vicissitudes of political fortune.

Lessons of the War not to be lost in peace.

The ideal of national unity to be taught in schools and advocated from pulpits.

No hindrance to unity greater than social or political privilege which cannot be overcome; caste a bar to all progress.

Glory of Great Britain that the humblest citizen may rise to the highest places.

Presidents of the United States, *e.g.* Lincoln.

National unity to be regarded as a means of upholding right.

15. *Patriotism.*

The sentiment natural to civilised humanity.

Pride in nationality and national life. Each citizen a member of the Nation and Empire.

Spirit of service, sacrifice and sympathy—traditions of achievements in application of ideals—atmosphere.

Children to learn at school patriotic poetry, *e.g.* Shakespeare and Scott. Value of learning poetry by heart as inspiring noble ideas.

Patriotism either false or true. Chauvinism and Jingoism, forms of false patriotism.

German patriotism before the War both aggressive and immoral, as taking no account of the rights or claims of other nations than Germany. Evil tradition of military power descending from Frederick the Great in Germany. Influence of modern historians, *e.g.* Treitschke.

Issue of the War.

The collapse of false patriotism.

True patriotism recognises an ascending scale of duties from family to city, from city to country, from country to humanity; as the interest of family at times must give way to that of city or country, so must the interest of city or country give way to that of humanity.

No patriotism justifiable unless it is such as can be inculcated in all countries without injury to any one country. True patriotism independent of politics.

Patriotism and Imperialism—not the same, but often confused. Patriotism, however, not complete without including something of the Imperial spirit.

The League of Nations the supreme instrument for moralising international life.

Citizens to be all instructed in the obligation of service to the State. Example of Japan.

The Public School spirit which has so signally vindicated itself in the War to be encouraged in all secondary and elementary schools.

16. *Industry and Commerce.*

Industry the life-blood of a nation. Upon it depend the interest and influence of national life—its value in development of character. But neither industry nor commerce free from danger.

The plea 'Business is business,' like the plea 'War is war,' may be used to justify evil means and evil ends.

No nation secure without trade, yet trade by itself may lower the national standard of duty. Free Trade expresses the natural relation between countries, each country supplying what other countries need and getting in return from them what it needs itself.

The world would be happiest if all the world were pacific and all Free Trading. Speeches of Cobden and Bright. But so long as there is danger of one nation attacking another, Free Trade qualified by the necessity of a country being, or so far as possible being made to be, self-supporting.

Thus the decay of agriculture might imperil the national safety, as the War has shown. It may be worth while to support agriculture even if the support somewhat raises the price of bread.

Value of coal-fields.

Change in the character of great industries.

Personal relations between employers and employed greatly impaired.

Origin of 'combines.'

Necessity for restoring a friendly feeling and confidence among all persons engaged in the same industry.

Co-partnership and profit-sharing.

Arguments for and against the Nationalisation of main industries.

Nationalisation not a question of right or wrong, but of expediency; will it tend to the efficiency of the industries nationalised? To be considered from point of view of national, not sectional, interests.

True conception of wealth. Adam Smith.

Exports and imports. Invisible exports.

Increased production the remedy for high prices.

Creation of new industries, application of science—electric, gas, dye industries. Without progressive science, labour and capital cannot play their part in modern life.

Discouragement of fraud in all relations of life and business.

Importance to nation of effective, honest, and intelligent working of all forms of business or industry.

Disasters resulting from mismanagement or fraud.

The credit attaching to British honesty and thoroughness the chief asset in the British trade.

Industrial and social reconstruction.

Development of various resources.

Co-operation and co-operative societies.

Crafts and Industrial Unionism. Arbitration. Wage Boards. Factors determining rates of wages. The living wage. The duty of every member of a Union to abide by its agreements.

Industrial Councils. Employers' Liability, Workmen's Compensation, Factory Acts. Welfare work. Strikes. Direct action.

Guild Socialism. Syndicalism.

Duty of community to sympathise with every effort of the workers to improve their conditions and develop their intelligence.

17. *International Relations.*

Nations have historically regarded each other as enemies, but they are really friends. Their interests are reciprocal, if not identical.

Different origins of wars between nations, racial, territorial, religious, commercial, but all proceeding from the same spirit.

The comity of nations an ideal newly acquired or newly realised.

The word 'international' not found earlier than in Bentham's writings.

'International law' a misleading phrase, as it implies a sanction which does not exist.

History of the *Alabama* Case.

Mr. Gladstone's attempt to substitute arbitration for war.

Geneva Convention. Hague Conferences. Behaviour of the Powers, especially Germany.

Diplomacy. Sir H. Wotton's definition of an Ambassador.

President Wilson's plea for open diplomacy.

The Balance of Power a rude attempt to stave off war by equalising the forces of combatant or rival nations.

The League of Nations an attempt to bring the moral senses of civilised humanity to bear upon one offending nation.

Appeal of Chili and Argentina to Queen Victoria for arbitration.

Statue in memory of the arbitration.

Owing to facility of intercommunication the world becoming one family.

18. *The Press.*

History of the Press. Its importance in the present day.

Nations no longer hearing but reading nations; hence the decay of the pulpit, and even the platform, in point of influence, but increase in the power of the Press.

The Press most powerful in a society in which men and women have learnt to read but not to set a just value upon the news which they read.

Newspapers play the most distinctive rôle in the enlargement of human nature— a potent weapon in the creating of public opinion, replacing chatter and gossip of earlier periods.

Advertisements as a means of success.

False credit given to vendors of patent medicines or tipsters in respect of horse-racing.

Remedy lies in better education.

Responsibility of the Press. Possible misuses of its influence. One danger lies in the control of an individual over many newspapers.

Importance of the law against slander or libel. The question whether the publication of false news should not be punishable.

Danger of sensationalism.

Incorruptibility an honourable feature of the Press in Great Britain.

Contrast subsidised newspapers in foreign countries, most of all in Germany.

Purity and decency another honourable feature.

Freedom of the Press essential to constitutional liberty. Prynne and Cobbett

Help given by the Press in the detection of crime.

Check to be imposed on reports of divorce and murder cases, as of certain other cases.

Training for a journalistic career.

19. *Housing.*

The homes of the people the sources and centres of virtue.

Difficulty of the housing problem. Value of space in slums of great cities. Statistics relating to occupants of single rooms. Morality almost impossible where persons of all ages and both sexes are herded together.

Cellar dwellings nearly extinct. Need of houses never greater than to-day.

The question of housing both physical and moral.

Importance of light, space, and sanitation.

Municipal authorities now invested with requisite powers. Duty of voters to see that these powers are exercised.

Sanitary inspection essential. Owners of insanitary property not to escape responsibility.

Rivalry of the home and the public-house.

The best counter-attraction to the public-house lies in good private houses.

Infantile mortality the result of drinking and of bad housing.

Good lighting efficient as a means of lessening crime.

Advantage of Garden Cities constructed on scientific principles, *e.g.* Bournville, Port Sunlight.

Love of home one of the most potent forces in human nature, but impossible unless there are comfortable homes.

Need of provision, especially in those parts of Great Britain which are rapidly becoming vast cities.

Difficulty of constructing houses for which it is possible to charge a remunerative rent.

The whole strength of a municipality to be employed under Parliamentary sanction in improving the houses of the poor.

20. *Temperance.*

Drink the greatest national evil. The source of three-fourths of the crime and misery in the nation. Physiological effect of alcohol.

Amount of the national bill for drink even during the War.

Waste of foodstuffs.

No private interest to be allowed to stand in the way of reform.

The nation cannot afford to be a drunken nation.

Question of the drink trade not local but national. Local option to be the outcome of national control.

Local trade and politics. Tied houses. Relation of brewers to publicans. Clubs to be treated like public-houses and beer-houses.

Effect of prohibition of vodka in Russia.

Prohibition in U.S.A. Not so much a social as an industrial measure. A guarantee for industrial efficiency. Estimated to increase efficiency by 10 per cent.

Two influences making for temperance : (1) Women's votes, (2) Education in elementary schools.

Work of the Central Liquor Control Board during the War.

Similar, if not the same, control necessary in peace.

Nationalisation or State purchase of the liquor trade.

Argument for nationalisation. So long as private interest in the sale of liquor exists, the State is exposed to inevitable danger. Take away motive of self-interest and improvement will become possible.

The late Earl Grey's project of disinterested management.

Owners of public-houses to be made responsible for drunkenness occurring in them.

Duty of State to remove temptation as far as possible from citizens.

Gain of excluding children from public-houses.

In the present rivalries of the nations, Great Britain must become sober, or it will lose its pride of place.

Temperance societies and their campaign for national sobriety.

21. *Leisure and Recreation.*

Daily life and its division into working, leisure, and sleeping periods.

Necessity for useful and strenuous work as opposed to slothfulness, idleness, and luxury.

Problems of idleness and luxury. Gossiping. Street-corner and public-house idlers.

The danger of morbid introspection.

The influence of habit upon development.

Many persons ruined through inability to employ non-working periods properly.

Importance of proper amount and kind of recreation.

Change from, and foil to, work.

Suitable recreation for manual workers, sedentary workers, and brain-workers.

Demand for more leisure-time from physical work.

Leisure-time not to be wasted in idleness but to be profitably occupied in necessary rest, home duties, civic duties, amusements, and self-development.

Due proportion of leisure-time to be given to self-improvement or self-development.

Self-development—hobbies—literature—music—art, &c.

Adequate provision of facilities—libraries, &c.

Amusements of the people : (1) Old-time : Morality plays, mummers, strolling players, revels, fairs, morris dancers, May Day. (2) Present time : Processions, sports, regattas, racing, picture palaces, theatres and music-halls, athletics, &c.

The habit of looking at, as opposed to taking part in, sports.

Provision of open spaces in towns.

Enjoyment of open air and interest in natural history.

Games and their value in the development of *esprit de corps*, co-operation, responsibility, perseverance, emulation, fair play, leadership, discipline.

Team-games for children.   Importance of organisation and supervision of games at school.

The evil of gambling—its effects upon sports.

Provision of play-grounds and playing-fields.

Boy Scouts.   Girl Guides.

The proper use of holidays.

Summer camps and schools.

Co-operative holidays and tours.

Juvenile organisations.   Committees.   School clubs.

APPENDIX II.—ANALYSIS OF THE SCOUT SCHEME OF TRAINING TOWARDS CITIZENSHIP.
By Lieut.-Gen. Sir Robert Baden-Powell.

I.—INDIVIDUAL.

1. CHARACTER AND INTELLIGENCE.		2. HEALTH AND STRENGTH.		3. HANDCRAFT AND SKILL.		4. SERVICE AND CITIZENHOOD.	
QUALITIES AIMED FOR	THROUGH PRACTICE OF	QUALITIES AIMED FOR	THROUGH PRACTICE OF	QUALITIES AIMED FOR	THROUGH PRACTICE OF	QUALITIES AIMED FOR	THROUGH PRACTICE OF
INTELLECTUAL. Observation, Deduction.	Scouting, Woodcraft, &c.	Health.	Responsibility for personal, Hygiene, Continence, Temperance, &c.	Hobbies, Technical Skill, Inventiveness.	Tests in technical proficiency. Rewards by badges, &c., in over 50 forms of handicraft.	Unselfishness, Civic Duty, Patriotism, Service for Country, Service for Humanity, Service for God.	Good Turns, First Aid, Life Saving, Fire Brigade, Accident Corps, Hospital assistance, 'Missioner' work.
CIVIC. Fair Play, Discipline, Leadership, Responsibility, Respect for rights of others, Justice.	Team games, Patrol work, Court of "Honour," ,, ,,	Strength.	Physical development, Games, Swimming, Climbing and Natural activities.				
MORAL. Honour, Chivalry, Self-Reliance, Courage, Capacity for Enjoyment, Self-Expression in Art, Higher Tone of Thought, Religion.	Scout Law, ,, ,, Scout work and activities, Appreciation of Nature, Nature lore and study, Astronomy, &c., Kindness to animals, Service to others (q.v.).				N.B.—Apart from promoting efficiency and occupation for idle hours, this department leads to fitting the square peg in the square hole for his career.		

The INDIVIDUAL qualities having thus b.en formed in Numbers 1, 2, and 3, they are then harnessed for the good of the COMMUNITY in Number 4. Here they may be further promoted by Collective development of the right SPIRIT with its expression in patriotic ACTION.

II.—COLLECTIVE.

DEVELOPMENT OF No. 4 SERVICE IN CIVIC DUTY.

SPIRIT

CIVIC PRIDE

Interest of the Unit in local history, natural history, local industry.

THROUGH

Compiling and setting up historical record of parish or locality. Keeping record of Natural History, e.g. migration of birds, &c. Formation of Historical and Natural History Museum, Photos, Sketches, Outings. Knowledge of local Public Memorials. Keeping roads and parks clean. Historical tree planting. Memorial Shakespeare Pageants, Plays, &c. Knowledge of local industries.

CIVIC KNOWLEDGE

General History, Geology, Nature, Arts, Manufacture, Municipal, Government, Parliamentary, Constitutional, Government, Balance of mind, Broad-minded, Patriotism, and World Peace.

THROUGH

Visiting, studying, and comparing History, Natural and Civic qualities of other places. Visiting factories. Practice of Court of Honour. Mock trials, &c. Attending Town Council Meetings. Interchange of correspondence with British and Overseas State foreign countries.

ACTION

CIVIC SERVICE

PRACTICE	PREPARATION
Juniors (Wolf Cubs). Messenger duty, Elementary First Aid, Cleaning up house, premises, gardens. Clearing up paper, orange peel, &c., in streets and parks. — THROUGH	Knowledge of streets and short cuts. Ability to remember instructions. Cleanly spirit.
Boys (Scouts). 'Missioner' work (i.e. House-work for aged and invalid). Orderly duty to civic authorities, police, fire brigade, hospitals, &c. First Aid assistants. Guides to strangers. — THROUGH	Compiling local map showing fire, police, ambulance stations, &c. Special organisation and training in these duties and in despatch riding, signalling, wireless telegraphy, &c., for public service.
Seniors (Rovers). Accident Corps, Hospital Fire Brigade, Rocket Troop, Special Constables, Leadership, Coastguards, &c. — THROUGH	Sea Scouts and other special organisation and training for every kind of accident and for these duties.

N.B.—The details must necessarily be graded according to the psychological capacity of the pupil.

NOTE.—The sacrifice of time and pleasure and the refusal of tips or reward of any kind in preparing for and performing these duties is an important point in the training. It is a first step in education towards self-sacrificing patriotism.

APPENDIX III.—SELECTED EXAMPLES OF COURSES ON
CITIZENSHIP.

A.—Devon County Education Committee.

Suggestions as to Moral Instruction and Training in Citizenship.

Infants (under 7 years).

1. Cleanliness.
    (a) Clean hands, faces, and clothes.
    (b) Clean habits—*e.g.* the proper use of the lavatory.

2. Tidiness.
    (a) In the home, school, and street.
    (b) Personal tidiness.
    (c) Care of furniture, books, toys, and other property.

3. Manners.
    (a) Greetings at home and at school.
    (b) Behaviour at meals.
    (c) Punctuality and promptness.

4. Fairness.
    (a) Mine and thine.
    (b) Fairness towards others.

5. Kindness.
    (a) Love to parents.
    (b) Kindness to each other in the home, school, and street.
    (c) Kindness to animals.

6. Truthfulness.
    (a) Telling the truth.
    (b) Confidence in parents and teachers to be encouraged.
    (c) 'Dramatic' untruths to be discouraged.

7. Courage.
    (a) When alone.
    (b) Darkness, shadows, and strange noises.

Lower Standards (7 to 11 years).

1. Cleanliness.
    (i) (a) Use and care of parts of the body—*e.g.* hair, eyes, ears, nose, lips, teeth, hands, and feet.
    (b) Care of clothing.

    (ii) (a) In the home.
    (b) In the school, playground, and street—*e.g.* to desist from scattering paper and orange peel.
    (c) Neatness in person and in work.

2. Order.
    (a) The value of system—*e.g.* a place for everything and everything in its place.
    (b) The value of punctuality.
    (c) The value of promptness.

3. Manners.
    (i) (a) In eating and drinking: moderation.
    (b) In question and answer: politeness.
    (c) In bearing: quietness, unobtrusiveness, patience in waiting.
    (d) Punctuality in the home and the school.

    (ii) (a) In speech: courtesy and clearness; refinement of language.

    (b) In bearing: orderliness in the streets, behaviour in public places.
    (c) How to perform a simple service—*e.g.* how to carry a message.
    (iii) (a) Unselfishness.
    (b) Respectfulness towards the aged.
    (c) Cheerfulness: evil of grumbling and fault-finding.
    (d) Modesty.
    (e) Self-respect.

4. Obedience.
    (a) Immediate and hearty obedience to parents and teachers.
    (b) Respect for rules and regulations.

5. Kindness.
    (a) To companions at play.
    (b) To pet animals.
    (c) To flies, worms, and other harmless creatures.
    (d) To birds: their nests.

6. Gratitude.
    To parents and teachers.

7. Fairness.
    Ungrudging disposition, especially when favours are distributed, or when the success of others is under notice.

Lower Standards (7 to 11 years).—(Cont.)

8. Honesty.
   - (a) Respect for the property of others.
   - (b) Restoration of lost property.
   - (c) Preserving and protecting property at home, at school, in parks, and other public places.
   - (d) In work.

9. Truthfulness.
   - (i) (a) In speech : the importance of exactness; the avoidance of exaggeration.
     - (b) In manner : the importance of simplicity; the avoidance of affectation.
     - (c) Promises.
   - (ii) (a) In reporting : correctness; avoidance of slander and gossip.
     - (b) In action : candour; not to act a lie.
     - (c) In thinking : eagerness for the truth.
     - (d) Not to shirk a difficulty by a pretence of understanding.
   - (iii) (a) All the truth and nothing but the truth.
     - (b) Avoidance of prevarication and withholding part of the truth.
     - (c) Avoidance of deception through manner or gesture.
     - (d) The importance of frankness.

10. Honour.
    - (a) In the eyes of others : trustworthiness.
    - (b) In the eyes of self : self-respect.
    - (c) Avoidance of false pride.

11. Courage.
    - (i) (a) Cheerful endurance of little pains and discomforts; manliness and womanliness.
      - (b) On behalf of the weak or innocent.
      - (c) In relation to creatures inspiring instinctive fear in children—*e.g.* mice, frogs, spiders, and beetles.
    - (ii) (a) To follow good example and to resist bad example.
      - (b) To confess faults or accidents.
      - (c) Under difficulties : self-reliance.
      - (d) In bad weather—*e.g.* not to fear thunder and lightning.

    - (iii) (a) The importance of courage, avoidance of bravado.
      - (b) Presence of mind, avoidance of panic.

12. Temperance.
    See Syllabus issued by the Board of Education.

13. Self-Control.
    - (a) In bearing : the avoidance of wilfulness, peevishness, obstinacy, sulkiness, violent temper, and quarrelling.
    - (b) In speech : the avoidance of rudeness and hastiness.
    - (c) In thought : checking of evil thoughts.

14. Work.
    - (i) (a) Helping in the home.
      - (b) The value of industry in the school.
    - (ii) (a) Pride in thorough work.
      - (b) Use of leisure time; value of hobbies.

15. Perseverance.
    - (a) In work : hard or distasteful tasks.
    - (b) In play, fighting out a losing game.
    - (c) In self-improvement.

16. Humanity.
    - (i) (a) Personal help to those in need.
      - (b) Making other people happy.
    - (ii) As shown by public institutions— *e.g.* the fire brigade, lifeboat, lighthouses, hospitals, asylums, Red Cross Society.

17. Justice.
    - (i) (a) To companions, in the school, playground, and home.
      - (b) To the less fortunate—*e.g.* the weak, imbeciles, stammerers, deformed.
    - (ii) (a) To others—*e.g.* not to spread infection.
      - (b) Avoidance of cruelty to animals in pursuit of amusement or sport.
      - (c) The justification for restraint and punishment in the home and the school.
    - (iii) (a) In thought, word, and act.
      - (b) Forbearance.
      - (c) Forgiveness, remembering our own faults.

Upper Standards (11 *to* 14 *years*).

1. Habits.
    (a) How acquired.
    (b) How cultivated and avoided.
    (c) Harmfulness of juvenile smoking.
2. Manners.
    (i) (a) Courtesy and respect towards all.
    (b) Self-restraint.
    (ii) (a) As shown by dress.
    (b) By choice of friends, literature, and amusements.
    (c) By kindness to younger children.
    (d) In boys : by special courtesy to all women and girls.
3. Truthfulness.
    (i) (a) Respect for differences of opinion.
    (b) Living for truth : readiness to receive new truths.
    (c) What men have sacrificed for truth.
    (ii) (a) Conquest of science over ignorance and superstition.
    (b) Progress of truth.
    (c) Love of truth.
4. Temperance.
    See Syllabus issued by the Board of Education.
5. Courage.
    (a) Heroic deeds done in the service of man : self-sacrifice.
    (b) Everyday heroism.
    (c) Chivalry : devotion of the strong to the weak.
    (d) Moral courage.
6. Justice.
    (i) (a) To all human beings, irrespective of sex, age, creed, social position, nationality or race; and to animals, tame and wild.
    (b) Charitableness in thought.
    (c) The value of courts of justice.
    (ii) (a) Love of justice.
    (b) Just and unjust relations between employers and employed.
    (c) The rights of animals.
    (iii) (a) The development of the idea of justice from the earliest times.
    (b) The development of the humane spirit in laws.
    (c) The development of the idea of equality.

7. Zeal.
    (a) The value of zeal and energy in overcoming difficulties.
    (b) The dangers of misdirected zeal—*e.g.* bigotry, fanaticism.
8. Work.
    (a) The necessity for and dignity of labour.
    (b) The earning of a living : different pursuits—their responsibilities and social value.
9. Patriotism.
    (i) (a) Pride in one's school and loyalty to it.
    (b) Duty of local patriotism : how to serve one's town or village.
    (c) The value of local institutions.
    (ii) (a) What our forefathers have earned for us—*e.g.* liberty, social and political institutions.
    (b) How each may serve his country and posterity.
    (iii) (a) The vote : its nature and responsibilities.
    (b) Local government.
    (c) The nation and its government.
    (d) Society as an organism : its development through the family, tribe, and nation.
    (e) Universal brotherhood.
10. Peace and War.
    (i) (a) The value of peace and her victories.
    (b) The duty of citizens in time of war.
    (c) The evils of war.
    (ii) (a) International relations : how nations can help each other.
    (b) Value of arbitration.
11. Thrift.
    (i) (a) Money : its uses and abuses.
    (b) Economy in little things.
    (c) Wise spending : avoidance of extravagance and wastefulness.
    (ii) (a) How and why to save : Savings Banks.
    (b) The cost of drink to the nation.
    (c) Clubs and insurance.
    (iii) (a) Simplicity of living.
    (b) The evils of debt.

Upper Standards (11 *to* 14 *years*).—(*Cont.*)

(c) The evils of betting and gambling : meanness of the desire to get without rendering service.

12. Co-operation.
   (a) Between citizens.
   (b) Between nations : in commerce, art, and thought.

13. Ownership.
   (i) Talents and opportunities : responsibility for their use.
   (ii) (a) Individual and collective ownership.
   (b) Responsibilities of ownership.
   (c) Care of borrowed books, tools, &c.

14. Self-knowledge.
   (a) The need to know ourselves and to test our moral progress.

(b) The claims of conscience, individual and social.
(c) The enlightenment of conscience.

15. The Will.
   (a) The training of the will.
   (b) The right to be done intelligently, unhesitatingly, thoroughly, cheerfully, and zealously.
   (c) Danger of mental and moral sloth.

16. Self-Respect.
   (a) Self-respect and self-restraint in thought, word, and act.
   (b) Avoidance of impure literature.

17. Ideals.
   The value and beauty of an ideal for life.

B.—*Outlines of a Scheme of History (Six Months' Course) at Ellerslie Road, Hammersmith, London.*

St. II. A. Stories of primitive men. Past and present.
   ,, B. Fifteen notable warriors in the world's history.
St. III. A. Fifteen famous women in history.
   ,, B. Fifteen pioneers in English history.
St. IV. A. The making and welding of the British Race.
   ,, B. England under Absolute Monarchs.
St. V. A. King v. Parliament.
   ,, B. Reaction—Kings again—Limited Monarchy.
St. VI. A. Industrial revolution and growth of democracy.
   ,, B. Growth of British Empire.
St. VII. A. How England is governed. (The syllabus for this course is appended.)
   ,, B. History of other countries.
Necessity for Government. Forms of Government.
Absolute Monarchy, Republic, Limited Monarchy.
Hammersmith Borough Council.
Mayor, Alderman, Councillors' Duties.
L.C.C. Chairman, Committees' Duties.
Parliament : House of Commons. How elected. Parties.
Cabinet. Passing of Bills.
The King and Lords : Their Functions. Lords v. Commons.
Colonies and Great Britain—An Imperial Parliament.
The Exchequer. Direct and indirect taxation.
Free Trade and tariffs.
Policeman. Soldier. Sailor. Civil Service.
Crime and its Punishment (model trial).
Model election.

C.—*From Blackley, Manchester.*

Plans adopted to make men and good citizens.

1. Institution of Arbor Day in 1907.
2. Colonial and American Correspondence.
3. A system of class government throughout the school based upon the Patrol system and Scouting.
4. Exchange of flags between the school and a school in Adelaide, South Australia, and also with the Rhode Island Normal School, Providence, U.S.A.
5. The provision of a sledge and forty pairs of clog-soles to the Trans-Antarctic Expedition (Sir E. Shackleton).
6. Special lessons and ceremonies on Empire Day, St. George's Day, Nelson Day, Armistice Day, and other special occasions.
7. Telegrams to H.M. The King on each Empire Day, to Admiral Beatty, Sir Douglas Haig during the War, to M. Deschanel on the occasion of the first celebration of Empire Day in Paris.
8. Establishment of a link between the school and a unit of the Fleet—a cruiser upon which an old boy was serving. The children presented a mirror and other accessories, and wrote Christmas letters to the men. They responded by presenting a Union Jack and a White Ensign (15 ft. by $7\frac{1}{2}$ ft.) to the school. Interesting ceremony.
9. The publication of *The Torch* containing a record of the activities of the school and honourable service.
10. Christmas letter from the Headmaster to each child.
11. School Gardens, Flower and Bulb Shows, Exhibitions of Work.
12. Old Scholars' Association. Reunion of Old Boys. Reunion of Old Scholars (boys and girls) under the age of nineteen.
13. Blackley War Memorial. Special eight weeks' effort to raise 10*l.* by personal gifts and work. Total sum realised, 12*l.* 12*s.*
14. Old Boys' War Memorial. Combined effort—Old Boys and present scholars to raise 200*l.* for a School Memorial.
15. The planting of a tree in the school grounds by a George and a Mary, which, by permission of The King, was called 'A Coronation Tree.' His Majesty accepted a photograph of the ceremony, and sent his thanks to the children.
16. Evening Classes for Boy Scouts (each winter since 1911).
17. A troop of Boy Scouts in connection with the Day School.

D.—*Caerau Mixed School, Caerau, Bridgend.*

Scheme for Training of Citizenship.

1. Our School.—How we are governed; Education Committee and Local Managers; Attendance Officer and his duties; Why we attend school.
2. Our Town.—How our town is governed; The Urban District Council; Wards; Elections; Duties of Urban District Council.
3. The County Council.—Its work—roads, bridges, &c.; Education, &c.
4. Our Country.—(*a*) How governed; Parliament—its Houses and its origin.
   - (*b*) How order is kept—Police, Law Courts, Justice.
   - (*c*) Our duties as Electors—Seriousness of elections; Why thought is necessary rather than mere following of leader.
   - (*d*) Our duties as Burghers—in the street, the park, and on enclosed property.
   - (*e*) Our duties as Citizens of the Empire—'What we are in the days to come, that will Britain be'; Our future work, &c.

Temperance Instruction and Thrift (War Savings Association at School), as per C.C. scheme.

Civic Practice (Self-Government).

Class Committees.

1. General Committee.—Look after general welfare of school; appoint Sub-Committees, monitors for corridors, cloak-rooms, &c.
2. Sub-Committees.—(a) Sports Committee.
   (b) Debating Committee.—To arrange Debates. Typical subjects—Party Government—Advantages and Disadvantages; Use and abuse of letter-writing to papers; Is the working-man better off to-day than fifty years ago? Up to what age should children attend school? &c.
   (c) Sanitary Committee.—Report choked gullies, &c., during school hours, &c.
3. Scholars' Committee (each class).—Three girls and four boys elected by class. Each responsible for class-room generally. Laziness, bad work, &c., reported to Committee, who report their decision to Class Teacher. Committee also appoint Class-room Monitors.

E.—*Roath Park Boys' School, Cardiff.*

Citizenship.

The scheme for History [St. V.-VII.] is framed with the definite object of rousing interest in—
   (a) The growth of liberty.
   (b) England's position among the nations.
   (c) Value of sea-power.
   (d) Growth of the Empire.
   (e) Citizenship.
Part (e) includes :
   (i) The Rights of a Citizen.
   (ii) The Duties of a Citizen.
   (iii) How an Act of Parliament is made.
   (i) Rights of a Citizen.
      1. The right to vote. Representative Government.
      2. The right to protection.
      3. The right to justice. The Courts of Justice and the administration of justice.
   (ii) Duties of a Citizen.
      1. Municipal government. How a city like Cardiff governs itself.
      2. Obedience to laws.
      3. Service to the community. The best service to the community is to be honest and industrious in the sphere in which we are placed.
      4. The protection of public property.
      5. Ignorance a bar to progress and good citizenship. Every person should take an intelligent interest in the affairs of city and State.

IRELAND.

F.—*First Derry Boys' School.*

1. Society—what ' society ' implies.
   (a) Necessity of ' Social System ' of living.
   (b) Duties arising from ' Social System ' of living.
   These classified as A, B C, &c., as follows :
2. A. Liberty.
   B. The Consequent Renunciation.
   C. Duties must be discharged, rather than rights sought for.
   D. Obedience to Law; loyalty to King and Constitution, issuing in.
   E. Development of a ' Spirit of Law.'
   F. Personal duty in regard to thought, speech, act.
   G. Limits to exercise of these.
   H. Public duty of each in regard to these.
   I. Spirit of carefulness, neatness, and cleanliness in person and in environment.
   J. Personal and Public; hence strive to abolish ugliness.
   K. Reaction on character—public and private—if we observe these rules, &c.

Scotland.

G.—*Stobswell School.*

A. A series of lessons on (1) the Rights and Privileges, (2) the Responsibilities
and Obligations, and (3) the Organisation, Finance, and Management of
      (*a*) The Home,
      (*b*) The School,
      (*c*) The Municipality,
      (*d*) The State, and
      (*e*) The Empire.
B. Incidental instruction in Local and Central Government arising out of
History Course and passing events, including miniature elections con-
curring with Municipal and Parliamentary Elections.
C. Training in habits of punctuality, regularity, cleanliness, orderliness, thrift,
self-respect, consideration for others, mutual responsibility, and regular
assembly and dismission devotions.
D. Placing the discipline of the School as far as possible on the honour of the
pupils and their supervision outwith teaching periods in charge of Monitors,
nominated and elected by the pupils themselves at the beginning of each
session.

APPENDIX IV.—SCHEMES OF SCHOOL MANAGEMENT.

Skerton Council School, Lancaster—Boys' Side.

' *Order is Heaven's First Law.* '

Our Constitution.

First comes H.M. The King and his Government.
Next comes the Board of Education.
Next comes the L.E.A.
Next come the School Managers.
And . . . next . . . we! Now, who are we?
We are :
    (1) The Headmaster and the Staff of Teachers.
    (2) The School Prefects : viz. Captain, Sub-Captain, Secretary, Sergeant-
Major, Football Captain, Football Sub-Captain, Cloak-room Curators, Sani-
tary Inspectors (Outdoors), Assembly Sergeants, Corporals, Lance-Corporals,
Bugler(-Sergeant), Drummer(-Sergeant).
    (3) The Senior School (Standard V. upwards) who are on the Roll of
Voters, and these only are eligible for Prefectship (*Nota Bene* : Only mem-
bers of the Top Class are eligible for Captaincy and Sub-Captaincy) : Full
Citizens.
    (4) Disfranchised Members of the Senior School : Not Full Citizens.
    (5) The Junior School—none of whose members are entitled to vote in
School Elections.

The Prefects.

Appointed by Election :
    Captain, Sub-Captain, Secretary; each of whom retires at the end of the
last school day of each month (except December and August), but is eligible
for re-election.
Appointed, immediately after his election, by the Captain :
    Sanitary Inspectors (outdoors), Cloak-room Curators.
Selected by Class Teachers from a list of Qualified Instructors in Drill :
    Sergeant-Major, Sergeants and Corporals.
Appointed by Captain :
    Corporal and Lance-Corporal for tardy scholars; two supernumerary Lance-
Corporals.
Appointed by the Master in charge Football Team :
    Football Captain, Football Sub-Captain.
Qualifications for admission to full Citizenship :
    (1) To be in Standard V. or upwards.
    (2) To have undertaken, and signed, the Citizens' obligation, *vide* Girls'
Constitution.

Disfranchisement : Persistent idleness or misbehaviour, or a serious act of insubordination or meanness, renders a boy liable to disfranchisement by his class teacher until after the monthly election next ensuing.

The Prefects—their Privileges, &c. :

(1) They are, whilst holding office and wearing the badge of their rank, immune from any corporal punishment, detention, or public reprimand ;

(2) They may be either suspended, degraded, or dismissed from office by their class master or the Headmaster, if, by neglect of duty or other serious misconduct, they have merited such punishment; in such cases, the badge of rank will not be worn and the privileges of prefectship will be withdrawn for the time being;

(3) No Prefect may assume the badge of rank and benefit by the privileges of prefectship until he has

(a) been constitutionally appointed;

(b) publicly undertaken and signed an obligation to bear in mind the Prefects' motto, *Noblesse oblige.*

This Constitution, so far as it concerns the Prefects and the Citizens, may be modified—or withdrawn entirely—by the Headmaster, at any time he thinks such modification or withdrawal necessary.

Skerton Council School, Lancaster—Girls' Side.

' Order is Heaven's First Law.'

First comes H.M. The King and his Government.

Next comes the Board of Education.

Next comes the L.E.A.

Next come the School Managers.

And . . . next . . . we! Now, who are we?

We are :

(1) The Headmaster and the Staff of Teachers.

(2) The School Prefects : viz. Captain, Sub-Captain, Secretary, Games Captain and Sub-Captain, Superintendent of Playground Mothers, Sanitary Inspectors (Outdoors and Indoors), Sergeants, Corporals, and Lance-Corporals, Cloak-room Curators.

(3) The Senior School (Standard V. upwards) who are on the Roll of Voters, and these only are eligible for prefectship.

(4) Disfranchised members of the Senior School.

(5) The Junior School—none of whose members are entitled to vote in school elections.

The Prefects.

Appointed by Election :

Captain, Sub-Captain, Secretary; each of whom retires at the end of the last school day of each month except December and August, but is eligible for re-election.

Appointed, immediately after her election, by the Captain :

Sanitary Inspectors (outdoors), Sanitary Inspectors (indoors), Cloak-room Curators.

Selected by Class Teachers from a List of Candidates duly qualified (by examination) as N.C.O.s :

Sergeants, Corporals.

Appointed by Captain :

Corporal and Lance-Corporals for tardy scholars; two supernumerary Lance-Corporals.

Appointed by Playground Mothers each month end :

Superintendent of Playground Mothers.

Appointed by Headmaster each month :

Games Captain and Sub-Captain, two Lance-Corporals as time-keepers.

Qualifications necessary for inclusion on Roll of Voters :

1. To be in Standard V. or upwards ;

2. To have undertaken, and signed, the Citizens' Obligation; viz.

(a) To be courteous and considerate to all ;

(b) To speak and vote without meanness, spite, fear, or favour ;

(c) To try to be a good citizen.

Disfranchisement : Persistent idleness or misbehaviour, or a serious act of in-
subordination, renders a girl liable to disfranchisement by her Class
Teacher until the Monthly Election next ensuing.

Prefects:

1. They are, whilst holding office and wearing the badge of their rank,
immune from any corporal punishment, detention, or public reprimand;

2. They may be either suspended, degraded, or dismissed from office, if
the Headmaster considers that by neglect of duty or other serious misconduct
they have merited such punishment; in cases of suspension from office, the
badges of rank will not be worn, and the girls suspended will no longer be
immune under the conditions stated above.

3. No Prefect may wear the badge of office or assume the privileges or
immunities of prefectship until she has

(1) been constitutionally appointed;

(2) publicly undertaken to bear in mind the Prefects' motto, *Noblesse
oblige.*

The conditions relating to the Senior School, the Junior School, the Prefects,
and the Roll of Voters may be modified by the Headmaster, or withdrawn
entirely, if he considers such modification or withdrawal necessary.

Badges of Office: Boys' Side.

Captain	red plush : letter ' C.'
Sub-Captain	,, ,, letter ' s.'
Secretary	,, ,, letter ' S.'
Sergeant-Major	blue plush : letters ' S.M.'
Cloak-room Curators	blue disc, yellow centre.
Sanitary Inspectors (outdoors)	blue disc, red centre.
Football Captain	black shield, yellow letters ' F.C. '
Football Sub-Captain	black shield, yellow letters 'F.C. '
Assembly Sergeants	red disc, blue centre.
Assembly Corporals	yellow disc, black centre.
Assembly Lance-Corporals	green disc, red centre.
' Crack ' Sergeant and Corporal	red ribbon.
Passed Qualifying Examination for Assembly N.C.O.s	green ribbon.

The Girls' Secretary is responsible for collection of the badges and their
redistribution at the proper times, and must in particular see that the badges
of Prefects who are absent do not get lost. Prefects are not entitled to any
of the privileges or immunities attaching to prefectship unless they are wearing
their badge of rank.

Badges of Office: Girls' Side.

Captain	red plush : letter ' C.'
Sub-Captain	,, ,, letter ' s.'
Secretary	,, ,, letter ' S.'
Superintendent of Playground Mothers	plush, letters 'S.O.M.' in monogram.

Note.—Playground Mothers are not Prefects, but are entitled to wear a blue
badge.

Cloak-room Curators	blue disc, yellow centre.
Sanitary Inspectors :	
Outdoors	blue disc, red centre.
Indoors	blue disc, pale-blue centre.
Games Captain	green rosette, red centre and edging.
Games Sub-Captain	red rosette, green centre and edging.
Assembly Sergeants	red disc, blue centre.
Assembly Corporals	yellow disc, black centre.
Assembly Lance-Corporals	green disc, red centre.
' Crack ' Sergeant and Corporal	red ribbon.

Qualified by examination as N.C.O.s.—These are entitled to wear a green ribbon.

The Secretary is responsible for collection of the badges and their redistribution at the proper times, and must in particular see that the badges of Prefects who are absent do not get lost. Prefects are not entitled to any of the privileges or immunities attached to prefectship unless they are wearing their badges of rank.

COWLEY SCHOOLS, ST. HELENS.

Civic Government by Boys.

(Written by a Boy at School.)

There is at Cowley a system of self-government by the boys, whereby every boy of average intellect is given a chance of commanding his smaller school-fellows.

Description of System.

School of 300 divided into 8 houses, and forms as usual.

Each house has a captain, a vice-captain, and a house-master, in addition to house-prefects.

Inter-house competitions take place between houses in Rugby, cricket, boxing, swimming, and work. To each of the champion houses there are cups awarded.

The school itself has about eight prefects and eight sub-prefects, who have a private room of their own. These are led by a head-prefect.

The games in the school are compulsory, and take place after 4.15 on Monday, Tuesday, Thursday, and Friday, and also on Wednesday afternoon.

On Monday, Tuesday, Thursday, and Friday, for twenty minutes after 12 o'clock, the school does 'drill,' not physical (takes place under tutorship of master) but military. It is an actual fact in this school that the 'boys' do self-govern and masters are in the school only to teach in certain periods. Why are the boys able to do this? For the following reasons :—

Spirit of Command is fostered.

(a) Captains of Forms are responsible for their forms, and as form matches take place between forms the captain has ample scope of the exercising of his authority.

(b) The games are run in sets. Every set has two or three captains, and almost every boy in the school has a chance of captaining a side in Rugby or some other game at some time or other.

(c) As a boy grows older, having had practice as a form or set captain, he becomes, perhaps, if he shows energy, vice-captain and the captain of a house. This means work and plenty of it. When a boy becomes a house-captain he begins to understand the spirit of responsibility.

(d) Minor and then chief commanding position in drill.—This point cannot be too greatly emphasised. It is a well-known fact that chaps in this school hate military drill until they obtain a minor command. This enlivens their interest, and the worst grouser after a year of commanding realises the value of the drill—not for its military value, but for the effect it has on one's personality. A boy afraid to enter a room containing a master may come out so much in a year by commanding a squad in the drill that a whole squad of masters would not and do not frighten him.

(e) After these preliminary steps to the top a boy becomes first a sub-prefect and then a prefect, a captain of games (either Rugby or cricket) or a chief commander in the drill.

In this way the spirit of command is fostered and a boy leaves Cowley fit to rule a kingdom (as well as the Coalition do England). A prefect may give a boy lines, he may, in special cases, whack a boy, but what backing does the prefect get from the powers that be?

(a) The Head supports him.

(b) The masters tolerate him.

(c) The governors don't know what he is and will not admit his rights.

Still the Head's power goes a long way, and it is very few foolish parents who seriously object to the prefect system. (In one or two cases only each year is a boy beyond the control of prefects.)

The prefect's chief weapon at Cowley is public opinion :

This shows out most clearly in the houses.

A boy transgresses (fumbles a ball in a house match or some other terrible thing). The house-captain appeals to the house. Is the boy worthy of a whacking? The house decides—the boy is whacked. All the parent can say against this system is that the house are bullies, but can forty boys be all bullies? The house whackings are, therefore, in most cases fair, and become, as has already been said, the prefect's chief weapon.

Public opinion :

Some prefects are liked very much—others are tolerated. Some are disliked.

It is safe to say that most prefects are not disliked, and if a prefect is not disliked then public opinion helps him.

Duties of a prefect :

With these weapons a Cowley prefect sallies forth. No master challenges his right. Masters teach and disappear. The prefects go in lessons and then appear for duty in keeping the school in order, in arranging games, &c. But no master interferes. Not even a house-master has much power in his own house. A Cowley prefect has no masters to rival his authority. He can give lines and whack (Only me! T.B.) (when backed by public opinion or in special cases). A boy, with the help of set captains, through the school, manages the games. The prefects manage the drill (alone). Do not the prefects then rule the school, and, as public opinion either supports or does not support a prefect, then is not the school self-governed by the boys?

PENARTH COUNTY SCHOOL FOR GIRLS, WALES.

(Written by a Girl at School.)

The aim of self-government in school is an attempt to train the child as a responsible citizen without taking away his delight in childish things. It is an attempt to train him to be that type of child who will later on in life be a self-respecting, responsible citizen, and, as such, it includes the true purpose of education.

It is impossible for any man or woman to be capable of controlling even a local committee if he has not been trained to think for himself and act as a responsible member of society. If children are not made to think for themselves, if teachers and parents insist on doing all the directing and thinking for them, they cannot expect that the child, when he grows up, will be capable of looking after himself and his fellow-men.

It is difficult in an ordinary county school to carry on self-government to a great extent, because external examinations demand so much time, and there is a certain amount of work to be got through in a limited time. And the space and means at the disposal of the Head-mistress are so limited that it needs clever organising to be able to stray from the beaten track, but there is a certain amount of experimenting possible even in the busiest school.

Practically all that we have done originated in our English Composition lessons, but to give a definite account of the gradual growth of self-government in school is impossible.

In IV.c and IV.b there is a Form Committee which is elected by the form to manage all matters connected with the form.

The III., V.b, and V.a have themselves arranged a programme of English Composition lessons for this term, and every form has a chairman who is prepared to take the class and direct affairs at any moment. The lessons are varied and are all such as will increase the pupils' knowledge of and command over the English language and literature, and will give them confidence to act on their own initiative. It is significant that the younger girls soon got over their shyness of facing a class and being called upon to give an 'Oral Composition,' but that older girls, who when working for examinations had little

time to spare, afterwards when they tried took far longer to get used to the idea, and their attempts were often feeble.

It gradually became felt that something ought to be done to bring the scattered committees together, and with the guiding help of the staff the idea of a Girls' Representative Council was thought of.

It was decided that the Council should be formed consisting of the secretaries of the different committees and a form representative from every form, except the two lowest forms.

The voting for Secretaries and Representatives was done by ballot by the whole school, with the head girl, chosen by Miss Lloyd, as President.

The Council sat for the first time in September 1919 and meets on an average once a month. As the Girls' Representatives they carry out as far as possible all new ideas and desires of the girls which have received the Head-mistress's sanction. They act entirely on their own responsibility and as a rule are successful. The last Prize-giving was practically all the work of the girls.

One of the most important features is the General Knowledge Club. Any girl from IV.c upwards may belong, and it is held weekly. At the beginning papers were read by courageous individuals on varying subjects. Lately whole forms have combined to give a form entertainment.

At the end of last term Miss Lloyd asked the Council if they felt prepared to take the responsibility of looking after a blind girl in school. The whole Council agreed that it would be good for the girls to look after somebody who lacked something they all enjoyed, and they appointed a Committee to arrange her lessons, &c.

The girls have been, if anything, too kind, but now they seem to be realising that it will be kinder to show her how to get about school on her own, rather than that they should take her. This training is excellent for the girls, for no one can be a good citizen unless he is gentle and courteous to those older or weaker than himself.

ROATH PARK BOYS' SCHOOL.

Prefect System.

The Prefect system has been in use for seven years and has passed altogether out of the experimental stage. It has proved its great value :—

1. In developing a sense of responsibility.
2. In training boys to lead and manage others.
3. In improving the tone of the school.

Duties of prefects :

1. The head-prefect has a general supervision over the work of the other prefects.
2. The other prefects take entire charge of
   (a) Bell-ringing.
   (b) Letters for staff.
   (c) The lavatories, towels, soap.
   (d) Latrines (regularly inspected).
   (e) The stairs during entrance and dismissal.
   (f) Lost and found articles.
   (g) Late comers (lateness nearly stamped out by the system).

3. On rainy days, during interval, the prefects take charge of classes.

Self-government :

Prefects, with help of teacher, elect to vacancies, and also elect their own head-prefect.

Offences committed by boys are reported to head-prefect, who investigates, and reports, if necessary, to teacher of offender. If case is serious, to Head-master.

Inattention to duties on the part of, or offences committed by, prefects, are, first of all, investigated by a 'tribunal' of prefects. If case is proved, the offender is admonished by the head-prefect. Serious cases are reported to the Head-master.

Scout-craft.

For two years, 1916-18, one hour per week of the school time-table was devoted to scout-craft. Boys were trained and drafted into regular troops. The advantages of working as a preparatory organisation for feeding other troops instead of running a regular school troop are :—

1. Boys over 14 years of age could continue as members of the troops they join, and so avoid the 'break' when leaving school.
2. Most of the better boys of the school leave for secondary schools, and could continue scout-training only as members of regular troops.
3. A troop run as part of school-work would always be associated with 'compulsion.'

Objects of system :
1. To develop character, work, self-training.
2. To counteract 'cinemas,' discourage idleness and thriftlessness.
3. To interest boys in Boy Scout and Boys' Brigade organisations.
4. To draft boys into local troops.

Membership :
1. Any boy in Standard V. and upwards may be a candidate.
2. He is admitted after one month's home-work as a 'test.'

Method :
I. Weekly meeting of one hour for instruction, including :
    1. Talks on lines of Scout yarns.
    2. Preliminary tests for Tenderfoot badge.
    3. Talks on troops in Cardiff.
    4. Practical work, such as
        (a) Knot-making ;
        (b) Flag-sketching ;
        (c) Signalling ;
        (d) Map-reading ;
        (e) Use of the compass.
    5. In most cases the leader of each patrol was responsible for training the boys in his section.
II. Patrols.
    1. There were six patrols—A, B, C, D, E, F.
    2. Each patrol had its leader, second, third, &c.
    3. Leaders and seconds acted as school prefects.
    4. Each boy had a note-book (ordinary exercise book) in which he kept a record of work done at home.
    5. The books were marked by the leaders.
    6. In addition to ordinary school-work the following were accepted as satisfactory 'home' work :
        (a) Model-making ;
        (b) Sketching ;
        (c) Hobbies ;
        (d) Choir-practice ;
        (e) Music lessons ;
        (f) Attendances at churches ;
        (g) Attendances at Scout or other organisations.

The High School of Glasgow.

Extract from Prospectus for 1914-15.

Prefects.

Prefects are divided into two classes—Form and School.

Each Form has two prefects, one chosen by the master, the other by the Form, subject to the veto of the master. These should be boys who have most distinguished themselves for public spirit. In consultation with the master they are responsible for—

    1. The efficient government of the Form in the master's absence.
    2 The general welfare of the Form.

They will be under the command of the school prefects for police duty.

School prefects are appointed by the Rector.   They are divided as follows : The Captain; the Lieutenant; four Buildings Sergeants; four Privates.

The captain of the school is *ex officio* head of the games.   Disobedience to a prefect's order is an offence against school discipline.

The duty of a prefect is to see that the welfare of the school is maintained.

The purpose of this class is the gaining of an intelligent acquaintance with the facts connected with our system of government, both local and imperial, and with the rights and duties devolving on a citizen.   It is intended primarily for those just leaving school, and it forms a valuable introduction to the study of economics and industrial history :—

Methods of election of various governing bodies; powers and duties of Parish, County, and Town Councils; Local Government; Education, Poor Law, Public Health, Licensing, and Harbour Authorities; judicial and financial systems, local and national; election of a member of Parliament; the Party system; the Cabinet and the Departments of the Executive Government; Governments within the British Empire; relations with Foreign States.

APPENDIX V.

Suggestions for Organising Regional Study and Maintaining a Permanent Regional Record in a Parish.

(By the Earl of Lytton.)

I. Objects to be aimed at.

1. To prepare and keep up to date a complete historical survey and history of the area.
2. To establish a local Regional Museum illustrative of the survey.
3. To secure the maximum educational advantages from the work.
4. To enlist the services of the children in the work, thus helping to train their observation, stimulate their interest in their surroundings, and develop their faculties.
5. To make the school and its work a centre of interest to all who live in the neighbourhood.
6. To secure co-operation between the teachers, the children, and the adult population.

II. Steps should be taken to :

1. Hold a meeting for the discussion of the subject.
2. Form a Regional Association for the study of the parish.
3. The school to be recognised as the Regional Museum of the parish.
4. Enlist the interest and co-operation of the teachers and children of the school.
5. The survey and record to be kept at the school; the teachers and older children to become the first members.
6. Secure the interest and co-operation of the local Boy Scouts or any other local organisation.
7. All members of the Association will be expected to keep notes of any observations they may make and give them to the school to be incorporated in the central record.
8. Members will also be expected to present to the school museum any articles of interest they may find, such as flint implements, pottery, coins, fossils, &c., also contemporary objects of local historical interest.
9. Members will also be expected to assist in the preparation of historical records of any old buildings in the parish and in supplying information about the houses which they occupy, as well as in keeping the annual record up to date.
10. It is further hoped that members will help to provide in the school books of reference on natural history or archæological matters, materials for exhibit cases, &c., to be made by the children, illustrations in the form of photographs or pictures of matters of local interest.   They can also help by giving facilities to the children to make observations on their property by assisting them to pay periodical visits to local museums and by lending books or objects for any special branch of study.

III. Things which in course of time the school should contain.

1. A map of the parish, or area to be surveyed, on which every field, hedgerow, road, lane, wood, river, pond, or house should be given a distinctive number, and its separate name, where these can be ascertained.
2. A permanent register or record of the past history of every place in the area, and of every fact which can be observed concerning the life of these places year by year, under their distinctive names and numbers, &c.—e.g. changes of property, rotation of crops, habits of animals, arrivals and departures of migrants, striking weather conditions, &c.
3. A series of traced maps showing separately—

> (a) The geology.
> (b) The vegetation.
> (c) The water supply.

4. A summarised history of all ancient buildings and monuments, and a bibliography of books referring to them.
5. A brief description of every modern building, containing the date of its construction, alterations, and the names of the families occupying it.
6. A classified list of all animals, birds, butterflies, moths, insects, trees, shrubs, plants, and wild flowers found in the area.
7. Specimens of any articles of historical interest, whether ancient or contemporary, found in the area.
8. Illustrations of local plant and animal life, or, if desired, actual specimens of birds, nests, eggs, butterflies, moths, &c.   If such specimens are collected they must be properly preserved, mounted, and catalogued.
9. A list as complete as possible of Lords of the Manor, Rectors of the parish, Ministers of other denominations, Head Teachers of the school, Parish Councillors, distinguished natives of the parish, &c.
10. The record of the activities of the parish in the Great War 1914-1918, or in any other period of special interest.

APPENDIX VI.

Suggestions for Local Survey for Town Schools.

(By Valentine Bell.)

The environment of the child plays such an important part in its education that it is of the utmost importance that the school should be brought into closer touch with the school district. The school should not be a cloister. One of the first points to be driven home in the training of teachers should be that a teacher cannot become a really successful influence unless he or she is thoroughly acquainted with the school district. Local surveys are the best practical means of teaching live citizenship. Other advantages of survey work are palpable to any teacher.

Teachers should take advantage of the stores of information at the Town Hall, Public Library, local museum, and local Societies (Archæological, Botanical, Photographic, &c.). In most towns valuable information, e.g. old prints, plans, maps, &c., is stored away in local libraries and museums, and is rarely ever asked for. Local surveys can well be taken if the locality is made the means of approach to the education of the child.

The Geography Lesson.—Physical features, means of communication, industries, population, &c.

The History Lesson.—The old manors; the old views and maps of the district; the pastimes; the evolution of the means of travelling; old toll-gates; old buildings (manor-house, church, castle, abbey, gates, inns, &c.); the punishment of crime (police-stations, old watch-house); public-house and street names; old industries, &c.

The Drawing Lesson.—Sketches of objects connected with locality (industries, &c.); details of old church, castle, &c. (Norman, Gothic, and Tudor arches, tiles, &c.); pictures from history books; flowers in locality, &c.

The Writing and Composition Lesson.—Examples of local interest.

The Literature Lesson.—Interest should be aroused in the work of celebrities connected with the district by birth or residence. Local memorials, as statues or tablets, are frequently unknown by the inhabitants.

The Arithmetic Lesson.—This lesson can be made most practical when approached locally. Exercises on the park (areas); local shopping centres (compound rates); the river; the industries; Report of M.O. of Health (practical percentages, decimals, and graphs), &c.

The Science and Nature Study Lesson.—In elementary schools children should have the phenomena that surround them explained (*vide* Board of Education latest circular). *Lessons.*—Why doesn't a factory chimney fall? Why are water-tanks often above the roofs? How does a motor-bus go? Why do we get gravel here, chalk there, &c.? The dispersal of seeds of local plants, &c.

Lollard Street L.C.C. School, N. Lambeth.

Class Work (in most cases taken after Outdoor Work).

1. The Ordnance Map of the district (explanation of conventional signs, &c.).
2. The district as viewed from some 'outdoor tower' (top floor of school, summit of local hill, &c.), and directions driven home with use of the map. Important buildings noted, *e.g.* churches, factories, gasometers, &c.
3. The Physical Features of the district. (In crowded areas, the district before the houses. Map to be made.)
4. The Simple Geology of the district. (Digging operations for sewers, &c.; sections of local borings for wells.)
5. The Botany of the district. (In crowded areas, the vegetation in local parks and other open spaces is of great interest.)
6. The Growth of the district as traced from old maps. (The manors; the old houses marked on modern map. The influence of railway, trams, or new 'bus route can be seen.)
7. A chat on old views of the district. (Often of value in discussing No. 3.)
8. The Parish Registers and what they teach us.
9. The Streets. (Street names. Style of house; when built.)
10. The Public-houses. (Value of names. The Breweries.)
11. The Amusements of the district. (Cricket, football, &c. Comparison with Amusements of our forefathers. The revival of old dances.)
12. The Good and Bad Influences at work in the district. (Picture Palaces, Public-houses, Recreation-grounds, Home-gardening, Churches, Scouts, Bands of Hope, Polytechnics, &c.)
13. The Means of Communication in the district. (Railways, 'buses, tubes, trams, canals, &c. Suggestions for improvement.)
14. The Open Spaces and Recreation-grounds. (Various features compared, &c.)
15. The Local Industries. (Decayed, decaying, and modern. Causes of growth and decay.)
16. The Important Buildings. (Town Hall, Library, Public Baths, Churches, &c.)
17. The Local Authorities (work of Borough Council, Board of Guardians, Police Authority, &c.).
18. The Feeding of the locality. (Markets, Milk Supply, &c.)
19. The Health of the district. (Report of M.O.H., Graphs of Birth and Death Rates, &c.)

Architectural Development :
Lambeth Palace. Lambeth Church. Eighteenth and early nineteenth century houses.

The Amusements of the People :
Survey of Local Amusements. Sports. Kennington Oval. Open spaces. Picture Palaces; Music Halls; Public-houses; the Old Pleasure Gardens; Vauxhall Gardens; Duck-shooting in Lambeth Marsh; the last London Maypole; the Regatta—old University Course from Westminster to Putney.

The Fight against Disease :
Report of Medical Officer of Health. Old Parish Registers. Survey of Hospitals, Dispensaries, and Medical Missions.

Local Government :
Survey of Lambeth's Local Authorities. The Old Church Vestry—Old Vestry Hall—Old and New Town Halls.

History for Four Upper Classes.

The classes will make a survey of the school district and by means of this approach an elementary knowledge of the social and industrial history of England will be attempted.

Introductory Lessons :
    (a) The Scroll of History.
    (b) The Centuries.
    (c) Leading Dates.

The Britons :
    Local Items.—Lambeth before the Houses. Boadicea's statue.

The Romans :
    The Old Kent Road. The Roman Boat found when digging the County Hall foundations. Stangate. The Roman Bath in the Strand.

The Land :
    Local notice-boards advertising sites for sale. Local examples of Freehold, Copyhold, and Leasehold. Local entries in Domesday Book. The increased value of Pedlar's Acre (site of County Hall). Notice summoning General Court Baron for the Manor of Kennington. Enclosure of Lambeth Green.

The Growth of Towns :
    The 1593, 1750, 1797, 1820, and 1870 maps of Lambeth. Old views of Lambeth.

The Growth of Industries :
    The present and past industries of Lambeth. Survey of local trade unions and friendly societies.

What we owe to Foreigners :
    The introduction of glass- and pottery-making into Lambeth (Venetians and Dutch).

The Evolution of the Means of Travelling :
    Survey of Lambeth's means of communication. The railways, tubes, trams, 'buses, taxi-cabs, motors, &c. The Brighton Road *via* Kennington Gate. The old inns.

The Punishment of Crime :
    The police-station. Lambeth Police Court. The Old Watch-house in High Street. The old Surrey Gallows at Kennington.

The Religious Life of the People :
    Survey of the various local churches and chapels, &c. Lambeth Palace. Bunyan's Hall.

The Growth of Education :
    Survey of local schools, evening institutes, polytechnics, colleges, &c. Lambeth parochial school (Archbishop Temple's). The Tradescants and Elias Ashmole.
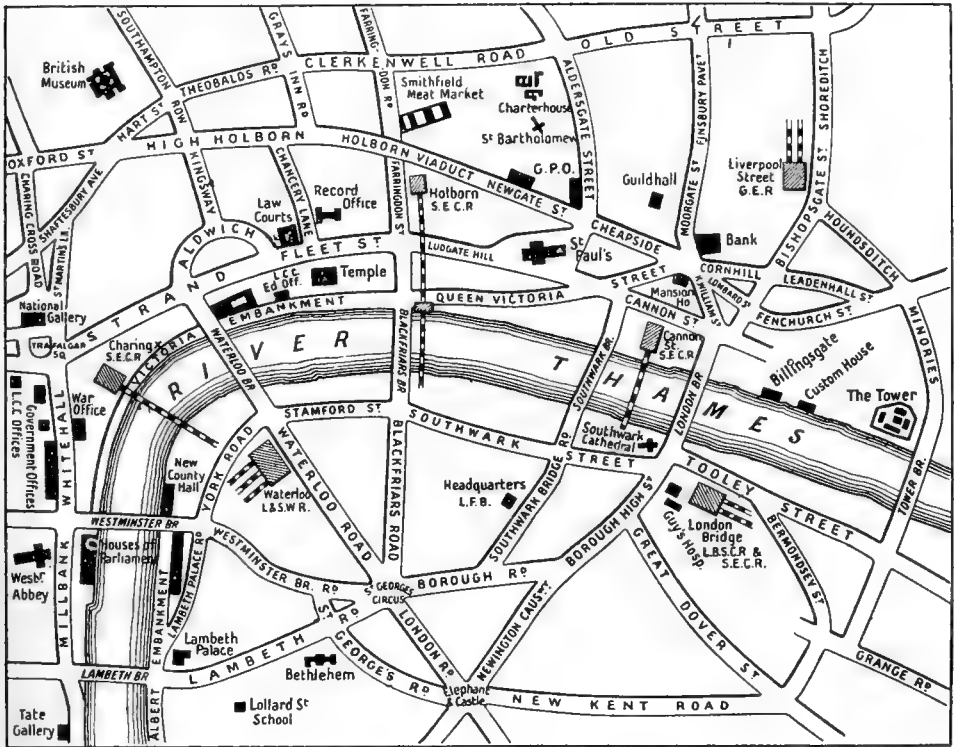
Places for Educational Visits.

Lambeth Palace.	The Temple.	St. Bartholomew the Great.
Tate Gallery.	Westminster Abbey.	The Guildhall.
National Gallery.	St. Margaret's Church.	The Tower.
National Portrait Gallery.	St. Paul's Cathedral.	Houses of Parliament.
British Museum.	Southwark Cathedral.	

Find the nearest and cheapest way of reaching the above.   Study the tram map.

VISIT TO WESTMINSTER ABBEY.
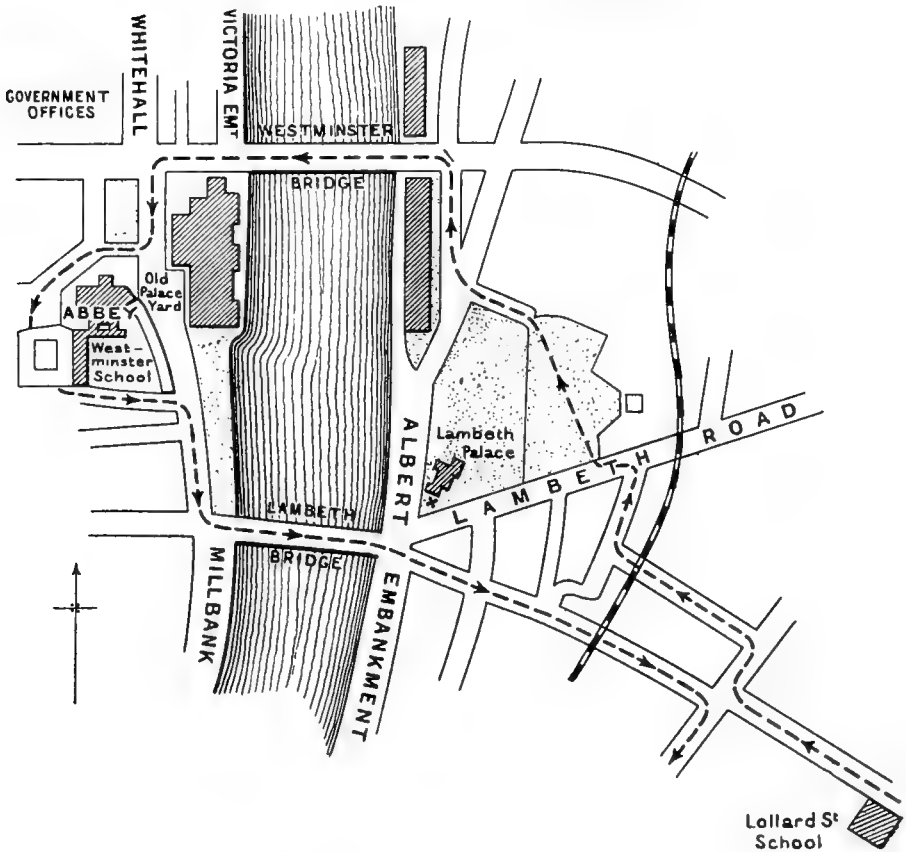
Write down the Route we shall follow.

Points of interest to be noted.

Georgian Houses in Pratt Street.
View of Lambeth Palace from Archbishop's Park.
Old Village Houses in Park Place.
Names of Public-houses in and near Paris Street.
The views up and down the river from Westminster Bridge.
The statues in Parliament Square.
The dwarfing of St. Margaret's by the Abbey.
The link-extinguishers in Dean's Yard.
The view of Lambeth from Millbank.
The Toll-house on Lambeth Bridge.

Kinds of Questions to ask Yourselves :—

Why is Juxon Street so named?   Who controls the Archbishop's Park?
Who built Lambeth Palace?   When were the Embankments constructed?
Who is responsible for the upkeep of Westminster Bridge?

ROUGH PLAN OF WESTMINSTER ABBEY.

NORTH PORCH

NORTH TRANSEPT

Sᵗ ANDREW Sᵗ MICHAEL Sᵗ JOHN BAPT

ISLIP

Sᵗ JOHN THE BAPTIST

Sᵗ PAUL

BELFRY

NORTH AISLE

AMBULATORY

WESTERN DOOR

NAVE

CHOIR

SANCTUARY

EDWARD CONFESSORS CHAPEL

HENRY VIIᵗʰ CHAPEL

SOUTH AISLE

Sᵗ NICHOLAS

JERUSALEM CHAMBER

NORTH CLOISTERS

SOUTH TRANSEPT

POETS CORNER

Sᵗ BENEDICT

Sᵗ EDMUND

COLLEGE HALL

DEANERY

WEST CLOISTER

CLOISTERS

EAST CLOISTER

Sᵗ FAITH

VESTIBULE

CHAPTER HOUSE

SOUTH CLOISTERS

CHAPEL OF THE PYX

Dimensions.

Length, with Henry VII.'s Chapel, 513 ft. Length of transepts : 200 ft.
Height of towers : 225 ft. Height of Church : 102 ft.

Find in dictionary the meanings of the following :—

Aisle, nave, sanctuary, ambulatory, cloister, transept, vestibule, deanery, chapter, pyx.

Find out any other terms used in architecture.

Draw a simple sketch illustrating a Norman and a Gothic arch.

VISIT TO THE TOWER OF LONDON.
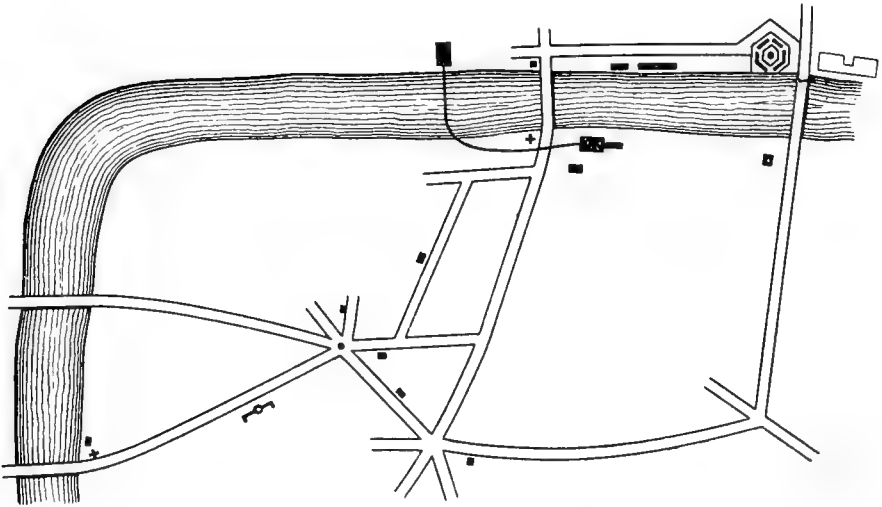
1. Name our route and the buildings marked.

2. What tram services run through the Obelisk and the Elephant and Castle?

3. What bridges can be seen from London Bridge? In what ways are the banks of the Thames at London Bridge different from those at Lambeth Bridge?

4. What steamships did you notice? What flags were flying? What evidences existed in the Pool of the war being on?

5. Where was the Fishmongers' Hall? What is it? How do you account for its site?

6. What particularly struck you in Lower Thames Street? Name some of the public-houses. What type of shop was open?

7. Describe the dress of a Billingsgate fish-porter.

8. Draw a rough plan of the Tower of London, marking the Moat, Inner and Outer Bailey, and the Keep.

9. What are the following : The arquebus, matchlock, flintlock, mace, lance, stink-pot, rack, scavenger's daughter, visor, pike, chain-shot, grape-shot, halberd?

10. Describe briefly the historical growth of the Tower.

11. Name any notable folk connected with the Tower.

12. In what Tower are the Crown Jewels guarded? Explain K.G., K.T., K.P., V.C., D.S.O., G.C.B., K.C.M.G., G.C.S.I., C.V.O.

13. What regiments were stationed at the Tower? Describe the dress of a ' Beefeater.'

Tidal Institute at Liverpool.—Report of Committee (Prof. H. LAMB, *Chairman*, Dr. A. T. DOODSON, *Secretary*, Sir S. G. BURRARD, Sir C. F. CLOSE, Dr. P. H. COWELL, Sir H. DARWIN, Dr. G. H. FOWLER, Admiral F. C. LEARMONTH, Sir J. E. PETAVEL, Prof. J. PROUDMAN, Major G. I. TAYLOR, Prof. D'ARCY W. THOMPSON, Sir J. J. THOMSON, Prof. H. H. TURNER).

1. *Report on Harmonic Prediction of Tides.* *By* A. T. DOODSON, *D.Sc.*

THE present state of harmonic prediction of tides cannot be regarded as very satisfactory, and this report has been written with the object of calling attention to the matter. For some of the information the writer is indebted to the Hydrographic Department of the Admiralty.

While real accuracy in the prediction of tides is not obtainable at present, owing to inability to predict effects of meteorological variations, yet one would expect that the normal, or undisturbed, or periodic tide could be accurately given. That such is not the case is well known to those who have compared observations with predictions ; there are periodic or systematic differences in height and time of high water which are sufficiently serious in many cases to cause distrust. This is especially the case with harbours in river estuaries or in comparatively shallow seas, and, in fact, the distrust has led in many cases to the complete abandonment of the method of harmonic prediction. Thus the Hydrographic Department of the Admiralty report that the German and Netherlands tidal authorities have found the methods of harmonic prediction so seriously in error that they have abandoned them, and the experience of the Hydrographic Department for the North Sea has also been very unfavourable to the continuance of this method. For many ports situated in estuaries, and catered for by British authorities, it is customary to apply non-harmonic corrections to the results of harmonic prediction.

It is generally admitted, however, that harmonic predictions for oceanic ports (*i.e.*, ports open to the free influence of the deep water oceanic tide wave) reach a high degree of accuracy. The general continuance of the harmonic method of prediction, therefore, will depend very largely upon the solution of the 'shallow-water problem.' This calls for scientific research, and, concerning it, reference may be made to future reports of the Committee.

That the method of harmonic prediction for river ports should be in danger of discontinuance could be taken as sufficient evidence regarding the unsatisfactory state of harmonic prediction, but a few figures may serve to show what degree of inaccuracy is considered by authorities to be unsatisfactory. For instance, at Quebec the average error, regardless of sign, is as high as 16 minutes for high water, and 28 minutes for low water predictions, though the harmonic constants for Quebec are based on over 19 years' continuous observation. Again, a comparison of observations with predictions of high water at Liverpool shows a very marked oscillation in the differences, of which the following is an example :—

10, 6, 11, 3, 9, 4, 13, −1, 8, −7, 8, 2, 8, 2, inches ;

thus successive high waters are alternately predicted too low or too high by as much as seven inches. (The predictions are taken from the tables of 1918. It should be stated, however, that these are not purely harmonic predictions, but are corrected by non-harmonic methods.) This is probably due to incomplete or faulty analysis, and, concerning this, reference may be made to Prof. Proudman's Report on Harmonic Analysis, where comparisons between observation and prediction of hourly heights are recorded for Liverpool, 1869 ; it has been shown that this represents also the present day state.

If, however, the above evidence were not available an estimate of the value of

harmonic prediction could be obtained from comparisons of the predictions made independently for the Admiralty and by the United States Coast and Geodetic Survey. One would naturally expect that predictions ostensibly obtained from the same harmonic constants would be fairly concordant, even if slightly different mean values were used, and that the lengthier the series of observations and analyses the better would be the correspondence in prediction. But an examination of the independent predictions shows that this is not so. The following table was prepared by taking one month's predictions from the Admiralty tables and comparing with the corresponding predictions published in the United States tables. The maximum difference and average difference, taken without regard to sign, are given for the heights and times of high and low waters.

Port	Number of years analysed	Mean range of spring tide	Heights		Times	
			Greatest difference	Average difference	Greatest difference	Average difference
		feet	inches	inches	mins.	mins.
Liverpool . . .	7 or over	28	11	5	21	10
St. John, N.B. .	15 ,, ,,	23	12	3	16	4
Bombay . . .	17 ,, ,,	15	7	1·4	32	4
Aden	16 ,, ,,	7	5	1·6	30	8
Wellington . .	2	4	7	2·5	34	16
Balboa . . .	1	16	3	1·2	34	10

In the case of Aden, the range of tides on certain days is small, so that the time of high and low water is uncertain; these days have been ignored.

The above table in itself bears testimony to the unsatisfactory state of harmonic prediction, even if we had no further evidence. We are not at the moment concerned with the cause of any discrepancy between the two independent predictions so much as with the general results. Where, as in the case of Liverpool, we may have two different predictions, each supposed to be authoritative, which differ occasionally by nearly a foot and on an average by five inches in height, then one's confidence in the accuracy of prediction is badly shaken. The differences in times are also serious and are considered so by all authorities; these provide the best test of the accuracy of prediction in all cases, since the height differences are naturally small when the mean range of spring tides is small.

While the maximum and average differences are conclusive, there is also much interest in a fuller examination of the differences that do occur. It is found that there are periodic differences of some magnitude in practically all cases. For Liverpool, on certain days both high waters have a positive difference and both low waters a negative difference; that is, there is a semi-diurnal oscillation (in the differences), of which the amplitude is about seven inches; moreover, there are diurnal oscillations also present in the differences, as is shown by one set of high-water differences being consistently lower than the alternate set, e.g.

$$4, 8, 0, 4, -4, -1, -7, -3 \text{ inches.}$$

Similar results are found in the differences in times of high and low water. As another example, in the case of St. John, N.B., there is a semi-diurnal oscillation of nearly a foot on certain days, as is shown by the following series of differences in heights for successive high and low waters :—

$$-7, 12, -12, 11, -8, 12, -11, 12 \text{ inches.}$$

The effects vary with different machines, but, generally speaking, for all the ports there are systematic differences which should not be allowable.

Enough evidence has now been given to show why it is considered that harmonic predictions are not at present satisfactory; two independent methods of judging them have been considered, the first being based upon the experience of tidal authorities in comparing predictions with observation, and the second being based upon the disagreements that exist between two sets of predictions calculated independently of one another.

It is quite certain that the chief cause for the failure of harmonic methods for shallow-water ports has been the incompleteness of analysis, and the absence of a reliable method of dealing with the shallow-water constituents. This is under investigation.

Regarding the differences in predictions, it is necessary to consider four possible causes:

(1) differences in the values of harmonic constants used by the two authorities furnishing the predictions,
(2) differences in the number of harmonic constituents that can be taken into account on the machines used for the calculation,
(3) application of corrections, as at Liverpool,
(4) uncertainty in the behaviour of machines.

It will be admitted that the choice of harmonic ' constants ' that can be made ought not to be a serious matter, especially where there are lengthy series of observations, as in the first four examples quoted.

In the case of Balboa, it is believed that the harmonic constants used are identical, and that no corrections have been made to the machine results, so that one seems driven to assign the fourth cause. There is evidence from other sources which tends in some cases to throw a certain amount of doubt on the behaviour of predicting machines, but this also is under investigation by the Committee.

The machines used for the purpose of predicting tides differ considerably in the number of constituents that can be taken into account, but it is not much palliation even to know that a machine is as accurate as it can possibly be, when important constituents have necessarily to be treated as non-existent. Investigations at the Tidal Institute point to the conclusion that the number of constituents required to deal adequately with the shallow-water problem is considerably greater than that allowed for in the building of tide-predicting machines. It is partly for this reason that corrections by non-harmonic methods are sometimes applied, though their success is very doubtful.

The whole problem of the harmonic prediction of tides is being investigated by the Committee, in collaboration with the Liverpool Tidal Institute and other bodies interested in tidal work, and further information concerning progress will be presented in their reports to the Association.

2. *Report on Harmonic Analysis of Tidal Observations in the British Empire. By* J. PROUDMAN.

1. The following report was undertaken for the Association through Prof. Lamb, who has collected information from all the authorities concerned, and has been in consultation with the author during the whole time of its preparation.

It is based on information very kindly supplied by the Admiralty, the National Physical Laboratory, Messrs. Roberts, Prof. D'Arcy Thompson, the Survey of India, the Tidal Survey of Canada, Prof. R. W. Chapman of Adelaide, the Government Astronomer of Western Australia, and the Tidal Survey of New Zealand, as well as on the published literature of the subject.

In the first place an indication is given of the origin of the various harmonic constituents, which aims at explaining more than the customary popular accounts, while avoiding the heavy mathematical formulæ required for the analysis itself.

In the second place a table is given of the results of analysis, the inconsistencies in which show that the subject is in an unsatisfactory state.

In the next place an account is given of the various methods of analysis that have been used hitherto, with the object of making prominent their essential features, and providing the basis of a critical examination of them. To complete this critical examination requires a large amount of computative labour.

Finally, a complete historical account is given, with bibliographies and lists of analyses made.

It is to be remarked that the principle of the harmonic analysis is part of the theory of the *small* oscillations of a dynamical system, and its application becomes less accurate as the range of tide becomes a larger fraction of the depth of the water, or as the tidal currents become greater. It yet remains to be found to what

precise extent the purely astronomical tide at any station may be expressed as a series of a reasonable number of harmonic constituents. When this has been done and the methods of analysis and prediction refined so as to give predictions correct to this extent, a hopeful investigation may be made into the residual astronomical tide and the whole of the meteorological disturbance.

In a preliminary report * presented to the Geophysical Discussion of June 1918, it was stated that tide tables as at present produced appear to be adequate for practical needs. This was based on the facts that the practically important constituents can be determined fairly accurately, and that harmonic prediction presents no theoretical difficulties like those of harmonic analysis. The investigations of Dr. A. T. Doodson show, however, that the published tables of harmonic predictions are also very unsatisfactory.

Harmonic Tidal Constituents.

2. The gravitational forces generating the tides are derivable from a potential which is everywhere proportional to what the height of the tide would be if water covered the whole earth and had lost its inertia without losing its gravitational properties.

Such a tide—the equilibrium tide—may be calculated by adding the amounts by which a certain pair of nearly spherical surfaces of revolution project above the mean water-level. Each of these surfaces encloses a volume equal to that of the earth, and is slightly variable in shape. They move so that their axes, while always passing through the centre of the earth, pass also always through the centres of the sun and moon respectively.

The tides due to either of these spheroids may be expressed as a series of constituents, each of which varies harmonically in a period determined by astronomical data. From dynamical principles it follows that to each of these constituents there will correspond a similar constituent in the actual tides, that is, a constituent varying harmonically in the same period.

To find, in the actual tides at any station, the amplitude of each of these constituents, together with the lag of its phase behind that of the corresponding constituent of the generating potential, is the object of the harmonic analysis of tidal observations.

Let us consider the speeds of the constituents of lunar origin; we have to examine the motion, relative to any point on the earth's surface, of the spheroid whose axis passes always through the moon.

The pole of this spheroid which is nearer the moon is a little further from the earth's centre than is the opposite pole, while the whole departure from sphericity depends on the distance of the moon.

Let γ denote the angular speed of the earth's rotation and σ the mean motion of the moon.

If the moon moved with constant angular speed in the plane of the equator and at a constant distance from the earth, we should have, at any station, high water occurring regularly at intervals of $\pi/(\gamma - \sigma)$, with a maximum range of tide at the equator. The rise and fall of the water would not quite be simply harmonic, but could be resolved, with sufficient accuracy, into a harmonic constituent of speed

$$2(\gamma - \sigma),$$

of amplitude inversely proportional to the cube of the moon's distance, and two much smaller constituents of speeds

$$\gamma - \sigma, \qquad 3(\gamma - \sigma)$$

and of amplitudes inversely proportional to the fourth power of the moon's distance. The fact that the moon does not move as here supposed causes many modifications, but it is only on the constituent of speed $2(\gamma - \sigma)$ that their effect need be considered.

Let us still suppose the moon to move in the equator, but take into account the

* *Brit. Assoc. Report* for 1918, pp. 15, 16.

elliptic, evectional and variational inequalities in her distance and motion. These inequalities have speeds

$$\sigma - \varpi, \qquad 2(\sigma - \varpi), \qquad \sigma - 2\eta + \varpi, \qquad 2(\sigma - \eta),$$

where ϖ denotes the mean motion of the lunar perigee and η that of the sun. The effect of each is to make the moon's sidereal motion increase and decrease with the reciprocal of her distance, and thus to make the period of the tides increase and decrease with their range.

The effect of the first order elliptic inequality and the evectional inequality is the introduction of new harmonic constituents of speeds

$$2(\gamma - \sigma) \pm (\sigma - \varpi), \qquad 2(\gamma - \sigma) \pm (\sigma - 2\eta + \varpi)$$

of which, for the reason just given, the greater are those of speeds

$$2(\gamma - \sigma) - (\sigma - \varpi), \qquad 2(\gamma - \sigma) - (\sigma - 2\eta + \varpi).$$

The effect of the second order elliptic and variational inequalities is sufficiently represented by the introduction of new harmonic constituents of speeds

$$2(\gamma - \sigma) - 2(\sigma - \varpi), \qquad 2(\gamma - \sigma) - 2(\sigma - \eta).$$

The daily mean level of the water depends slightly on the departure from sphericity of the spheroid, so that we have long-period elliptic, evectional, and variational constituents of speeds,

$$\sigma - \varpi, \qquad \sigma - 2\eta + \varpi, \qquad 2(\sigma - \eta),$$

respectively.

3. If the moon moved with constant angular speed in a parallel of latitude other than the equator, consecutive high tides would be unequal except at the equator, and we should require the introduction of a new constituent of speed

$$\gamma - \sigma,$$

with an amplitude vanishing at the equator. Also, the amplitude of the constituent of speed $2(\gamma - \sigma)$ would be less than when the moon was in the equator.

But since the declination of the moon changes, the diurnal constituent requires modification. If its amplitude could be regarded as changing harmonically with speed σ, it would be replaced by two harmonic constituents of equal amplitudes and speeds

$$\gamma - \sigma \pm \sigma.$$

Owing to the fact that this is not quite so, the amplitude of the constituent of speed $\gamma - 2\sigma$ is a little greater than that of speed γ, and there is another smaller constituent of speed

$$\gamma + 2\sigma.$$

Again, introducing the first order elliptic inequality we get new harmonic constituents of speeds

$$(\gamma - 2\sigma) \pm (\sigma - \varpi), \qquad \gamma \pm (\sigma - \varpi),$$

of which those of speeds

$$\gamma - \sigma \pm \varpi$$

are regarded as forming a single constituent of speed

$$\gamma - \sigma$$

and slowly varying amplitude. The second order elliptic, the evectional and variational inequalities give rise to new constituents of speeds

$$(\gamma - 2\sigma) - 2(\sigma - \varpi), \qquad (\gamma - 2\sigma) - (\sigma - 2\eta + \varpi), \qquad (\gamma - 2\sigma) - 2(\sigma - \eta).$$

Also, the changing declination of the moon causes the amplitudes of the semi-diurnal constituents to vary, but it is sufficiently accurate to take mean values in all cases except that of speed $2(\gamma - \sigma)$. As the effect is to make the speed and range of tide increase or decrease together, we get a new constituent of speed

$$2(\gamma - \sigma) + 2\sigma.$$

Again, the changing declination of the moon introduces the principal variation in daily mean level, in the form of a constituent of speed

$$2\sigma,$$

which with the first order elliptic inequality gives two more of speeds

$$2\sigma \pm (\sigma - \varpi).$$

The amplitudes of all the constituents depending on the inclination of the moon's orbit to the equator vary with the position of the node on the ecliptic. As the monthly mean level also depends on the inclination of the moon's orbit to the equator, we have a small constituent with a speed $\dot{N}$ equal to that of revolution of the moon's nodes.

The speeds of the constituents of solar origin may be similarly determined, but only the declinational and first order elliptic effects on the primary constituent need be considered.

4. On collecting the results we have the following tables. The constituents of the same species have similar geographical distributions of generating potential; they are arranged in decreasing order of magnitude. The symbols are the same as those in general use, with the exception of σ_1, which is now introduced for the first time. The corresponding amplitude in the generating potential is larger than that of some constituents given in the other species. All the constituents given in the tables have, according to Darwin, larger amplitudes in the generating potential than any omitted.

Semi-diurnal Species.

Symbol			Name					Speed
M_2	.	.	Principal lunar	.	.	.	.	$2(\gamma - \sigma)$
S_2	.	.	Principal solar	.	.	.	.	$2(\gamma - \eta)$
N_2	.	.	Larger lunar elliptic	.	.	.	$2\gamma - 3\sigma + \varpi$
K_2	.	.	Luni-solar	.	.	.	.	2γ
ν_2	.	.	Larger lunar evectional	.	.	$2\gamma - 3\sigma + 2\eta - \varpi$
L_2	.	.	Smaller lunar elliptic	.	.	$2\gamma - \sigma - \varpi$
T_2	.	.	Solar elliptic	.	.	.	$2\gamma - 3\eta$
$2N_2$	.	.	Second order lunar elliptic	.	.	$2\gamma - 4\sigma + 2\varpi$
μ_2	.	.	Lunar variational	.	.	.	$2\gamma - 4\sigma + 2\eta$
λ_2	.	.	Smaller lunar evectional	.	.	$2\gamma - \sigma - 2\eta + \varpi$

Principal Diurnal Species.

Symbol			Name					Speed
K_1	.	.	Luni-solar	.	.	.	.	γ
O_1	.	.	Larger lunar	.	.	.	.	$\gamma - 2\sigma$
P_1	.	.	Larger solar	.	.	.	.	$\gamma - 2\eta$
Q_1	.	.	Lunar elliptic	.	.	.	$\gamma - 3\sigma + \varpi$
J_1	.	.	Supplementary lunar elliptic	.	.	$\gamma + \sigma - \varpi$
OO_1	.	.	Second order lunar	.	.	.	$\gamma + 2\sigma$
ρ_1	.	.	Lunar evectional	.	.	.	$\gamma - 3\sigma + 2\eta - \varpi$
$2Q_1$	.	.	Second order lunar elliptic	.	.	$\gamma - 4\sigma + 2\varpi$
σ_1	.	.	Lunar variational	.	.	.	$\gamma - 4\sigma + 2\eta$

Long Period Species.

Symbol			Name					Speed
Mf	.	.	Lunar fortnightly	.	.	.	2σ
Mm	.	.	Lunar monthly	.	.	.	$\sigma - \varpi$
Ssa	.	.	Solar semi-annual	.	.	.	2η
—	.	.	Nineteen yearly	.	.	.	$\dot{N}$
—	.	.	Ter-mensual	.	.	.	$3\sigma - \varpi$
—	.	.	Monthly evectional	.	.	.	$\sigma - 2\eta + \varpi$
MSf	.	.	Fortnightly variational	.	.	$2(\sigma - \eta)$
Sa	.	.	Solar annual	.	.	.	η

Besides the above there is the constituent M_1 of speed $\gamma - \sigma$ which consists partly of that of variable amplitude of the principal diurnal species and partly of that of amplitude inversely proportional to the fourth power of the moon's distance. There is also the ter-diurnal constituent M_3 of speed $3(\gamma - \sigma)$ and amplitude inversely proportional to the fourth power of the moon's distance.

Shallow Water Constituents.

In shallow water a harmonic constituent is accompanied by others having for their phases multiples of the phase of the primary constituent. Also, two harmonic constituents are accompanied by two others, having for their phases the sum and difference of the phases of the primary constituents. Some of these shallow water constituents have speeds the same as those of certain primary constituents. In the following tables only those shallow water constituents are mentioned which it has been the custom to consider hitherto.

Primary Constituents affected by Shallow Water Constituents.

Symbol	Primary Constituents of Shallow Water Effect				Speed
M_2	K_1,	O_1			$\gamma + \gamma - 2\sigma$
L_2	N_2,	M_4			$4(\gamma - \sigma) - (2\gamma - 3\sigma + \varpi)$
μ_2, $2MS_2$	S_2,	M_4			$4(\gamma - \sigma) - 2(\gamma - \eta)$
K_1	M_2,	O_1			$2(\gamma - \sigma) - (\gamma - 2\sigma)$
O_1	M_2,	K_1			$2(\gamma - \sigma) - \gamma$
P_1	K_1,	S_2			$2(\gamma - \eta) - \gamma$
Q_1	K_1,	N_2			$(2\gamma - 3\sigma + \varpi) - \gamma$
Mf	K_1,	O_1			$\gamma - (\gamma - 2\sigma)$
Mm	M_2,	N_2			$2(\gamma - \sigma) - (2\gamma - 3\sigma + \varpi)$
MSf	M_2,	S_2			$2(\gamma - \eta) - 2(\gamma - \sigma)$
M_1	O_1,	N_2			$(2\gamma - 3\sigma + \varpi) - (\gamma - 2\sigma)$

Other Shallow Water Constituents.

Symbol	Primary Constituents of Shallow Water Effect				Speed
M_4	M_2				$4(\gamma - \sigma)$
M_6	M_2				$6(\gamma - \sigma)$
M_8	M_2				$8(\gamma - \sigma)$
S_4	S_2				$4(\gamma - \eta)$
S_6	S_2				$6(\gamma - \eta)$
MS_4	M_2,	S_2			$4\gamma - 2\sigma - 2\eta$
MK_3	M_2,	K_1 ;	M_4,	O_1	$3\gamma - 2\sigma$
$2MK_3$	M_2,	O_1 ;	M_4,	K_1	$3\gamma - 4\sigma$
SK_3	S_2,	K_1			$3\gamma - 2\eta$
MN_4	M_2,	N_2			$4\gamma - 5\sigma + \varpi$
$2SM_2$	S_4,	M_2			$2\gamma + 2\sigma - 4\eta$

Meteorological Constituents.

The observed values of Ssa and Sa are largely of meteorological origin, as also those of S_1 of speed $\gamma - \eta$.

Results of Analysis.

5. In order to show how far the results of harmonic analysis of hourly heights represent harmonic constants we give some figures relating to ten different analyses for Bombay. The tidal observatory chosen is regarded as one of the most satisfactory, and the results of the analyses of the records taken there are about the most consistent from year to year.

Each entry in the tables refers to ten different determinations of what ought to be the same constant; by 'standard deviation' is meant the square root of the mean of the squares of the differences from the mean.

It will be noticed that apart from

$$M_2, \quad S_2, \quad N_2, \quad K_2, \quad K_1, \quad O_1, \quad P_1$$

the deviations from year to year are so great as to prohibit any reliance being placed on the results of the customary analysing processes applied to a single year's record. In fact, instead of the results from all analyses for the same constant being equal, the apparent amplitude for one year is sometimes more than ten times that for another year, while the apparent lags are sometimes distributed through more than three quadrants.

We also give some figures relating to three different analyses of tidal currents at Smith's Knoll (Lightship off Norfolk). In 1911 six weeks' observations were analysed, but in 1912 and 1913 only two weeks' observations were analysed.

Tidal Heights at Bombay (Apollo Bandar) 1906–1915.

Consti-tuent	Greatest Amplitude	Least Amplitude	Difference	Mean Amplitude	Standard Deviation in Amplitudes	Maximum Difference in Lags	Standard Deviation in Lags
	Feet	Feet	Feet	Feet	Feet	Degrees	Degrees
M_2	4·017	3·934	·083	3·970	·021	1·51	·49
S_2	1·569	1·548	·021	1·560	·007	2·94	·78
N_2	1·011	·943	·068	·976	·023	4·67	1·69
K_2	·448	·374	·074	·404	·019	9·60	2·98
ν_2	·318	·033	·285	·200	·093	134·55	56·3
L_2	·139	·019	·120	·079	·033	197·09	—
T_2	·240	·022	·218	·151	·066	93·28	29·2
$2N_2$	·213	·056	·157	·136	·049	43·33	12·50
μ_2	·257	·177	·080	·208	·026	22·87	7·68
λ_2	—	—	—	—	—	—	—
K_1	1·394	1·376	·018	1·386	·006	1·19	·34
O_1	·664	·643	·021	·653	·007	1·31	·40
P_1	·431	·403	·028	·412	·008	3·07	1·00
Q_1	·174	·125	·049	·154	·014	18·98	5·77
J_1	·148	·052	·096	·096	·031	48·13	14·45
OO_1	—	—	—	—	—	—	—
ρ_1	—	—	—	—	—	—	—
$2Q_1$	—	—	—	—	—	—	—
σ_1	—	—	—	—	—	—	—
Mf	·057	·011	·046	·032	·013	91·71	25·2
Mm	·127	·010	·117	·058	·030	237·91	—
Ssa	·196	·106	·090	·142	·024	69·94	20·1
Sa	·163	·024	·139	·109	·043	252·71	—
MSf	·075	·014	·061	·032	·019	287·58	—
M_1	—	—	—	—	—	—	—
M_3	—	—	—	—	—	—	—
M_4	·109	·085	·024	·097	·007	33·93	10·36
M_6	·019	·013	·006	·017	·002	61·99	21·1
M_8	—	—	—	—	—	—	—
S_4	—	—	—	—	—	—	—
S_6	—	—	—	—	—	—	—
MS_4	·116	·064	·052	·090	·015	30·60	9·42
MK_3	·083	·007	·076	·054	·025	281·23	—
$2MK_3$	·068	·040	·028	·053	·010	25·56	10·62
SK_3	—	—	—	—	—	—	—
MN_4	·022	·002	·020	·014	·006	136·00	45·3
$2SM_2$	·045	·030	·015	·037	·004	32·65	10·71
S_1	·097	·062	·035	·079	·011	11·78	3·35

M_2 Constituent of Tidal Currents at Smith's Knoll.

Depth in mètres	Direction of Maximum Current			Maximum Current in cms. per sec.			Minimum Current in cms. per sec.			Phase of Equil. M_2 at max. current		
	1911	1912	1913	1911	1912	1913	1911	1912	1913	1911	1912	1913
2·5	S 32°E	S 4°E	S 3°E	74·0	98·0	85·3	− 8·5	− 7·4	−11·6	251°	247°	255°
5	,,28 ,,	,, 5 ,,	,, 3 ,,	90·3	105·2	91·6	,, 7·4	,, 8·6	,,12·3	253	247	254
10	,,27 ,,	,, 7 ,,	,, 4 ,,	90·2	98·7	92·5	,, 9·1	,, 7·2	,,11·8	255	246	254
15	,,27 ,,	,,10 ,,	,, 4 ,,	84·6	94·7	89·3	,, 7·5	,, 8·4	,,10·9	255	248	255
20	,,24 ,,	,,11 ,,	,, 4 ,,	80·4	88·8	84·0	,, 7·1	,, 8·9	,, 9·9	255	249	255
25	,,23 ,,	,,12 ,,	,, 4 ,,	75·8	85·3	78·5	,, 4·1	,, 7·4	,,10·4	258	249	257
30	,,25 ,,	,,14 ,,	,, 5 ,,	65·6	80·7	73·3	,, 5·3	,, 5·7	,, 9·9	260	248	258

The negative sign prefixed to the minimum current indicates that the current turns in the clockwise direction.

Harmonic Analysis of Hourly Heights.

6. We assume that the height of the water at any station may be expressed as the sum of a number of Fourier's series in time. In each of these series only a few terms are important; for instance, in the largest (M) series, the important terms are the constituents

$$M_1, M_2, M_3, M_4, M_6, M_8,$$

while in others (e.g. N or O) there is only one important term (N_2 or O_1).

A typical short period series will have speeds

$$\sigma_r, 2\sigma_r, 3\sigma_r, \quad . \quad . \quad . \quad n\sigma_r, \quad . \quad . \quad .$$

where σ_r is that of the diurnal constituent. We shall take $r = 0$ for the principal solar series, so that $2\pi/\sigma_0$ is a mean solar day.

The data for analysis consist in a number of heights at intervals of time equal to one mean solar hour.

The Isolation of the Principal Solar Series.

Arithmetic means are taken of the heights at the times

$$\frac{h}{24}\frac{2\pi}{\sigma_0}, \left(1 + \frac{h}{24}\right)\frac{2\pi}{\sigma_0}, \left(2 + \frac{h}{24}\right)\frac{2\pi}{\sigma_0}, \quad . \quad . \quad . \quad \left(N - 1 + \frac{h}{24}\right)\frac{2\pi}{\sigma_0}, \quad . \quad . \quad . \quad (1)$$

for each of the values $0, 1, 2, \quad . \quad . \quad . \quad 23$ of h.

For any value of h this process will leave unchanged the term

$$R \cos (m\sigma_0 t - \epsilon) \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

and consequently the whole S series.

From the term

$$R \cos (n\sigma_r t - \epsilon) \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

however, it will give

$$\frac{R}{N} \sum_{s=0}^{N-1} \cos \left\{2n\left(s + \frac{h}{24}\right)\frac{\sigma_r}{\sigma_0}\pi - \epsilon\right\}, \quad . \quad . \quad . \quad (4)$$

which is equal to

$$\frac{R}{N} \sum_{s=0}^{N-1} \cos \left\{2n\left[s\left(\frac{\sigma_r}{\sigma_0} - 1\right) + \frac{h}{24}\frac{\sigma_r}{\sigma_0}\right]\pi - \epsilon\right\}, \quad . \quad . \quad . \quad (5)$$

and to

$$\frac{1}{N} \frac{\sin\{Nn(\sigma_r/\sigma_0 - 1)\pi\}}{\sin\{n(\sigma_r/\sigma_0 - 1)\pi\}} \ R \cos \left\{ 2n\left[\frac{N-1}{2}\left(\frac{\sigma_r}{\sigma_0} - 1\right) + \frac{h}{24}\frac{\sigma_r}{\sigma_0}\right]\pi - \epsilon \right\} \quad . \quad (6)$$

on summation.

If σ_r/σ_0 is sufficiently near to unity, (6) may be replaced by

$$\frac{\sin\{Nn(\sigma_r/\sigma_0 - 1)\pi\}}{Nn(\sigma_r/\sigma_0 - 1)\pi} \ R \cos \left\{ 2n\left[\frac{N-1}{2}\left(\frac{\sigma_r}{\sigma_0} - 1\right) + \frac{h}{24}\frac{\sigma_r}{\sigma_0}\right]\pi - \epsilon \right\}, \quad . \quad (7)$$

which is exactly what we should have obtained if we had replaced (5) by the integral

$$\frac{R}{N} \int_{-\frac{1}{2}}^{N-\frac{1}{2}} \cos\left\{ 2n\left[s\left(\frac{\sigma_r}{\sigma_0} - 1\right) + \frac{h}{24}\frac{\sigma_r}{\sigma_0}\right]\pi - \epsilon \right\} ds \quad . \quad . \quad . \quad (8)$$

It is the general practice to choose N so as to make as small as possible the residue (6) from some one large term of the type (3).

When a year's record is to be analysed the value of N chosen is 369, and it is then generally assumed that all the residues (6) may be neglected; in other words, that the isolation of the S series is complete.

With a month's record Darwin's plan is to use for N both 30 and 27, and with a fortnight's record both 15 and 14. It is assumed that the amplitudes of K_2 and P_1 bear to those of S_2 and K_1 respectively the equilibrium ratios, and that the lags of K_2 and P_1 are equal respectively to those of S_2 and K_1, while T_2 is supposed simply proportional to S_2. The residues from all other constituents are neglected.

For N = 369 and the constituents

$$M_2, \quad N_2, \quad K_2, \quad K_1, \quad O_1, \quad P_1$$

the coefficient

$$\frac{1}{N} \left| \frac{\sin\{Nn(\sigma_r/\sigma_0 - 1)\pi\}}{\sin\{n(\sigma_r/\sigma_0 - 1)\pi\}} \right| \quad . \quad . \quad . \quad . \quad . \quad (9)$$

in (6) takes the values

$$\cdot 000, \quad \cdot 008, \quad \cdot 010, \quad \cdot 010, \quad \cdot 000, \quad \cdot 010$$

respectively, and the corresponding coefficient in (7) takes the same values.

For N = 15 and the constituents

$$M_2, \quad N_2, \quad K_2$$

(9) takes the values

$$\cdot 016, \quad \cdot 204, \quad \cdot 989,$$

respectively, and the only effect of replacing (6) by (7) is to give $\cdot 200$ instead of $\cdot 204$.

For N = 14 and the constituents

$$K_1, \quad O_1, \quad P_1,$$

(9) takes the values

$$\cdot 998, \quad \cdot 014, \quad \cdot 998$$

respectively, which are not affected by replacing (6) by (7).

The Isolation of the other Series.

7. We shall now consider the isolation of the series which has σ for the speed of its diurnal constituent, and shall refer to $2\pi/\sigma$ as a 'special day.' For the isolation to be carried out exactly like that of the S series we should require the heights at intervals of time equal to a 'special hour.' The method of summation we should then have is made the basis of the actual methods now under consideration. With each place in the summations is associated a definite time, and this in general is necessarily different from that of the height which is assigned to the place.

There are two ways in general practice of making the assignment, and we shall refer to these as the B.A. and the Abacus assignments. The first is used by Roberts and the Survey of India, while the second is that of Darwin's tidal abacus.

In the B.A. assignment all the given heights are used and the maximum difference between the time of a place in the summations and that of the height assigned to it is $\pi/24\sigma$ or half a special hour. When the times of two of the given heights are nearer to that of a place than this interval, both are assigned to it, while when no height has a time as near as this interval, none is assigned to the place. The former case occurs regularly when $\sigma < \sigma_0$, the latter when $\sigma > \sigma_0$. In both cases the mean is formed by dividing by the total number of heights used in the summation.

In Darwin's tidal abacus the height at each mean solar noon is assigned to that place in the summation whose time differs from this noon by less than half a special hour, while any two consecutive heights belonging to the same mean solar day are assigned to consecutive places in the summation. As in the B.A. assignment all the given heights are used so that two heights are assigned to certain single places . . . when $\sigma < \sigma_0$ and no heights are assigned to certain places . . . when $\sigma > \sigma_0$.

Let us now examine the effects of these averaging processes on the various constituents.

Taking firstly the term

$$R \cos (m\sigma t - \epsilon), \qquad . \qquad . \qquad . \qquad . \qquad . \qquad (10)$$

the summation for the special hour h will give

$$\frac{R}{N} \sum_{s=0}^{N-1} \cos \left(\frac{mh\pi}{12} + m\sigma\tau_s - \epsilon \right) \qquad . \qquad . \qquad . \qquad . \qquad (11)$$

where τ_s denotes the excess of the time of the $(s+1)$th height in the summation over that of the place to which it is assigned, and N the number of terms in the summation.

Now, for given values of σ, m, and N, the sums

$$\frac{1}{N} \sum_{s=0}^{N-1} \cos (m\sigma\tau_s), \qquad \frac{1}{N} \sum_{s=0}^{N-1} \sin (m\sigma\tau_s) \qquad . \qquad . \qquad . \qquad (12)$$

may be determined accurately by direct addition. This would be laborious and the sums are replaced by integrals. For the B.A. assignment they are replaced by

$$\frac{1}{2T} \int_{-T}^{T} \cos (m\sigma\tau) d\tau, \qquad \frac{1}{2T} \int_{-T}^{T} \sin (m\sigma\tau) d\tau, \qquad . \qquad . \qquad . \qquad (13)$$

respectively, where T is half a special hour. The effect is to replace (11) by

$$\frac{\sin m\sigma T}{m\sigma T} R \cos \left(\frac{mh\pi}{12} - \epsilon \right). \qquad . \qquad . \qquad . \qquad . \qquad (14)$$

For the abacus assignment (11) is replaced by

$$\frac{\sin m\pi/24}{m\pi/24} \frac{\sin \{ m(\sigma/\sigma_0 - 1)\pi \}}{m(\sigma/\sigma_0 - 1)\pi} R \cos \left\{ \frac{m}{12} \left[\tfrac{1}{2} \left(1 - \frac{\sigma}{\sigma_0} \right) + h \right] \pi - \epsilon \right\}. \quad (15)$$

Taking next the term

$$R \cos (n\sigma_r t - \epsilon), \qquad . \qquad . \qquad . \qquad . \qquad . \qquad . \qquad (16)$$

the summation for the special hour h will give

$$\frac{R}{N} \sum_{s=0}^{N-1} \cos \left\{ n\sigma_r \left[\left(s + \frac{h}{24} \right) \frac{2\pi}{\sigma} + \tau \right] - \epsilon \right\}. \qquad . \qquad . \qquad . \qquad (17)$$

which we shall refer to as the residue from (16).

Again, for given values of σ, σ_r, n and N, the series

$$\frac{1}{N} \sum_{s=0}^{N-1} \cos \left\{ n\sigma_r \left(\frac{2s\pi}{\sigma} + \tau_s \right) \right\}, \qquad \frac{1}{N} \sum_{s=0}^{N-1} \sin \left\{ n\sigma_r \left(\frac{2s\pi}{\sigma} + \tau_s \right) \right\}, \qquad . \qquad (18)$$

may be calculated accurately by direct addition, and although this would be very laborious it could be done once for all.

Again, it is the general practice to try to choose N so as to make as small as possible the residue due to some one large constituent, and then to neglect all the residues. For example, when a year's record is available the B.A. plan is to take N = 357 for the M series and N = 343 for the O series. Darwin's plan for a month's record is to replace these numbers by 29 and 25 respectively, and for a fortnight's record by 14 and 13 respectively. These values are taken on the basis of the formula (17) with τ_s omitted.

Analysis of the Separate Series.

8. From the isolation process for each series we have 24 values

$$\zeta_0, \quad \zeta_1, \quad . \quad . \quad . \quad \zeta_h, \quad . \quad . \quad . \quad \zeta_{23}$$

associated with times which differ by intervals of one special hour. Certain fractions of these values are residues from other series.

The usual method of analysing the Fourier's series into its separate terms is by what we may call the 'least square rule.' If the series is expressed by

$$\zeta = A_0 + R_1 \cos (\sigma t - \epsilon_1) + R_2 \cos (2\sigma t - \epsilon_2) \quad . \quad . \quad + R_m \cos (m\sigma t - \epsilon_m) \quad . \quad (19)$$

the rule is given by

$$A_0 = \frac{1}{24} \sum_{h=0}^{23} \zeta_h,$$

$$R_m \cos \epsilon_m = \frac{1}{12} \sum_{h=0}^{23} \zeta_h \cos \frac{mh\pi}{12}, \qquad \qquad \qquad (20)$$

$$R_m \sin \epsilon_m = \frac{1}{12} \sum_{h=0}^{23} \zeta_h \sin \frac{mh\pi}{12},$$

and its application to the cases in question would be quite accurate if the isolation were perfect.

We must therefore consider the effect on the results of imperfect isolation.

Taking the S series, the effect of a term of the type

$$R \cos \left(\frac{h}{24} \frac{2n\sigma_r \pi}{\sigma_0} - \epsilon \right) \quad . \quad . \quad . \quad . \quad . \quad (21)$$

on A_0 is

$$\frac{1}{24} \frac{\sin (n\sigma_r \pi/\sigma_0)}{\sin (n\sigma_r \pi/24\sigma_0)} \ R \cos \left(\frac{23}{24} \frac{n\sigma_r \pi}{\sigma_0} - \epsilon \right), \quad . \quad . \quad (22)$$

so that the effect of the term (3) on A_0 is

$$\frac{1}{24N} \frac{\sin (Nn\sigma_r \pi/\sigma_0)}{\sin (n\sigma_r \pi/24\sigma_0)} \ R \cos \left\{ \left(N - 1 + \frac{23}{24} \right) \frac{n\sigma_r \pi}{\sigma_0} - \epsilon \right\}. \quad . \quad (23)$$

The effect of (21) on $R_m \cos \epsilon_m$ is

$$\frac{R}{24} \left[\frac{\sin\{(n\sigma_r/\sigma_0 + m)\pi\}}{\sin\{(n\sigma_r/\sigma_0 + m)\pi/24\}} \cos \left\{ \frac{23}{24} \left(\frac{n\sigma_r}{\sigma_0} + m \right) \pi - \epsilon \right\} \right.$$

$$\left. + \frac{\sin\{(n\sigma_r/\sigma_0 - m)\pi\}}{\sin\{(n\sigma_r/\sigma_0 - m)\pi/24\}} \cos \left\{ \frac{23}{24} \left(\frac{n\sigma_r}{\sigma_0} - m \right) \pi - \epsilon \right\} \right]. \quad . \quad (24)$$

When $n = m$ and σ_r/σ_0 is near unity the second part of this is much larger than the first, and it reduces to

$$R \cos \left\{ \frac{23}{24} m \left(\frac{\sigma_r}{\sigma_0} - 1 \right) \pi - \epsilon \right\} \qquad \qquad (25)$$

approximately; similarly the effect of (21) on $R_m \sin \epsilon_m$ reduces to

$$- R \sin \left\{ \frac{23}{24} m \left(\frac{\sigma_r}{\sigma_0} - 1 \right) \pi - \epsilon \right\} \qquad (26)$$

approximately.

On combining (7) with (25) and (26) we obtain as the corrections to

$$R_m \cos \epsilon_m \quad \text{and} \quad R_m \sin \epsilon_m$$

required by (3)

$$- F R \cos (\kappa - \epsilon), \quad F R \sin (\kappa - \epsilon), \qquad (27)$$

respectively, where

$$F = \frac{\sin\{Nn(\sigma_r/\sigma_0 - 1)\pi\}}{Nn(\sigma_r/\sigma_0 - 1)\pi}, \quad \kappa = n \left(N - \frac{1}{24} \right) \left(\frac{\sigma_r}{\sigma_0} - 1 \right) \pi. \qquad (28)$$

If these corrections are small compared with R_m, they give

$$\left. \begin{array}{l} \delta R_m = - F R \cos (\kappa + \epsilon_m - \epsilon), \\ \delta \epsilon_m = F R \sin (\kappa + \epsilon_m - \epsilon), \end{array} \right\} \qquad . \qquad . \qquad . \qquad . \qquad (29)$$

which we notice involve only the *relative* phase $\epsilon_m - \epsilon$.

Darwin's Method for Solar Constituents.

9. This method consists in applying the S series isolation process and the least square rule to different sets of 30 consecutive days' record and then analysing the resulting sets of values for yearly and half-yearly harmonic variations.

When a year's record is available, 12 sets of 30 days are used, and from the results values of

$$S_2, \quad K_2, \quad T_2, \quad K_1, \quad P_1, \quad Ssa, \quad Sa, \quad A_0, \quad S_4, \quad S_6, \quad S_1$$

are immediately taken. Residues from M_2 are allowed for. When less than a year's but as much as half a year's record is available, Ssa is neglected.

Analysis of Hourly Heights for Long Period Constituents.

10. There are two methods in general use, and we shall refer to them as the B.A. method and Darwin's short method. The B.A. method is used by the Survey of India.

The principle of the B.A. method is the least square rule applied to daily mean heights, using one decimal place for the multiplying sines and cosines. The residues from all primary astronomical constituents are allowed for.

Darwin's short method uses the principle of isolation and proceeds on a plan similar to the assignments, the daily means taking the place of the given hourly heights. Residues from M_2 are allowed for, but no great accuracy is claimed for the method; Darwin gave it as a much less laborious process than the B.A. method.

Darwin's Method for Harmonic Analysis of High and Low Water Observations.

11. If ζ denote the height of the water at time t, it will be given by an equation of the type

$$\zeta = R_0 \cos (\sigma_0 t - \epsilon_0) + \sum_r R_r \cos (\sigma_r t - \epsilon_r), \qquad . \qquad . \qquad . \qquad (30)$$

where σ_0 is no longer the speed of S_1, but that of any constituent conveniently chosen to play a special part in the analysis.

At the time of high or low water we have

$$0 = R_0 \sin (\sigma_0 t - \epsilon_0) + \sum_r \frac{\sigma_r}{\sigma_0} R_r \sin (\sigma_t t - \epsilon), \qquad . \qquad . \qquad . \qquad (31)$$

and if we let

$$\left. \begin{array}{llll} t_1, & t_2, & \ldots & t_s, & \ldots \\ \zeta_1, & \zeta_2, & \ldots & \zeta_s, & \ldots \end{array} \right\} \qquad . \qquad . \qquad . \qquad (32)$$

denote respectively the times and heights of consecutive high and low tides, we deduce from (30) and (31)

$$
\left.\begin{aligned}
&\frac{1}{N} \sum_{s=1}^{N} \zeta_s \cos \sigma_0 t_s - R_0 \cos \epsilon_0 \\
&= \frac{1}{N} \sum_{r} \sum_{s=1}^{N} R_r \left[\tfrac{1}{2}\left(1 + \frac{\sigma_r}{\sigma_0}\right) \cos \left\{ (\sigma_r - \sigma_0)t_s - \epsilon_r \right\} \right. \\
&\qquad\qquad\qquad\qquad \left. + \tfrac{1}{2}\left(1 - \frac{\sigma_r}{\sigma_0}\right) \cos \left\{ (\sigma_r + \sigma_0)t_s - \epsilon_r \right\} \right], \\
&\frac{1}{N} \sum_{s=1}^{N} \zeta_s \sin \sigma_0 t_s - R_0 \sin \epsilon_0 \\
&= -\frac{1}{N} \sum_{r} \sum_{s=1}^{N} R_r \left[\tfrac{1}{2}\left(1 + \frac{\sigma_r}{\sigma_0}\right) \sin \left\{ (\sigma_r - \sigma_0)t_s - \epsilon_r \right\} \right. \\
&\qquad\qquad\qquad\qquad \left. - \tfrac{1}{2}\left(1 - \frac{\sigma_r}{\sigma_0}\right) \sin \left\{ (\sigma_r + \sigma_0)t_s - \epsilon_r \right\} \right].
\end{aligned}\right\} \quad (33)
$$

If in the terms

$$
\frac{1}{N} \sum_{s=1}^{N} \cos \left\{ (\sigma_r - \sigma_0)t_s - \epsilon_r \right\}, \qquad \cdot \qquad \cdot \qquad (34)
$$

we may approximate by substituting

$$
t_s = t_1 + (s-1)\frac{\pi}{\sigma}, \qquad \cdot \qquad \cdot \qquad \cdot \qquad (35)
$$

we get

$$
\frac{\sin\left(N \dfrac{\sigma_r - \sigma_0}{\sigma} \dfrac{\pi}{2}\right)}{N \sin\left(\dfrac{\sigma_r - \sigma_0}{\sigma} \dfrac{\pi}{2}\right)} \cos \left\{ (\sigma_r - \sigma_0)\left(t_1 + \frac{N-1}{2}\pi\right) - \epsilon_r \right\} \qquad \cdot \qquad (36)
$$

This vanishes when N is an exact multiple of $2\sigma/(\sigma_r - \sigma_0)$, or when the tides taken cover an exact number of synodic periods of the constituents of speeds σ_0 and σ_r, while if N has the value of $\sigma/(\sigma_r - \sigma_0)$, or the tides taken cover exactly half a synodic period of the constituents of speeds σ_0 and σ_r, and $(\sigma_r - \sigma_0)/\sigma$ be small, (36) is equal to

$$
\frac{2}{\pi} \cos \left\{ (\sigma_r - \sigma_0)\left(t_1 + \frac{N-1}{2}\pi\right) - \epsilon_r \right\}, \qquad \cdot \qquad \cdot \qquad (37)
$$

approximately.

These are the equations and relations on which the method is based.

When analysing for M_2, $R_0 \cos (\sigma_0 t - \epsilon_0)$ is taken as this constituent, and N is chosen so that the tides considered cover an exact number of semi-lunations. It is then assumed that the summations on the right of (33) may be neglected, so that

$$
\left.\begin{aligned}
R_0 \cos \epsilon_0 &= \frac{1}{N} \sum_{s=1}^{N} \zeta_s \cos \sigma_0 t_s, \\
R_0 \sin \epsilon_0 &= \frac{1}{N} \sum_{s=1}^{N} \zeta_s \sin \sigma_0 t_s,
\end{aligned}\right\} \qquad \cdot \qquad \cdot \qquad (38)
$$

When analysing for N_2 and L_2, $R_0 \cos (\sigma_0 t - \epsilon_0)$ is again taken as M_2, but N is chosen so that the tides considered exactly cover a semi-lunar-anomalistic period. Two series of 13 such sets of tides are taken, the tides in each series being

consecutive, and one series beginning a quarter-lunar-anomalistic period after the other. By subtracting the equations (33) for consecutive sets in each series, M_2 is eliminated from each series, and the results are then combined so as to eliminate first N_2 and then L_2. In these final equations it is assumed that all other constituents may be neglected, and even for N_2 and L_2 that the terms involving $(1 - \sigma_r/\sigma_0)$ may be neglected, while those of the type (36) may be replaced by terms of the type (37).

To find S_2, $R_0 \cos(\sigma_0 t - \epsilon_0)$ is taken as S_1 and N is chosen so that the tides considered cover an exact number of semi-lunations. All constituents except K_2 and T_2 are then neglected in the summations. It is assumed that the amplitude of K_2 bears to that of S_2 the equilibrium ratio, and that their lags are equal, while T_2 is supposed simply proportional to S_2. In the summations involving K_2, the terms containing $(1 - \sigma_r/\sigma_0)$ are neglected.

To find K_2, $R_0 \cos(\sigma_0 t - \epsilon_0)$ is again taken as S_1 and two sets of the same number of tides are taken so that one begins 3 months after the other. Again T_2 is taken proportional to S_2, the factor of proportionality changing a little from one set to the other. By subtracting corresponding equations (33) for the two sets, the terms involving S_2 become small, and when the value of S_2 already found is used, we have two equations for the amplitude and lag of K_2. All other constituents are again neglected in the summations.

In finding K_1 and O_1 the procedure is analogous to that for finding N_2 and L_2, M_1 taking the place of M_2 and a semi-lunar period taking the place of a semi-lunar anomalistic period. This time, however, the contributions of M_2 and S_2 to the summations are accurately computed with the values of these constituents already found, while K_1 and P_1 are supposed to have their amplitudes in the equilibrium ratio and their lags equal.

The finding of P_1 is analogous to that of K_1, the process for K_1 and O_1 taking the place of that for S_2.

Only the constituents M_2, S_2, N_2, K_2, L_2 and K_1, O_1, P_1 are considered.

Correlation with Generating Potential.

12. The amplitudes of some of the harmonic constituents of the generating potential are really variable, though the variations are very slow. Also, some of their speeds have not quite the values scheduled, though again the deviations are small. In both cases the effects take a long time to accumulate and the changes may be neglected over a year, while a longer record is not subjected to a single analysing process.

When the amplitude and phase of a constituent observed at any particular time are connected with the generating potential, the deviations mentioned have to be taken into account. There is one type of case, however, in which this does not appear to have been adequately done. It is the case in which shallow water constituents have speeds equal to those of constituents of other origin. When the ratio of the amplitudes and the difference of the phases of two constituents are slowly changing, it is clear that true tidal constants cannot be obtained until the constituents have been separated in some way. Such separation has never been made, and this fact may possibly account for some of the irregularities in the results from year to year, especially for some of the long period constituents.*

Historical Account.

13. In 1866 Prof. Thomson (afterwards Sir William Thomson and Lord Kelvin) applied to the Admiralty for a year's record of any trustworthy tide-gauge, and received that of Ramsgate for 1864. This he began to analyse harmonically with the aid of Messrs. E. Maclean, J. Smith and W. Ross of the University of Glasgow, at first using three-hourly heights but afterwards hourly heights. Approximate values for M_2 and S_2 were found and the existence of Ssa and Sa detected.

* In the preliminary report it was stated that the inconsistencies in the results for long period constituents are not due to defects in the method of analysis. This was based on the fact that residues are allowed for; the subject of the present section was not there considered.

At the Meeting of the British Association in 1867 Thomson obtained the appointment of a committee which, during the following nine years, carried out harmonic analyses. The grants from the Association to the Committee amounted to £1,000, and the work was done, under the superintendence of Thomson, by Mr. E. Roberts, of the Nautical Almanac Office, and assistant computers working under his immediate direction.

To the members of this committee, in December 1867, Thomson issued a circular, containing, among other things, the speeds of the constituents

M_2	K_1	Mf	M_1	S_1
S_2	O_1	Mm	M_3	
N_2	P_1	Ssa		
K_2	Q_1	Sa		
L_2	J_1			
T_2	'$\gamma - 3\eta$'			
R_2				

We have explained the nature of all these except R_2 and '$\gamma - 3\eta$,' which are solar constituents analogous respectively to the lunar constituents L_2 and Q_1. In the development of the generating potential their amplitudes are less than those of constituents which have always been neglected. Thomson gave S_1 as an astronomical constituent analogous to M_1, and the speed of Mm as σ instead of $\sigma - \varpi$, as was afterwards pointed out by Roberts. He thought that while all the above constituents would be sensible on our shores, the effects of evection and variation would be negligible. He also stated the 'equilibrium principle' of allowing for the changing inclination of the moon's orbit to the earth's equator.

From the Ramsgate 1864 record the terms of the

$$M, K, S, O, N, L$$

series were first found. Special hourly means for the year were formed using the B.A. assignment, and then analysed by the least square rule using the tabular forms given by Archibald Smith for the deviation of the ship's compass.

This analysis revealed the shallow water constituents

$$M_4, M_6, M_8, S_4,$$

and this then suggested the possible presence of MS_4 and MSf.

By means of these first approximations a complete calculation of residues was made, but it was afterwards concluded that first-approximations were sufficient for short-period constituents, and no other residues for such constituents were calculated by the Committee.

From the same record numbers for

$$Mf, Mm, Ssa, Sa, MSf,$$

were found, by using daily means for the year purified of lunar short-period influence before analysis.

As a test, an hourly tide-table for one day in 1864 was made and compared with the original record. The errors reached a foot at half-tide (mean spring amplitude $= 8$ ft.).

A series of records taken at Liverpool were supplied by the Board of the Mersey Dock Estates, and from that for 1857–8, values for the

$$M, K, S, O, N, L$$

series were found.

A set of personal observations taken at Bombay were supplied by Mr. W. Parkes, a member of the Committee, and special quarter-hourly means for 127 days were formed and analysed for the

$$M, K, S, O, N, L$$

series.

14. From the Liverpool 1857–8 record the constituent P_1 was found, and then the 1858–9 and 1859–60 records were analysed similarly. The constituents T_2 and R_2 were also treated, each from two two-yearly records.

Now that records taken in different years at the same station were considered, correlation with the generating potential was made, and a Liverpool hourly tide

table for 13 specially chosen days in 1869 was constructed and compared with the corresponding record.

To examine the variation of certain constituents with the inclination of the moon's orbit, the record for 1866–7 was analysed. It was decided that the variation was not quite according to the equilibrium principle, but was afterwards always treated as if it were so.

The new results of analysis were incorporated in the tide-table and then the determination of ν_2, μ_2 and λ_2 was suggested and carried out for each of the four years taken.

The tide-table was again emended and compared with the record : the discrepancies over two typical days are shown in Fig. I. The determination of MS_4 was suggested and the analysing process applied to each of the four yearly records. The tide-table was emended accordingly and the results again compared with the record. The final discrepancies over the two typical days are shown in
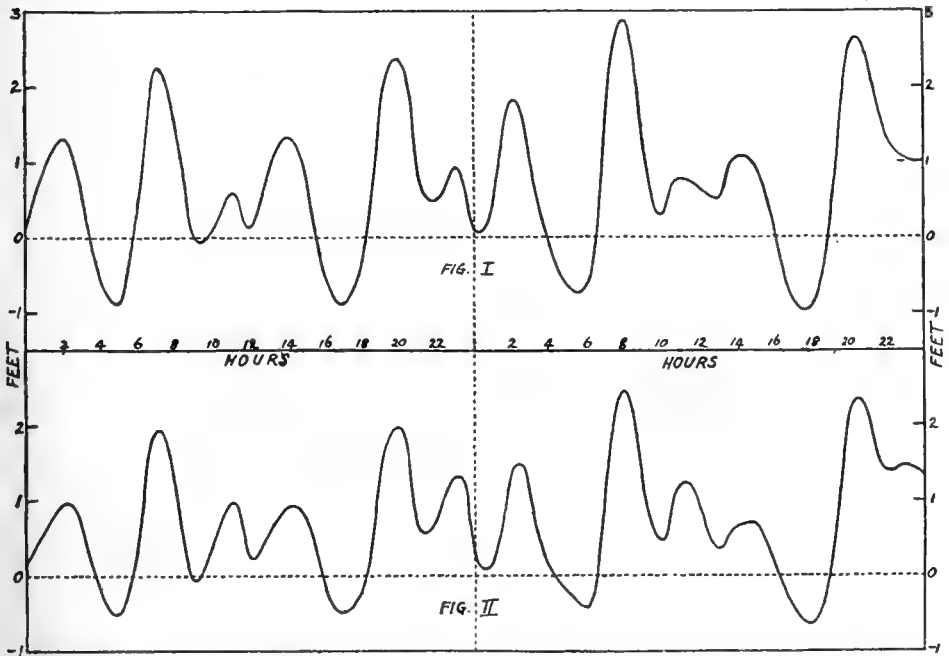
FIGS. I. AND II. —Difference between Predictions and Record for Liverpool, April 26 and 27, 1869.

Fig. II. It should be stated that the tide-table contained a number of constituents of amplitudes less than 0·1 ft.

Numbers for

$$Mf, \quad Mm, \quad Ssa, \quad Sa, \quad Msf$$

were also obtained from each of the four yearly records, but were inconsistent from year to year.

The Ramsgate 1864 record was next further analysed for

$$P_1, \quad \nu_2, \quad \mu_2, \quad \lambda_2, \quad MS_4.$$

A set of quarter-hourly observations taken during several periods within four days in the Fiji Islands, and supplied by Lieut. Hope, R.N., were partially analysed. The difficulties presented by the shortness of the periods led Thomson to apply to the U.S. Coast and Geodetic Survey for a trustworthy record of Pacific tides. He received that of Fort Point, California, for 1858–9, and it was analysed for all the constituents considered at Ramsgate. As a test an hourly tide-table for 14 days was made without μ_2, λ_2, the long-period constituents and MS_4, but including several

constituents of amplitudes less than 0·1 ft. The discrepancies over two typical days are shown in Fig. III.

A Karachi record for 1868–71 was supplied by Mr. Parkes and analysed for all the constituents considered at Liverpool, together with

$$Q_1, \qquad J_1.$$

An hourly tide-table for 29 days in 1868 was made without T_2, Q_1, J_1, the long-period constituents and MS_4, but including several constituents of amplitudes less than 0·1 ft. The discrepancies over two typical days are shown in Fig. IV.
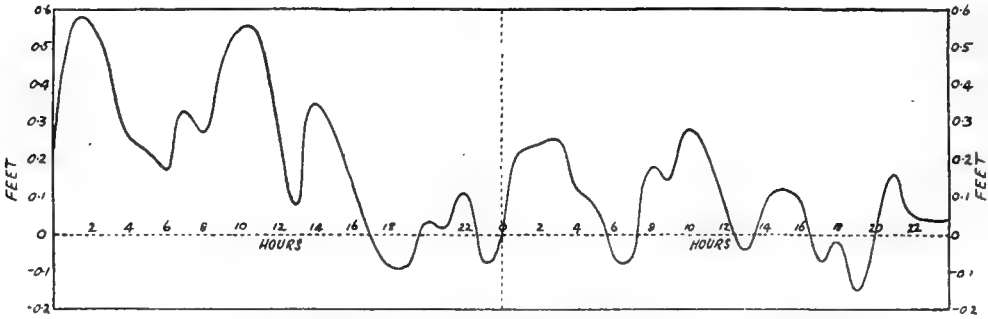
FIG. III.—Difference between Predictions and Record for Fort Point,
March 16 and 17, 1859.
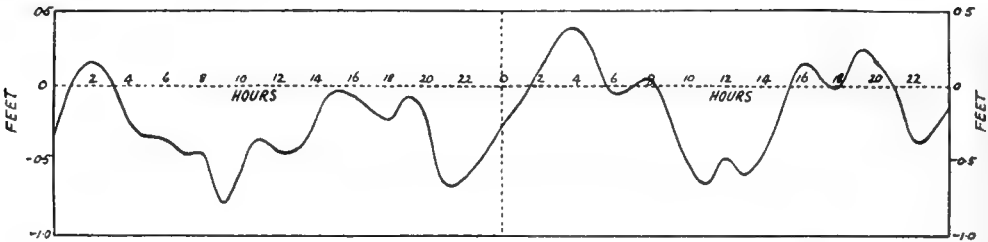
FIG. IV.—Difference between Predictions and Record for Karachi,
November 11 and 12, 1868.

Next, three further yearly records of Liverpool tides were analysed for the same constituents as before, with the exception of those of long period.

Also, two further yearly records from Fort Point were analysed, the constituents Q_1 and J_1 being added.

A thirteen months' record from Cat Island, in the Gulf of Mexico, was obtained from the U.S. Survey, and analysed for the

$$M, \quad K, \quad S, \quad O, \quad N, \quad L, \quad P, \quad Q, \quad J$$

series, as well as the long-period constituents.

From the Ramsgate 1864 record the constituents $2SM_2$ and $3MS_4$ were found, the latter being a shallow-water constituent of speed $4\gamma - 6\sigma + 2\eta$, derived from S_2 and M_6.

Another yearly record from Karachi was analysed for all the constituents yet considered.

Four yearly records from Portland were analysed for the same constituents as at Ramsgate, with the exception of those of long period.

In the British Association Report for 1872, Roberts gave a harmonic development of the semi-diurnal tide generating potential.

15. In 1874 Lieut. (afterwards Col.) A. W. Baird, of the Survey of India, set up tidal observatories at Hanstal, Navanar, and Okha Point. He was afterwards deputed to Europe to study the practical details of tidal registration and harmonic analysis. With the help of Roberts in England, he analysed the records from the observatories he had set up.

The British Association Committee further analysed records for Hilbre Island,

Karachi, San Diego and Fort Clinch. When its funds were exhausted Thomson tried to obtain £150 from the Government but was unsuccessful. The amount was provided by the Royal Society out of their Government Grant Fund, and with it records from West Hartlepool, Port Leopold, Beechy Island, Brest and Toulon were analysed.

For the 1876 British Association Meeting Thomson drew up a final report and gave an investigation into the generating potential, tabulating speeds, arguments and amplitudes.

A record taken at Freemantle by the Admiralty provided the first case of harmonic analysis for a station in the Southern Hemisphere. Under the influence of Thomson the Hydrographic Office was searched for other Southern Hemisphere records, but the only ones found were from Port Louis, Mauritius and Port Louis, Berkely Sound. Harmonic constants from these as well as from records taken at Toulon, Marseilles, and Malta, were published by Thomson and Capt. Evans, R.N.

On Baird's return to India in 1877 the systematic observation and analysis of tides was there begun, he training the original staff of observers and computers.

Thomson next constructed the first mechanical harmonic analyser with the aid of grants from the British Association and the Royal Society. It was designed to determine the constituents

$$\mathrm{M_2, \quad S_2, \quad K_1, \quad O_1, \quad M_4}$$

but has never been used for this purpose. It is deposited in the Museum at South Kensington.

At the 1882 British Association Meeting Prof. Darwin (afterwards Sir George Darwin) communicated a paper in which he pointed out that the methods of analysis which had been used for the long-period constituents might be seriously in error. A committee consisting of Profs. Darwin and Adams was appointed to examine the whole subject of harmonic analysis.

In 1883 and 1885 Darwin presented reports which have ever since formed the standard manual on the subject. They contain an elaborate analysis of the generating potential (several errors in the report of 1876 being indicated) and a complete treatment of the methods of analysing hourly heights. These methods, except for $\mathrm{T_2}$, $\mathrm{R_2}$ and the long-period constituents, do not differ essentially from those which had been used by Thomson's Committee; they complete the evolution of what we have called the B.A. methods.

In 1885 Baird and Darwin published an up-to-date collection of results of analysis; the number of stations considered was 43.

16. In 1885–6 the Canadian Expedition to Hudson Bay under Lieut. Gordon, R.N., was made and short series of observations taken at five stations in Hudson Straits were afterwards harmonically analysed by Gordon with the aid of Prof. Carpmael of Toronto.

In a final report (1886) of the British Association Committee Darwin gave his methods of analysing short records.

In 1886 Baird left the Tidal Department of the Survey of India and published his ' Manual of Tidal Observations.' The methods of observation and analysis which he had established have been continued without modification up to the present time; they consist precisely of the B.A. methods, Baird constructing auxiliary tables.

In 1889 Darwin published a second up-to-date collection of results from all sources, showing an increase of 27 stations since 1885.

In 1890 Darwin gave his method of harmonically analysing observations of high and low water, and in 1892 his new method for solar constituents and his short method for long-period constituents. In the same year he published his design of the 'tidal abacus,' an apparatus for facilitating the computations in the analysis of hourly heights. This apparatus has since been much used.

In 1891 the Australasian Association appointed a committee to report on the tides of South Australia. Two of the members of this committee, Prof. R. W. Chapman and Capt. A. Inglis, afterwards analysed records from Port Adelaide and Port Darwin.

In 1894 the Survey of Tides and Currents in Canadian Waters was instituted by the Canadian Government, and this organisation has worked continuously up to the present time under the direction of Dr. W. Bell Dawson. The harmonic analyses have been made for the Survey by Messrs. Roberts, a firm of computers founded by Mr. E. Roberts of Thomson's British Association Committee, but only a few of the results have been published.

Mr. T. Wright, of the Nautical Almanac Office, has analysed records by Darwin's methods with aid from the Royal Society Government Grant Fund.

Darwin and Messrs. Selby and Hunter have analysed with the abacus records for Antarctic expeditions, and Selby has similarly analysed records for the National Physical Laboratory.

Roberts has made new analyses for Liverpool and Dover, the former for a British Association Committee, the latter with the aid of a small grant from the Royal Society Fund.

In New Zealand tidal work began in 1909 and has been entirely under the charge of Mr. C. E. Adams, the Government Astronomer. Records have been analysed by the use of Darwin's abacus and computation forms, and the results checked by Mader's mechanical analyser. This appears to be the only use that has been made of mechanical analysers, in spite of the number of different machines that have been invented.

In Western Australia records have been taken and analysed by the Government, under the direction of Messrs. Cooke and Curlewis, Government Astronomers.

Prof. D'Arcy Thompson has studied averages of consecutive high and low waters at Aberdeen, Dundee and Milford Haven, giving values for Ssa and Sa at these Stations. He has also published lists of Ssa and Sa constants obtained from various sources.

The Admiralty has announced that for the North Sea the customary methods of harmonic analysis lead to predictions which are entirely in error.

In 1911 the British Government, in connection with the 'Conseil Permanent International Pour l'Exploration de la Mer,' began the taking and harmonic analysis of continuous observations of tidal currents in the North Sea.

British Work on Harmonic Analysis of Tidal Heights.

BIBLIOGRAPHY.

1868.* SIR W. THOMSON, Report of 'Committee for the purpose of promoting the extension, improvement, and harmonic analysis of tidal observations.'
Brit. Assoc. Report, pp. 489–505. [Thomson's Circular is reprinted in *Pop. Lect. and Add.*, pp. 209–223].
E. ROBERTS, Report supplementary to Thomson's, *Brit. Assoc. Report*, pp. 505–510.

1870. SIR W. THOMSON, Report of same Committee for 1869, *Brit. Assoc. Report*, pp. 120–123.
SIR W. THOMSON, Report of same Committee for 1870, *ibid.* pp. 123–125 ; 148–151.
E. ROBERTS, 'Statement of work performed by him,' *ibid.* pp. 125–148.

1871. E. ROBERTS, Report of same Committee, *Brit. Assoc. Report*, pp. 201–207.

1872. E. ROBERTS, Report of same Committee, *Brit. Assoc. Report*, pp. 355–395.

1876. SIR W. THOMSON, Report of same Committee, *Brit. Assoc. Report*, pp. 275–307.

1878. CAPT. EVANS and SIR W. THOMSON, 'On the Tides of the Southern Hemisphere and of the Mediterranean,' *Brit. Assoc. Report*, pp. 477–481. [*Nature*, v. 18, pp. 670–672.]
SIR W. THOMSON, 'Harmonic Analyser,' *Proc. Roy. Soc.*, v. 27, pp. 371–373.

1881. SIR W. THOMSON, 'The Tide Gauge, Tidal Harmonic Analyser and Tide Predicter,' *Proc. Inst. Civil Engrs.*, v. 65, pp. 2–25.

1882. G. H. DARWIN, 'On the Method of Harmonic Analysis used in deducing the numerical values of the Tides of Long Period . . . ,' *Brit. Assoc. Report*, pp. 319–327.

1883. G. H. DARWIN, 'Report of a Committee for the Harmonic Analysis of Tidal Observations,' *Brit. Assoc. Report*, pp. 49–118. [*Sci. Papers*, v. 1, pp. 1–68.]

1884. G. H. DARWIN, Second Report of same Committee, *Brit. Assoc. Report*, pp. 33–35.

1885. G. H. DARWIN, Third Report of same Committee, *Brit. Assoc. Report*, pp. 35–60. [*Sci. Papers*, v. 1, pp. 70–96.]
A. W. BAIRD and G. H. DARWIN, 'Results of the Harmonic Analysis of Tidal Observations,' *Proc. Roy. Soc.*, v. 39, pp. 135–207.

1886. G. H. DARWIN, 'Fourth Report of Committee for Harmonic Analysis . . ,' *Brit. Assoc. Report*, pp. 41–56. [*Sci. Papers*, v. 1, pp. 98–116.]

* For *Brit. Assoc. Reports*, the dates are those of the meetings to which the volumes refer ; in other cases the dates are those of publication.

1886. G. H. DARWIN, 'Instructions for the Reduction of Hourly Tidal Observations
. . .' *Admiralty Scientific Manual*, 'Tides.' [*Sci. Papers*, v. 1, pp. 122–139.]

1889. G. H. DARWIN, 'Second Series of Results of the Harmonic Analysis of Tidal
Observations,' *Proc. Roy. Soc.*, v. 45, pp. 556–611.

1890. G. H. DARWIN, 'On the Harmonic Analysis of Tidal Observations of High and
Low Water,' *Proc. Roy. Soc.*, v. 48, pp. 278–340. [*Sci. Papers*, v. 1, pp. 157–215.]

1892. G. H. DARWIN, 'On an Apparatus for Facilitating the Reduction of Tidal
Observations,' *Proc. Roy. Soc.*, v. 52, pp. 345–389. [*Sci. Papers*, v. 1,
pp. 216–257.]

1902. T. WRIGHT, 'Harmonic Tidal Constants for certain Australian and Chinese
Ports,' *Proc. Roy. Soc.* (A), v. 71, pp. 91–96.

1904. J. N. SHOOLBRED, 'The Tidal Régime of the River Mersey,' *Brit. Assoc. Report*.

1906. J. N. SHOOLBRED, 'The Tidal Régime of the River Mersey . . ,' *Proc. Roy.
Soc.* (A), v. 78, pp. 161–166.

1907. SIR G. H. DARWIN, 'On the Antarctic Tidal Observations of the "Discovery,"'
Sci. Papers, v. 1, pp. 372–388. [*Nat. Antarctic Exp.* 1901–1904, *Phys. Obs.*,
pp. 3–12.]

1908. F. J. SELBY and J. DE G. HUNTER, 'Tidal Observations of the "Scotia," 1902–
1904,' *Nat. Antarctic Exp.*, 1901–1904, *Phys. Obs.*, pp. 13–14.

1909. T. WRIGHT, 'Harmonic Tidal Constants for certain Chinese and New
Zealand Ports,' *Proc. Roy. Soc.* (A), v. 83, pp. 127–130.

1910. SIR G. H. DARWIN, 'The Tidal Observations of the British Antarctic Expedition
1907,' *Proc. Roy. Soc.* (A), v. 84, pp. 403–422.

1911. F. J. SELBY, 'Analysis of Tidal Records for Brisbane for the year 1908,' *Proc.
Roy. Soc.* (A), v. 86, pp. 64–66.

1913. E. ROBERTS, 'Re-reduction of Dover Tidal Observations for 1883-4, &c.,' *Proc.
Roy. Soc.* (A), v. 88, pp. 230–233.

1914. D'ARCY W. THOMPSON, 'On Mean Sea Level and its Fluctuations,' *Fishery Bd.
Scotland, Sci. Invest.* iv.

British Work on Harmonic Analysis of Tidal Currents.

The work has all been done in connection with the 'Conseil Permanent Inter-
national pour l'Exploration de la Mer,' and the results published in the *Bulletin
Hydrographique* for 1911, 1912, 1913.

Harmonic constants are given for the northerly and easterly components of the
current and also for the current-ellipses, at various depths at each station.

The English observations have been made on steamships and lightships under
instructions from the Board of Agriculture and Fisheries. The Scottish observations
were taken on board the 'Goldseeker' by the 'North Sea Investigation Committee,'
under instructions from Prof. D'Arcy Thompson.

Indian Work on Harmonic Analysis of Tidal Heights.

The work has all been done by the 'Survey of India,' a Government institution.
The results are published in the annual volumes of the 'Records of the Survey of
India'; prior to 1908 these volumes were called 'Extracts from Narrative Reports
of the Survey of India.'

In 1901, vol. 16 of the 'Great Trigonometrical Survey of India' was published,
being written by Mr. J. Eccles. It deals exclusively with tidal work and gives full
accounts of methods used, work done and results obtained up to 1892.

Canadian Work on Harmonic Analysis of Tidal Heights.

A few results are published in the annual 'Reports of Progress' of the 'Survey
of Tides and Currents in Canadian Waters.'

Australian Work on Harmonic Analysis of Tidal Heights.

BIBLIOGRAPHY.

1892. R. W. CHAPMAN and A. INGLIS, 'The Tides of the Coast of South Australia,'
Aust. Assoc. Report, iv., pp. 230–232.

1894. R. W. CHAPMAN and A. INGLIS, 'The Tides of Port Adelaide,' *Aust. Assoc.
Report*, v.

1898. R. W. CHAPMAN and A. INGLIS, 'The Tides of South Australia,' *Aust. Assoc. Report*, vii., pp. 241–244.

1902. R. W. CHAPMAN and A. INGLIS, 'The Tides of Port Darwin,' *Aust. Assoc. Report*, ix., pp. 67–68.

1903. R. W. CHAPMAN, 'The Tides of Port Darwin,' *Nature*, v. 68, p. 295.

1914. H. B. CURLEWIS, 'The Tides, with special reference to those of Freemantle and Port Hedland,' *Journ. R.S. West Aust.*, v. 1.

1916. H. B. CURLEWIS, 'Tide Tables for Port Hedland, 1917' (Perth, W.A.).

Work done in New Zealand on Harmonic Analysis of Tidal Heights.

This work is done by the Government Department of Lands and Survey. The results are published in the annual 'Reports of Survey Operations' and in papers by C. E. Adams.

Papers by C. E. Adams.

1909. 'The Wellington Tide Gauge,' *Trans. N.Z. Inst.*, v. 41, pp. 406–410.

1912. 'Harmonic Tidal Constants of New Zealand Ports—Wellington and Auckland,' *Aust. Assoc. Report*, v. 14.

1913. 'Harmonic Tidal Constants of New Zealand Ports—Wellington and Auckland,' *Trans. N.Z. Inst.*, v. 45, pp. 20–21.

1914. 'Harmonic Tidal Constants of New Zealand Ports—Dunedin and Port Chalmers,' *Trans. N.Z. Inst.*, v. 46, pp. 316–318.

British Work on Harmonic Analysis of Tidal Heights.

Analyses Made.

Date of Publication	Station	Dates of Record Analysed	Authority	Authority for Record
1868	Ramsgate ...	1864	1	Admiralty.
	Liverpool ...	1857–8	2	Mersey Dock Bd
	Bombay	1867	2	Parkes.
1870	Liverpool ...	1857–8, 1858–9, 1859–60, 1866–7	2	Mersey Dk. Bd.
	Ramsgate ...	1864	2	Admiralty.
	Fort Point ...	1858–9	2	U.S. Survey.
	Karachi	1868–9, 1869–70	2	Parkes.
1871	Liverpool ...	1866–7, 1867–8, 1868–9, 1869–70	2	Mersey Dk. Bd.
	Fort Point ...	1859–60, 1860–61	2	U.S. Survey.
	Cat Island ...	1848	2	,,
1872	Ramsgate ...	1864	2	Admiralty.
	Karachi	1870–1	2	Parkes.
	Portland ...	1851, 1857, 1866, 1870	2	Sir J. Coode.
1876	Hilbre Island ...	1858, 1859, 1860, 1861, 1862, 1863, 1864, 1865, 1866, 1867	2	Mersey Dk. Bd.
	Karachi	1871–2, 1872–3	2	Parkes.
	San Diego ...	1860, 1861	2	U.S. Survey.
	Fort Clinch ...	1860–1	2	,,
	W. Hartlepool ...	1858–9, 1859–60, 1860–1	2	—
	Port Leopold ...	1848–9	2	Clark Ross.
	Beechy Is. ...	1858–9	2	Capt. Pullen.
	Brest	1875	2	French Marine.
	Toulon	1853	2	,,
1878	Freemantle ...	1873–4	2	Admiralty.
	Port Louis (Mauritius)	1838–9	–	,,
	Port Louis (Berkely Sd.)	1842	–	,,
	Toulon	1847, 1848	–	French Marine.
	Marseilles ...	1850–1	–	,,
	Malta	1871–2	–	Ad. Cooper Key

British Work on Harmonic Analysis of Tidal Heights—(cont.).

Date of Publication	Station	Dates of Record Analysed	Authority	Authority for Record
1889	Dover	1883, 1884, 1885	3	Bd. of Trade.
	Ostend	1883, 1884, 1885	3	Belgian Ministry of Public Works.
	Singapore ...	1882	2	—
	Hong Kong ...	1883	2	—
1902	Princess Royal Har.	1876–7	4	Admiralty.
	Newcastle (N.S.W.)	1900	4	,,
	Ballina	1898	4	,,
	Hong Kong ...	1889	4	Chinese Customs
	Swatow	1897–8	4	,,
	Wampoa ...	1894–5	4	,,
	Brisbane ...	1865–6	4	Admiralty.
	Sydney	1888	4	,,
	Cooktown ...	1890	4	,,
	Cairns Harbour	1892–3	4	,,
1904	Liverpool ...	1902	2	Mersey Dk. Bd.
1906	Liverpool ...	1902	2	,,
1907	Ross Island ...	1902–3	5	' Discovery ' Exp.
1908	S. Orkneys ...	1903	6	' Scotia ' Exp.
1909	Port Chalmers, N.Z.	1901	4	Admiralty.
	Port Lyttleton, N.Z.	1901–2	4	,,
	Wellington, N.Z.	1901	4	,,
	Auckland, N.Z. ...	1900–1	4	,,
	Wei-Hai-Wei ...	1898–9	4	,,
	Woosung... ...	1902	4	Shanghai Customs.
1910	Ross Island ...	1908	5	' Nimrod ' Exp.
	Ross Island ...	1902–3	5	' Discovery ' Exp.
1911	Brisbane ...	1908	6	Local.
1913	Dover	1910–11	2	Admiralty.
Unpublished	Cuxhaven ...	1841–2	2	Admiralty.
	Gibraltar... ...	—	2	,,
	Oban	1910–11	2	,,
	Shatt-el-Arab ...	—	2	,,
	Stromness ...	1910–11	2	,,
	Georgetown (Brit. Guiana)	1915–16	2	Colonial Office.
	London Bridge ...	1911, 1912	2	Port of London.
	Tilbury Dock ...	—	2	,,
	Royal Albert Dk.	—	2	,,
	Southend ...	—	2	,,
	Immingham ...	1911–12	2	G.E. Railway.
	Avonmouth ...	1910–11, 1911–12	2	Bristol Harbour
	Penang	1906–7	2	Local.
	Hong Kong ...	1887, 1888, 1889	2	,,
	Port Swettenham	—	2	,,

Authorities for Analysis.

1. Thomson and Roberts.   3. E. Connor.   5. Darwin.
2. Roberts.   4. Wright.   6. Selby and Hunter.

The number of stations considered is 57 ; the aggregate record analysed amounts to about 90 years.

British Work on Harmonic Analysis of Tidal Currents.

Date	Station	Analyses made
1911	Sc	M_2 at 3 depths.
	E_1	M_2, S_2 ,, 3 ,,
	Smith's Knoll	M_2 ,, 9 ,,
	Varne	M_2 ,, 6 ,,
1912	E	M_2 ,, 2 ,,
	Smith's Knoll	M_2 at same depths as in 1911.
	Varne	M_2 ,, ,, ,, ,,
1913	E52	M_2 at 2 depths.
	Outer Dowsing	M_2 ,, 5 ,,
	Swarte Bank	M_2 ,, 6 ,,
	Smith's Knoll	M_2 at 7 of depths taken in 1911, 1912.
	Varne	M_2 at same depths as in 1911, 1912.

Sc, off Caithness.   E, further out than E_1.
E_1, off Northumberland.   E52, further out than E.

Smith's Knoll, Lightship off Norfolk.
Varne,   „   in Straits of Dover.
Outer Dowsing,   „   off Lincoln.
Swarte Bank,   „   about midway between Grimsby and Texel.

The number of stations considered is 8 ; the aggregate duration of the observations analysed amounts to about 34 weeks.

Indian Work on Harmonic Analysis of Tidal Heights.
Gauge Records Analysed.

Station	Dates of Record	Station	Dates of Record
Hanstal	1874 to 1875	Galle	1884 to 1890
Navanar	,, ,, ,,	Colombo	,, ,, ,,
Okha Point ...	,, ,, 1875, 1905 to 1906	Cochin	1886 ,, 1892
Bombay (A.B.) ...	1878 ,, present	Cocanada ...	,, ,, 1891
Karwar	,, ,, 1883	Chittagong ...	,, ,, ,,
Beypore	,, ,, 1884	Akyab	1887 ,, 1892
Pamban Pass ...	,, ,, 1882	Bombay (P.D.) ...	1888 ,,
Aden	1879 ,, present		present
Vizagapatam ...	,, ,, 1885	Tuticorin ...	,, ,, 1893
Madras	1880 to 1890, 1895 to present	Bhaunagar ...	1889 ,, 1894
Rangoon ...	,, ,, present	Mergui	,, ,, ,,
Amherst	,, ,, 1886	Trincomalee ...	1890 ,, 1896
Moulmein ...	,, ,, ,, 1909 to present	Minicoy	1891 ,, ,,
Port Blair ...	,, ,, present	Bushire	1892 ,, 1901
Karachi	1881 ,, ,,	Muscat	1893 ,, 1898
Negapatam ...	,, ,, 1882, 1886 to 1888	Diamond Is. ...	1895 ,, 1899
False Point ...	,, ,, 1885	Suez	1897 ,, 1903
Duolat	,, ,, 1886	Perim	1898 ,, 1902
Diamond Har. ...	,, ,, ,,	Porbandar ...	,, ,, ,,
Kidderpore ...	,, ,, present	Port Albert Victor	1900 ,, 1903
Elephant Pt. ...	1884 ,, 1888	Bassein	1902 ,, 1903
Goa	1884 ,, 1889		

Personal Observations Analysed.
Port Albert Victor, 1881 to 1882 ; Porbandar, 1893 to 1894.

Besides the above a record for Basrah was analysed in 1916–17 and the results published with those for the Indian stations.

The number of stations considered is 43. The aggregate amount of record analysed up to the present amounts to about 480 years.

Canadian Work on Harmonic Analysis of Tidal Heights.

Gordon's Analyses.

The results were published in 1887.

Station	Date and Length of Record	Station	Date and Length of Record
Port Burwell ...	1885, 2 weeks	Stupart's Bay ...	1886, 2 weeks
Ash Inlet ...	1886, 4 ,,	Port Laperrière ...	,, 2 ,,
Nottingham Is. ...	,, 4 ,,		

Analyses made for Survey.

The results are mostly unpublished.

Station	Date of beginning and Length of Record	Station	Date of beginning and Length of Record
Halifax	1851, 13 years	Forteau Bay ...	1898, 5 years
Quebec	1894, 18 ,,	Vancouver ...	1902, 11 ,,
St. John, N.B. ...	,, 20 ,,	Port Simson ...	,, 10 ,,
St. Paul Is. ...	1895, 17 ,,	Clayoquet ...	1905, 9 ,,
Sand Heads ...	,, 6 ,,	Prince Rupert ...	1906, 8 ,,
Victoria, B.C. ...	,, 11 ,,	Wadhams ...	,, 5 ,,
Father Point ...	1897, 15 ,,	Charlottetown ...	1907, 8 ,,
		Point Atkinson ...	1912, 5 ,,

The number of different stations considered is 19, Point Atkinson being practically identical with Sand Heads, which it has replaced; the aggregate length of record analysed amounts to about 160 years.

Australian Work on Harmonic Analysis of Tidal Heights.

Analyses Made.

Date of Publication	Station	Date of Record	Authority
1892	Port Adelaide ...	1889–90	Chapman & Inglis.
1894	,, ...	,,	,, ,,
1898	,, ...	1889–90, 1893	,, ,,
1902	Port Darwin ...	1896	,, ,,
1914	Freemantle ...	1908–9, 1909–10	Cooke.
,,	,, ...	1911, 1912	Curlewis.
,,	Port Hedland ...	1913	,,

The aggregate length of record analysed amounts to about eight years.

Work done in New Zealand on Harmonic Analysis of Tidal Heights.

Analyses made by the Survey.

The prefixed dates are those of publication, the others those of the records.

1911 Wellington.	1912 Auckland.	1914 Dunedin.
1909	1908–9	1911–12

The Urgent Need for the Creation within the Empire of a Central Institution for Training and Research in the Sciences of Surveying, Hydrography, and Geodesy.

By Dr. E. H. GRIFFITHS *and* Major E. O. HENRICI.

(Paper opening joint discussion in Sections A and E, August 27. Ordered by the General Committee to be printed *in extenso*.)

Good maps are necessary for the development of a country, for such purposes as defining property boundaries, limits of mining and other concessions, and so on, as well as for such engineering purposes as railway, road, and canal schemes, hydro-electric schemes, water supply, irrigation, &c. The importance of good charts, as well as reliable information as to tides and currents, hardly needs emphasising. An incorrect or out-of-date chart will cause losses due to delays to shipping, even if it does not lead to more direct loss. Anything that will assist in the production of up-to-date and accurate charts is of great and direct benefit to the shipping industry, and through it to the nation. Even when such work has once been completed there is no finality, as both maps and charts require periodical revision at more or less frequent intervals, according to circumstances.

The economical and speedy production of such maps and charts necessitates a thorough knowledge of the principles on which all survey work is based, and of the best means of applying such principles under varying conditions. Apart from revision work, there is still a very great deal of survey work waiting to be carried out. Enormous areas still exist in the Empire which are surveyed very inadequately or not at all.

Very large sums have been misapplied in the past owing to a lack of appreciation of the principles which should underlie all survey work. The following quotation from the official account of the 'Cadastral Survey of Egypt,' by Captain H. G. Lyons (Cairo, 1908), is an example of this :

'Surveying has been carried on in Egypt to a considerable extent during the last ninety years, and the work of Muallim Ghali and M. Masi, 1813-1822, of Mahmud Pashael Falaki, 1861-1874, of the cadastre of 1878-1888, of the Hydrographic Survey of 1889-1898, amounting to a total of some forty years' work on the geographical measurement of the country, had been accomplished before the cadastral survey, which has just been completed, commenced. That more permanent results were not obtained from them is mainly due to want of scientifically organised control and supervision, so that inferior work was not detected, and the standard of accuracy was allowed to fall below that which was necessary in so densely populated a country. The circumstances of the time have usually been responsible for this, and want of funds, urgent demands for maps to be prepared within a minimum length of time, and other similar causes led to much repetition of work without producing reliable maps of the country.

'During the time that the present Survey Department has been engaged in measuring the cultivable lands in Egypt, much inconvenience has been experienced from the want of any complete account of these earlier surveys. . . . When the formation of the Survey Department was undertaken in 1898, no complete account existed of the work of this kind which had been previously undertaken. References to it existed in various reports, but the detailed information concerning the methods employed, their cost, the recruiting and training of staff, and relative values of different ways of executing the work was not available. There was no time then to undertake its compilation, but had such a work existed, subsequent work would have been greatly expedited and facilitated, and a considerable economy would have resulted.'

The Egyptian Survey of 1878-1888, mentioned above, cost some £360,000, and produced incomplete maps of some 2,000 square miles. Almost the whole

of the work had to be repeated in 1892-1907, when, owing to the adoption of proper methods, and in spite of many difficulties, some 13,000 square miles were satisfactorily mapped at a cost of under £450,000.

The methods to be adopted depend upon circumstances, the nature of the country, and the objects of the survey. The difficulties to be overcome vary in different parts of the world. The experiences of the various surveyors have been published in their records and reports, but these are not in an easily accessible form, nor is there any general index or summary to be found. The originals are circulated to a limited number of persons and institutions, and are buried in libraries, even if their existence is not forgotten. When a new difficulty arises in any survey it has to be tackled *de novo*, though it is quite likely that similar circumstances have arisen before. In such a case it is probable that the surveyor in question does not know of it, and even if the reports are accessible to him (which they frequently are not) the actual information he wants is most effectually buried. This leads to much waste of effort, as there is no central body to which he can refer.

As regards existing Departments and Institutions, the Dominion, Indian, and Colonial Surveys are all independent, and, broadly speaking, train their own staff. There are, however, good survey schools in some of the Dominions. The Ordnance Survey produce their well-known maps, which are revised periodically, and they are so complete that no extensive survey work is required by outsiders in this country. This accounts for the lack of attention paid to the subject outside Government Departments, but the result has been that the development of the science of surveying has largely stagnated in this country, the centre of the Empire.

There is, therefore, a distinct need for a school and institution where students can be trained in the principles of survey work, and where the subject is studied as a whole. This school would also serve as a central information bureau, enabling the scattered surveyors of the Empire to keep in touch with developments, and to which they could apply for information and assistance.

It might seem at first sight that this could and should be undertaken by a Government Department, but this is hardly possible for various reasons. There is no central authority which deals with the Government Surveys of the Empire, though a link is kept between the Colonial (as distinct from the Dominion) Surveys by the Colonial Survey Committee. The various Surveys and Departments naturally have to consider their own immediate needs first; they are usually short of funds, and consequently are not in a position to carry out the work now being discussed. Even if a central authority were formed for this purpose it could deal only with Government Surveys, and could not train surveyors and engineers for private work.

There seems little doubt that most of the Government Surveys would welcome a school from which they could recruit their staff, and an institution to which they could apply for information, and which could keep them in touch with the activities and progress in other parts of the world.

The existence of such an establishment would also encourage the production of improved designs of instruments, and the invention of new time-saving devices; there have been many such improvements of late years, but mostly from abroad—*e.g.*, Invar tapes and wires for base measurement (France), improved levelling instrument (Germany). There are also many developments in view which require working out—*e.g.*, the use of wireless time signals for the determination of longitude in the field, survey from aircraft, &c. At present makers have little inducement to bring out new and improved patterns of instruments; their largest customers are engineers, who as a rule have had a very elementary training as surveyors, and are shy of adopting a new instrument or method.

The above remarks apply particularly to land surveying, but are largely true also of hydrographic work. India and Canada have their own Hydrographic Services, but apart from this the Hydrographic Department of the Admiralty has to deal with all the seas and coasts of the Empire, and also with such others as are not dealt with by their own Governments. The task is a large one, and the resources available are all too small for the work. There is much work waiting to be done, and anything that assists in getting this work done quicker

and better will be of great value to the shipping industry and the country as a whole. Even in home waters there is much to be done, if only due to the changes continually taking place in all estuaries. The Thames, the Humber, Portsmouth, Plymouth, and Liverpool have to be resurveyed annually. The Bristol Channel is badly in need of resurvey, which it is hoped will be carried out shortly (it was last done about 1890). The approaches to Liverpool, the Solway Firth, and the Clyde badly need revision. Most of the East Coast of England and the North Sea has not been surveyed for fifty years, and some of the work is as old as 1830. Apart from the shifting of sandbanks, &c., much of the earlier work is not up to the standard of modern requirements.

As regards the rest of the world, the coast of Brazil has not been surveyed since about 1852, and that survey suffered from the poor facilities available at the time, and is very out of date. The approaches to Monte Video have not been done since 1849, and the charts are bad. The Falkland Islands are partly unsurveyed, and South Georgia and the South Shetlands almost entirely so. The Straits of Magellan, other than the main routes, are largely unknown. The coasts of China are yet imperfectly charted; even the approaches to Hong Kong are incomplete. Siam and the Straits Settlements require resurvey; the charts are not up to modern requirements and are out of date. The Red Sea coasts are at present almost entirely charted from the original sketch surveys. The Grecian Archipelago, the Dardanelles, and the Black Sea all require resurveying.

There is no school where hydrographic surveyors can receive instruction in the principles and theory of their work, and no staff available for studying methods and instruments and bringing them up to date. The Hydrographic staff of the Admiralty is recruited from volunteers amongst the younger officers of the Executive Branch of the Royal Navy who have passed in navigation. They learn their surveying in the surveying ships while work is in progress, and the staff of trained surveyors is at present so limited that they can give little instruction to the beginners. Many officers, after serving in a surveying ship for two or more years, return to ordinary duties afloat, or specialise in other branches where their knowledge of survey work is of great benefit to them. The remainder are advanced in rank with the officers of H.M. fleet. The existence of a school where the theoretical side of the question could be studied would be of great benefit to all.

The principles involved in survey are the same, whether applied by land or sea, and the instruments are largely the same. One establishment could usefully study and give instruction in both sides of survey work.

Survey cannot be carried out over large tracts of country without considera-tion of the science generally known as geodesy, which is really only survey as applied to the earth as a whole. The problems involved in this require not only world-wide data but high mathematical skill. Problems interconnected with these are those concerning the tides and terrestrial magnetism, both of great importance to navigation. These, again, connect with the study of the earth's structure in its wider sense, and so connect with seismology and geology. These problems may all be summed up in the word geophysics.

While a knowledge of geophysics is not necessary for every surveyor, no survey authority can function satisfactorily without it. At the same time few such authorities have the staff available for its proper study. A central institu-tion, which could be referred to for information, would add greatly to the efficiency of the Survey authorities.

The need for a British Geodetic Institute is admitted by all who are acquainted with the nature and importance of the pressing Imperial and scientific problems which depend on the great surveys. The study of such problems has hitherto been left, in characteristic British fashion, to the initiative of enthu-siastic individuals or neglected altogether. Take, for example, the case of the tides, so vital a matter to our sailors. While the late Sir George Darwin still lived it could at least be said that one master-mind was devoted, with some approach to continuity, to the study of the great problems which must be attacked and solved if tidal prediction is to advance beyond its present elementary and scrappy state, but since his lamented death in 1912 the subject has lacked attention.

At the request of the B.A., Prof. Horace Lamb recently reviewed the whole

situation with regard to tides, and in a masterly report indicated the number and importance of the problems awaiting solution. Problems comparable in insistence are connected with the land surveys of our Empire, and a similar review of the general situation, also initiated by the B.A. under the stimulus of war, drew attention to the pressing need of some determined effort to attack them. The report opened with this cogent sentence : ' There is no institution, association, or department whose business it is to deal with the higher Geodesy.' Consideration of the report by a special committee, subsequently enlarged, developed in the direction of urging the establishment of a Geophysical Institute. The need for such an Institute has been formally recognised as urgent by the Conjoint Board of Scientific Societies (formed during the war for the study of urgent questions), who appointed a small executive committee (which included the President and Secretary of the Royal Society) to press for the immediate establishment of such an Institute.

A committee promoted by the ex-Vice-Chancellor of Cambridge was subsequently formed, and issued an appeal calling attention to the national importance of the matter. Amongst its members are to be found the Astronomer-Royal, the President of the Royal Society, Sir Charles Parsons, Col. Lyons (formerly Director-General of the Survey of Egypt), Prof. Turner, Sir Charles Close (Director-General of the Ordnance Survey), Sir Napier Shaw, Sir Joseph Larmor, and other authorities on scientific matters.

In an appeal issued by that committee it is stated that ' It is the widespread British territories which are most closely concerned in the great international surveys of the future, and indeed of the past ; and the consolidation and extension of their special surveys is most necessary to the solution of the Geophysical problems of the world. . . . It would be a matter for regret if, from omission of the relevant scientific development at home, British official surveyors were again compelled to rely on the Prussian Geodetic Institute at Potsdam for information with regard to international work in the higher Geodesy.'

The following are extracts from letters received by this committee :—

Admiral Parry (then Hydrographer to the Admiralty)—' Such an Institution would be warmly welcomed by the Hydrographic Department, and it is suggested that courses of instruction should be available, not only for geodesists and land surveyors, but also for the cognate Naval Service, so that these services would be able to collaborate more closely than at present as regards geodetic problems, and as regards tidal problems would assist in bridging the gap between the practical and theoretical sides which at present exists.   I am convinced that the establishment of such an Institute would be of the greatest benefit to the Empire at large, more especially as the latter is so widely distributed, and it seems most essential that there should be such an Institute where surveyors, geodesists, etc., of the Empire could not only receive instruction but to which they could also refer any practical and theoretical problems which may arise.'

A letter from the Army Council states that—' The war which is now drawing to a conclusion has shown the great value to the Army of trained surveyors from the skilled geodesist to the topographer and draughtsman. I am to say, therefore, that the Council would view with great satisfaction the establishment of an Institute which would encourage the study of Geodesy and Survey in all its branches, and that such an Institute would undoubtedly be of immense assistance to that study of survey work which it is the wish of the Council to promote in the Army.'

It will be seen from the above that both the Navy and Army authorities are anxious to see a combined Survey and Geodetic School and Institute established.

Sir Charles Close (Director-General of the Ordnance Survey) writes—' I have no doubt that it is in the national interest that a Geodetic Institute should be created, and I think it would be a very satisfactory arrangement if it were established at Cambridge, and, in connection with it, a Professorship of Geodesy.'

We think it would be difficult to find, in any scientific matter, greater unanimity amongst all the authorities concerned therein. We trust that sufficient evidence has been given both as to the national importance of the subject and the urgency of the need for action. We await the advent of the ' Vivus Benefactor,' for—as already indicated—there is a consensus of opinion that

such an Institution should be established within a University by private bene-
factions, although assistance might, as a consequence, be forthcoming from
national funds.   The wide ramifications of Survey, Geodesy, and Geodynamics
into mathematical, physical, and engineering sciences call for their study in a
University, rather than in a Departmental, atmosphere. 'Undue withdrawal
from the Universities to official special Institutes of the men who show promise
of power would hamper their own development by removal from their proper
environment; moreover, it would weaken the efficiency of the Universities as
the national nurseries of scientific ability and genius at a time when, by more
intimate relations with the Dominions and increased contact with other nations,
they ought to be preparing for the discharge of imperial functions.' [1]

We trust that this conference of the Physical and Geographical Sections
will forward to the Council of the B.A. a resolution calling attention to the
urgency of this matter.

[1] Sir Joseph Larmor.

SECTIONAL TRANSACTIONS.

SECTION A.—MATHEMATICAL & PHYSICAL SCIENCE.

(For references to the publication elsewhere of communications entered in the following list of transactions, see p. 380.)

Tuesday, August 24.

1. Presidential Address by Prof. A. S. EDDINGTON, F.R.S. See p. 34.

2. Mr. J. EVERSHED, F.R.S.—*Measures of the Shifts of the Fraunhofer Lines and their Interpretation, particularly with relation to the Einstein Theory.*

3. Major P. A. MACMAHON, F.R.S.—*A New Binomial Theorem and its Arithmetic Interpretation.*

4. Prof. H. HILTON.—*Plane Algebraic Curves of Degree n with a Multiple Point of Order n−1, and a Conic of 2n-point Contact.*

5. Prof. G. H. BRYAN, F.R.S.—*The Graphical Solution of Spherical Triangles.*

6. Mr. T. C. LEWIS.—*Is there in Space of three Dimensions an Analogue to Feuerbach's Theorem? Is there anything corresponding to the Hart System?*

Wednesday, August 25.

7. Dr. F. W. ASTON.—*Mass Spectra and the Constitution of Chemical Elements.*

8. Sir E. RUTHERFORD, F.R.S.—*The Building up of Atoms.*

9. Prof. R. WHIDDINGTON.—*The Ultra-Micrometer.*

10. Lieut.-Col. F. J. M. STRATTON.—*Spectra of Nova Aquilæ III.*

11. Rev. Father A. L. CORTIE, S.J.—*Comparison of Drawings of Solar Faculæ and Photographs of Calcium Flocculi.*

Thursday, August 26.

12. *Discussion on The Origin of Spectra,* opened by Prof. A. FOWLER, F.R.S., and Prof. J. W. NICHOLSON, F.R.S.

13. Report of Seismology Committee. Prof. H. H. TURNER, F.R.S., and Mr. J. J. SHAW. See p. 215.

14. Sir OLIVER LODGE, F.R.S.—*Controversial Note on Popular Relativity.*

This note concerns the assumed necessary constancy of the observed velocity of light in free space, as contrasted with the universally admitted constancy of its true velocity. The author contends that there is no experimental evidence for the dogma that wave-fronts are concentric with a travelling observer initially situated at the source. The Michelson-Morley experiment is consistent with such concentricity, but does not necessitate it. The FitzGerald-Lorentz contraction of matter is a perfectly valid alternative explanation. Einstein's equations exercise no physical discrimination and are consistent with either mode of expression. In interpreting them verbally it is safer for a physicist to postulate a special property of matter than to attempt to foist complications upon time and space.

15. Prof. F. HORTON and Miss A. C. DAVIES.—*The Ionisation of Atmospheric Neon.*

It has been found[1] that the following are critical electron velocities for atmospheric neon :—11·8 volts and 17·8 volts for the production of radiation, and 16·7 volts, 20·0 volts, and 22·8 volts for the production of ionisation. The conditions under which the different critical points were indicated showed that the radiation velocity, 11·8 volts, is associated with the ionisation velocity, 16·7 volts, and that the radiation velocity, 17·8 volts, is associated with the ionisation velocity, 22·8 volts. No third critical velocity for radiation, corresponding to the ionisation velocity, 20·0 volts, was detected, but it is possible that such a critical velocity occurs too close to one of the other radiation velocities to be distinguished separately.

The conditions under which the various points were obtained also showed that none of the critical velocities mentioned can be attributed to the displacement of a second electron from an already ionised atom. Neon is the only gas so far investigated which has shown more than one critical velocity for the removal of a first electron from the atom.

Further information as to the ionisation of neon was sought by observing the spectrum of the luminosity produced in the gas as the electron velocity was gradually increased; for on the generally accepted view the line spectrum of a gas results from the recombination which occurs when ionisation has taken place. It was found that the lines of the first and second subordinate series types never appeared below 22·8 volts, but that under certain conditions the lines of the Principal series type came in at 20·0 volts. No lines in the visible spectrum were ever observed below 20·0 volts, although the earlier experiments show that considerable ionisation must have been occurring.

The results of the ionisation experiments might be interpreted as indicating that atmospheric neon is a mixture of different elements, since isotopes would be expected to have the same critical velocities for electrons. Such a supposition is, however, not borne out by the investigation of the spectrum of the luminosity produced in atmospheric neon, for if there were more than one element present it would be expected that the complete spectrum of the element of lower ionising velocity (including some lines in the visible spectrum) would

[1] *Proc. Roy. Soc.* A, 1920.

be obtained for lower electron velocities than the spectrum of the other element, and this was not found to be the case. We therefore conclude that the different critical velocities found for neon correspond to the displacement of differently situated electrons within the atom, or, in other words, that the external electrons in neon are not all symmetrically situated about the nucleus.

16. Report of Committee on Gravity at Sea.

Friday, August 27.

17. Prof. S. CHAPMAN, F.R.S.—*Terrestrial Magnetism, Auroræ, Solar Disturbance, and the Upper Atmosphere.*

18. Dr. A. E. OXLEY.—*Magnetism and the Structure of the Atom.*

19. Mr. J. H. SHAXBY.—*Vapour Pressures.*

20. Reports of Committee on Tides. See p. 321.
  (1) Prof. PROUDMAN.—*Harmonic Analysis.*
  (2) Mr. A. T. DOODSON.—*Prediction.*

21. Dr. P. V. WELLS.—*The Thickness of Stratified Soap Films.*

22. Mr. H. P. WARAN.—*A New Type of Interferometer.*

23. Joint discussion with Section E on *Geodesy.* See p. 346.

SECTION B.—CHEMISTRY.

(For references to the publication elsewhere of communications entered in the following list of transactions, see p. 380.)

Tuesday, August 24.

1. Presidential Address by Mr. C. T. HEYCOCK, F.R.S. See p. 50.

2. Capt. A. DESBOROUGH.—*Industrial Alcohol.*

Wednesday, August 25.

3. Joint Meeting with Section A for the discussion of papers 7 and 8 in the programme of that Section (which see).

4. *Discussion on Lubrication.*—Mr. A. E. DUNSTAN, Mr. H. M. WELLS, Mr. J. E. SOUTHCOMBE, Mr. H. T. TIZARD, Prof. W. C. McC. LEWIS.

Thursday, August 26.

5. *Papers on the Metallurgy of Tungsten.*—Mr. J. L. F. VOGEL, Prof. C. H. DESCH. *Electrolytic Zinc.*—Mr. S. FIELD.

6. Dr. R. V. STANFORD.—(a) *New Method for the Estimation of Carbon by Combustion in Organic Compounds, using very small quantities of substance. (b) Estimation of Amino-acids, using very small quantities of substance.*

7.   *Report of Committee on Fuel Economy* (Prof. W. A. Bone, F.R.S.), and *Discussion* thereon.   See p. 248.

Friday, August 27.

8.   Dr. J. S. Owens.—*Researches on Atmospheric Pollution and its Measurement.*

9.   Prof. F. M. Jaeger.—*Research Work at High Temperatures, and the Determination of Surface Tension and Electrical Conductivity between* $-100°$ *and* $+1650°$ *C.*

The following excursions were arranged for members of the Section : Melingriffith Tinplate Co. ; South Wales Indiarubber Co. ; Cardiff Gas Co. ; Cardiff Dowlais Works ; exhibits, etc., in Chemical Laboratories, Mental Hospital, Radyr ; Powell Duffryn Co. ; Tharsis Sulphur and Copper Co.

SECTION C.—GEOLOGY.

(For references to the publication elsewhere of communications entered in the following list of transactions, see p. 380.)

Tuesday, August 24.

1.   Presidential Address by Dr. F. A. Bather, F.R.S. See p. 61.

2.   Prof. A. Hubert Cox.—*Address on the Geology of the Cardiff District.*

3   Dr. J. W. Evans, F.R.S.—*The Origin of the Alkali Igneous Rocks.*

> These rocks, distinguished by unusually high proportion of alkalies, relatively to alumina and lime, occur mainly where the crust of the earth is thick, the heat-gradient low, and there has been no folding since remote times. Magmas appear to have reached the surface by fault fissures from great depths where high pressures are associated with comparatively low temperatures.   Crystallisation proceeding under such circumstances, there would be an early formation of minerals with small molecular volumes, garnets, kyanite, epidote, and zoisite, minerals rich in lime and alumina. Zoisite may be regarded as the high-pressure representative of anorthite, but there is no corresponding representative of albite or orthoclase.   Consequently we should expect a residual magma exceptionally rich in alkalies which would furnish the material necessary for formation of alkali rocks.

4.   Reports of Research Committees.   See p. 261.

Wednesday, August 25.

5.   Joint Meeting with Sections D and K.   *Discussion.— Mendelism and Palæontology: The Factorial Interpretation of Gradual Changes, especially when New Characters appear late in the Individual Life-cycle.*

Dr. F. A. BATHER, F.R.S. :—

The question posed. Can characters be regarded as independent, *i.e.* as manifestations of independent factors in the germ? Does evolution take place solely by addition or loss of such factors? Is there not also a gradual modification of the body, resulting in a continuous transition? Palæontologists find such transition to be the rule in those cases where the geological record is sufficiently complete. (See President's Address, Section C, heading 'Continuity in Development.') Palæontologists support the theory of Recapitulation, and believe that, in many cases, gradual modification of the adult and senile body is, in the course of race-history, pushed back to earlier growth-stages. (See President's Address, Section C, heading 'Recapitulation.') Can such cases be explained by independent factors in the germ? Does not that hypothesis involve, first, an alteration of the germ through change in the body; secondly, the determination of that germinal change in a direction harmonious with bodily change?

Dr. R. RUGGLES GATES :—

According to mutationist hypothesis, germinal characters arise as alterations of single elements of the germ plasm. This conception avoids the difficulties involved in considering the change as due to the loss or addition of a factor. It recognises on the one hand the solidarity of the germ plasm as a whole, and on the other the independent origin of variations in its several parts. Such variations are termed karyogenetic, since they apparently arise in the nuclei and are perpetuated by mitotic division. Mutations of this nature are almost universal amongst wild plants and animals, and some of them are so small that for general purposes they are practically continuous. They differ from the Darwinian conception of continuous variation, however, in that (i.) they do not arise in any regular order, (ii.) they are inherited as separate units. But Recapitulation is an almost equally widespread phenomenon in animals, and to a less extent in plants. The recapitulation in animal embryos, and in such fossil groups as the Ammonites, implies the addition of terminal stages to the development of the organism. From the standpoint of organic structure this process is clearly different from a mutation by which the nuclear unit is modified throughout the organism. Recapitulatory characters thus fall into two groups : (i.) embryonic, which appear always to imply adaptation of the organism to different conditions, and are best explained by the neo-Lamarckian principle; (ii.) orthogenetic, which appear late in the life-cycle but are germinal in origin and non-adaptational.

Prof. J. E. DUERDEN.—*Mendelism; Palæontology; Evolution.*

Recent investigations in genetics in general give support to the factorial hypothesis, namely, that the characteristics of the body are represented in the germ plasm, in all probability in association with the chromosomes. Supporting evidence is forthcoming from sex, crossing-over and localisation. Any hereditary change in an organism must therefore be associated with factorial change in the germ plasm. Casual mutations readily admit of Mendelian interpretation, but evolution in general does not take place by changes of this kind. Evolution of species often seems to call for a similar change in the whole assemblage of individuals within an area, while palæontology and the study of numbers of

related forms calls for gradual successional changes in the same direction as regards any particular structure (orthogenesis). Mendelian experiments do not yet afford any great support for either of these demands. Observed mutational changes do not call for environmental influence, and are wholly apart from any adaptive considerations; natural selection plays no part in the origin or preservation of variations, but may be eliminative. It is highly questionable whether somatic or environmental influences can modify the germinal factors in definite directions, but disruptive changes and gradual loss of factorial vigour, or perhaps senility, may be contemplated, continued over long ages. As the common germ plasm of a race may at any one time be presumed to be in somewhat the same condition, evolutionary changes on somewhat similar lines may be expected.

Prof. A. DENDY, F.R.S.

Thursday, August 26.

6. Dr. T. FRANKLIN SIBLY.—*The Old Red Sandstone of the Mitcheldean District, Gloucestershire.*

Mitcheldean lies on the Gloucestershire-Herefordshire border ten miles west of Gloucester, and in the latitude of the Breconshire Beacons. In this neighbourhood persistent westerly dips determine an outcrop of the whole of the Old Red Sandstone, with a thickness of some 7,500 feet, in a band scarcely two miles wide, bounded on the east by the Silurian strata of the May Hill anticline and on the west by the Carboniferous of the Forest of Dean coal-basin. The sequence of strata determined in this locality offers a possible key to the wilderness of Old Red Sandstone in Herefordshire.

7. Prof. W. M. FLINDERS PETRIE, F.R.S.—*The Continuance of Life on the Earth.*

If by any process of aggregation the earth has been at a red heat, all the lime and soda would be combined with the silica (now sandstone) and all the carbonic and hydrochloric acids would be in the atmosphere (now locked up in limestone and salt). The changes from that condition would consist in the acids gradually decomposing the silicates; at present there is only a minute fraction of the original carbonic acid left in the atmosphere. The decomposition of a few more inches of silicates over the globe would exhaust the carbonic acid, and life could not exist. This may take place in a few hundred thousand years, and such is the limit to vegetable and therefore to animal life, irrespective of solar cooling. The amount of carbon in the strata is probably enough to combine with all the oxygen of the air; hence land-breathing animals were impossible until after the carbon had become separated and left oxygen free. This agrees with the appearance of air breathers after the Carboniferous age.

8. Dr. A. E. TRUEMAN.—*The Liassic Rocks of Somersetshire and their Correlation.*

The Liassic rocks of Somerset are thin but richly fossiliferous, yielding many large Ammonites. When followed towards the Mendips there is considerable reduction in thickness and marked lithological change. At several localities a white limestone resembling the Sutton Stone of Glamorgan is seen to rest on the Carboniferous Limestone;

it is developed at various horizons and usually contains no Ammonites, but correlation can be made by means of species of *Ostrea* and *Gryphea*. In the numerous exposures near Radstock many non-sequences can be located, and maps showing the movement of intra-Liassic folds have been prepared.

9. Dr. J. K. CHARLESWORTH.—*The Glaciation of the North-West of Ireland.*

The major part of the region investigated, including the Donegal Highlands and the Sperrin Mountains, was never invaded by the Scottish ice as currently supposed, but the Donegal mountains, in particular the Barnesmore Hills, formed a most powerful centre of radiation, whence ice streamed westwards to the Atlantic and eastwards over the Sperrin Mountains to Cookstown and beyond. In a south-easterly direction the ice passed obliquely across the Clogher Valley in Slieve Beagh to the Central Plain of Ireland, where was located the 'central axis' of Hull and Kilroe. This axis of dispersal existed at no period of the glaciation.

10. Mr. L. DUDLEY STAMP.—*On Cycles of Sedimentation in the Eocene Strata of the Anglo-Franco-Belgian Basin.*

The Eocene deposits of the great Anglo-Franco-Belgian Basin can be grouped naturally into a series of cycles of sedimentation—the Montian, Landenian, Ypresian, Luetetian, Ledian, and Bartonian. Each cycle commences with a marine invasion and passes from marine to estuarine and continental conditions. In England the changes are closely connected with the gentle, intermittent uprise of the Weald.

Friday, August 27.

11. Dr. J. W. EVANS, F.R.S.—*The Geological Structure of North Devon.*

In early Permian times the Devonian and Carboniferous were thrown by pressure from the south into overfolds, with overthrust faults. A subsequent relaxation of pressure resulted in a slip back on the same fault-planes. There were also oblique tear-faults striking between north and west. A mountain region then sloped southward from the Welsh Coast to Mid-Devon and much material was transported in that direction. In the Triassic period, however, the Palæozoic had, as a whole, its present contours, including the great Glastonbury and Bristol Channel depression descending to the west, and its subsidiary valleys still partly filled with Mesozoic deposits. In Tertiary times there was renewed pressure from the south. This met with less resistance in the west, and there was consequently a relatively forward and downward movement on that side along the old tear-faults and possibly new fractures with the same general direction. In Pliocene times the land was more submerged than now and the subsequent emergence seems to have continued in most places till a comparatively recent date.

12. Prof. W. L. BRAGG.—*Crystal Structure.*

The investigation into crystal structure, which has been made feasible by the discovery of the diffraction of X-rays by crystals, has led to a determination of the precise positions of the atoms in a number of the simpler crystalline forms. Recent theories of atomic structure, such as those put forward by Bom and Landé, Debye, Lewis, and Langmuir,

are largely based on the arrangement of the atoms in crystalline solids, since this arrangement affords an insight into the nature of the forces acting between the atoms. In such compounds as sodium chloride, it is probable that the atoms exist as ions of sodium and chlorine, and that the crystal is held together by the electrostatic attractions of these ions, thus accounting for the fact that there is no grouping of the atoms into molecules in the solid. In other compounds, such as those of two electronegative elements, the molecular arrangement persists in the solid state and the chemical combination appears to be of a different type from that of sodium chloride. A consideration of the distances between the atomic centres in crystals supports the conception of the two types of chemical combination.

13. Mr. D. C. Evans.—*The Ordoviceo-Valentian Succession in North-east Pembrokeshire and North Carmarthenshire.*

14. Mr. David Davies.—*Palæontology of the Westphalian and lower part of the Staffordian Series of the Coal Measures as found at Clydach Vale and Gilfach Goch, East Glamorgan.*

Recorded : 45,000 plants; 1000 shells; 2 insects and one fish scale. Plants yielded 154 species, shells 6 species, insects 2 species, fish scale one; 45 of these are new to South Wales and 7 new to Britain. Ecology of ten horizons : Equisetales predominate in four; Filicales and Pteridosperms in three; Lycopods in two, and Cordaitales in one horizon. When Lycopods predominate, Fern and Fern-like plants are weak, and *vice versa.* 37 per cent. plants are common to both series; 31 per cent. distinctly Staffordian; 32 per cent. distinctly Westphalian. The Pennant Sandstone produced smooth round coal pebbles, giving evidence of a geological break. A significant feature is the appearance of new species at this period.

SECTION D.—ZOOLOGY.

(For references to the publication elsewhere of communications entered in the following list of transactions, see p. 381.)

Tuesday, August 24.

1. Presidential Address by Prof. J. Stanley Gardiner, F.R.S. (see p. 87). Followed by a Discussion.

Afternoon.

2. Prof. J. Stephenson.—*The Polyphyletic Origin of Genera in the Oligochæta, and its bearings.*

3. Prof. P. Fauvel.—*The Affinities of the Annelidan Fauna of the Abrolhos Islands.*

Wednesday, August 25.

4. Joint Meeting with Sections C and K. *Discussion on Mendelism and Palæontology: the Mendelian Interpretation of gradual changes, especially when new Characters appear late in the individual Life-cycle.* For speakers see Programme of Section C, p. 354.

Afternoon.

5. Reports of Committees.

6. Prof. J. E. DUERDEN.—*A Caudal Vesicle and Reissner's Fibre in the Ostrich.*

7. Mr. J. H. LLOYD.—*The Early Development of the Pronephros in Scyllium.*

Thursday, August 26.

8. *Discussion on the Need for the Scientific Investigation of the Ocean.* Opened by Prof. W. A. HERDMAN, C.B.E., F.R.S. Other speakers: Prof. J. STANLEY GARDINER, F.R.S., Dr. E. J. ALLEN, F.R.S., Mr. C. TATE REGAN, F.R.S., Prof. C. A. KOFOID, Prof. J. E. DUERDEN, Sir FRANCIS OGILVIE, Mr. F. E. SMITH, Dr. E. C. JEE.

Section E (p. 361) took part in this discussion.

Afternoon.

9. *Discussion on the Need for the Scientific Investigation of Fisheries.* Opened by Mr. H. G. MAURICE, C.B. Other speakers: Prof. A. MEEK, Prof. JAMES JOHNSTONE, Mr. CRAWFORD HERON, Prof. G. GILSON, Dr. E. J. ALLEN, F.R.S., Prof. W. GARSTANG, Mr. C. TATE REGAN, F.R.S., Mr. NEALE, Prof. J. STANLEY GARDINER, F.R.S.

Friday, August 27.

10. Prof. J. E. DUERDEN.—*The Pineal Eye in the Ostrich.*

11. Prof. A. MEEK.—*The Physiology of Migration.*

12. Prof. C. A. KOFOID.—*The Neuro-motor System of Ciliate and Flagellate Protozoa, and its Relation to the process of Mitosis and to the Origin of Bilateral Symmetry in certain Flagellates.*

13. Dr. CRESSWELL SHEARER, F.R.S.—*The Influence of Salts on Growth.*

14. Mr. E. HERON-ALLEN, F.R.S., and Mr. A. EARLAND.—*Protoplasm and Pseudopodia.*

Afternoon.

15. Prof. C. A. KOFOID.—*Hookworm and Human Efficiency.*

16. Prof. R. W. HEGNER.—*The Relations between Nucleus, Cytoplasm, and External Heritable Characters in the genus* Arcella.

17. Prof. E. B. POULTON, F.R.S.—*A Preliminary Account of the Hereditary Transmission of a minute, extremely variable, generally asymmetrical marking in the fore-wing of the Currant Moth* (Abraxas grossulariata).

Saturday, August 28.

Excursion to Southerndown and Merthyr Mawr.

Exhibits.

There were on exhibition throughout the meeting :—

(*a*) A series of plates for ' A Monograph of the Unarmoured Dinoflagellates,' by Prof. C. A. KOFOID.

(*b*) Living specimens of Amphidinium, by Miss C. HERDMAN.

SECTION E.—GEOGRAPHY.

(For references to the publication elsewhere of communications entered in the following list of transactions, see p. 381.)

Tuesday, August 24.

1. Presidential address by Mr. J. McFARLANE.   See p. 98.

2. Mr. D. LLEUFER THOMAS.—*Some Geographical Aspects of the Distribution of Population on the South Wales Coalfield.*

> The narrow valleys of the plateau provoke feelings of imprisonment and isolation—originally the coal attracted the raw material of industry, *e.g.* copper, iron—after 1850 coal was worked in the interior valleys—the Rhondda valley became populous after 1871 and caused the growth of Cardiff and Barry.

Discussion opened by Dr. A. E. TRUEMAN.   Other speakers: Prof. H. J. FLEURE, Mr. H. J. RANDALL, Mr. A. E. L. HUDSON, &c.

3. Dr. A. E. TRUEMAN.—*The Iron Industry of South Wales.*

> Iron ore either hæmatite or ironstone nodules—the iron-stone worked all over the coalfield, but especially the east—the growth of Merthyr Tydfil—phosphatic nature caused a decline in mining—ore brought from Spain, despite transport costs the iron industry persists in its original location.

Wednesday, August 25.

4. Lieut.-Col. W. J. JOHNSTON, C.B.E., R.E.—*Small-scale Maps of the United Kingdom.*

> The demand for coloured maps is increasing.   Engraving on copper for map reproduction is moribund.   Of three methods in use at the Ordnance Survey that dependent on photo-zincography is probably best.

5. Mr. A. E. L. HUDSON.—*Some Methods of Using Ordnance Maps in School Teaching.*

6. Capt. H. ALLAN LLOYD.—*The Pictorial Factor in Aërial Map Design.*

7. Joint Meeting with Section L (see p. 377) in the rooms of Section L. Prof. J. L. MYRES.—*The Place of Geography in a Reformed Classical Course.* Discussion by Mr. G. C. CHISHOLM, Mr. H. O. BECKIT, &c.

Afternoon.

8. Dr. VAUGHAN CORNISH.—*Imperial Capitals.*

> Paris, like other capitals, occupies a position between the centre of the country and the middle of the most important frontier—such a situation compromises between the best site for civil (home) administration and the best site for military defence (foreign).

Excursion.

Vale of Glamorgan, visiting Barry, Llantwit Major, and Cowbridge.

Thursday, August 26.

9. Rev. W. J. BARTON.—*The Oases and Shotts of Southern Tunis.*

10. Joint Meeting with Section D in the rooms of Section D (p. 359).

Dr. E. C. JEE.—*The Movements of the Sea.*

Afternoon.

11. Prof. E. H. L. SCHWARZ.—*The Kalahari and the Possibilities of its Irrigation.*

> The Kalahari. The changed course of the Cunene river. The three great depressions. Weirs on the Cunene and Chobe and their utility for irrigation.

Excursion.

The Upland of Glamorgan and the Taff and Rhondda Valleys.

Friday, August 27.

12. Dr. T. ASHBY.—*The Water Supply of Ancient Rome.*

> The four chief aqueducts of Ancient Rome. The Anio valley, its river and springs. Recent exploration along the course of the aqueducts—geographical considerations.

13. Joint Meeting with Section A in the rooms of Section E. Dr. E. H. GRIFFITHS and Major HENRICI.—*The urgent need for the creation within the Empire of a Central Institution for Training and Research in the Sciences of Surveying, Hydrography, and Geodesy.* See p. 346.

Exhibit.

A collection of maps illustrating various aspects of the geography of South Wales, arranged by the Cardiff branch of the Geographical Association, was exhibited in the City Hall throughout the meeting.

SECTION F.—ECONOMIC SCIENCE AND STATISTICS.

(For references to the publication elsewhere of communications entered in the following list of transactions, see p. 381.)

Tuesday, August 24.

1. Mr. H. ALLCOCK.—*A Criticism of the Majority Report of the Royal Commission on Decimal Coinage.*
Discussion.

2. Mr. L. SMITH GORDON.—*Agriculture as a Business.*
> Suggestions as to the possibility of reconciling the psychological and economic demands of agriculture with modern conditions by treating it as an industry to be organised upon a scientific basis.

3. Mr. J. LASSEN.—*Danish Credit Corporations.*
> Details concerning a system of co-operative borrowing of money on first-class mortgage security. The interest of the system lies in the fact that whereas all financing is usually performed through the medium of Lenders (bankers, trust companies, &c.), the scheme under review is directly reversed and relates to a ' Corporation of Borrowers.'

Discussion, introduced by Mr. C. R. FAY.

Wednesday, August 25.

4. Presidential Address by Dr. J. H. CLAPHAM, C.B.E. (See p. 114.)

5. Mr. J. O. CHEETHAM.—*The present Supply of Coal and its effects on the Shipping Interests of Cardiff.*

6. Mr. R. F. ADGIE.—*The Conduct of the Mining Industry.*
> An examination of some of its economic and psychological aspects, with regard to the bearing on the socialisation of ownership and control.

Discussion.

Thursday, August 26.

7. Mr. A. H. GIBSON.—*Credit: Inflation and Prices.*
> A short review of the early beginnings of bank credit, its growth and elasticity in modern times, its relation to commodity prices; and the abuse of bank credit during the recent war.

8. Mr. A. J. BEAMISH.—*Deflation and the National Balance Sheet.*

> A consideration of the alternatives before the Chancellor of the Exchequer.

Discussion.

9. Mr. R. TROUTON.—*The Liquidation of International Debts.*

> The fact that it was advantageous during the war for some countries to furnish mainly finance and others mainly men is not a reason for making the latter pay a tribute to the former.

Discussion.

Friday, August 27.

10. *Communications from Research Committees of Section F.* —Sir E. BRABROOK, C.B.

11. Mrs. WOOTTON.—*The Future of Earning.*

> An examination of the principles on which payment for labour is now based and their probable future development; with special reference to the relations of wages and prices and the influence of Government expenditure (by way of subsidies, &c.) on real income.

Discussion, introduced by Prof. W. J. ROBERTS.

SECTION G.—ENGINEERING.

(For references to the publication elsewhere of communications entered in the following list of transactions, see p. 381.)

Tuesday, August 24.

1. Presidential Address by Prof. C. F. JENKIN, C.B.E. (See p. 125.)

2. Prof. F. C. LEA.—*Testing Materials at High Temperatures.*

3. Col. R. E. CROMPTON, C.B.—*The Cutting Edges of Tools.*

Excursion.

In the afternoon a visit took place to the Bute Docks of the Cardiff Railway Co.

Wednesday, August 25.

4. Mr. S. F. EDGE.—*Farm Tractors—Regarded from the Viewpoint of the User and Potential User.*

> The writer traces the earliest appearances of the British farm tractor, explains its failure to earn the encouragement it deserved, and relates his experience of excellent work, to-day, from a ten-year-old machine built by pioneers of twenty years ago. He explains his preference, at present, for a certain type of machine, touches upon the psychological effect of the use of the tractor on the labour question, and then discusses the various main classifications of tractors

now in common use. Giving reasons for the greater success
of one generic type, he confesses his inability to name any
one machine which will do all sorts of work equally efficiently,
mentions the poor quality of knowledge often possessed by
those who actually operate tractors, but proceeds to encourage
designers and manufacturers to give us better and better
tractors, especially of better material, despite the temptation
to turn out less serviceable machines at low prices. Road-
haulage is a phase of tractor-work he asks designers to bear
in mind, but he admits that 'one machine for one job' is a
principle that designers and users may have, for some years,
to respect. Tracing the history of the British industry, he
sympathises with tractor-manufacturers in the past, he holds
out hope of a fine future, if they will strive to give the
farmer the best machines, in design, material, and workman-
ship alike. He expresses his belief in the future of the
tractor industry.

5. Mr. H. R. RICARDO.—*A High-speed Internal-combustion
Engine for Research.*

6. Prof. W. H. WATKINSON.—*A Dynamical Method of Rais-
ing Gases to High Temperatures.*

7. Dr. C. BATHO.—*The Partition of the Load in Riveted
Joints.*

Excursion.

In the afternoon a visit took place to the Melingriffith Tinplate
Works.

Thursday, August 26.

8. Prof. J. T. MACGREGOR-MORRIS.—*A Portable, Direct-
reading Anemometer for Measuring Ventilation in Coal-
mines.*

9. Mr. H. T. TIZARD and Mr. D. R. PYE.—*Specific Heat
and Dissociation in Internal-combustion Engines.*

10. Sir J. B. HENDERSON and Prof. H. R. HASSÉ.—*The
Indicator Diagram of a Gun.*

11. Prof. A. L. MELLANBY and Mr. W. KERR.—*Steam Action
in Simple Nozzles. A Short Study of the Variants in
Nozzle Expansion.*

This paper gives an exposition of a simple method of
dealing with the latter in 'straight' nozzle expansion. It
is intended as an introduction to a more detailed considera-
tion, from the experimental point of view, of the same
problems. The general methods of investigating steam flow
are analysed, and it is shown that the somewhat neglected
search-tube experiments are the most promising. The
paucity of information regarding internal effects of steam
expansion is noticed, and it is contended that the ordinary
experiments on steam flow cannot give much further informa-
tion upon interior happenings. A series of equations are
derived from which the pressure ratio curve along a nozzle
can be calculated, and experimental evidence is brought
forward to show that the underlying theory is, at least,

approximately correct. Arising from a desire to study jet conditions, the necessity of dealing with any type of expansion is postulated, and some discussion is given on the general influences of certain arbitrary laws of expansion on throat and flow conditions.

Excursion.

In the afternoon a visit took place to the Dowlais (Cardiff) works of Messrs. Guest, Keen & Nettlefolds, Ltd., at East Moors.

Friday, August 27.

12. Prof. W. CRAMP.—*The Pneumatic Conveying of Materials.*

13. Wing-Commander T. R. CAVE-BROWNE-CAVE.—*Airships for Slow-speed Heavy Transport and their Application to Civil Engineering.*

> The use of airships as a means of transporting considerable loads over impassable country is developed from the operations actually carried out up to the present. The application to Civil Engineering, Surveying, and Transport is discussed, and a review is made of the uses of airships for purposes other than those of the Mail and First-class Passenger Services, which have been fully dealt with by Air Commodore Maitland before the Royal Society of Arts.

14. Prof. G. W. O. HOWE.—*The Efficiency of Transmitting Aërials and the Power required for long-distance Radio-Telegraphy.*

15. Dr. J. S. OWENS.—*The Removal of Reefs in the Rio Guadiana.*

> This paper describes the removal by drilling and blasting of about 11,000 tons of rock reefs from the bed of the Rio Guadiana at Pomaron, the port of the San Domingos Mines. There is at this point a rise of tide of 11 ft. springs, maximum normal current up to about three knots, and a maximum depth over the reefs of 30 ft. Drilling was done from a floating drill barge using a 5-in. steam drill. Charging was done without divers and by means of specially prepared 'sausages' of dynamite dropped through a pipe and fired electrically in groups of about eight holes.

Excursion.

In the afternoon a visit took place to the surface plant of the Great Western Colliery Co., near Pontypridd, including large electric winder.

SECTION H.—ANTHROPOLOGY.

(For references to the publication elsewhere of communications entered in the following list of transactions, see p. 381.)

Tuesday, August 24.

1. Presidential Address, by Prof. KARL PEARSON, F.R.S. (See p. 135.)

2.   Prof. H. J. FLEURE.—*The Welsh People: Physical Types.*

Nine distinct physical types are found in Wales. Speaking generally the Welsh show more longheadedness, more dark pigment, and shorter statures than the English, but both are complex minglings of different breeds which in some cases can be correlated with migrations of prehistoric and historic times.

3.   Miss M. L. TILDESLEY.—*Preliminary Notes on the Burmese skull.*

4.   Prof. F. G. PARSONS.—*The Modern Londoner and the Long Barrow Man.*

Afternoon.

5.   Dr. THOMAS ASHBY.—*The Roman Site at Caerwent.*

6.   Mr. WILLOUGHBY GARDNER.—*Roman Site at Abergele.*

Wednesday, August 25.

7.   Dr. W. H. R. RIVERS, F.R.S.—*The Statues of Easter Island.*

In San Cristoval stone images represent the dead chief buried in the pyramidal structures with which the images are associated. It is suggested that the statues of Easter Island represent a hypertrophy of one element of a similar association.

8.   Captain L. W. G. MALCOLM.—*The Anthropogeography of the Cameroons, W. Africa.*

9.   Prof. E. H. L. SCHWARZ.—*The Ovambo.*

10.   Signor BAGNANI.—*Recent Archæological Discoveries in Rome.*

11.   Dr. T. ASHBY.—*Further Observations on the Roman Roads of Central and Southern Italy.*

The roads now described are the Via Valeria, and its prolongation the Via Claudia Valeria, which, with the Via Tiburtina, formed a continuous highway from Rome to the Adriatic, and the Via Latina. An attempt to discover the course of the Via Herculia from Venusia to Potentia was unsuccessful. One of the finest stretches of Roman pavement in Italy was discovered on the Via Cassia, which leads north from Rome through Etruria.

12.   Prof. A. M. WOODWARD.—*Note on Excavations on a Hill Fort at Ilkley.*

Afternoon.

Excursion to Caerwent.

Thursday, August 26.

13. Mr. L. H. DUDLEY BUXTON.—*The Physical Anthropology of Ancient Greece and Greek Lands.*

The present essay is a continuation of a report rendered to the British Association at Bournemouth in 1919, on the ethnology of Cyprus.

In classical times a clear distinction was made between 'Greeks' and 'Barbarians,' and this distinction was believed to have a physical basis. Modern anthropologists are generally in agreement that at least two races are represented in Greek lands, 'Mediterranean' and 'Alpine.' The aboriginal population remains at present uncertain, and in the absence of early material it became necessary to reverse the time process and to study the ancient population in the light of the modern. In order to make the matter clearer, the material has been divided into classes. First, head form is considered; secondly, facial form; and thirdly, stature; a fourth class, pigmentation, has been added for the living. Evidence which can be treated statistically is available from Albania, Leukas, Messenia (Meligala), Peloponnese (Mani), Crete, Lycia, and Cyprus.

The mean Cephalic Index varies from 79·20 in Crete to 87·51 among the Bektash of Lycia. None of the measurements are in agreement with the pure Mediterranean type as represented, for instance, in Corsica. Among the people who claim to be Greeks we have three classes—the Cretans, Maniotes, and Lycians have a cephalic index under 81, the Messenians and Cypriots a cephalic of 82, and the Leukadians and Albanians an index over 84. This classification does not appear to be of any real significance when we come to examine the standard deviations, as these are sufficiently great to suggest considerable admixture, especially in Lycia. Cranial evidence, such as it is, confirms this theory. The ancient crania which have survived form too small a series for statistical treatment. It appears as a general rule that the modern Greeks are slightly more brachycephalic than the ancient inhabitants of the same places—possibly sufficient correction has not been made for the difference between crania and living heads. From such scanty evidence as we have it appears that there is a general closer approximation between the earliest cranial indices and the modern ones than between either of the former and those of intermediate date. In Crete it would appear as though there was an immigration or an extension of the longheads in early times, who were later supplanted by a mixed round- and long-headed population.

The following tentative conclusions have been drawn: First, the cranial indices of the Greeks exhibit great variety, sufficient to suggest ethnic admixture. Secondly, this admixture has not been evenly distributed, and local and distinct sub-races have been formed, so distinct that where crania over a long period have been obtained the cephalic index of one modern village more closely resembles that of their Bronze Age predecessors than that of a neighbouring area. As a corollary to this, any sweeping statements about the cephalic index of a modern administrative area, based on measurements made on sixty individuals, as has been done by Clon Stephanos, is unjustifiable. Thirdly, the mixture of race is early, possibly Neolithic in Leukas, certainly Bronze Age (or before) in Cyprus or Crete.

The living stature is available, in large numbers, from Crete and Cyprus only, and in both cases the stature is practically identical. Three other small series are available, all of which fall into a single shorter group—Leukas, Mount Parnon, and Lycian gypsies. The modern stature appears to be slightly greater than the ancient. The conditions, whatever they may be, which make for heterogeneity in cephalic index, appear also to make for a similar condition in stature. The small numbers represented, combined with the large degree of variation, suggest that great caution is needed in ascribing high or low stature to any race in our area.

Considerable evidence has been brought forward to suggest that the Upper Facial Index is unreliable. If we accept it provisionally we find that it is a factor which, while agreeing to a large extent in showing the same degree of 'ethnic stability' as the cephalic index, in some cases shows wide divergencies; for instance, the cephalic index of the Cretans is most allied to

that of the modern Egyptians, whereas the index of Cyprus is the upper facial index which most closely approximates to that of the modern Egyptian. The standard deviations again suggest that there may be a mixture of race.

In dealing with pigmentation there are two points of special importance. First, in Albania and Cyprus about one man in ten has blue eyes, and even in dark Crete one man in twenty; and secondly, there is evidence to show that pigmentation distinguishes the Western Alpines from their Eastern congeners the Armenoids. Pigmentation does not appear to bear a definite correlation to cephalic index.

Summing up the evidence, we may say that at both boundaries of the Greek world there are two racial types of comparative homogeneity, and that those intermediate peoples who present local divergencies are very variable.

As far as our present evidence goes, the division into numerous local types would appear to serve no useful purpose. We have not at present sufficient information to discuss the physical anthropology of Greece proper; such as we have would appear to justify the assertion that the numerous small communities of the ancient Mediterranean differed physically; that is to say, that there was a physical background to the struggles between Amathus and Salamis, Athens and Sparta. To suppose that it is possible to establish a Greek type and to distinguish between Hellene and Barbarian does not appear justifiable.

It has been suggested that the Nordic race has contributed to the population of Greek lands, but the presence of fair Alpines would account for the blue-eyed people of ancient Greece.

In conclusion, then, while admitting the presence of numerous minor differences sufficiently great to make it necessary to know the exact provenance of any anthropological data we may wish to examine, it would not seem possible at present to assign any definite racial position to the Greeks, but rather to class them as representing a combination, probably early in date, of Alpines and Mediterranean stocks, both of which are found sporadically in a comparatively unmixed state in some parts of the Greek world.

14. Mr. S. C. CASSON.—*Excavations of the British School at Athens at Mycenae,* 1920.

In the area known as the Grave Circle on the Acropolis traces of early Bronze Age and Neolithic cultures were discovered. It seems certain that there was a continuous mainland civilisation stretching back to the beginning of the second millennium B.C. It has been possible to classify chronologically the works of the different generations of dynasts, and it seems that all the greater and more impressive monuments of Mycenae belong to the latest phase of Mycenaean culture.

15. Mr. J. WHITAKER.—*Excavations at Motya, N.-W. Sicily.*

16. Mr. P. E. NEWBERRY.—*Some early connections between Egypt, Syria, and Babylonia.*

Afternoon.

17. Prof. W. M. FLINDERS PETRIE, F.R.S.—*Recent Work in Egypt.*

18. Mr. R. CAMPBELL THOMPSON.—*The Earliest Inhabitants of Babylonia.*

Friday, August 27.

19. Prof. H. J. FLEURE.—*The Scheme of the Welsh Department of the Board of Education for the collection of Rural Lore through the agency of Schools.*

20. Mr. H. KIDNER.—*Round Barrows in the New Forest that do not conform to either of the three generally recognised types.*

21. Mr. G. A. GARFITT.—*Rock Sculptures from Eyam Moor Stone Circle, Derbyshire.*

22. Mr. D. MacRITCHIE.—*Greenland in Europe.*

Afternoon.

23. Dr. LLOYD WILLIAMS.—*Welsh Traditional Music.*

24. Dr. H. WALFORD DAVIES.—*Euphony and Folk Music.*

SECTION I.—PHYSIOLOGY.

(For references to the publication elsewhere of communications entered in the following list of transactions, see p. 382.)

Tuesday, August 24.

1. Presidential Address by Mr. J. BARCROFT, C.B.E., F.R.S. (See p. 152.)

Joint Meeting with Subsection I.

2. Dr. C. S. MYERS, F.R.S.—*The Independence of Psychology.*

3. Miss MAY SMITH and Dr. W. McDOUGALL, F.R.S.—*The Mental Effects of Alcohol and other Drugs.*

Afternoon.

4. Dr. W. H. R. RIVERS, F.R.S.—*An Independent Section of Psychology* (Discussion).

Wednesday, August 25.

5. Visit to the New Laboratory of Physiology, Newport Road.

6. Dr. T. LEWIS, F.R.S.—*Auricular Flutter*

7. Prof. A. D. WALLER, F.R.S.—*Plant Electricity.*

8. Prof. A. D. WALLER, F.R.S.—*Emotive Response of the Human Subject* (Lantern Demonstration).

Thursday, August 26.

9. Dr. T. LEWIS, F.R.S.—*The Relation of Physiology to Medicine.*

10. Prof. P. T. HERRING.—*The Effect of Pregnancy on the Organs of the White Rat.*

11. Miss E. BEDALE (in collaboration with others).—*Report on Caloric Value of the Ordinary School Meals and on the Energy Output of the School Day for Ages 10 to 18 Years.*

12. Miss HILDA WALKER, Prof. A. R. LING, and Mr. E. A. COOPER.—*On the Estimation of Sugar in Blood* (preliminary communication).

The authors have investigated methods of estimating sugar in blood, and consider that of Maclean to be convenient and accurate. They have improved its technique somewhat. As regards the nature of the cupric-reducing substance present in normal blood, they show that it is neither creatinine nor uric acid; that it is insoluble in ether; that its reducing power is destroyed by boiling with ammonia; that it is completely dialysable, and when dialysed against aqueous glucose solutions, equilibrium is adjusted as it should be with glucose; it forms a crystalline osazone morphologically similar to glucosazone. The authors conclude, therefore, that the reducing substance is glucose.

13. Dr. F. W. EDRIDGE GREEN.—*The Prevention of Myopia.*

The direct exciting cause of myopia appears to be increase of intra-ocular tension through back pressure on the eye, therefore lengthening its antero-posterior diameter. There is no satisfactory evidence that the use of the eyes for near work either increases or causes myopia.

In the prevention of myopia any cause which will increase the intra-ocular tension by obstructing the outflow should be avoided. All exercises in children which involve strain with the eyes pointing downwards should be avoided. A typical example of this is the exercise in which a child moves itself up and down from the floor with the eyes pointing downwards.

Physical training is of great importance, and it will be noticed that the ordinary forms of exercise—cricket, football, golf, &c.—do not cause myopia, whereas lifting heavy weights, dumbbells, wrestling, boxing, or riding a bicycle uphill in a stooping position, do. It is particularly in those who have sedentary occupations, and who are not in a fit physical condition, and have an hereditary tendency to myopia, that these forms of exercise should be avoided. Exercises which after inquiry are found to cause a feeling of pain or tension in the eyes or distension of the veins of the neck should be strictly forbidden to myopes. Reading in a recumbent or stooping position with the eyes pointing downwards is not advisable.

Afternoon.

14. Visit to Cardiff City Mental Hospital at Whitchurch. Dr. R. V. STANFORD demonstrated various Biochemical Methods.

Friday, August 27.

15. Prof. A. D. WALLER, F.R.S.—*The Energy of the Human Machine as Measured by the Output of Carbon Dioxide.*

16. Demonstration by Prof. J. B. HAYCRAFT.—*A New Electrokymograph.*

17. Joint Discussion with Section K on *Biochemistry and Systematic Relationship.*

SUBSECTION I.—PSYCHOLOGY.

(For references to the publication elsewhere of communications entered in the following list of transactions, see p. 382.)

Tuesday, August 24.

Joint Meeting with Section I.

Wednesday, August 25.

1. Miss L. C. FILDES.—*Word-blindness in the Mentally Defective.*

2. Miss V. HAZLITT.—*Conditions of Learning compared in Man and Rats.*

3. Prof. C. LLOYD MORGAN, F.R.S.—*The Territory Instinct in Birds.*

4. Mr. F. B. KIRKMAN.—*The Experimental Study of Animals in the Wild State.*

Afternoon.

5. Dr. E. PRIDEAUX.—*A Psychologist's Attitude towards Telepathy.*

6. Prof. AGNES ROGERS.—*Mental Tests in American Universities.*

Thursday, August 26.

7. Joint Meeting with Section L. (See p. 378.)

Afternoon.

8. Dr. W. H. R. RIVERS, F.R.S.—*The Complex and the Sentiment.*

Discussion, opened by Miss SAXBY.

9. Mr. F. C. BARTLETT.—*The Function of Images.*

Friday, August 27.

10. Mr. F. WATTS.—*Some Problems of Vocational Selection.*

11. Mr. S. WYATT.—*The Psychology of Industrial Life: Observations on Operatives in the Cotton Industry.*

12. Prof. E. L. COLLIS.—*The Psychology of Industrial Convalescence.*

Afternoon.

13. Mr. HENRY BINNS.—*Psychological Skill in the Wool Industry.*

14. Prof. F. S. LEE.—*Some Phenomena of Industrial Fatigue.*

15. Dr. G. H. MILES.—*Aims and Work of a National Institute of Applied Psychology.*

16. Dr. H. M. VERNON.—*The Influence of Adaptation after Altered Hours of Work.*

SECTION K.—BOTANY.

For references to the publication elsewhere of communications entered in the following list of transactions, see p. 382.)

Tuesday, August 24.

1. Presidential Address by Miss E. R. SAUNDERS. (See p. 169.)

2. Professor LLOYD WILLIAMS.—*Alternation of Generations in the Laminariaceæ.*

3. Mrs. ELEANOR M. REID.—*The History of the West European Pliocene Flora as Deciphered by the Study of Fossil Seeds.*

Afternoon.

4. Prof. R. CHODAT.—*Some Aspects of Plant Ecology and Biology in Paraguay.*

5. Prof. F. J. LEWIS and Miss GWYNNETH M. TUTTLE.— *On the Phenomena attending Seasonal Conversion of Reserve Food Materials in the Leaves of* Picea canadensis.

6. Miss GWYNNETH M. TUTTLE.—*On the Nature of Reserve Food Materials in the Tissues of some Plants of Northern Alberta.*

Wednesday, August 25.

7. *Joint Discussion with Sections C and D on Mendelism and Palæontology: The Factorial Interpretation of Gradual Changes, especially when New Characters appear late in the Individual Life-cycle.*

(See programme of Section C, p. 354.)

8. Dr. E. A. NEWELL ARBER.—*The Leaves of the Irids and the Phyllode Theory.*

Afternoon.

9. Dr. HAROLD WAGER. *Geotropism of Foliage Leaves; Geotropic and Nastic Growth; Localisation and Differentiation of Geotropic Stimulus. Are Leaves dia-geotropic?*

10. Prof. J. SMALL and Miss W. REA.—*Further Evidence for the Differentiation in Hydrion Concentration in stem and root as the explanation of Positive and Negative Geotropism, with evidence for Carbon-Dioxide Balance as the cause of that Differentiation.*

11. Miss K. B. BLACKBURN.—*Anomalies in Microspore Formation in 'Rosa' and its possible connection with Hybridity in the Genus, including a description of normal meiosis in three species for comparison with abnormal features found in ten forms, including two hybrids.*

Excursion.

A short Botanical Expedition took place to visit local plants of considerable interest.

Thursday, August 26.

Joint Discussion with Section M on *Soil and Plant Survey Work*.

Botanical part of the discussion centred round :

(i.) Types of grassland in view of importance in Wales and in general in agriculture—the possibility of recognising a number of types in different parts of the country.

(ii.) Possibility of fixing on some standard symbols or colours for ecological mapping.

(iii.) Relation of ecological data to geological data.

(iv.) Study and representation of arable.

12.   Mr. G. W. ROBINSON.—*Soil Types of North Wales*.

It is suggested that while uniformity in sampling and analytical methods should be secured, the classification of soils must depend on the local conditions. In extreme humid conditions it would appear that differences due to geological factors tend to be obliterated. Large numbers of soil samples should be collected and the types should be worked out from actual observations, correlation with geology may follow afterwards. The soil survey gives information as to *one* of the factors affecting plant growth and, ultimately, agriculture in a particular area. The survey in its widest sense should take cognisance of all the other factors, including climate and soil-water conditions. The vegetation survey gives the results of the operation of all these factors.

13.   Mr. E. A. FISHER.—*Soil Acidity*.

14.   Prof. R. G. STAPLEDON.—*Surveys of Grassland Districts*.

Re (i.) A method of obtaining quantitative data ; subdivision of grassland ; Fescue Agrostis pastures in detail. Effect of grazing animals.

Re (ii.) Colour scheme must allow for transitions ; subtypes by ink symbols on a ground colour.

Re (iv.) Use of weed flora in representation of arable.

Importance of primary survey.

15.   Miss W. H. WORTHAM.—*The Vegetation of Anglesey and N. Carnarvonshire, with special reference to the Grasslands*.

Until about 700 years ago the vegetation of Anglesey and N. Carnarvonshire comprised :

(1) an area of moorland stretching up from the 1000-1700 contours over the Carnarvonshire mountains, interrupted only by the associations of the summits, rock-ledges and screes ;

(2) A zone of woodland extending from the edge of the moorland to sea-level, interrupted by marshes and by lowland moors. The plant formations are closely related to the geological structure of the district. The greater part of the uncultivated area is now grassland and may be summarised thus : (i) the sub-alpine grassland which has been derived from upland moor. (ii) The siliceous, schistose, and calcareous grasslands which have been derived from the corresponding woodlands. (iii) Molinia grassland formed (*a*) as a product of the degeneration of woodland, or (*b*) as a primary association developed on wet rocks ;

(3) Grassland, siliceous, schistose, or calcareous, according to the nature of the soil, has also been formed by the degeneration of lowland moor, the draining of the marshes, the colonisation of screes, and the colonisation of sand-dunes

16. *Discussion.*—Sir A. D. HALL, K.C.B., F.R.S., Dr. E. J. RUSSELL, F.R.S., Mr. C. G. T. MORISON, Mr. T. J. JENKIN, Mr. C. T. GIMMINGHAM, Mr. R. ALUN ROBERTS, Dr. E. N. THOMAS, Prof. LLOYD WILLIAMS, Mr. T. W. FAGAN.

In the Section Room.

17. Mr. D. PATTON.—*The Vegetation of Beinn Laoigh situated on the West of the Breadalbane Mountains. Its Configuration, Geological Formation, Climatic Conditions, &c., and the Relation of these to the Conditions of Plant Life, with special reference to the Associations.*

Excursion.

Botanical expedition to Mynydd y Glew and Wenvoe, *via* Cowbridge. Lunch was provided by the kind invitation of Mr. and Mrs. T. Mansel Franklen, and tea at Duffryn by Mr. and Miss Cory.

Friday, August 27.

18. Prof. F. J. LEWIS.—*Distribution of Vegetation types in the Eastern Canadian Rocky Mountains* (Lantern Demonstration).

19. Mr. P. E. MARTINEAU.—*Records of Growth of Pit Mound Plantations.*

Joint Discussion with Sections I and B on *Biochemistry and Systematic Relationship*, opened by

20. The Hon. Mrs. ONSLOW.—Introductory paper on *Biochemistry and systematic relationship in the plant kingdom.*

> Brief consideration of systematic relationship.—Biochemical aspect of the plant.—Possibility of expression of reproductive and vegetative characters in chemical terms.—Lines of plant metabolism.—Aromatics.—Catechol and pyrogallol plants.—Oxidase and peroxidase plants. Suggestions as to connection with relationships of other substances—*e.g.,* anthocyan pigments, flavones, &c.

21. Dr. F. F. BLACKMAN, F.R.S.—*Photosynthesis and carbohydrate metabolism from the point of view of systematic relationship in plants.*

> The pigments of chloroplasts : their uniformity in phanerogams and their diversity in certain phyla of algæ.
> The primary products of photo-reduction of CO_2 : the balance of hexoses and pentoses in different groups : pentoses as the basis of the succulent habit.
> Diversity of condensation-products : saccharose, starch, and inulin, their occurrence in relation to species and families.
> Riechert's work on the individuality of the starch grains of every species of plant : its bearing on the chemical specificity of protoplasms.

22. Prof. G. H. Nuttall, F.R.S. — *Precipitive Reactions as a means of determining Systematic Relationships in Animals and Plants.*

23. Mr. J. Barcroft, F.R.S.—*Correlation of properties of the oxygen-carrying power of blood (essentially the properties of hæmoglobin) with the functions and habits of the animal in question rather than with its phylogeny.*

Afternoon.

24. Prof. C. J. Chamberlain.—Semi-popular lecture on *The Origin and Relationships of the Cycads.*

25. Mr. Kingdon Ward.—*On the Distribution of Floras in S.-E. Asia as affected by the Burma-Yunnan Ranges.*

26. Sir J. C. Bose, F.R.S.—*Plant Autographs and their revelation, with demonstration of growth by means of the Magnetic Crescograph.*

Exhibition.

There was an exhibition during the Meeting of microscope preparations, drawings, specimens, maps, &c. (many illustrative of the papers).

SECTION L.—EDUCATION.

(For references to the publication elsewhere of communications entered in the following list of transactions, see p. 383.)

Tuesday, August 24.

1. Presidential Address by Sir Robert Blair.   (See p. 191.)

2. Report of the Committee upon Training in Citizenship. (See p. 281.)   Speakers: Bishop Welldon, Mr. J. Clarke, Mr. A. Patterson.

Afternoon.

3. Mr. Spurley Hey.—*The Supply of Teachers.*

There is a serious shortage in the supply of teachers. The actual supply is insufficient to repair wastage, makes little contribution towards increase in quality of staff, and makes no increase whatever towards reduction of classes and other necessary reforms, or towards additional requirements arising under the Education Act, 1918.   Boards of Education policy has led to a decrease in supply; some L.E.A.'s have done nothing, whilst most L.E.A.'s have failed to supply their own wastage; the teaching profession has for some years been often indifferent, sometimes hostile, to the creation of an adequate supply.   The Board of Education should provide adequate money and should penalise defaulting L.E.A.'s.   L.E.A.'s must provide good scales of salaries and better school conditions, and must utilise the wider avenues now allowed by the Board of Education.   Teachers

should actively co-operate in creating an adequate supply.
Rural areas must be more thoroughly tapped for young
recruits, educated manhood and womanhood for adult recruits.

Wednesday, August 25.

(4-7).   Papers on *The Relation of Schools to Life*, as follow-
ing :—

4.   Mr. A. LINECAR.

Schools should induce capacity for life as well as, or
before inducing, ability to earn a livelihood.   A general
governing principle guiding all the school work is wanted,
as ' Induce into pupils the power of concentration of mind.'
Then multiplicity of subjects disappears; we have instead
various postures of one endeavour.   The broader the curri-
culum the broader the culture.   We shall find we can vary
our postures so as to cultivate capacity to appreciate art,
literature, beauty, nature, nobility; we shall no longer find
ourselves struggling to attain sectional high efficiency.   We
shall give power to be happy, the greatest gift; we shall give
possibility of broad, tolerant, healthy and full mental life :
we shall find that unusual efficiency has come, unsought, so
that while giving a splendid chance of real life we have also
provided an added probability of livelihood.

5.   Mr. J. M. McTAVISH.

(1) Primary function of schools to assist in cultivating
such practical emotional and intellectual habits, systemati-
cally organised as will fit man to his social and physical
world.   (2) Necessity of re-interpreting in terms of man's
widening social relations individualistic conceptions of edu-
cation which hinder us from understanding the importance
of their social function.   (3) Education as being the develop-
ment of the physical and mental capacity too limited.   It
dissociates education from social change.   (4) Each historic
epoch carries with it corresponding changes in educational
motives and methods.   (5) During periods of social stability
this creates no problems, but man's social world is to-day in
a condition of rapid flux.   (6) The world's problems due to
conscious social conduct being primarily determined by
sentiments which determine the behaviour of the conscious
stream and their influence upon consciousness may be
regarded as the psychological analogue of the conception of
force in physics.   (7) The disintegrating sentiments in civili-
sation are egoism, patriotism, and class sentiment.   (8) The
need for cultivating a human sentiment powerful enough
to hold these in check.   (9) The relation of the above con-
siderations to secondary education.   (10) Schools most closely
related to life through adolescent education.

6.   Mr. R. O. BRAY.

Paper concerned with industrial aspect of life.   Need for
industry to regard the entrants as persons in training.   Ten-
dency of industry to regard them as adults.   No adequate
provision made for trade teaching, for physical welfare, or
for general training.   Figures justifying statement.   Work-
shop training neglected both by employers' associations and
trade unions.   Tendency to regard the schools as offering a
substitute for training in workshop.   The workshop, the
stronger influence, undermines the training in the schools.
The most urgent educational problem lies not inside the

schools but in industrial life outside. The solution must be found by industry, trade unions, and employers, regarding the juvenile worker as a person in training. The double duty of the schools : first to educate industry itself by securing a change of attitude towards its method of regarding the juvenile worker. Secondly to assist industry in the selection and training of its entrants. The first the most important and the most neglected of the tasks. The schools must prepare industry for the child before it can prepare the child for industry.

7. Miss STRUDWICK.
General Discussion.

8. Joint Meeting with Section E.   (See p. 361.)
Prof. J. L. MYRES.—*The place of Geography in a Reformed Classical Course.*

> Recent decisions about 'compulsory Greek' compel drastic revision of classical teaching. With language courses restricted and postponed, the aim must be earlier acquaintance with ancient conduct and thought, through closer coordination between history, literature, and geography. The Mediterranean region being exceptionally suited to supplement, by contrast, Homeland notions of geography, and being also the physical cradle of those ancient cultures, Hebrew and Græco-Roman, which have most influenced our own : reformed 'classical' education would begin by illustrating, through ancient narrative and description, in translations, man's behaviour under these conditions, both normally and in great crises; and his solutions of social and moral problems in ancient times compared with ours. Later, these episodes would be linked, chronologically and topographically, to illustrate historical growth and interaction between local types. But study of 'special periods' would be reserved till these outlines were familiar, and ancient languages until required for appreciation of literature.

Afternoon.

9. Dr. VINCENT NASER.—*The International Intellectual Relations.*

> Wealth of a nation, a function of its directing energies. All students to organise locally, creating committees at each University representing both undergraduates and postgraduates, and having complete modern office equipment at their disposition. These committees to take the initiative of establishing local bureaux of information under the supervision of University authorities, and acting as local branches to central State-authorised bureau of all the nation's Universities.   Local committees to elect National Council of Students. Necessity of caring for undergraduates travelling abroad by established bureaux of information, and creating facilities for introduction to families, etc.   Tendency at modern Universities to specialise in certain post-graduate specialities to be encouraged to avoid overlapping. Idea of World's University to be realised locally by the internationalisation of post-graduate education with lectures in English and French being prepared by the teaching of English and French in all schools.   Introduction of official students, international identity cards with photo of bearer

affixed, etc. Railroad and shipping companies to provide special rates, after being shown the necessity for the keeping up of the standard of instruction and life generally of intellectual classes.

Thursday, August 26.

Joint Meeting with Subsection of Psychology.

10. Prof. T. P. NUNN.—*The Tendency towards Individual Education.*

Education must effect a *modus vivendi* between two principles : (i.) the principle of 'mental discipline,' and (ii.) the principle of spontaneity which requires the pupil to be his own educator. The second implies that the individual pupil, not the class, is the proper unit for instruction, and that he should be free to go his own way at his own speed. The former demands expert control of his studies and at least a minimum prescribed curriculum. The problem of reconciling the two principles in the case of young children is relatively simple, and has been solved (*e.g.*) by Dr. Montessori. For older pupils it seems to require (*a*) the reduction of formal class-teaching to a minimum, and (*b*) setting free a large part of the school week for 'elective' work under tutorial supervision. Cautious experiments in this direction are immediately practicable, and should be encouraged.

11. Prof. G. H. THOMSON.—*Do the Binet-Simon Tests measure General Ability ?*

12. Dr. C. W. KIMMINS.—*The Dreams of Children who are Physically Abnormal.*

General Discussion upon the above Papers.

Afternoon.

13. Report of Committee upon the Educational Value of Museums. (See p. 267.)

Friday, August 27.

14. Right Hon. H. A. L. FISHER.—*The Universities in a National System of Education.*

15. Mr. FRANK FLETCHER.—*The Public Schools in a National System of Education.*

16. Miss H. M. WODEHOUSE.—*The Training Colleges in a National System of Education.*

Do we aim at requiring that all teachers, including all those teaching ages 2-11, shall be graduates? (Estimate of numbers.) *If not*, training colleges other than university departments ought still to exist : (i.) To take non-matriculated students, (ii.) to take students who, even though qualified for matriculation, do not wish to take a full-length university course. From staff, from learning, and from social life, these students gain more in a college of their own than in a university. Most desirable that training colleges should be widened by amalgamating the training of teachers with other work. *E.g.*, physical training, arts, crafts, agriculture, engineering, preparation for business or secretarial

work, for social service, &c., &c. Question of connection with universities (absorption in universities being rejected). (a) Advantage to staff; (b) should improve some governing bodies. The university should *not* act as external examiner to the college. Services apart from official connection.

17. Mr. J. C. MAXWELL GARNETT, C.B.E.—*Higher Technical Schools in a National System of Education.*

The paper included a description of his diagram of a national system of education. The diagram illustrated certain recommendations recently published by the Federal Council of Lancashire and Cheshire Teachers' Associations. The diagram represented sixteen different types of education; nine different types of school or college; a system of scholarships and maintenance allowances sufficient to secure that every kind of education is brought within the reach of all young people of sufficient educational promise; and the numbers of young people who should be attending any particular type of school or receiving any particular type of education at any given age. As indicated by its title, the paper was especially concerned with the provision of the highest technological education in the national system of education represented in the diagram.

A *General Discussion* followed.

Afternoon.

Excursion to Barry Summer School.

SECTION M.—AGRICULTURE.

(For references to the publication elsewhere of communications entered in the following list of transactions, see p. 383.)

Tuesday, August 24.

1. Presidential Address by Prof. F. KEEBLE, C.B.E., F.R.S., on *Intensive Cultivation.* (See p. 200.)

2. Mr. H. V. TAYLOR (Ministry of Agriculture).—*The Distribution of Wart Disease in Potatoes; (b) Some Results of the Ormskirk Potato Trials.*

3. Mr. F. J. CHITTENDEN.—*Experimental Error in Potato Trials.*

4. Mr. T. WHITEHEAD.—*A Preliminary Report on the Parasitic Fungi of North Wales.*

5. Mr. C. L. WALTON.—*Agricultural Zoology of North Wales.*

Wednesday, August 25.

6. Captain R. WELLINGTON (Ministry of Agriculture).— *Orchard Survey of West of England.*

7. Mr. R. G. HATTON.—*Fruit Tree Stocks.*

8. Mr. S. P. WILTSHIRE.—*Methods of Infection of Apple Trees by Nectria ditissima, Tul.*

9.　Prof. T. WIBBERLEY.—*Experiments in Intensive Corn-growing.*

10.　Prof. A. HENRY.—*The Artificial Production of Vigorous Trees.*

Afternoon.

Excursion to St. Fagans.

Thursday, August 26.

11.　*Joint Discussion* with Section K (see p. 373) on *Plant and Soil Survey Work.*

Friday, August 27.

12.　Mr. GEORGE S. ROBERTSON.—*Result of Experiments with Rock Phosphates.*

13.　Captain H. J. PAGE, M.B.E.—*Experiments on Green Manuring of Light Soils.*

14.　Mr. S. HOARE COLLINS.—*Sugar Content of Straw.*

15.　Mr. C. B. V. MARQUAND.—*The Varieties of Oats.*

REFERENCES TO PUBLICATION OF COMMUNICATIONS TO THE SECTIONS.

AND OTHER REFERENCES SUPPLIED BY AUTHORS.

Under each section, the index-numbers correspond with those of the papers in the sectional programmes (pp. 351–380).

References indicated by ' cf.' are to appropriate works quoted by the authors of papers, not to the papers themselves.

SECTION A.

2. Cf. *Bulletins* Kodaikanal Observatory, 1913–18 ; also *Observatory*, Apr. 1920.

4. To be published in *Proc. London Math. Soc.*

6. To be published in *Messenger of Mathematics ;* cf. recent papers, *ibid.*, on Pentaspherical Co-ordinates.

7. Cf. *Phil. Mag.*, **39**, p. 707, Dec. 1919 ; **40**, p. 451, Apr. 1920 ; p. 611, May 1920.

9. Expected to be published in *Phil. Mag.*, Nov. 1920.

10. Cf. J. H. Moore, ' Recent Spectrographic Observations of Nova Aquilæ III.,' *Astronom. Soc. of Pacific*, **32**, p. 232.

11. *Observatory*, **43**, No. 558, Nov. 1920.

14. To be published in *Nature.*

17. Cf. *Proc. Roy. Soc.*, **95**A ; *Camb. Phil. Trans.*, Oct. 1919 ; also forthcoming *Encyclopædia of Physics*, ed. Sir R. T. Glazebrook (Macmillan), s.v. *Terrestrial Magnetism.*

18. To be published in *Proc. Roy. Soc. ;* cf. *Phil. Trans.*, **214**A, p. 109 (1914) ; **215**A, p. 79 (1915) ; **220**A, p. 247 (1920) ; *Proc. Roy. Soc.*, **95**A, p. 58 (1918) ; *Science Progress*, Mar. 1920.

SECTION B.

2. *Journ. Soc. Chem. Ind.*, Sept. 15, 1920.

8. Results to be published in 6th Rep. Advisory Committee on Atmospheric Pollution.

SECTION C.

2. See *Handbook to Cardiff* (B.A. Meeting, 1920); also *Proc. Geol. Assoc.*, **31**, p. 45 (1920).

3. Expected to be published in *Mineralogical Magazine.*

10. To be published (abbreviated) in *Geol. Mag.*

12. Cf. ' The Arrangement of Atoms in Crystals,' *Phil. Mag.*, **40**, Aug. 1920 ; ' Crystal Structure,' *Royal Inst.*, May 28, 1920.

Section D.

8. For summary of discussion, see *Nature*, **106**, p. 30.

10. *Nature*, **105**, p. 516.

11. *Nature*, **105**, p. 197.

16. *Journ. Exp. Zool.*, **30**, p. 1.

Section E.

2. To be published in *Yearbook Welsh Housing and Development Assoc.*, 1921.

3. Cf. *Geog. Journ.*, 1919, p. 410.

8. Volume, *The Sites of Imperial Cities*, in preparation.

10. Cf. Fishery Investigations *Reports* (new series), III. *Hydrography*, **1** ; *Hydrography of the English Channel*, pt. 1 (Start Point to Channel Is.), 1904–17 ; pt. 2 (Isle of Wight to Cape de la Hague), 1904–18 ; pt. 3 (Isle of Wight to Havre), 1904–18 ; pt. 4 (Newhaven to Caen), 1903–12 ; pt. 5 (Plymouth to Brest), 1907–11. **2.** *Hydrographical Observations at English Lightships*, pt. 1 (Seven Stones Lightship). **3.** *Hydrography of the Atlantic Ocean*, pt. 1 (Area centred on 50° N., 20° W.) **4.** *Hydrography of the North Sea*, pt. 1 (Section from R. Tyne towards Naze of Norway).

11. Cf. *The Kalahari*, by E. H. L. Schwarz (Blackwell, Oxford, and Miller, Cape Town), 1920.

12. Cf. *Builder*, **94.** (1908, i.), pp. 37, 64, 89, 111, 121, 142, 153, 174, 184, 203, 234; *Neue Jahrbücher für das Klassische Altertum*, **23** (1909), pp. 246 -260 ; *Public Works*, **2**, iii. (Jan. 15, 1904), pp. 193–201 ; also ' Livellazione degli Antichi Acquedotti Romani, memoria del Prof. V. Reina, degli Ing. G. Corbellini e G. Ducci,' in *Memorie Soc. Ital. d. Scienze detta dei XL.*, series 3a, **20**.

Section F.

1. *Ways and Means*, Oct. 16, 1920, p. 62.

2. *Better Business* (Co-op. Ref. Library), Nov. 1920. Cf. *Rural Reconstruction in Ireland* (P. S. King & Son), *Co-operation for Farmers* (Williams & Norgate).

5. *Cardiff Journal of Commerce*, Aug. 26, 1920; *Colliery Guardian*, Aug. 27, 1920.

7. *Bankers' Magazine*, Oct. 1920.

9. *Economic Journ.*, Dec. 1920.

Section G.

2. *Engineering*, Aug. 27, 1920, p. 293.

3. ,, Aug. 27, 1920, p. 292.

4. *Farm Life*, Sept. 4, 1920.

5. *Engineering*, Sept. 3, 10, 1920, pp. 325, 361.

6. ,, Aug. 27, 1920, p. 276.

7. ,, Sept. 3, 1920, p. 314.

8. ,, Aug. 27, 1920, p. 279 ; *The Electrician*, Aug. 27, 1920.

9. ,, Sept. 3, 1920, p. 325.

11. ,, Sept. 3, 1920, p. 310.

12. ,, Sept. 3, 1920, p. 330.

13. ,, Sept. 3, 1920, p. 331.

14. *Radio Review*, Aug., Sept., Nov. 1920.

15. *The Engineer*, Aug. 27, 1920, p. 201.

Section H.

2. Cf. *Journ. Roy. Anthrop. Inst.*, **46** (1916), **48** (1918), **50** (1920).

3. Cf. (forthcoming) *Biometrika*, **13** (1921).

5. Cf. *Archæologia*, **57**, pp. 295–316 (1901); **58**, pp. 119–152 (1902), 391–406 (1903); **59**, pp. 87–124 (1904), 289–310 (1905) ; **60**, pp. 111–130 (1906), 451–464 (1907) ; **61**, pp. 565–582 (1909) ; **62**, pp. 1–20 (1910), 405–448 (1911) ; **63**, pp. 437–452 (1913).

7. Expected to be published in *Folk Lore*.

11. Cf. (forthcoming) R. Gardner, ' The Via Valeria,' in *Papers Brit. School a Rome*, **10**, and future articles by T. Ashby and R. Gardner, also (forthcoming) T. Ashby on an ancient Lucanian hill-fort in *Journ. Roman Studies*.

13. To be published in *Biometrika* ; also forthcoming article in *Nature*.

14. To be published in *Annual of the British School of Archaeology at Athens,* XXIV. ; see also *Times Literary Supplement,* June 24, 1920 (A. J. B. Wace), and July 15, 1920 (Sir Arthur Evans).

15. *Man,* Dec. 1920.

17. Results to be published in *Lahun II.,* Brit. Sch. in Egypt.

18. Cf. (forthcoming) *Semitic Mythology* (series, *Mythology of all Races,* Marshall Jones, Boston, U.S.A.) ; also ' The British Museum Excavations at Abu Shahrain,' in *Archæologia.*

20. *Proc. Hampshire Field Club and Archæological Soc.,* 1920.

21. To be published in *Man.*

22. Expected to be published (abbreviated) in *Discovery.* Cf. ' The Lapps in Scotland.' *The Link,* Sept. 1917 (Bellows, Gloucester) ; ' Kayaks of the North Sea.' *Scott. Geog. Mag.,* Mar. 1912 ; ' The Kayak in North-Western Europe.' *Journ. Roy. Anthrop. Ins.,* **42,** 1912 ; ' The Abudeen Kayak and its Congeners.' *Proc. Soc. Antiq. Scot.,* 1912 ; ' Les Kayaks dans le Nord de l'Europe,' *Compte Rendu XIVe Session (Genève,* 1912) *Cong. Internat. d'Anthrop. et d'Archéol. Préhist.,* **2,** 1914 ; ' Der Kajak im nördlichen Europa,' *Petermanns Mitt.,* 1911 ; ' Notes on a Finnish Boat preserved in Edinburgh,' *Proc. Soc. Antiq. Scot.,* 1890 ; ' The Testimony of Tradition ' (Kegan Paul, 1890).

24. Cf. *Musical Times,* Oct. and Nov. 1920.

SECTION I.

2. To be published in *Discovery.*

3. To be published in special reports, Medical Research Council. Cf. communications to Brit. Assoc., 1913 and 1915, published as ' A Contribution to the Study of Fatigue,' in *Brit. Journ. Psychology,* **8.**

9. *Brit. Med. Journ.* 2, pp. 439-462 (1920).

10. *Brit. Med. Journ.,* Dec. 11 (1920), p. 886.

SUBSECTION I (Psychology).

3. Cf. ' Instinctive Dispositions,' *Scientia,* Oct. 1920.

4. Cf. (forthcoming) *The Intimate Life of a Seagull* (T. C. & E. C. Jack, 1921) ; paper to be published probably in *Brit. Journ. Psychology.*

6. To be published in *Brit. Journ. Psychology.*

10. To be published in *Brit. Journ. Psychology,* **11,** pt. ii. (Jan., 1921).

11. Cf. forthcoming Reports of Industrial Fatigue Board.

12. To be published in *Engineering and Industrial Management.*

13. *Wool Record and Textile World,* Aug. 26 and Sept. 2, 1920 ; Cf. ' The Human Factor in the Judgment of Yarn and Cloth,' to be published in *Journ. Bradford Textile Soc.* and *Wool Record.*

14. Cf. ' Studies in Industrial Physiology : I. Comparison of an Eight-Hour Plant and a Ten-Hour Plant : Report by Josephine Goldmark and Mary D. Hopkins on an Investigation by P. S. Florence and Associates under the general direction of Frederic S. Lee,' *Public Health Bulletin,* No. 106, U.S. Public Health Service, Washington.

16. *Report* No. 6, Industrial Fatigue Research Board, 1920.

SECTION K.

3. Cf. Paper to be published by Geological Soc.

4. Cf. R. Chodat and W. Vischer, ' Végétation du Paraguay,' in *Bull. Soc. Botan. Genève* (1916-20).

8. Abstract to be published in *Journ. Indian Botany,* and it is hoped that the full paper will appear in *Annals of Botany.*

10. Summary in *New Phytologist,* **19,** p. 208 (1920) ; cf. ' A Theory of Geotropism,' *ibid.* **19,** p. 49 (1920).

11. Results to be published (with Dr. J. W. H. Harrison) in *Annals of Botany.*

12. Cf. ' *A Survey of the Soils and Agriculture of Shropshire,*' published by Salop County Council, 1913 ; ' Studies on the Palæozoic Soils of N. Wales ' in *Journ. Agric. Science,* **8,** 3 ; ' Further Studies on the Soils of N. Wales ' (in collaboration with C. F. Hill), *ibid.,* **9,** 3 ; Dr. Edward Greenly in *Geol. Survey Memoir* on Anglesey, pp. 877–881.

13. To be published in *Journ. Agric. Science.*

19. Cf. Communication to Brit. Assoc., Newcastle, 1916 ; also annual *Reports* Midland Reafforesting Assoc., 1905–19.

20. Cf. (forthcoming) *Biochemical Journ.* ; also ' Nature of the Peroxide naturally associated with certain direct oxidising Systems in Plants,' in *Biochemical Journ.*, **13**, p. 1 (1919).

21. To be published in *New Phytologist*, Jan. 1921.

24. Cf. C. J. Chamberlain, *The Living Cycads* (Univ. of Chicago Press) ; *Botanical Gazette*, passim.

SECTION L.

4. *Education*, Sept. 24, 1920.

5. *Highway*, Oct. and Nov., 1920 ; *Education*, Oct. 1, 1920.

6. Cf. R. O. Bray, *Boy Labour and Apprenticeship* (Constable).

7. *Journ. of Education*, Oct. 1920.

11. Cf. ' General *versus* Group Factors in Mental Activities,' in *Psychological Review*, 1920.

14. *Education*, Sept. 3, 1920 ; *The Times Educational Supp.*, Sept. 2, 1920.

15. *Journ. of Education*, Oct. 1920.

16. To be published in *Education*. Cf. *Athenæum*, July 1917.

17. Cf. *An International System of Education* (Manchester University Press).

SECTION M.

2. *Journ.* Ministry of Agriculture and Fisheries, Nov.–Dec. 1920.

5. Cf. (for Mid-Wales). ' The Liver Rot of Sheep, and Bionomics of Limnæa truncatula in the Aberystwyth Area,' and ' Some Results of a Survey of the Agricultural Zoology of the Aberystwyth Area ' (both in) *Parasitology*, **10**, 2 (1917) ; ' Farm Insects Observed in the Aberystwyth Area, 1913–16,' and ' A Note on Agricultural Decology in Mid-Wales,' in *Annals of Applied Biology*, **4**, 1 and 2 (1917).

7. *Journ. of Pomology*, **5**, Nov. 1920 ; *Gardeners' Chronicle*, Oct. 16, 1920 ; cf. *Journ. R.H.S.*, **42**, p. 261 (1917), **44**, p. 89 (1919), **45**, pp. 267, 269 (1920) ; *Bulletin*, by R. G. Hutton, ' Summary of the Results in selecting and propagating Paradise Stocks.'

9. *Farm Life*, Oct. 2, 1920.

10. *Quarterly Journ. Forestry*, **14**, pp. 253–257 (Oct. 1920).

12. Cf. ' Solubility of Mineral Phosphates in Citric Acid,' pt. ii., *Journ. Soc. Chem. Industry* **35** (1916) ; Notes on the Nature of the Phosphates contained in Mineral Phosphates,' in *Journ. Agric. Science*, **8** ; ' Trials on Grass Land with Open-hearth, Basic Slag and Rock Phosphates,' in *Journ. Board of Agriculture*, **24** (1918).

13. Summary in *Gardeners' Chronicle*, Sept. 18, 1920, p. 139. Results expected to be published in *Journ. Roy. Horticultural Soc.*

EVENING DISCOURSES.

I.

Some Requirements of Modern Aircraft.
By Sir R. T. Glazebrook, K.C.B., F.R.S.

The differences between the requirements of military and of commercial flying are not merely due to the fact that the military flying machine has developed into a formidable weapon, whilst commercial aircraft must be a means of rapid transit and transport.

The lecturer quoted from a paper, recently communicated by Squadron Leader R. M. Hill, to characterise these differences : ' If commercial aeroplanes are to compete successfully with other forms of transport, they must compete on grounds of economy, speed, and reliability, but such achievement will not be of the slightest value until a standard of safety nearer to that reached by railways and shipping is attained. The most pressing difficulties seem to be those of flying to a place and landing when there is a mist down to the ground, of the comparative unreliability of the light aero-engine, of the space which any aeroplane requires to land in, and of the imperfect control of small aeroplanes at low speeds and of large ones at any speeds.'

The lift, the upward force on an aeroplane, is greatest for an approximately rectangular wing when the long side is horizontal and at right angles to the direction of flight, when the lift $W = k_1 \rho s v^2$, where ρ is the air density, s the area, v the speed of the machine, and k_1 the lift coefficient, depending both upon the section of the wing and on the angle between the bottom chord of the

Fig. 1. POSSIBLE LOADING OF WINGS & BODY OF HIGH SPEED AEROPLANE IN FLIGHT.
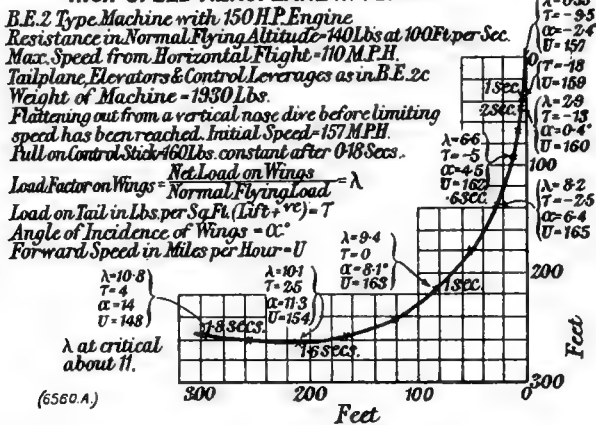
wing and the flight direction, *i.e.*, the angle of attack. As the angle of attack increases, and the wing is held more and more obliquely, the lift coefficient increases until the angle is about 15 deg. ; at this, known as the stalling angle, k_1 suddenly drops, and the wing ceases to support the weight it has borne. The actual values reached by k_1 before stalling differ appreciably ; 0·6 or 0·7 is about

the usual maximum, but 0·9 has been exceeded by Messrs. Handley Page and by the Blackburn Company.

The stalling speed, the minimum speed at which the machine can fly, is inversely proportional to the square root of the maximum lift coefficient, and since $W = k_1 \rho s v^2$, $W/s = k_1 \rho v^2$, this quotient expressing the 'loading,' i.e., the weight carried per unit wing area; the loading varies between 5 lb. and 10 lb. per sq. ft. The drag or resistance to the motion of the wing is equal to $k_2 \rho s v^2$, the drag coefficient k_2 also depending upon the shape of the wing section and the angle of attack. As that angle increases, k_2 decreases at first, then increases more and more as the angle grows steeper. But k_2 is always smaller than k_1, and the force D required to move the wing is given by $D = W k_2/k_1$. The resistance of the rest of the machine is about the same in amount as that of the wing. The resistance is entirely due to the friction

Fig. 2. POSSIBLE LOADING OF WINGS & BODY OF HIGH SPEED AEROPLANE IN FLIGHT.
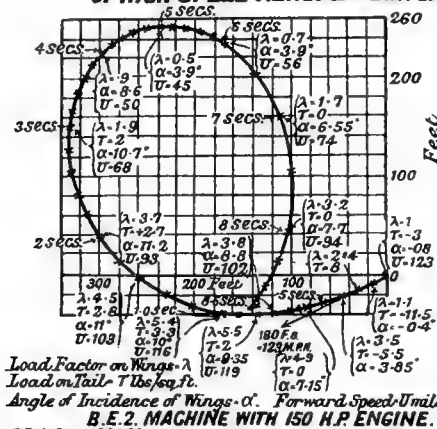
Load Factor on Wings - λ
Load on Tail - T lbs./sq. ft.
Angle of Incidence of Wings - α. Forward Speed - U miles/hr.
B.E.2. MACHINE WITH 150 H.P. ENGINE.
Weight - 1930 lbs.
Resistance in normal flying attitude - 340 lbs. at 100 f.s.
Max. speed for horizontal flight - 83 m.p.h.
Control leverages as in B.E. 2c.

Pull in lbs. on Control Stick during the above loop.

(6560 B.) Seconds.

between the moving body and the air, and to the viscosity of the air, the friction itself is due rather to the eddies, which owe their origin to the viscosity, than to the direct motion of the machine through the air.

As regards the engines, the weight per horse-power is smaller for radial air-cooled engines than for water-cooled motors; 3 lb. per horse-power is perhaps a reasonable figure. According to Bairstow, a light aeroplane capable of travelling at 125 miles p.h. would be able to carry a useful load of 270 lb. out of a total weight (machine, engine, and pilot) of 2,100 lb. The modern Bristol Pullman triplane for fourteen passengers, capable of a best speed of 130 m.p.h., weighs empty 11,000 lb., and fully loaded 18,000 lb., 4,375 lb. of that total being available for cargo or passengers. The engines are not yet reliable in the sense that marine engines are reliable. The useful load can be increased by making the structure light. A light machine can be flown at a lower angle of attack, but the structure must remain sufficiently strong, and the calculation of the stresses is not an easy matter, except when the machine is moving horizontally. In steep diving, at twice normal speed, the stresses might rise to sixteen times their magnitude for normal flight, if the pilot were able to change his controls instantaneously, and although that is impossible, the controls can be changed within one-fifth of a second. For these calculations the 'load factor' is

important, this factor indicates the ratio of the stress in any part of the structure under given conditions of flight to the stress under normal conditions of horizontal flight. The load factor during recovery from a dive can be determined in two ways. Starting with an aeroplane diving at a given speed we could calculate the initial stresses on the parts, and if we knew the aerodynamic forces, the values of k_1 and the resistances of the various parts, and were able to make some assumption as to the rapidity at which the pilot altered his controls, we could determine mathematically the rest of the path and the stresses on the machine at any point of that path. Fig. 1 shows the results of that calculation for a machine flattening out from a dive, and Fig. 2 the results from a machine doing a loop. In both cases the machines were moving at high speeds, and high load factors were required for safety. In the former case the pull on the control levers was 160 lb., reached after 0·18 second; in the latter case it was 100 lb., attained in about the same period.

The other method of determination relies upon the experimental measurement of the forces on the machine as a whole by the aid of accelerometers. On aeroplanes pendulums of heavy weights or springs of high inertia, which would go on swinging, would be useless. The accelerometer used took the form illustrated in Fig. 3. It consisted of a quartz fibre, 1/2,000th in. in diameter,

Fig.3. ACCELEROMETER
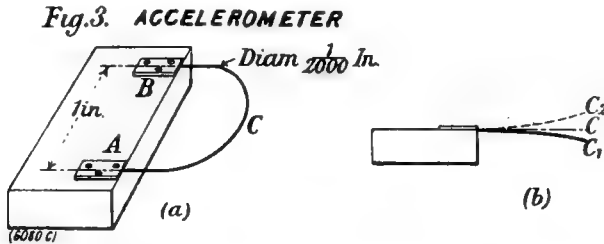
bent to an arc A B C, and held, normally, in a horizontal position. The fibre would be slightly deflected downwards by its weight, and would be still more deflected by a vertical acceleration, and this additional deflection of the point C to positions C_1 or C_2 was recorded photographically. The accelerometer camera and the lamp were contained in a small box strapped to the observer's knee. The curves thus obtained indicates only the accelerations of the centre of gravity, without taking account of the twists and turns about this centre. The kinematograph has also been utilised at the Royal Aircraft Establishment for the determination of the stresses. A kinematograph was fitted on to the tail of one machine rising steadily, while the second machine following close behind performed the loop spin, or other evolution, it was desired to analyse. Series of these photographs were exhibited.

A commercial machine should be stable, as it would be impossible, without a great expenditure of the pilot's energy, to fly through a continuous cloud. Yet stability pushed too far gave the machine a will of its own and made it sluggish, and military pilots, to whom rapid control meant life or death, differed as to the desirability of stability. On the other hand, the stable machine would fly on, if the pilot lost control, and, if the engine stopped, the machine glided down, while the unstable machine got into a spin and crashed down to earth. Stability may be automatic or inherent. Automatic stability, secured by contrivances which came into play when a deviation from the steady course occurred, is unsatisfactory because the device necessarily takes some time to operate, overshoots the mark, and hunts. An inherently stable machine would be brought back to the steady state by the disturbing wind forces. The lecturer indicated how, thanks mainly to the labours of Bryan and Bairstow, longitudinal stability, at any rate, had been successfully secured; our knowledge of the conditions for lateral stability is unfortunately far less complete. Longitudinal stability is important in connection with looping. It is known that an unstable aeroplane has a stable flying position on its back. If it got into that position

by some accident, it could only be righted again with the greatest difficulty, if at all.

The lecturer finally referred to the difficulties which the aviator, as distinct from the sailor, had with his navigational instruments. The aviator does not know whether or not he is moving uniformly. A bubble level indicator will only tell him that the acceleration, if any, is at right angles to the surface of the bubble. The compass is disturbed by the vibrations of the engine, to damp the effect of these the whole compass is encased in a closed vessel usually filled with alcohol. In the ordinary compass, the card carrying the magnets is fitted with a cup resting on a pivot. The vibrations make the card rotate and set the liquid in motion; to reduce these movements the vessel is made spherical, the pivot and cup are interchanged, the cup being attached to the support, and the card itself is replaced by a short length of a cylinder having the cup on its axis. Even thus improved the R.A.F. spherical compass, of which Fig. 4 gives a diagrammatic section, will, on an initially northern course, move to the east or west, only, say, 10 deg., when a turn of 15 deg. has been made, because, in describing the curve, the aeroplane is banked inwards so that the axis of the magnet card does not remain vertical, and the directive force on the magnet is no longer the full horizontal component of the earth's field. Moreover, the compass cannot take up its position instantaneously. When the compass turns more quickly than

Fig.4.
R.A.F. SPHERICAL COMPASS. DASHBOARD MOUNTING.
DIAGRAMMATIC SEC.

Bezel.
Filling Screw.
Rubber Jointing Strip.
Glass with Spherical Inner Surface.
Lubber Line.
Diaphragm
Retaining Wire.
Outer Case.
Brass Bowl.
Expansion Chamber.
Cylindrical Card.
Float Containing Magnets
Liquid
Stem Carrying Sapphire Cup.
(6560.D)
Holder for Adjusting Magnets.
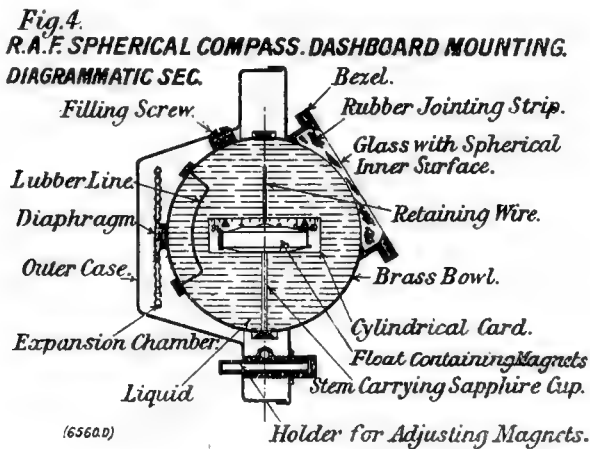
the aeroplane, the observer would at first imagine that he was steering towards the west when he was really turning eastward. This trouble could be remedied by making the time of swing of the needles long compared with the time taken by the machine to complete its turn. On a northerly course the turn would then appear in the right direction, though too small in amount; on a southerly course the turn would also appear in the right direction, but too large in amount.

The aviator's trouble with the sextant is that he is rarely able to take a horizon reading, and even if he can that reading would have to be corrected for the dip of the real horizon below the level of his machine. In the bubble sextant, illustrated in the diagram Fig. 5, the image of the bubble is visible at the same time as the distant object. The observer has to bring the object to appear in the centre of the bubble and to bring the two on the axis of his observing telescope. But, as in the case of the compass, the observer should be sure that, at the moment of reading, he is flying uniformly without acceleration. His speedometer gives him his rate of motion through the air, and, with a steady wind, he would know that he was flying at constant speed. His altimeter aneroid, or his climbmeter, would enable him to keep on a horizontal path, but he still needs something to tell him whether he is flying straight or on some curve. The turn indicator gives him this information. When he banks on a curve, the extremity of the outer wing is higher up and moving faster than the inner-wing extremity, and there is a difference of pressure at these two points; a small difference, of course, yet measurable by Sir Horace Darwin's

turn indicator, which marks zero when the aeroplane is travelling straight. In the gyro-turn indicator, developed at Farnborough, the principle of which is explained by the diagram Fig. 6, the gyro-wheel is placed outside the machine

Fig.5. R.A.E. SEXTANT MARK IIA.
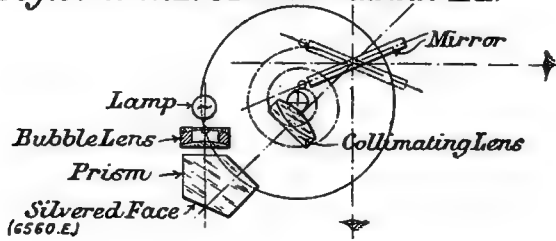
so as to rotate in a vertical plane at right angles to the direction of flight, the rotation being maintained by the wind playing upon suitable holes in the wheel. If the machine when moving straight horizontally begins to turn about a vertical axis, a pointer attached to the gyrostat indicates that motion or, if controlled by a spring, shows the force tending to make the gyrostat move.

There remains the dangers from mists and fogs, which are more formidable to the aviator than to the sailor. The sailor has fog signals, leader cables,

Fig.6.
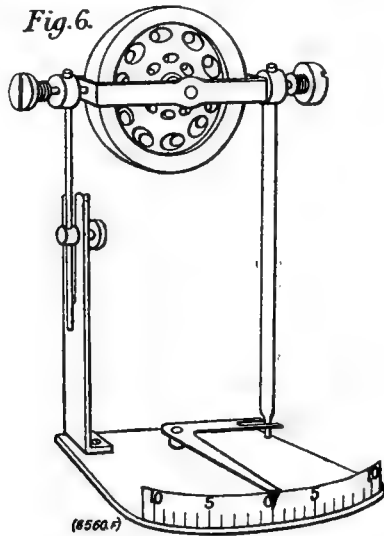
buoys, and harbour lights to guide him; the aviator frequently has little warning that he is within a few feet of the ground. Captive balloons, if feasible, are themselves sources of danger; sound signals would not be appreciated by the neighbours of aerodromes; signal beams do not penetrate far through mists; radio-telegraphy and radio-telephony promise best. But the landing troubles and means of control at low speeds, as well as the construction of reliable aero-engines, remain urgent problems of modern aircraft.

II.

FRIDAY, AUGUST 27.

A *Grain of Wheat from the Field to the Table.*
By Sir DANIEL HALL, *K.C.B.*, *F.R.S.*

In the history of mankind there are no processes older, more essential, or more universal than the growing, grinding, and baking of wheat and its kindred food grains.

What, then, has the British Association to do with so fundamental a business, brought to something like perfection long before anything we can call science existed?

That is precisely what I want to tell you to-night.

Countless years have elapsed since primitive man took the momentous step of sowing a little of the wild grain he had hitherto been content to gather, in the hope of saving himself some trouble in collecting the next year's crop. Millions of men have spent their lives in growing wheat. All sorts of rewards—nay, the very life of the community—have attended on improvements in the crop. What can there be to learn about it now?

Yet at every stage in the story of the grain of wheat from the seed-bed to the breakfast table we find that we do not know what we need to know in order to get on with the business of making two grains grow where one grew before. I want to show you that, however old, however fundamental the industry, science comes in at every turn, and research, calling for all our imagination, skill, and determination, is required if progress is to continue.

All biologists would agree that development demands an abundant food supply, just as fine flowers want a fat soil. Now, the population of the world is rapidly growing up to, if it has not for a time exceeded, its available food supply, and only by research and the utilisation of the fruits of that research are we going to obtain more food. If I had to name one remedy for the present discontents it would be more wheat, and as we are nearing the limits of the potential wheat land we must therefore set about the other problem of getting more from what land we have. Beginning with the grain of wheat, we find it consists of a tiny embryo, that part that possesses life, and the endosperm or food store, which is to nourish it until it can push a green leaf above the ground and begin to feed upon the air and the soil. The embryo's food store is our food supply; flour is only the powdered endosperm.

In its dry state, when it cannot draw upon the endosperm, the embryo soon dies, and with it the whole grain; some in one year, more in two; few can survive for ten years. Mummy wheat is a myth. Can you excite the seed before sowing by electricity or other means to grow better and give a bigger crop? Experiments are being made, but the results are dubious. Probably not, because the seed only starts the plant in life; its growth and yield depend on development after the start, on the soil, the manure, the weather.

It is usual in England to sow two and a-half bushels of seed wheat to the acre; properly managed, half a bushel or less would cover the field with the necessary plants for a maximum crop. Experiments are on foot to get a machine that will sow economically. Even if we can save a bushel an acre of seed the country would gain 3 per cent. of its output of wheat, worth well over a million pounds a year.

There are hundreds of kinds of wheat—early and late, tall and short, close-packed or open in the ear, varying in colour and size and in other ways. Each sort breeds true because the flower is self-fertilised. If we pick out each year the longest ears in the field, or the plumpest berries, and grow only from them, no improvement results. Selection of this kind has been tried for fifty years without result. Change, and with it improvement, only comes when varieties are *crossed*; then we get new varieties. The scientific breeder working on Mendel's principles can in a few years raise and fix a new wheat, combining the good points of both parents. New English wheats have been bred in the

last few years which raise the produce per acre of Eastern County farms by at least 10 per cent.

For all its vigour, wheat cannot stand the competition of weeds. At Rothamsted a crop was left unharvested to sow itself without cultivation. In three years the wheat had entirely disappeared in the wilderness that grew up.

Nevertheless, wheat has, more than any other cultivated plant, the capacity of growing upon all sorts of soils, even the poorest. At Rothamsted, on one of the plots, wheat has now been grown for seventy-seven successive years without any manure, and it still yields about twelve bushels to the acre, pretty much the average crop of all the wheat lands of the world. Wheat is the crop for breaking in the wilderness. In the new countries the settler always begins with a succession of wheat crops before he resorts to mixed farming.

Experiments have long since settled what manures wheat wants. The real trouble now is to get the big crops grown with plenty of manure to stand up, and this is a problem now being attacked in various fashions—stiffer-strawed varieties, special cultivation, and corrective manures, etc.

The wheat plant practically finishes growing a month or five weeks before it is harvested. In the last period the valuable material is being moved from stem and leaves to the seed. The migration is incomplete; half or less of the stuff manufactured by the plant gets into the seed, and here are great possibilities of improvement.

The object of the modern flour miller is not to grind wheat into a meal and then sift out the flour, but to crack the berry without breaking the husk (bran) and let the endosperm fall out. The best white flour is pure endosperm. It is the most digestible part of the grain, and weight for weight yields the most food. In peace times only two-thirds of the grain is recovered as flour, but under war conditions it was necessary to use the less digestible portions as well, and the extraction was raised from 68 to well over 90 per cent. Though imperfectly digested and not suited to all constitutions, the higher extraction was equivalent to an extra two months' supply of wheat.

Flour from most English wheats produces small dense loaves; certain Canadian and other foreign wheats give big spongy loaves, which the public prefer. A wheat was found that retains this property of strength in the English climate. This wheat crops badly, but the wheat-breeder is at work combining the strength of this Fife wheat with the cropping power of English wheats. Professor Biffen's ' Yeoman ' wheat, on suitable soils, is now the biggest cropper known, and gives flour as strong as Canadian wheat

Before the war we only grew one-fifth of the wheat we ate; the rest came from North and South America, Russia, India, and Australia. Some of these foreign supplies have been cut off, and the world's supply of wheat will be short for years to come. As a national insurance we must grow more at home, and this can only be done by better skill and more knowledge, because we cannot expand our land indefinitely. We must not grudge expenditure on knowledge; our food supply in the future depends upon the advancement of science, which is the purpose of the British Association.

CORRESPONDING SOCIETIES COMMITTEE.

Corresponding Societies Committee. Report of the Committee consisting of Mr. WILLIAM WHITAKER, *F.R.S.* (*Chairman*), Mr. WILFRED MARK WEBB (*Secretary*), Mr. P. J. ASHTON, Dr. F. A. BATHER, *F.R.S.*, *the* Rev. J. O. BEVAN, Sir EDWARD BRABROOK, *C.B.*, Sir H. G. FORDHAM, Mr. T. SHEPPARD, *the* Rev. T. R. R. STEBBING, *F.R.S.*, Mr. MARK SYKES, *and the* PRESIDENT *and* GENERAL OFFICERS *of the Association.* (*Drawn up by the Secretary.*)

I.

THE Committee reports that the following are the officers of the Conference of Delegates to be held at Cardiff : *President*, Mr. T. Sheppard, M.Sc., F.G.S. ; *Vice-President*, Mr. F. W. Sowerbutts ; *Secretary*, Mr. Wilfred Mark Webb, F.L.S. ; and that the programme is as follows :—

Wednesday, August 25, at 2 P.M.—(1) Presidential Address by Thomas Sheppard, M.Sc., F.G.S., on ' The Evolution of Topographical and Geological Maps.' (2) Paper on ' Railways and their Obligations to the Community,' by A. H. Garstang, Secretary of the Railway Facilities Sub-Committee of Section F.

Friday, August 27, at 2 P.M.—Discussion on ' The Status of Local Societies : the means of developing their objects, of getting new members, of publishing papers and making announcements,' which will be opened by William Whitaker, B.A., F.R.S.

After the meeting the Delegates will be entertained to tea by Principal A. H. Trow, D.Sc., F.L.S., President of the Cardiff Naturalists' Association, and will have an opportunity of seeing the Exhibition, illustrating the Presidential Address and the work of local Societies.

The Committee recommends that the Offa Field Club, Oswestry, be admitted as an Affiliated Society, and the Scottish Natural History Society, the Darlington and Teesdale Naturalists' Field Club, and the Greenock Philosophical Society as Associated Societies.

The Committee asks to be reappointed, with a grant of 40*l.*

II.

At the first meeting of the Conference of Delegates on Wednesday, August 25, the President, Mr. T. Sheppard, delivered the following address :—

The Evolution of Topographical and Geological Maps.

One of the secrets of successful collecting—and every scientific man is a collector in some form or other—is to secure series of certain specimens or objects for which few people, if any, are in search. In this way it is possible to contribute something tangible towards the advancement of science.

On a previous occasion I had the privilege of bringing before your notice information relating to the past difficulties in connection with the exchange of currency, clearly demonstrating the necessity for the decimal system of weights and measures. (See ' Rep. Brit. Assoc. for 1917,' pp. 228-235.) That paper was made possible by collecting old boxes of money scales and weights, a few years' work resulting in the finest series of English examples in existence being gathered together.

In the same way, and for somewhat similar reasons, collecting old topographical and geological atlases and maps was indulged in, and by methods familiar to experienced collectors, examples of old road-books, charts, and geological plans, diagrams, and maps began to accumulate to an extent which was positively alarming !

On the topographical side, a predecessor of mine in this chair, Sir George Fordham, exhibited before the British Association at the Dublin and Dundee meetings in 1908 and 1912 respectively, a fine series of old road-books. With regard to geological maps, however, which naturally follow the topographical series, nothing systematic seems yet to have been accomplished, and it is extraordinary how this most valuable source of information has been neglected—even by our special geological societies. A few years ago the Geological Society of London asked me to prepare a Catalogue of British Geological Maps, and the work has occupied nearly all my 'spare time' ever since, as will be understood when I say that the Catalogue, as yet in manuscript, contains details of thousands of such maps. The preparation of this, and the collecting of old maps and charts, has resulted in the accumulation of facts which will, I think, be of interest to the delegates of the Corresponding Societies, and, it is hoped, will give them an idea of the method of obtaining, from sources which are usually neglected, information relating to the physical geography and geology of their respective areas.

I possess ' Edward, Duke of Norfolk, Earle Marshall of England's ' copy of Moll's ' New Description of England and Wales,' dated 1724. It consists of fifty maps, measuring 10 by $7\frac{1}{2}$ inches. Each map is folded, and mounted in the middle on guards, so that no information is lost or distorted in the binding. The volume measures $7\frac{3}{4}$ by $8\frac{1}{4}$ inches. Bound up with it is a map of ' The Roads of ye South Part of Great Britain Called England and Wales,' by Herman Moll, Geographer. It is dedicated to Frederick Prince of Wales (and therefore must be after 1729), and contains the following engraved upon it : ' *Note* : This map has been copied four times *very confused and Scandalously* '! Presumably by other ' enterprising ' publishers.

On the margins of each of the county maps various ' curios ' are engraved, usually bearing upon the district. For example, the first map, Bedford Shire, is decorated by representations of obverses and reverses of six Roman coins, in the execution of which—as in other illustrations—the artist has had very fair licence.

Besides giving evidence of changes due to coast erosion, alterations in estuaries and river channels and lakes; these old maps yield much interesting geological information, albeit the ' Geographer, Moll,' knew not the science of geology. On the map of Cornwall, for instance, not only are the mines indicated, and their names and depths given, but details occur of the various metals obtained—tin, copper, and lead; there are ' lead mines producing much silver,' and even ' ancient lead mines.' On the margin is engraved ' The Wring Cheese Stones near Rillington,' the ' Hurlers' Stones,' etc. Thus this map not only indicates the extent to which mining for metals was practised precisely two centuries ago, but gives evidence of mining in still earlier times.

Black-lead mines, copper mines, and lead works are shown on the Cumberland map. That of Derbyshire is dotted over, to a surprising extent, with triangular marks indicating lead mines : the margins being devoted to representations of geological ' curios,' as well as engravings of ' Poole's Hole ' and another cave named after a portion of the anatomy of the devil. These illustrate the characteristic weathering of the local Carboniferous Limestone. The fossils are better engraved than described. An obvious *Euomphalis* is ' A Petrify'd Cockle'; a *Productus* is ' A Petrify'd Oyster '; a piece of encrinital limestone is ' A Terrene course Fluor or Spar found in the Lead Mines '; a Silurian Brain-Coral has somehow crept in, and is ' Bufonites or Brain-stone, viz. from ye Representations; it Bears to a Toad.' A chalk echinoderm—which has also strayed—is ' One of the Echini Petrify'd with the Representations of Trees,' the ' Trees ' being the lines between the plates forming the test. Two sharks' teeth, suspiciously Tertiary, are ' A Glossopetra or Crowbill found in ye Lead Mines,' and ' A Glossopetra found in the like Mines both in this County and Wales,' respectively.

The Devonshire map indicates lead, tin, and copper mines, ancient lead mine, and ' *The Most Ancient* Copper Mine.' Lead mines and ' Coal Pitts ' occur on the Durham map, as well as ' Hell Kettles ' and other natural features. On the Essex map are two engravings labelled ' Near Tilbury are several Artificial spacious Caverns built with Stone in a Chalky cliff to ye height of 10

Fathom as nere represented,' but the artist had not quite understood the structure of the dene-holes (as now named); his plans are all right, but the shafts are drawn above ground like factory chimneys. The Lincolnshire map indicates the former appearance and extent of the Fens and the Wash, and with it is 'A Perpetual Tide Table for Foss-Dyke and Cross-Keys Washes in the county, showing from the Moon's Age the exact time of Full Sea of ye beginning and ending of the Wash, or when Travellers may safely pass over.'

The Northamptonshire plate shows a pear-shaped mass of Serpulæ labelled ' A Vertebra or single Joint of the Back-Bone of a large fish. 'Tis two inches and a half in length, and near as much in breadth. Digged up at Peakirk almost 4 foot deep in the Earth.' A well-known Oolitic coral is ' Astroites, or Star Stone, with round radiated holes in its surface found at Cartenhall '; two Kimmeridge Clay Ammonites are ' A Five wreath'd double straited (*sic*) *Ammonia* found in Oxenden ' and ' The Studded Ammonites Modiolaris found near Towceter.' Another is ' A Four wreath'd Ammonites found at Marston Trussel.' ' Lead Mines and Coal Pitts ' are indicated on the Northumberland map. On that of Shropshire it is amusing to read that ' *For want of antiquities, &c., in the county* we have inserted some out of ye Neighbouring County of Staffordshire.' Five ' Form'd Stones partly Cylindrical ' are clearly encrinite-stems, and four organ-pipe and similar corals are labelled ' Minerall Coral,' ' Museus Pyreidatus found near Stansop,' ' Honey Comb Stone,' and ' A Form'd Stone like a Stool of Reeds' respectively. For the same reason the Worcester map is decorated by specimens alleged to have been found in Staffordshire, though in this case minerals as well as fossils are given. In addition to ' Lead Mines and Coal Pitts,' ' Allom Works ' are indicated on the ' North Riding of York Shire ' map, the last referring to a one-time flourishing industry in the Whitby area. On the East Riding of the same county ' Sunk Island ' is shown as an island in the middle of the Humber—an area now joined to the mainland—as a result of which one-time seaport towns are now far inland; similarly, on the sea-coast, towns are shown which have since been entirely washed away by the sea.

On the South Wales map a frond of *Neuropteris* and a fragment of *Sigillaria* are given as ' Mock Plants out of a Cole Pit near Neath in Glamorganshire,' and presumably examples of

> ' The sport of Nature, aided by blind chance,
>   Rudely to mock the works of toiling man.'

I have mentioned only a few contents of this old atlas, but I trust it has been demonstrated that, two centuries ago, information now of great value, both geographically and geologically, was being placed upon these maps. Moll's work is only one of scores which were issued, some earlier, some later. What I wish to emphasise is the necessity for preserving these maps and atlases before it is too late. Each county society should collect, store; and eventually catalogue and describe the maps relating to its area.

During the past few weeks three different booksellers have sent me books wrapped up in county maps, or, more annoying still, parts of maps. In one case I asked if any others were available similar to those used for packing. I secured those I required, but at a price which clearly indicated that the ' packing ' was about as valuable as the books !

I am sorry to say that the systematic compilation of lists of county maps has only as yet been accomplished for a very few areas, and we thus have many more ' imperfections in the geological record ' than are really necessary. A few have been published, and as they contain a fair proportion taken from atlases which contain charts of all the counties, a careful examination of these— or the latest one issued—will give a good basis for a catalogue, to which the locally-published archæological, geological, and topographical maps should be added. Mr. T. Chubb, of the Map Room, British Museum, would, I feel sure, give every assistance to anyone seriously taking the matter up.

Sir George Fordham appears to have led the way, his admirable papers on Hertfordshire maps being published by the Hertfordshire Natural History Society and Field Club (1901–1914); he followed with ' Cambridgeshire Maps ' (' Communications Cambridge Antiquarian Society,' 1905–1908).

Mr. William Harrison was early in the field with his 'Early Maps of Lancashire and their Makers' (1908), published in the 'Transactions' of the Lancashire and Cheshire Antiquarian Society, and in the same year this society published his **'Early Maps of Cheshire.'**

Mr. T. Chubb prepared a catalogue of Wiltshire maps, published by the Wiltshire Archæological and Natural History Society in 1911, a more substantial catalogue of Gloucestershire maps issued by the Bristol and Gloucestershire Archæological Society in 1913, and in 1916 the Somersetshire Archæological Society printed his catalogue of Somersetshire maps, which, having the advantages of all the lists previously published, is remarkably complete.   In 1918 the Cumberland and Westmorland Antiquarian and Archæological Society's 'Transactions' contained a list of the maps of those two counties by Mr. J. F. Curwen. This brief list exhausts our record of county catalogues, though Miss Ethel Gerard has dealt with early Sussex maps (*Library*, 1915), Miss M. Frost with early Sussex Geological maps (in manuscript), and the present writer with East Riding maps ('Transactions' East Riding Antiquarian Society, 1912), but in these instances complete catalogues were not attempted.

A careful perusal of these various compilations shows that the history of British cartography can be divided roughly into three periods, perhaps best classified by Fordham as—

1. 1579–1673 (Saxton to Blome).   The early and archaic maps : Period of the
            Dutch School, and of the meridian of the Azores or Canaries.
2. 1673–1794 (Seller to Cary).   The modern and detailed maps with roads :
            Period of the English School, and of the meridian of London.
3. 1794–1900.   Period of the Ordnance Survey, and of the meridian of Greenwich.

With regard to the earliest maps of the British Islands : these are usually on vellum and preserved in one or other of our great libraries.   Useful reproductions of some of these are given in Richard Gough's 'British Topography, or, An historical account of what has been done for illustrating the Topographical Antiquities of Great Britain and Ireland,' 1780, two valuable volumes which are not used by students so much as they deserve to be.   Gough's illustrations are especially serviceable, as the originals of some of his plates have since faded to such an extent that portions are entirely useless. These maps of the thirteenth, fourteenth, and fifteenth centuries, however, are principally remarkable for their quaintness, and are historically of service from the place-names they record and the rough sketches of ecclesiastical buildings, castles, and fortifications.   But, as for illustrating any geographical or geological features, their scale is too small and the conditions under which they were prepared were too primitive.   But they were the stepping-stones to greater cartographical achievements.

The first engraved map of England and Wales (1573) was by Humphrey Lloyd, a Welshman ; the first county maps were produced by a Yorkshireman, Christopher Saxton, who had special facilities for surveying granted to him by Queen Elizabeth.   The work was carried out between 1574 and 1579.   Various maps were engraved during this period, the whole being brought together and issued as an atlas, with title page, etc. in 1579.

Between 1584 and 1593 John Norden surveyed seven counties and issued maps thereof.

The next important series was by John Speed, said to have been a native of Cheshire.   He issued a 'History of Great Britaine' for which fifty-four county maps were prepared between 1608 and 1610.   The maps alone were published in 1610 in an atlas entitled 'The Theatre of the Empire of Great Britain.'

Speed's maps were based upon Saxton's and Norden's.   Copies of these were issued by W. Kip and W. Holl, in the various editions of Camden's 'Britannia,' and still further copies appeared in numerous other works.   Also, the remarkable maps of English counties, printed in Holland, and often gorgeously coloured, made their appearance.   This was due to the anxiety of two publishers, Blaeu and Jansson, in Amsterdam, vieing with each other in the reproduction of an enormous atlas of the world.

But in all these various maps the geographical details were essentially the same, the same errors of spelling and of positions of townships were faithfully

copied one after another; and additional misspellings and other mistakes crept in as fresh 'editions' appeared. True, the shipping, the scroll-work, borders, decorations, and dates were altered from time to time; a map dedicated to Queen Elizabeth bears a date long after that lady became an angel; and even the positions and attitudes of the various grotesque sea-monsters besporting themselves in the ocean were altered, but no advance was made in scientific cartography as a result of these various 'new editions.'

During the succeeding century the production of atlases and maps was tremendous : partly owing to the increased interest being taken in travel and exploration generally; partly, no doubt, from improved methods of engraving and printing. In this period occur maps by Seller, Lea, Morden, Moll, Blome, Overton, Kitchen, Bowen, Jeffery, Ellis, Carington, Bowles, Cary, and others, whose names are familiar to map collectors. Some were beautiful pieces of work, decorated by views or plans of the principal towns, cathedrals, or other items of interest. Many of the later maps had been specially surveyed, the roads were carefully portrayed, and even the smallest hamlet was indicated. Of these, the work of John Cary stands out with prominence. Between 1787 and 1832 he produced an extraordinarily large series of maps and atlases, all excellently ' performed.' His work consisted of various county and road atlases, while his large maps, on the scale of two miles to an inch, compare well with the Ordnance Survey map of the same scale.

Facilities for travelling between one point and another, in the way of stage coaches and improved roads, resulted in the appearance of 'road-books' of various descriptions. Among the first of these was John Ogilby's 'Britannia.' This consisted of details of all the items likely to be of interest to the traveller being represented on scrolls, engraved parallel to each other, particulars of a definite road being shown on one plate. At first these books were large and unwieldy, but improved as time went on, and eventually were issued small enough to fit the pocket.

Road-books alone provide a wealth of information relating to the former appearance of the country, and should be carefully examined by those interested in the past history or geography of any particular area. Streams, bridges, hills, good roads, moors, and commons, woods, and other details are given with wonderful precision. Beacons, gibbets, and similar by-gones are indicated; in those days the approach to the gibbet meant whipping up the horses in order to pass the ghastly spectacles hung in chains, with their accompanying stench, as quickly as possible. In Yorkshire these books have provided minute details of roads which have long since been swallowed up by the sea. Their perusal therefore yields valuable facts relating to the appearance of the country before it was changed by the Enclosures Acts, and all interested in coast changes, the reclamation of fen and bog land, the former extent of forests, moors, and commons will do well to consult them.

The period of really reliable and accurate mapping may be said to have started by the formation of the Board of Ordnance, the surveying and triangulations in connection therewith commencing in 1784, though nothing was actually published until 1801, seventeen years later. This Government work practically stopped all private enterprise in the way of surveying and publishing maps, a remarkable exception being the beautiful productions of the Greenwoods, who published several fine maps, and a county atlas in 1831. About the same time (between 1823 and 1835) A. Bryant surveyed and published maps of about a dozen counties. These were very well done and are eagerly sought by collectors.

From the early topographical maps illustrating coast changes and alterations in the physical features of the country, and recording the occurrence of various fossils and minerals, it becomes an easy process to imagine the evolution of the map upon which rocks and soils are indicated by signs or colours. Yet, although all the steps leading up to geological maps had been laid down, it was not until early in the nineteenth century that William Smith produced the first map upon which were definitely indicated the different rocks forming the earth's surface. From his primitive hand-coloured maps, upon which he 'plotted' what he personally investigated in the field, to the colour-printed geological and mineralogical maps now being issued in enormous numbers by every civilised State in the world, is a line of scientific progress rarely followed. Yet in few directions has the pet hobby of a scientific crank, as ' Strata Smith '

was then considered to be, become so necessary and so economically essential in every part of the world.

The early history of geological mapping shows that the foundations of this particular work are essentially British, and as usual these foundations were laid by amateurs, often at great pecuniary sacrifice to themselves, and without proper appreciation at the time.

That geological observations were made, long before maps recorded them, is obvious from various early writings. Even so long ago as 1595 George Owen of Henllys in Pembrokeshire, in a ' History of Pembrokeshire,' has a chapter on ' natural helpes which in this countrey to better the lande [" lyme " being the " chiefest "].' In this he states ' First you shall understand, that the lymestone is a vayne of stones running his course, for the most part right east and west, although sometimes the same is found to approach to the north and south. Of this lymestone there is found of ancient, two veynes, the one small and of no great account, and not of breadth above a butt length, or stones cast ; and therefore whosoever seeketh southward or northward over the bredth misseth it.'

The course of this ' veyne ' is then traced for a considerable distance ; and a third ' veyne of lymestone ' is referred to. We then read that ' For the veyne of coales which is found between these two vaynes of lymestone, as a benefit of Nature, without which the profit of the lymestone were neare lost ; betweene the sayd two vaynes from the beginning to the ending, there is a vayne (if not several vaynes) of coles, that followeth those of lymestone. This vayne of coal in some partes joineth close to the first lymestone vayne, as in Pembrokeshire, and Carmarthenshire ; and in some partes it is found close by the other vayne of lymestone, as in Glamorgan, Monmouth, and Somersetshire. Therefore whether I shall say that there are two vaynes of coles to be found between these two vaynes of lymestone, or to imagine that the cole should wreathe or turne itself in some places to one, in other places to the other ; or to think that all the land betweene these two vaynes should be stored with coles, I leave to the judgement of the skilfull miners, or to those which with deep knowledge have entered into these hidden secrettes.'

A comparison between these observations and a recent geological map will show that Owen's observations were quite reliable, although made over three centuries ago.

Later, but still long ago, Dr. Martin Lister in March 1683-1684 read a paper to the Royal Society entitled ' An Ingenious proposal for a new sort of Maps of Countrys, together with Tables of Sands and Clays, such chiefly as are found in the north of England.' The author commences : ' We shall be better able to judge of *the make of the earth*, and of many phenomena belonging thereto, when we have well and duly examined it, as far as human art can possibly reach, beginning from *the outside downwards*. As for the most inward and central parts thereof, I think we shall never be able to confute Gilbert's opinion, who will, not without reason, have it altogether iron.'

' And for this purpose it were advisable that *a soile or mineral map*, as I may call it, were devised. The same map of England may, for want of a better at present serve the turn. It might be distinguished into *countries*, with the rivers and some of the noted towns put in. The *soils* might either be coloured, or otherwise distinguished by variety of lines or etchings ; but the great care must be, very exactly to note on the map, where such and such soiles are bounded. As for example, in Yorkshire, (1) *The Woolds* : chaulk, flint and pyrites, &c. (2) *Blackmoor* : moores, sandstone, &c. (3) *Holderness* : boggy, turf, clay, sand, &c. (4) *Western Mountains* : moores, sandstone, coal, ironstone, lead-ore, sand, clay, &c. *Nottinghamshire* : mostly gravel pebbles, clay, sandstone, hall-playster or gypsum, &c. Now if it were noted how far these [soils] extended, and the limits of each soil appeared upon a map, *something more might be comprehended from the whole and from every part than I can possibly foresee*, which would make such a labour well worth the pains. For, I am of opinion, *such upper soils, if natural, infallibly produce such under minerals, and for the most part, in such order*. But I leave this to the industry of future times.'

Thus we get the first idea of preparing a map of England showing the

various soils and their boundaries by colours. Unfortunately the scheme was apparently not carried out until long after Lister's death.

A paper on 'The Somersetshire Coal District' was contributed to the 'Philosophical Transactions' in 1719, and was republished ten years later as 'Observations on the Different Strata of Earth and Minerals, more particularly such as are found in the Coal Mines of Great Britain.' This is accompanied by a carefully prepared section of some coal seams ten miles south-west of Bath. Though two centuries old, it clearly indicates the order and composition of the beds, their interruption by ridges [faults]; and the occurrence above the coal-seams of freestone [Oolite] lias, and red marl, lying unconformably upon the older beds. The author has some weird and wonderful theories to account for his facts, but he gives a section showing the proper relative order of the various beds between the Chalk and the Carboniferous Rocks.

'A new Philosophico-chorographical Chart of East Kent' was 'invented and delineated' by Christopher Packe in 1743. It is now very scarce, but there is a copy in the library of the Geological Society of London. In this the valleys and other physical features were shown, with the chalk districts, stone hills, clay hills, etc. There is, however, no reference to stratification. Dr. Packe was proud of his work. It was 'no dream or devise, the offspring of a sportive or enthusiastical imagination, conceived and produced for want of something else to do, at my leisure in my study, but it is a real scheme, taken upon the spot with patience and diligence, by frequent or rather continued observations, in the course of my journeys of business through almost every the minutest parcel of the country; digested at home with much consideration, and composed with as much accuracy, as the observer was capable of.'

John Woodward, in his 'Natural History of the Earth' (1723); Nicholas Desmarest, in the 'Encyclopédie Méthodique'; John Michel (1760), whose work has recently been described by Sir Archibald Geikie; John Whitehurst, in his 'Inquiry into the original State and Formation of the Earth'; John Smeaton, the engineer, in 1788; Prof. Jamieson ('Memoirs,' Wernerian Society), 1811; James Parkinson ('Transactions,' Geological Society), 1811, and other early investigators have left evidence that they were familiar with the various beds of the earth's surface, their relative positions, thicknesses, and economic contents. They were also aware of the various parts of the country in which the different beds occurred. But none of them recorded that information on a map or chart, although Prof. Jamieson got very near it; his paper being 'On Colouring Geognostical Maps,' but the enormous number of complicated signs and symbols he suggested proved unsuitable for practical purposes, though there were many good features in his colour-scheme.

In the library of the Board of Agriculture and Fisheries, Whitehall, is a very fine but incomplete series of the old county Agricultural Reports. These were prepared upon a definite plan, for most, but not all, of the counties in Great Britain, and principally date between 1790 and 1820. Occasionally two or more editions were issued. In most of them is a coloured or shaded map of the soils of the county. While these maps are usually but briefly described, and sometimes not described at all, their great bearing upon the geological features of the counties dealt with, together with their early date, make their consideration of some importance. They are also of value as it is obvious that they were seen by William Smith, and considerably influenced him in his work on his geological maps. The words used, e.g., brash, dunstone, freestone, etc., were also used by Smith. We know that he regularly attended the various meetings of the Agricultural Societies, exhibited his draft geological maps there, and, apparently, often bored his hearers by his talk on 'strata' and 'organised fossils.' 'Strata Smith' was a man to be avoided at these sheep-shearing meetings, and we learn that on one occasion he was made aware that no one was paying any heed to his remarks, so he folded up his maps and brought his discourse to an abrupt termination. Most of the maps in the Agricultural Reports were doubtless familiar to Smith; his intimate knowledge of rocks and their methods of disintegration into soils enabled him to extract much geological information from the soil maps, and there can be little doubt that they considerably assisted him in the preparation of his great geological map of England and Wales of 1815. I have carefully compared the soil maps of the areas which

we know were unfamiliar to Smith, and it is apparent that he depended upon them for the extent of the outcrops of the different beds, his masterly geological mind enabling him to translate ' limestone soil,' ' marl,' and other terms to their proper horizons.

In these circumstances it seems desirable briefly to refer to these old soil surveys.

In the ' Journal of the Royal Agricultural Society ' for 1898 Sir Ernest Clarke prints an account of ' The Board of Agriculture, 1793-1822,' in which he gives some information of value as to the dates of the appearance of the county surveys. He states that ' Sinclair commenced on too ambitious a scale with the comparatively small funds at his disposal.' Sir John's original estimate of the funds necessary for the Board's support had been 10,000 guineas per annum, which was reduced by degrees to 3,000*l.*, the actual sum annually voted by Parliament. But to a man of Sinclair's temperament it was impossible to ' hasten slowly,' and therefore the initial efforts of the Board were directed with an impetuosity for which an annual income of 10,500*l.* would not have been excessive. By the middle of the ensuing year, 1794, the whole of the kingdom had been divided into districts and assigned to different ' Surveyors,' and by July 1795 nearly all their reports had been received. They were then issued as what Sinclair called ' printed manuscripts,' in quarto size, with large margins for the corrections and additions of practical agriculturists. The plan was not a bad one, but it did not answer the expectations formed of it. This is not surprising when we consider the undue haste and bad judgment displayed by the President in the choice of the men employed. ' The result was the production of a huge mass of ill-digested articles of the most varying degrees of merit, from valuable and exhaustive monographs in a few isolated instances to scrappy memoranda of but a few pages in others, according to the writers' ability and thoroughness, or lack of these qualities. Though ostensibly drawn up for private circulation, the reports were entered at Stationers' Hall, and may be regarded as practically published documents. The issue of such un-reliable literature brought the Board at once into bad repute, and this unpopu-larity was accentuated by a belief, groundless it is true, that the inquiries of the surveyors were intended to lead to increased taxation. Another circum-stance which added to the Board's difficulties was the hostility of the Church, provoked by an attempt to obtain information on the subject of tithes. Sinclair had derived much help from the Scottish clergy in the preparation of his "Statistical Account" of Scotland, and he now hoped similarly to enlist the co-operation of the English clergy. But the mention of the vexed question of tithes excited their suspicion, and even led to an intimation by the Archbishop of Canterbury to Pitt, that any interference with this matter would alienate the support of the Church from the Government.'

' In view of the fact that every now and then appear in booksellers' cata-logues what are described as "large paper " copies of the reports to the Board of Agriculture on particular counties, it appears necessary to point out that these are the original imperfect drafts on quarto paper, circulated for correction amongst agriculturists of the district in the manner above described, and that the final reports were all printed (in most cases years after the original drafts and by different authors) in octavo size.'

Sir Ernest then supplies the following particulars of the publication of the Agricultural Reports, which I give here for the use of the delegates; though this list is not quite correct, my own collection containing a number of editions not here recorded :

Table showing Authors and Dates of Publication of (A) the Draft (Quarto) Reports, and (B) the Final (Octavo) Reports, on the several Counties of England and Wales.

County	(A) Draft (Quarto) Report Author	Date	No. of Pages	(B) Final (Octavo) Report Author	Date	No. of Pages
Bedford	Thomas Stone	1794	70	Thomas Batchelor	1808	651
Berkshire	Wm. Pearce	1794	74	Wm. Mavor	1808	559
Buckingham	Wm. James and Jacob Malcolm	1794	63	Rev. St. J. Priest	1810	420
Cambridge	Chas. Vancouver	1794	219	Rev. W. Gooch	1813	312
Cheshire	Thos. Wedge	1794	88	Henry Holland	1808	387
Cornwall	Robt. Fraser	1794	70	G. B. Worgan	1811	208
Cumberland	John Bailey and George Culley	1794	51	John Bailey and George Culley	1797	69
Derby	Thos. Brown	1794	72	John Farey (3 vols.)	1811–7	1901
Devon	Robt. Fraser	1794	75	Chas. Vancouver	1808	491
Dorset	John Claridge	1793	49	Wm. Stevenson	1812	498
Durham	Joseph Granger	1794	74	John Bailey	1810	426
Essex	Messrs. Griggs	1794	26 }	Arthur Young (2 vols.)	1807	873
„	Chas. Vancouver	1795	213 }			
Gloucester	George Turner	1794	57	Thos. Rudge	1807	416
Hampshire	Abr. and Wm. Driver	1794	78	Chas. Vancouver	1810	528
Hereford	John Clark	1794	79	John Duncumb	1805	181
Hertford	D. Walker	1795	86	Arthur Young	1804	255
Huntingdon	Thos. Stone	1793	47	R. Parkinson	1813	358
Kent	John Boys	1794	107	John Boys	1796	222
„	—	—	—	„ „	1805	306
Lancashire	John Holt	1794	114	John Holt and R. W. Dickinson	1795	253
					1814	668
Leicester	John Monk	1794	75	Wm. Pitt	1809	420
Lincoln	Thos. Stone	1794	108	Arthur Young	1799	462
Middlesex	Thos. Baird	1793	31	J. Middleton	1798	614
„	Peter Foot	1794	92	„	1807	720
Monmouth	John Fox	1794	43	Chas. Hassall	1812	154
				Nathaniel Kent	1796	252
Norfolk	Nathaniel Kent	1794	56	Arthur Young	1804	552
Northampton	Jas. Donaldson	1794	87	W. Pitt	1809	332
Northumberland	John Bailey and George Culley	1794	70	John Bailey and George Culley	1797	168
„	—	—	—	„ „ (3rd ed.)	1805	213
Nottingham	Robert Lowe	1794	128	Robert Lowe	1798	204
Oxford	Richard Davis	1794	39	Arthur Young	1809	374
Rutland	John Crutchley	1794	34	R. Parkinson	1808	194
Shropshire	J. Bishton	1794	38	Joseph Plymley	1803	390
Somerset	J. Billingsley	1794	192	J. Billingsley	1797	336
Stafford	W. Pitt	1794	168	W. Pitt	1796	264
„	—	—	—	„	1813	347
Suffolk	Arthur Young	1794	92	Arthur Young	1797	329
„	—	—	—	„ „ (3rd ed.)	1804	447
Surrey	Wm. James and Jacob Malcolm	1794	95	Wm. Stevenson	1809	624
Sussex	Rev. A. Young	1793	97	Rev. A. Young	1808	479
Warwick	John Wedge	1794	60	Adam Murray	1813	204
Westmorland	Andrew Pringle	1794	55	Andrew Pringle	1797	79
„	—	—	—	„ „ (3rd ed.)	1813	87
Wiltshire	Thos. Davis, Sen.	1794	163	Thos. Davis, Jun.	1811	287
Worcester	W. T. Pomeroy	1794	94	W. Pitt	1810	448

TABLE SHOWING AUTHORS AND DATES OF PUBLICATION, ETC.—*continued.*

County	(A) Draft (Quarto) Report			(B) Final (Octavo) Report		
	Author	Date	No. of Pages	Author	Date	No. of Pages
Yorks, N. Riding	Mr. J. Tuke, Jun.	1794	123	John Tuke	1800	370
„ E. „	Isaac Leatham	1794	68	H. E. Strickland	1812	340
„ W. „	Rennie, Brown, and Shirreff	1794	140	Robert Brown	1799	436
North Wales	Geo. Kay	1794	119	Walter Davies	1813	526
Brecknock	John Clark	1794	55			
Cardigan	T. Lloyd and Turnor	1794	37			
Carmarthen	Chas. Hassall	1794	52	" South Wales "		
Glamorgan	John Fox	1796	71	Walter Davies	1814	1170
Pembroke	Chas. Hassall	1794	63			
Radnor	John Clark	1794	41			
Isle of Man	Basil Quayle	1794	40	Thomas Quayle	1812	208
Channel Islands	—	—	—	„ „	1815	365

From the soil maps the advance to geological maps is not a very great step, but it was left to William Smith, 'a plain blunt man' and a civil engineer, born in Oxfordshire in 1769, to prepare the first geological map of any country; his maps have stood the test of time, and some of them, especially those of the country around Bath and Scarborough, compare very favourably indeed with the latest publications of the Geological Survey.

There is no doubt that Smith was inclined to be 'long-winded,' and even his nephew, John Phillips, records that he had a habit of running off into side-issues in such a way that it was exceedingly difficult to follow him or to form a correct idea as to the nature of his discourse. It was probably some such trait in his character which caused the men who formed the Geological Society to give Smith a wide berth, otherwise it seems difficult to account for the way in which he was at first ignored. However, Smith lived to see his work recognised by the Society, and the words uttered by the President, when the first Wollaston Medal was awarded to him in 1831, made amends.

I have dealt fully with Smith's work elsewhere ('William Smith : His Maps and Memoirs,' 'Proceedings,' Yorkshire Geological Society, 1917, reprinted 1920), but the great part he played in connection with the evolution of geological maps demands a brief reference here.

On the walls of the library of the Geological Society, Burlington House, in a circle fifteen inches in diameter, is 'A map of Five Miles round the City of Bath. . . . Presented to the Geological Society, February 18th, 1831. Wm. Smith, Coloured geologically in 1799.' This is the first geological map ever prepared, and is one of the greatest cartographical treasures we possess. By collecting old Bath Guides I was able to find that its basis was a plan appearing in 'The Historic and Local New Bath Guide,' published in that year. There are only three colours, but they illustrate Smith's well-known method of colouring the base of a formation with a deep tint, and shading this upwards towards the outcrop of the next overlying stratum. It also demonstrates how carefully he mapped all the geological lines around Bath.

A year later (1800) he coloured a geological map connecting the structure of the north of England with that of the south-west, recording the Oolitic series throughout England with remarkable accuracy. This map is lost, but we hope it may yet be found. In 1831 it was in the possession of John Phillips, the curator of the York Museum.

In 1801 a copy of Cary's 'Index Map of England' (11 in. by 9 in.) was coloured by Smith and labelled 'General Map of the Strata found in England and Wales.' This was also given to the Geological Society in 1831. It is coloured after the manner of Smith's later maps, the eight tints representing Chalk, Sand of Portland Rock, Oxford Clay, Oolite, Lias, Trias and Permian.

Carboniferous [and Magnesian] Limestone, Old Red Sandstone. It is the first geological map of England and Wales.

In 1805 Smith finished a large and detailed map of Somersetshire, which was publicly exhibited and described at meetings of the Agricultural Societies. In an incomplete state it had been shown at a meeting of the Bath Agricultural Society (of which he was a member) so early as 1799. This map is lost, but we hope search in old Somersetshire libraries may bring it to light.

Smith's great work was his wonderful map of England and Wales, measuring 6 ft. by 8½ ft., on the scale of five miles to an inch. It was projected in 1801 and published in 1815. Twenty different colours were used, and opposite the Humber Mouth is a 'Sketch of the Succession of Strata and their relative Altitudes,' often being accompanied by Smith's autograph, and the number of the map. In addition to the strata, collieries, various lead, copper, and tin mines, salt and alum works were indicated. The map, as might be expected, has defects, and these were pointed out by Judd ('Geological Magazine,' 1898, page 101), but they do not minimise the credit due to 'The Father of English Geology' for his masterpiece.

From a copy of the original prospectus it appears that the map was dedicated to Sir Joseph Banks, sanctioned by the Board of Agriculture, and was sold at prices varying from 5l. 5s. to 12l., according to the method of mounting. At least 96 numbered copies were published. Some idea of the amount of work connected with them may be gathered from the following extract from Smith's diary :—

> 'May 14, 1815.—Began at nine in the morning with an artist to colour for me the first printed copy of the "Map of the Strata" on canvas.
> 'May 22, 1815.—Finished colouring the first "Map of the Strata" on canvas.
> 'May 23, 1815.—Attended a meeting of the Board of Agriculture with the first finished copy on canvas of my "Map of the Strata."'

This was the map for which the Society of Arts awarded Smith the premium of 50l.

Between 1819 and 1824 Smith published six parts of a 'New Geological Atlas of England and Wales' twenty-one counties being represented in this work, which evidently did not receive adequate support, and was unfinished. The maps, which are very rarely met with, show much more detail than was possible on the large sheet of 1815. Yorkshire, in this series, is exceptionally accurate and complete, due to Smith's residence in the county from 1820, the map being issued in 1821.

In 1820 a reduced copy of Smith's large map of England and Wales was published by Cary. It measures 24 in. by 30 in. and is on the scale of 15 miles to an inch. It was entitled 'A New Geological Map of England and Wales, with the Inland Navigations, exhibiting the districts of Coal and other sites of Mineral Tonnage.' Information as to the economic products of the strata is here given. A second edition was contemplated, and I have a copy, engraved from the same plate as the 1820 issue, but with the date altered to 1824. The colours, however, have not been filled in. My copy, with another, was found in Scarborough a short time ago. So far I have not traced a copy of the 1824 edition coloured : it may never have appeared.

From 1828 to 1834 Smith was land steward to Sir John V. B. Johnstone, and resided at Hackness, near Scarborough. While there he prepared a geological map of the Hackness area, briefly referred to by his nephew Phillips. This map had not been seen, and neither the Geological Society nor any other society seemed to possess one or know anything about it. Through inquiries made in the Scarborough district, where Smith spent his last years, I was successful in securing two copies. A comparison between these and those of the Geological Survey shows a remarkable similarity, and the way in which Smith was able to map the intricate series of beds in the Scarborough area—nearly a century ago—is most creditable to him. It was lithographed by Day, London, hand-coloured by Smith, measures 23 in. by 36 in., scale 12 chains to 1 inch. Smith prepared a memoir describing the rocks represented upon it, which has recently been published.

In the Fourth Report of the Scarborough Philosophical Society, for 1833, there is a note 'that Mr. W. Smith has been kind enough to colour for the Society Knox's excellent map of the Vicinity of Scarborough.' This map could not be found at Scarborough, but on examining the maps in the possession of the Geological Society at Burlington House recently, I found it there, signed ' W. Smith, 1831.' How long it had been there, and how it got there at all, no one knows. I should imagine that Phillips, once curator of the Museum at Scarborough, later President of the Geological Society, had borrowed it! At the same time I found a copy of each of two second editions of Smith's county maps of Northumberland and Durham. These originally appeared in his Geological atlas in 1824, but the second issues are dated 1831, and were not previously known.

In addition to his maps Smith published a remarkable set of cross-country sections, the first being on his large map of 1815, eight others on a larger scale being published separately between 1817 and 1819. Geological sections hardly come within the scope of the present address, but as they form such an important feature on many maps subsequently published, it is as well to bear in mind the fact that Smith was the first to embellish a map in this way.

Improvements in engraving and in colour printing resulted in geological maps being issued which showed more detail and were less liable to error than the hand-coloured maps. Close upon Smith's heels came G. B. Greenough, President of the Geological Society, whose 'Geological Map of England and Wales' (63 in. by 75 in., scale 1 in. = 5 miles) was published 'under the direction of the Geological Society' and was accompanied by a memoir. Thirty-eight different strata were indicated, as well as various signs representing mines, etc. The first edition, though ready in 1814 and dated November 1819, appeared in May 1820. A second edition, with the colouring much improved, appeared in 1839, and a third in 1865, after Greenough's death. On this last edition appeared, for the first time, the words 'On the basis of the original Map of William Smith, 1815.'

As an indication of the enthusiasm displayed in connection with the issue of geological maps a century ago, the cost of the preparation of this map was 1,300*l.*, most of which was guaranteed before publication. I have recently had occasion to examine many reports of Philosophical Societies and Mechanics' Institutes of that time, and have been astonished at the number of items, varying from six to twenty pounds, shown in the accounts for the purchase of geological maps. Entries of this sort are very rare in these reports nowadays.

From the great wealth of Greenough's manuscript material in the possession of the Geological Society we can form some idea of the way in which he prepared his large maps, and of the great variety of sources from which he obtained information relating to the geological structure of our islands— even casual references in the daily papers supplied him with particulars. In his collection are some of the soil maps from the reports of the Board of Agriculture, clearly indicating, as with Smith, that Greenough could put these to good use.

Of a somewhat similar type, and certainly to be included as classics, are MacCulloch's map of Scotland (1834) and Griffith's map of Ireland (1839 and 1853). Of especial historical value is a manuscript map of 'Scotland coloured according to the Rock formations, presented to the Geological Society by Mr. A. L. Necker, Nov. 4th, 1808 ' (21 in. by 26 in., scale 1 in. = 12½ miles). There are seven different colours used to define the various rocks, these being placed upon a copy of Kitchin's map of 1778. The chart is far in advance of the soil-maps, is twenty-six years earlier than MacCulloch's map of Scotland, and even anticipates Smith's large map of England and Wales by seven years. Following these, the middle of the nineteenth century witnessed a veritable epidemic of geological maps of England and Wales, including those of Arrowsmith, Murchison, Walker, Ramsay, Ravenstein, Knipe, Phillips, Johnston, and others, many of which were reprinted and revised on numerous occasions. In addition to the geology, these maps vied with each other in the matter of the quantity of information of all sorts which was crowded upon every available space—the number of cross-country sections shown on some of them being extraordinary.

In addition were issued Palæontological maps, Mineralogical maps, and many others, specialising more and more as additional students came into the field. Then followed the maps of Geikie and his contemporaries, and finally those issued by the Geological Survey—at first hand-coloured, but now colour-printed, and as nearly perfect as it is possible to be, in this country.

Besides these separately published maps, mention should be made of the beautiful examples of mapping occurring in various valuable monographs issued in the early part of last century, MacCulloch's ' Western Isles of Scotland,' Phillips' ' Geology of Yorkshire,' and Mantell and Dixon's monographs on Sussex Geology being representative of their kind.

Privately-printed maps of the geology of portions of our country exist, the three most noteworthy perhaps being Sanders' map of the Bristol Coalfield, Jordan's ' London District,' and Elias Hall's ' Lancashire Coalfield.'

That of William Sanders was the most elaborate and complete geological map ever privately published in Great Britain. It was in nineteen sheets, each 30 in. by 24 in., on the scale of 4 in.=1 mile, and was issued to subscribers at 3l. 19s. plain, 4l. 19s. coloured. It was published by Lavars, of Bristol, from whom I obtained my copy, said to be a ' second edition,' though I cannot find that it differs in any way from the original issue. Two hundred and twenty parishes around Bristol are included ; every line is the result of Sanders' own work, which occupied the summer months for twelve years. Each of the nineteen sheets covers about 45 square miles. So reliable was his work that the Geological Survey practically adopted it, or at any rate included the information it contained, for the Government maps, as was fully acknowledged by De la Beche in vol. i. of the ' Survey Memoirs,' 1864, page 126, as follows : ' We are here anxious to acknowledge the great assistance the Geological Society [Survey] has derived from the labours of Mr. William Sanders, of Bristol, who most handsomely placed at the disposal of the Survey his beautiful maps of the country bounded by the Severn and Bristol Channel from Purton Passage to Clifden, and thence inland by Chipping Sodbury, in one direction, and by Keynsham and Newton on the other, to the Week rocks, a large area, and containing very complicated ground.'

Jordan's map first appeared as ' Stanford's Geological Map of London showing superficial Deposits, compiled by J. B. Jordan ' (22½ in. by 24½ in., scale 1 in.=1 mile). This was followed by ' Stanford's Library Map of London geologically coloured by James B. Jordan,' in 24 sheets, each 16 in. by 13 in., and on the scale of 6 in.=1 mile. Nine different beds between the Alluvium and the Chalk are defined on this fine map. In the following year, 1878, ' Stanford's Geological Map of London and its Suburbs ' was also in 24 sheets, and on the scale of 6 in.=1 mile. It includes the country between Wimbledon on the south-west and Hampstead on the north-west, Leyton on the north-east, and Beckenham on the south-east.

Contrasting with these two reliable and careful pieces of work is ' A Mineralogical and Geological Map of the Coal Field of Lancashire, with parts of Yorkshire, Cheshire, and Derbyshire, by Elias Hall [1832].' It measures 51 in. by 38 in., and is on the scale of 1 in.=1 mile. The representations of the strata are more peculiar than reliable, and the author's knowledge of the fossil contents of the beds, as shown by the ' Verticle [sic] Section,' is both ' extensive and peculiar.' Elias Hall copies Smith and Greenough by publishing sections of the rocks covered by the map, and in 1836 he issued a 32 pp. pamphlet as an ' Introduction to his Map.'

A handy modern volume is ' Stanford's Geological Atlas of Great Britain and Ireland,' edited by the late H. B. Woodward, which contains coloured maps of the counties, upon which are indicated the various collecting grounds for fossils, in addition to which is a general introduction to the geology of the country. But this atlas has ' evolved.' William Smith's incomplete Geological atlas of the counties was the start. In its present form the work began as ' Reynolds' Geological Atlas of Great Britain,' published in 1860, twenty-eight colours being used. But the plates, before being geologically coloured, were engraved by John Emslie and originally appeared in Reynolds' County Atlas in 1849. A ' New ' [second] edition of this work, entirely re-set throughout, was issued by Reynolds in 1864. In 1889 another edition was described as the ' second ' edition, but was really the third. In 1904 appeared ' Stanford's

Geological Atlas,' based on Reynolds' atlas, with plates of characteristic fossils, etc. In this Emslie's plates were still being used. Stanfords issued a 'second' edition of their atlas in 1907, and a 'third' (actually the sixth) edition in 1914. In 1913 was issued a 'Photographic Supplement' to the atlas. These different editions with their varying county maps are occasionally to be picked up at a reasonable figure, and the maps of any particular county might be exhibited in order to show the additions made to geological knowledge from time to time.

In urging the delegate, therefore, to collect, vigorously, while there is still an opportunity, I do so with every confidence that future students will be grateful for the efforts made. Few people yet realise the valuable information to be derived from an examination of a series of maps of any particular area.

It is quite possible at times that maps may be obtained which do not bear any date. In many cases the only clue to the date of the publication is in a review, or in the advertisements on the cover of the 'Geological Magazine' or other publications, though unfortunately the almost general practice of destroying the covers when binding the volumes results in much useful information being lost.

Except in a few rare cases even the publishers do not keep any particulars of the dates of the appearance of the various editions of maps they issue, nor can they supply lists of their own maps.

I trust in the preceding remarks the scientific value of the collection and study of maps has been demonstrated. In recent years there has been occasional evidence of their proper appreciation, but it should be more systematic and continuous. Certain districts have received careful and proper attention, others and far greater tracts of our islands do not seem to have received any. Without attempting to give a complete list of recent publications on the subject I may mention, as admirable examples of work : the Reigate sheet of the one-inch Ordnance Survey; 'A Study in the Geography of the Surrey Hills,' by Ellen Smith, published by A. & C. Black, London, in 1910, which is accompanied by six large maps; and a similar work issued by the same house in 1911, entitled 'Highlands of South-west Surrey : a Geographical Study in Sand and Clay,' by E. C. Matthews, with seven maps. In addition, there are the regional surveys such as those carried out by Mr. C. E. Fagg, of the Croydon Society, and a 'Regional Study of North-east England,' by C. B. Fawcett, in the 'Geographical Teacher' issued a few weeks ago. Of a more specialised character, but still indicating progress in the evolution of mapping, are the Botanical Survey sheets of Yorkshire by Drs. Smith and Woodhead, and similar publications relating to different areas in Scotland.

That more attention should be paid to maps is shown by the volumes which have recently appeared dealing entirely with the question of reading them. Probably at no period in the world's history have the advantages of accurate cartography been so vital as during the past few sad years, when it might safely be said that the future of the world's history largely depended upon the care expended in the preparation of the maps of North-west Europe.

Two centuries ago it was stated 'Most Students in Geographie take more delight to contemplate the remotest and most barbarous Countries of the earth, than lightly to examine the Descriptions of their owne.' I am afraid that the same remarks applied to the years which have since passed, but we now seem to be reaching the time when in our schools, in our Scientific Societies, and in the country generally, more attention is being paid to the geographical problems at home, and these can probably best be solved by an examination of our country's maps : this examination being facilitated if collections are made at convenient centres in various parts of the British Isles.

After a brief discussion Mr. A. H. Garstang, Secretary of the Railway Facilities Sub-Committee of Section F, read a paper on 'Railways and their Obligations to the Community.'

At the second meeting on Friday, August 27, a discussion on 'The Status of Local Societies : the means of developing their objects, of getting new members, of publishing papers and making announcements,' was opened by Mr. William Whitaker, and the following resolution was passed :—

Status of Societies.

'That this Conference suggests to the Council that a further meeting be held in London to which the Officers as well as the Delegates of the Corresponding Societies be invited.'

A further resolution arising out of Mr. Garstang's paper (read at the previous meeting) was proposed by Sir Edward Brabrook as follows, and carried :—

Railway Facilities.

'Having regard to the many important questions at issue with regard to Railway management this Conference recommends that the Association should urge upon the Government the reappointment of a Royal Commission such as that which was appointed in 1913, but in consequence of the War broke up without reporting.'

LIST OF PAPERS

BEARING UPON THE ZOOLOGY, BOTANY, AND PREHISTORIC ARCHÆOLOGY OF THE BRITISH ISLES, ISSUED DURING JUNE-DECEMBER, 1919.*
COMPILED BY T. SHEPPARD, M.SC., F.G.S., THE MUSEUM, HULL.

AT a recent meeting of the Corresponding Societies' Committee held at Burlington House, London, at which some of the General Officers of the British Association were present, the question of the continuance of the annual lists of publications received was discussed, and the present writer was asked to prepare a report on the subject, and to continue the preparation of the lists in a modified form. Subsequently the following report was submitted :—

' In the past the bibliography issued by the British Association has been limited to papers appearing in those publications forwarded by the Corresponding Societies, it has embraced practically all the sciences, related to any part of the earth (or the heavens), and it has ended each year on May 31. In this way the lists were incomplete, and contained much that was useless. For instance, of the fifty-eight papers relating to Mathematical and Physical Science, in the bibliography issued in our last report, thirty-three were published by the Royal Astronomical Society of Canada, five by the Royal Society of South Africa, leaving twenty titles to represent a year's work of British mathematicians and physicists. Similarly, in Chemistry, there are eight entries, five of which relate to South Africa, thus leaving three to represent the work of the British chemists—two of these papers being printed by the Mining Engineers, and one by the Cotteswold Club.

' True, as a catalogue of the publications sent to our library, the list is complete, but in its present scrappy form it is of little use scientifically, as indicated by the fact that the publications in the rooms of the British Association are seldom consulted.

' I would suggest that in future the bibliography be confined to papers and memoirs bearing upon the fauna, flora and prehistoric archæology of the British Isles, and include all published, whether sent to the Association or not, and that the lists close each year on December 31. This can be accomplished by printing the items June 1 to December 31, 1919, in our Report for 1920, each subsequent volume to contain the complete bibliography for the preceding year.

' It is not desirable to duplicate work already being accomplished by special societies. For example, Astronomy, Chemistry, Geography, Statistics, Electricity, Physiology, Agriculture and Engineering are already well catered for by the journals or other official organs issued by the supporters of those sciences. The lists of geological literature issued by the Geological Society of London, which have been in abeyance during the war, are, I understand, to be continued and completed, and if, in future, that Society could be prevailed upon to include *all* papers on British Geology, whether in its library or not (very few additional entries would enable this to be accomplished), the necessity for publishing geological items in the British Association's bibliography would be dispensed with.

' This would practically leave the Sciences of Zoology, Botany, and Prehistoric Archæology, in their various ramifications, to be dealt with in our lists.

' In addition to the papers on these subjects appearing in the publications of the various societies in the British Isles, and those relating to these islands which are published abroad, the contents of the weekly, monthly, and quarterly scientific magazines dealing with specific portions of the British Isles should be included. Similarly, suitable papers bearing upon the three sciences mentioned, appearing in any of the engineering, agricultural, geological or other publications, would, of course, be noted.

' With these suggested alterations, omissions and additions, the bibliography might be kept within reasonable limits and be of distinct value to British science.

' I may add that I have the list for June 1 to December 31, 1919, well in hand, and I do not anticipate any serious difficulty in the compilation of the lists for future years.

<div align="right">'T. SHEPPARD.'</div>

* The last list compiled by the late H. C. Stewardson included the months January to May 1919, but, as was stated, included only titles of papers in publications actually sent to the Corresponding Societies' Committee.

Some correspondence with the Geological Society ensued, resulting in receipt of the following letter, which enables us to leave papers bearing upon Geology to the Geological Society.

Geological Bibliography of the British Isles.

Dear Mr. SHEPPARD,—In reply to your letter of the 8th inst., I hasten to thank you for your kind offer to supply particulars of such British geological serials and papers as we do not receive here, and have no hesitation in undertaking to have them included in our future lists of geological literature, so that these may include everything published *in this country* relating to the British Isles.

<div align="center">Yours very truly,
L. L. BELINFANTE, Permanent Secretary.</div>

The preparation of the list here printed, as well as that for 1920, which is in progress, prompts the following observations :—

Titles of Papers.

I should like to appeal to contributors to journals, and particularly to editors, to insist on seeing, as far as possible, that the title of a paper gives some indication of the nature of its contents ; for if a bibliography is to be of service at all, it must, in addition to giving the titles of papers, indicate to what they refer. Thus, when Natural History and Archæological articles are headed, ' A Find ' ; ' A Puzzle ' ; ' A Combat ' ; ' Battle Royal ' ; or ' A Curious Find ' ; ' Dorset ' ; ' Vanessa io ' ; these headings have necessarily to be quoted, in addition to which the bibliographer has to insert what should have been the correct title to the paper, in this way considerably adding to the amount of work and to the cost of printing. Authors, and especially young ones, may be pardoned for making errors of this sort, but editors should be able to put them right.

There is a further difficulty to be contended with, which should be corrected, as the habit is growing more prevalent in our scientific journals. As far as possible the title should be brief and clearly convey the subject of the articles. For instance, a student searching for records, say, of the marten in Shropshire, could easily search through the lists for a heading, ' Marten in Shropshire.' Judging, however, from titles examined during the past few weeks, the record might easily occur under ' On,' ' Notes on,' ' Stray Notes on,' or even ' Memorandum of,' ' The Occurrence of,' or ' Record of ' a Marten in Shropshire, or many other varieties. It seems unnecessary to head a record, ' Notes on ' or ' Record of,' as this is obvious.

Another means of causing extra work and expense is the way in which authors appear under various names. In one journal with which we are familiar, during the last few months notes have appeared by a welcome contributor, as by, say, ' C. T. J.,' ' C. T. Jones,' ' Chris. T. Jones,' or ' Chris. Thomas Jones.' Had they all appeared under ' C. T. Jones,' or ' Chris. T. Jones,' one entry of the name would suffice, and the various contributions could be placed in alphabetical or datal order.

A further means of causing unnecessary confusion and search is when two authors who write conjointly change the names about, and one month a paper is by ' Smith and Jones,' and the next by ' Jones and Smith.' It is obvious there should be consistency, for when, as happens in one well-known journal, each joint author has three long names, which are given in different forms, and sometimes one set is placed first and sometimes another, the numbers of references and cross-references become irksome.

Another source of inconvenience in quoting references is the growing habit of issuing what are described as ' double numbers,' which are consequently double the usual price, presumably because they appear bi-monthly instead of monthly. There is not much objection to the words ' double number ' appearing on the cover for the benefit of the publisher and for the enlightenment of the subscriber, but when both months are named and both parts to the volume are indicated, and when every issue is a so-called ' double number,' the question of giving proper reference becomes exceedingly complicated, quite apart from the time and space occupied. One publication recently received is for July and October, but was ' published August 24.' It is also ' Nos. 7 and 8 ' of the volume, and a ' double number,' although it contains only twenty-four pages. It is difficult to say what objection there can be to numbering each part separately, and adding the month of publication. If, as in the

case of some societies' publications, a journal is behind-hand in its issues, there is no difficulty in adding that it is issued ' for ' such a month of such a year. This need not interfere with the actual number of the volume or part, or date.

To save expense and for convenience of reference, all varieties of an author's name will, in the following list, be included under the most complete form given (thus, C. T. J., C. T. Jones, Chris. T. Jones and Chris. Thomas Jones will all appear under the last named). Similarly, in the case of double numbers, the date of the first month and the number of the first part only will be given ; thus a journal Vol. 66, Parts 1 and 2, for July and October, issued in August, will be quoted as ' Vol. 66, Part 1, July.'

As the following list has been prepared at short notice, it may not be absolutely complete, but it includes references to all the papers relating to the British Isles occurring in the journals devoted to general natural history, entomology, botany, conchology, archæology, etc., as well as in the proceedings of the various London and Provincial scientific societies. Next year it is hoped to print a list of the publications which have been examined.

In the past the list of papers published by the Corresponding Societies' Committee has been classified according to the different sciences. Now that the number is much smaller, Zoology, Botany and Prehistoric Archæology only being dealt with, and that quite a large proportion of the titles refer to two or sometimes three of these subjects, it seems desirable to avoid repetition by making one alphabetical list, and indicating the subject dealt with at the front by the letters Z, B, and P respectively.

The references to journals, etc., are given in the usual abbreviated forms ; thus, *Ent.* = The Entomologist ; *New Phy.* = The New Phytologist ; *Geol. Mag.* = The Geological Magazine ; *Sci. Progr.* = Science Progress, and so on..

B Z ANON. Proceedings, Summer Session [Reports of Excursions]. *Ann. Rep. Proc. Belfast Nat. F. Club*, 1918–19, pp. 10–15.

B Z —— Winter Session, *loc. cit.*, pp. 16–26.

Z —— Report of the Council. *Ann. Rep. Yorks. Phil. Soc. for* 1918, pp. vi.–xxiv.

Z —— Additions and Corrections to the Hand-List of British Birds (Third List). *Brit. Birds.* June, pp. 2–4.

Z —— Spring Immigration of Jackdaws on the Hampshire Coast, *loc. cit.* Aug., p. 80.

Z —·— Recovery of Marked Birds, *loc. cit.* Oct., pp. 125–128.

Z —— Short Notes, *loc. cit.* June, p. 32 ; July, pp. 63-64 ; Aug., p. 87 ; Sept., pp. 111–112 ; Dec., p. 198.

Z —·— *Colosoma sycophanta* L. at Exmouth. *Ent. Mo. Mag.* Aug., p. 180.

B Z —— Insects and Fungi on Grass Land, *loc. cit.*, pp. 181–182.

Z —— The Dollman Collection, *loc. cit.* June, pp. 135–136.

Z —— Current Notes and Short Notices. *Ent. Rec.* June, pp. 113–115 ; July, pp. 134–136 ; Aug., pp. 173–175 ; Oct., pp. 188–191 ; Nov., pp. 210–211 ; Dec., pp. 226–229.

Z —— The South London Entomological and Natural History Society [Report], *loc. cit.* June, pp. 118–120 ; July, pp. 137–139 ; Aug., p. 176 ; Oct., pp. 191–192 ; Nov., pp. 211–212.

Z —— Lancashire and Cheshire Entomological Society [Report], *loc. cit.* June, p. 120 ; July, p. 140.

Z —·— The Entomological Society of London [Report], *loc. cit.* June, pp. 115–118.

B Z —— Essex Field Club : Reports of Meetings [May 11, 1918, to March 29, 1919]. *Essex Nat.* Vol. xix., pt. 1, pp. 31–47.

B Z —·— Proceedings of the [Glasgow Naturalists'] Society. *Glasgow Nat.* Sept., p. 82. Excursions [Aug. 21, 1915, to May 23, 1916], *loc. cit.*, pp. 93–111.

B —— *Potamogeton panormitanus* in Ireland [Note of record by Arthur Bennett, which see]. *Irish Nat.* Sept., p. 106.

B —— *Tolypella glomerata* var. *erythrocarpa* [Note of record by J. Groves and G. R. Bullock-Webster, which see], *loc. cit.*, p. 106.

B Z —— Dublin Microscopical Club [Report], *loc. cit.* June, p. 79 ; Nov., p. 133.

B —·— Watson Botanical Exchange Club Report [Notice of]. *Journ. Bot.* Nov., pp. 314–318.

Z   ANON.   Entomological [First Finds], *loc. cit.*
Z   ——   The Common Partridge (*Perdix cinera*), *loc. cit.*, pp. 10–11.
B P Z ——   Excursion to Hellingly, The Caburn Earthworks.   *Trans.*, etc., *East-bourne Nat. Hist. etc. Soc.*   Aug., pp. 91–92.
Z   ——   The Gardener's Friends and Foes, *loc. cit.*, pp. 96–97.
B   ——   Onion Mildew (*Peronospora Schleideni* Ung.).   *Ministry of Ag. & Fisheries.*   Leaflet No. 178, 4 pp.
P Z   ——   Instructions for Collectors.   *Brit. Mus.* (*Nat. Hist.*) No. 6, Mosquitoes (Culicidæ).   4th ed., 8 pp., No. 10.   Plants, 5th ed., 10 pp.
B Z   ——   Sectional Meetings and Field Work in 1918.   *Proc. Ashmolean N. Hist. Soc.* for 1918, pp. 17–18.
B Z   GEORGE ABBEY.   Variations in the Diet of Wild Birds.   *Journ. Wild Bird Inves. Soc.*   No. 1, pp. 10–11.
B Z   W. J. LEWIS ABBOTT.   Implements from Cromer Forest Bed and the Admiralty Section.   *Proc. Prehist. Soc. E. Anglia.*   Vol. III., pt. i., pp. 110–114.
Z   W. M. ABBOTT.   Large Flock of Ring Doves in Spring.   *Irish Nat.*   July, p. 94.
Z   B. G. ADAMS.   *Eupitheria albipunctata* var. *angelicata* in Surrey.   *Ent.* June, pp. 138–139.
P   S. O. ADDY.   House-Burial, with Examples in Derbyshire.   *Journ. Derby Arch. etc. Soc.*   Vol. XLI., pp. 86–161.
B Z   ROBERT ADKIN.   Devastation of Oak Trees by Spring Larvæ.   *Ent.*   Aug., pp. 187–188.
Z   ——   The Sydney Webb Collection, *loc. cit.*   Dec., pp. 275–276.
Z   ——   The Mitford Collection, *loc. cit.*, p. 276.
Z   H. E. ALDRIDGE.   Luminous Worms.   *Nature.*   Oct. 30, p. 174.
Z   H. G. ALEXANDER.   Winter Immigration of Goldcrests and Firecrests in Kent.   *Brit. Birds*, July, p. 56.
B Z   E. J. ALLEN.   A Contribution to the Quantitative Study of Plankton.   *Journ. Marine Biol. Assoc.*   July, pp. 1–8.
Z   J. W. ALLEN.   *Epuraea distincta* Grimmer, A Beetle New to Britain.   *Ent. Mo. Mag.*   June, pp. 128–129.
B   J. C. ALSOP.   Botanical Section [Report].   *Rep. Marlborough Coll. Nat. Hist. Soc.* No. 67, pp. 21–28.
         ——   Malacological Report, *loc. cit.*, p. 34
         ——   Ornithological List, *loc. cit.*, pp. 36–38.
Z   JOSEPH ANDERSON.   Saint Mark's Fly (*Bibio marci*).   *Ent. Rec.*   Nov., p. 207.
Z   ——   *Sirex gigas* at Chichester, *loc. cit.*, pp. 206–207.
Z   ——   *Colias edusa* at Chichester.   *Ent.*   Nov., p. 259.
Z   ——   Notes from Chichester [Entomology], *loc. cit.*   Dec., p. 279.
Z   PETER ANDERSON.   White Snipe and Ruff in Tiree.   *Scot. Nat.*   Sept., p. 156.
Z   W. H. N. ANDREWS.   Rough-legged Buzzards in Norfolk.   *Brit. Birds.*   June, p. 31.
Z   E. T. ANQUETIL.   *Opisthograptis luteolata* feeding on Laurel.   *Ent.*   Nov., p. 257.
Z   W. J. ARKELL.   *Polygonia c-album* in Wiltshire, *loc. cit.*   Nov., p. 261.
Z   ——   *Pocota apiformis* in Berkshire., *loc. cit.*
Z   ARTHUR ARNOLD.   Green Woodpecker in the Isle of Wight.   *Brit Birds.*   Sept., pp. 109–110.
B   S. ARNOTT.   The Colchicum, or Meadow Saffron.   *Trans. Dumfries. & Gall. Nat. Hist. & Ant. Soc.*   Vol. 6, pp. 27–30.
Z   W. J. ASHDOWN.   *Agrilus lunatus* in Surrey.   *Ent. Rec.*   Aug., p. 170.
Z   G. H. ASHE.   *Platypus cylindrus* in Worcestershire.   *Ent. Mo. Mag.*   Nov., pp. 260–261.
Z   A. ASTLEY.   Status of the Yellow Wagtail in Westmorland.   *Brit. Birds.*   Oct., pp. 134–135.
Z   ——   Crossbill in Westmorland, *loc. cit.*   Dec., p. 194
Z   HOWARD ATKINS.   Young Garden-Warbler fed on Moths, *loc. cit.*   Sept., p. 108.
Z   JASPAR ATKINSON.   White Grouse Chick.   *Nat.*   Sept., p. 294.
Z   ——   Accident to a Skylark, *loc. cit.*

Z   A. BACOT and L. LINZELL. The Incubation Period of the Eggs of *Haematopinus asini*. *Parasitology.* Oct., pp. 388–392.

Z   RICHARD S. BAGNALL. On the Discovery of two species of *Brachychœteumidœ*, a Minor Group of Millepedes peculiar to the British Isles. *Ann. & Mag. Nat. Hist.* Aug., pp. 79–84.

Z   —— Brief Description of new *Thysanoptera*, *loc. cit.* Oct., pp. 253-277.

Z   —.— A Curious Coccid-like Midge (*Rhabdophaga pseudococcus*). *Vasculum.* Dec., pp. 175–177.

Z   —— See J. W. H. HARRISON.

B Z   —— and J. W. H. HARRISON. Talks about Plant Galls. VI.—The Wasp Galls of the British Oak. *Vasculum.* July, pp. 127–134.

Z   R. H. BAILLIE. Grey Wagtail Nesting in Warwickshire. *Brit. Birds.* June, p. 27.

Z   G. E. BAKER, E. REID. Ornithology. Lepidoptera [Records]. *Proc. Liverpool Nat. F. Club* for 1918, p. 39.

B   —— See H. J. HOLME.

Z   C. L. E. BALCOMB. *Colias edusa* in Hampshire. *Ent.* Oct., p. 235.

Z   R. C. BANKS. *Pomatorhine Skua* in Monmouthshire. *Brit. Birds.* Dec., p. 197.

B P   W. BARCLAY. Report on Summer Excursions, 1918. *Trans. & Rep. Perth. Soc. N. Sci.* Vol. VII., pt. I., pp. vii–x.

B   —— [Presidential Address]. *Trans. Perth. Soc. Nat. Sci.* Vol. VI., pt. v., pp. cxci–cc.

Z   W. H. BARROW. Status of the Little Owl in Leicestershire. *Brit. Birds.* June, p. 30.

Z   —— See F. C. R. JOURDAIN.

Z   JAMES BARTHOLOMEW. Tree-sparrows nesting near Glasgow. *Scot. Nat.* Sept., p. 166.

Z   —— Number of Young in Stoat's Family, *loc. cit.* Nov., p. 181.

B   W. C. BARTON. Report of the Distributor for 1918. *Rep. Bot. Soc.* Vol. V, pt. iv., pp. 483-535.

Z   W. BATESON. The Progress of Mendelism. *Nature.* Nov. 6, pp. 214–216.

Z   LUCY JANE BAWTREE. Birds feeding upon Winged Ants. *Brit. Birds.* Nov., p. 160.

Z   EVELYN V. BAXTER. See LEONORA JEFFREY RINTOUL.

Z   —— and LEONORA JEFFREY RINTOUL. Behaviour of Birds under Abnormal Conditions. *Scot. Nat.* Nov., p. 182

B Z   PETER BAXTER. A Perthshire Naturalist—James Stewart McGregor of Glenisla. *Trans. Perth. Soc. Nat. Sci.* Vol. VI., pt. v., pp. 183–187.

Z   H. A. BAYLIS. On a curious Malformation in *Tœnia saginata*. *Ann. & Mag. Nat. Hist.* Sept., pp. 114–118.

Z   M. BEDFORD. Hobby in Bedfordshire. *Brit. Birds.* July, p. 58.

Z   H. BEESTON. The Non-Marine Mollusca of Llandudno and District. [*Journ. Conch.* Aug., pp. 128–132.

Z   ALFRED BELL. Fossil Shells from Wexford and Manxland. *Irish Nat.* Oct., pp. 109–114.

Z   H. BENDORF. See ANNIE DIXON.

B   ARTHUR BENNETT. Utricularia [size of]. *Journ. Bot.*, Sept., p. 260.

B   —— *Helosciadium inundatum* L. (Koch), f. *fluitans* (Fr.). Prahl., *loc. cit.*

B   —— *Vaccinium intermedium* Ruthe, *loc. cit.* Oct., pp. 284–285.

B   —— *Potamogeton dualus* Hagström, *loc. cit.*, p. 285.

B   —— *Carex montana* L., *loc. cit.*, p. 322.

B   —— *Calamagrostis stricta* Timm., forma *pilosior* Norman, *loc. cit.*, pp. 322-323.

B   —— *Cypripedium Calceolus* L. *Nat.* Nov., p. 372.

Z   ERNEST H. BENNIS. *Carabus clathratus* in Co. Clare. *Irish Nat.* July, p. 91.

Z   HOWARD BENTHAM. Large Flock of Hawfinches in Surrey and Kent. *Brit. Birds.* June, p. 26.

Z   H. DOUGLAS BESSEMER. Larvæ of *Papilio machaon* in Sussex. *Ent.* Aug., p. 189.

Z   MARY G. S. BEST. Smews off Aberdeenshire. *Brit. Birds.* June, p. 32

Z   FRED. S. BEVERIDGE. The Status of the Redshank in the Outer Hebrides. *Scot. Nat.* Nov., pp. 185–186.

Z   FRED. S. BEVERIDGE. Golden Eagle in North Uist., *loc. cit.*, pp. 195–196.

B   J. BINTNER. Silver Leaf Disease. *Bull. Misc. Information, Kew.* No. 6, pp. 241–263.

P   A. HENDERSON BISHOP. Note on a Burial after Cremation. *Trans. Dumfries. & Gall. Nat. Hist. & Ant. Soc.* Vol. 6, pp. 44–48.

Z   JAMES E. BLACK. Coleoptera at Dunster, Somerset. *Ent. Mo. Mag.* Oct., p. 231.

Z   JOHN E. BLACKIE. Variation in *Chrysophanus phlœas, Arica medon*, and *Nisoniades tages. Ent.* Oct., p. 234.

Z   —— *Attacus prometheus* in England, *loc. cit.*

B Z   F. M. BLADON. Oak Apples. *Sedgwick Soc. Rep* [Sedbergh]. No. 1, pp. 12–14.

Z   K. G. BLAIR. Some Notes on *Catonia aurata. Ent. Mo. Mag.* Sept., pp. 200–203.

Z   —— *Lytta vesicatoria* L. in Norfolk and in the Isle of Wight, *loc. cit.*, p. 207.

Z   F. L. BLATHWAYT. Vertebrata [Report]. *Trans. Lincs Nat. Union for 1918*, pp. 114–115.

Z   —— The Birds of Lincolnshire, Past, Present and Future, *loc. cit.*, pp. 121–133.

Z   —— The Late Dr. J. Wiglesworth [Obituary]. *Brit. Birds.* July, pp. 52–55.

Z   ARTHUR BLISS. Note on *Stauropus fagi* and *Smerinthus ocellatus. Ent.* Nov., p. 262.

Z   GEORGE BOLAM. Bank Vole (*Evotomys glareolus* Schr.) nesting above ground. *Lanc. & C. Nat.* Aug., pp. 51–54.

Z   —— Some Winter Wildfowl. *Vasculum.* Dec., pp. 145–152.

B Z   HERBERT BOLTON. The British Museum and Art Gallery. Report for . . . . 1919. 16 pp.

Z   C. J. BOND. On Certain Factors concerned in the production of eye colour in birds. *Journ. Genetics.* Dec., pp. 69–81.

Z   F. BOOTH. See ANON.

Z   H. A. BOOTH. A Wood-Pigeon Ruse. *Brit. Birds.* Nov., p. 165.

Z   HARRY B. BOOTH. H. A. Paynter, 1846–1919 [Obituary]. *Nat.* Aug., pp. 275–276.

Z   —— Blackbirds using the same nest twice, *loc. cit.*, p. 302.

Z   —— Common Seals in Morecambe Bay, *loc. cit.*, p. 370.

B   G. S. BOULGER. The Cryptogams of Andrews's Herbarium. *Journ. Bot.* Dec., pp. 337–340.

Z   MORRIS BOURNE. *Agrotis simulans* in Oxfordshire. *Ent.* Nov., p. 262.

B Z   F. O. B[OWER]. Prof. J. W. H. Trail, F.R.S. [Obituary]. *Nature.* Sept. 25, pp. 76–77.

Z   W. BRADBROOK. Newton Longville Parish Books [wild cats, polecat, etc.]. *Records of Bucks.* Vol. XI. No. I., pp. 1–10.

Z   HILDA K. BRADE-BIRKS and S. GRAHAM BRADE-BIRKS. Notes on Myriopoda, xviii. Report on Chilopoda and Diplopoda for the latter part of 1918. *Lanc. & C. Nat.* Oct., pp. 101–106.

Z   S. GRAHAM BRADE-BIRKS. Dragon-fly in the Darwen District [Correction]. *Lanc. & C. Nat.* Aug., p. 61.

Z   —— Luminous Worms. *Nature.* Sept. 11, p. 23 ; Oct. 2, pp. 93–94.

Z   —— See HILDA K. BRADE-BIRKS.

Z   F. G. S. BRAMWELL. The Larva of *Ino globulariæ. Ent.* Sept., pp. 214–215.

Z   —— Aberrations of *Argynnis aglaia, Brenthis euphrosyne* and *B. selene* at Brighton, *loc. cit.* Oct., p. 236.

Z   —— *Limenitis sibylla* at Brighton, *loc. cit.* Nov., p. 257.

B   WINIFRED E. BRENCHLEY. The Uses of Weeds and Wild Plants. *Sci. Progr.* July, pp. 121–133.

B   WILLIAM B. BRIERLEY. Some Concepts in Mycology—an attempt at Synthesis. *Trans. Brit. Mycol. Soc.* Sept., pp. 204 *et seq.*

Z   H. BRITTEN. Hemiptera, Heteroptera and Homoptera in Lancashire and Cheshire in 1918. *Lanc. & C. Nat.* July, pp. 15–19.

Z   —— Hymenoptera. Caraphractus cinctus Hal. in Manchester in 1918, *loc. cit.* Aug., pp. 47–51.

Z   —— Coleoptera. Interesting Records by Local Collectors in Lancashire and Cheshire during 1917 and 1918, *loc. cit.* Nov., pp. 126–130, and Dec., pp. 163-167.

Z   H. BRITTEN. Parasitic Worms in Sticklebacks from Whitworth Park, Manchester, *loc. cit.* Dec., p. 170.

Z   —— Reputed Nest of *Vespa crabo* L. in Lancashire, *loc. cit.*, p. 175.

B   [JAMES BRITTEN]. *Mimulus moschatus* L. *Journ. Bot.* Oct., p. 285.

B   C. E. BRITTON. Note on *Centaurea*, *loc. cit.* Dec., pp. 340–342.

Z   ARTHUR BROOK. The Merlin. *Animal World.* July, pp. 75–76.

Z   —— The Dipper, *loc. cit.* Oct., pp. 115–116.

B   F. T. BROOKS. An Account of Some Field Observations on the Development of Potato Blight. *New. Phy.* May, pp. 187–200.

Z   EDMUND R. BROWN. Teratological Variations in the Wings of Lepidoptera. *Lanc. & C. Nat.* Sept., pp. 70–72.

Z   JAMES MEIKLE BROWN. *Sirex gigas* in Sheffield. *Nat.* Sept., p. 292.

Z   —— Some Derbyshire Plant Galls, *loc. cit.* Oct., pp. 330–332.

P   G. BROWNEN. Hedenesbury or Hengistbury of Prehistoric Time. [Abs.]. *Man.* Oct., p. 158.

B   J. P. BRUNKER. Plants of Co. Louth. *Irish Nat.* July, p. 95.

Z   B. BRYAN. See ANON.

P   CHARLES BULLA. The Bronze Age in Ulster [Abs.]. *Ann. Rep. Proc. Belfast Nat. F. Club*, 1918–19, pp. 20–21.

B   G. R. BULLOCK-WEBSTER. See J. GROVES.

Z   P. F. BUNYARD. Weights of Cuckoos' Eggs. *Brit. Birds.* Oct., p. 144.

Z   —— Observations on the Cuckoo, *loc. cit.* Nov., pp. 166–167.

Z   J. P. BURKITT. The Wren. *Irish Nat.* July, pp. 85–89.

Z   —— Relation of Song to the Nesting of Birds, *loc. cit.* Sept., pp. 97–104.

Z   A. I. BURNLEY. Marine Biology at Scarborough. *Nat.* Nov., pp. 363–364.

Z   A. E. BURRAS. *Sciapteron tabaniformis. Ent.* Dec., pp. 276–277.

B   W. H. BURRELL. See W. E. L. WATTAM.

B   —— See T. SHEPPARD.

Z   C. R. N. BURROWS. Catalogue of Palæarctic Psychides. *Ent. Rec.* Aug., pp. 165–167.

Z   —— Records from Mucking, *loc. cit.*, pp. 168–169.

Z   —— The Larvæ of *Hydroecia crinanensis* and that of *Apamena leucostigma* (*fibrosa*), *loc. cit.*, pp. 177–178.

P   HENRY BURY. Some Palæolithic Problems. *Sci. Progr.* July, pp. 81–97.

Z   HERBERT BURY. Diptera. Supplementary List for 1918. *Lanc. & C. Nat.* Oct, pp. 107–110.

Z   E. A. BUTLER. Hemiptera in Jersey. *Ent. Mo. Mag.* June, pp. 137–138.

Z   —— *Lasiacantha capucina* Germ. A Tingid Bug new to the British List, *loc. cit.* Sept., pp. 203–204.

Z   E. P. BUTTERFIELD. *Cucullia verbasci* near Bingley. *Nat.* Aug., p. 278.

Z   —— Cause of Melanism in *Phigalia pilosaria* [with note by G. T. Porritt], *loc. cit.*, pp. 279–280 ; Oct., pp. 339–340 ; Nov., pp. 373–374.

Z   —— More *Plusia moneta*, *loc. cit.* Oct., p. 337.

Z   —— Blackbirds using the same nest twice, *loc. cit.*, p. 373.

Z   —— See W. GYNGELL.

Z   —— See W. E. L. WATTAM.

Z   R. BUTTERFIELD. See W. E. L. WATTAM.

Z   W. R. B[UTTERFIELD]. Note on Protura and how to collect them. *Mus. Journ.* June, pp. 196–197.

Z   W. T. CALMAN. Marine Boring Animals Injurious to Submerged Structures. *Brit. Mus. (Nat. Hist.). Econ. Ser.* No. 10, 34 pp.

Z   M. CAMERON. *Aleuonota egregia* Rye and *Ocypus cyaneus* Payk. in Norfolk. *Ent. Mo. Mag.* Aug., p. 178.

Z   BRUCE CAMPBELL. Quail in Midlothian. *Scot. Nat.* Sept., p. 166.

Z   D. C. CAMPBELL. Hoopoe in Innishowen. *Irish Nat.* July, p. 93.

B   F. CAMPBELL-BAYARD. Report of the Botanical Section for 1918. *Proc. Croydon N. & Sci. Soc.* Vol. VIII., pt. v., p. clxvi.

Z   HERBERT CAMPION. *Limenitis sibylla* Linn. at Burnham Beeches. *Ent.* Oct., pp. 236–237.

Z   A. E. J. CARTER. *Colias edusa* in Forfarshire. *Scot. Nat.* Nov., p. 199.

Z   —— Diptera in Perthshire. *Ent. Mo. Mag.* Oct., p. 233.

Z   A. E. J. Carter. [Diptera at Aberfoyle], *loc. cit.* Oct., p. 233. Also noticed in *Scot. Nat.* Nov., p. 202.

Z   B. A. Carter. Greenshank in Warwickshire. *Brit. Birds.* July, p. 61.

Z   J. W. Carter. *Sympetrum sanguineum* Mull., a Dragon-fly new to Yorkshire. *Nat.* Dec., p. 399.

B   Nellie Carter. On the Cytology of two Species of *Characiopsis*. *New Phy.* May, pp. 177–186.

B   —— Studies on the Chloroplasts of Desmids. *Ann. Bot.* July, pp. 295–304.

Z   H. E. Castens. Black-necked Grebe on Kent and Sussex Border. *Brit. Birds.* July, p. 60.

Z   G. C. Champion. Note on a dark form of *Liopus nebulosus* Linn. *Ent. Mo. Mag.* July, p. 158.

Z   —— Another note on the habits of *Melanophila acuminata* De Geer, *loc. cit.* Aug., pp. 177–178.

Z   —— Hemiptera, etc., in the New Forest, *loc. cit.* Sept., p. 209.

Z   Edgar Chance. Observations on the Cuckoo. *Brit. Birds.* Sept., pp. 90–95.

Z   Abel Chapman. Bird-Notes on the Borders, 1918–19. *Vasculum.* Dec., pp. 152–155.

Z   T. A. Chapman. Note on *Hoplocampa testudinea* Klug. *Ent. Mo. Mag.* June, p. 138.

Z   —— *Trichiosoma tibiale* and *Acampsia pseudospretella*, *loc. cit.*, pp. 138–139.

Z   S. A. Charters. *D. nerii* at Eastbourne. *Ent. Rec.* Oct., p. 188.

Z   —— Late appearance of *Agriades coridon*, *loc. cit.*, p. 226.

Z   Chris. A. Cheetham. Yorkshire Diptera Notes. *Nat.* July, p. 244.

Z   —— *Xiphura atrata* L. in Yorkshire, *loc. cit.* Dec., p. 380.

B   —— Yorkshire Naturalists' Union, Botanical Section, *loc. cit.*, p. 389.

Z   —— Additions to Yorkshire Diptera, *loc. cit.*, pp. 394–395.

B Z   —— See T. Sheppard.

B Z   —— See W. E. L. Wattam.

B Z   Miller Christy. Hornets, Wasps, and Flies sucking the sap of Trees. *Essex Nat.* Vol. XIX., pt. I., pp. 10–14.

Z   —— On the Arboreal Habits of Field Mice, *loc. cit.*, pp. 18–21.

B   —— Fungus on Stem of Oak Tree, *loc. cit.*, p. 48.

B   —— Samuel Dale (1659 ?–1739) of Braintree, Botanist, and the Dale family : some genealogy and some portraits, *loc. cit.*, pp. 49–64 [continued in 1920].

B   A. H. Church. The Plankton-phase and Plankton-rate. *Journ. Bot.* June Supplt., pp. 1–8.

B   —— The Building of an Autotrophic Flagellate. *Bot. Memoirs.* No. 1, pp. 1–27.

B   —— Thalassiophyta and the Subærial Transmigration. No. 3, pp. 1–95.

B   —— Elementary Notes on Structural Botany, *loc. cit.* No. 4, pp. 1–27.

B   —— Elementary Notes on Reproduction of Angiosperms, *loc. cit.* No. 5, pp. 1–24.

B   —— Brunfels and Fuchs. *Journ. Bot.* Sept., p. 233.

B   —— Historical Review of the Phæophyceæ, *loc. cit.* Oct., pp. 265–273.

B   —— Historical Review of the Florideæ.—I., *loc. cit.* Nov., pp. 297–304; II., Dec., pp. 329–334.

B   —— The Ionic Phase of the Sea. *New Phy.* Oct., pp. 239–247.

Z   Richard Clapham. Footprints of the Wild. *Animal World.* Dec., pp. 137–138.

B   J. Edmund Clark. Garden Phenology, Asgarth, Riddlesdown Road, Purley. *Proc. Croydon N. H. & Sci. Soc.* Vol. VIII., pt. v., pp. clxvii–clxviii.

Z   Wm. Eagle Clarke. The Starlings of Shetland, Fair Isle, and St. Kilda. *Scot. Nat.* Nov., pp. 183–185.

Z   —— Wild Swans observed on the Western Islands in Summer, *loc. cit.*, pp. 196–197.

B   W. G. Clarke. Three Old Essex Herbaria. *Essex Nat.* Vol. XIX., pt. I., pp. 23–25.

B   —— Some Essex Plant Records, *loc. cit.*, pp. 47–48.

P   —— W. Allen Sturge, M.V.O., M.D., F.R.C.P. [Obituary]. *Proc. Prehist. Soc. E. Anglia.* Vol. III., pt. I., pp. 12–13.

P W. G. CLARKE. The Distribution of Flint and Bronze Implements in Norfolk, *loc. cit.*, pp. 147–148.

Z W. J. CLARKE. Large Yorkshire Trout. *Nat.* July, p. 245.

Z W. G. CLUTTEN. Colour Variation of *Odezia atrata. Ent.* Nov., p. 256.

Z C. GRANVILLE CLUTTERBUCK. *Pselnophorus brachydactylus* in Gloucestershire, *loc. cit.* Dec., p. 275.

Z E. A. COCKAYNE. Inheritance of Colour in *Diaphora mendica* Cl. and var. *rustica* Hb. *Ent. Rec.* June, pp. 101–104.

P C. V. COLLIER. Thomas Boynton, F.S.A. [Obituary]. *Yorks. Arch. Journ.* Pt. 98., pp. 271–272.

Z WALTER E. COLLINGE. Some Remarks on the Food of the Barn-owl (*Strix flammea* Linn.). *Journ. Wild Bird Inves. Soc.* No. 1, pp. 9–10.

Z —— Two interesting cases of Melanism, *loc. cit.*, p. 15.

Z —— Strange accident to a heron, *loc. cit.*

Z —— Wild Birds and Distasteful Insect Larvæ. *Nature.* July 24, p. 404 ; Aug. 21, p. 483.

B E. J. COLLINS. Sex Segregation in the Bryophyta. *Journ. Genetics.* June, pp. 139–146.

Z A. H. COOKE. 'Ground' Clausilias. *Journ. Conch.* Aug., p. 102.

Z J. E. COOPER. See ANON.

Z A. STEVEN CORBET. Argynnids in Wiltshire. *Ent.* Dec., p. 278.

Z H. H. CORBETT. Woodcock near Doncaster. *Nat.* Oct., p. 336.

Z —— Cormorant at Doncaster, *loc. cit.* Nov., p. 371.

Z —— See W. E. L. WATTAM.

Z ERNEST CORNELL. Some Notes on the Butterflies of the South Coast of the Isle of Wight, 1919. *Ent.* Dec., pp. 279–280.

B A. D. COTTON. The Occurrence of Oak Mildew on Beech in Britain. *Trans. Brit. Mycol. Soc.* Sept., pp. 198–200.

B —— *Entomogenous* Fungi new to Britain, *loc. cit.*, pp. 200–203.

B —— and E. M. WAKEFIELD. A Revision of the British Clavariae, *loc. cit.*, pp. 164–198.

Z T. A. COWARD. Night-Heron in Anglesey. *Brit. Birds.* July, pp. 58–59.

Z —— One pair of Meadow-pipits feeding two young Cuckoos, *loc. cit.*, p. 139.

Z —— Notes on the Vertebrate Fauna of Lancashire and Cheshire. *Lanc. & C. Nat.* Oct., pp. 89–100 ; Dec., pp. 159–162.

Z E. CRISP. *Papilio machaon* in Sussex. *Ent.* Oct., p. 236.

Z —— *Hyloicus pinastri* in Suffolk, *loc. cit.* Nov., pp. 257–258.

Z S. G. CUMMINGS. Unusual site for Chiffchaff's nest. *Brit. Birds.* June, p. 27.

Z —— Breeding Habits of the Nightjar, *loc. cit.*, pp. 27–28.

Z JOHN CURRIE. Common Scoter on Duddingston Loch in May. *Scot. Nat.* Nov., p. 197.

Z J. W. CUTMORE. Some Notes on the Rats of the Port of Liverpool. *Proc. and Trans. Liverp. Biol. Soc.* Vol. XXXIII., pp. 68, 69.

Z J. G. D. Hereward Chune Dollman, F.E.S. [Obituary]. *Ent. Mo. Mag.* June, pp. 139–140.

Z CHARLES A. DALLAS. Woodcocks perching on Trees. *Brit. Birds.* Oct., pp. 142–143.

B A. A. DALLMAN. *Galium erectum* Huds. in Cheshire. *Lanc. & C. Nat.* Sept., p. 73.

B —— *Geranium versicolor* L. in North Wales, *loc. cit.* Oct., p. 111.

Z J. FFOLLIOTT DARLING. Pollan in Lough Ree. *Irish Nat.* July, p. 93.

Z —— Incubation of Birds, *loc. cit.*, p. 93.

Z FRANCIS DARWIN. A Phænological Study. *New. Phy.* Nov., pp. 287–298.

Z G. MacDONALD DAVIES. Excursion to Norbury, Mitcham Common and Beddington. *Proc. Geol. Assoc.* Vol. XXX., pt. II., pp. 75–81.

Z W. DAVIES. Pied Flycatcher in Staffordshire. *Brit. Birds.* Sept., pp. 107–108.

Z J. T. DAWSON. Art Gallery and Museum. [Additions.] *48th Ann. Rep. Rochdale Public Lib.* etc. *Committee*, pp. 11–13.

Z FRANK H. DAY. Carlisle Natural History Society. [Report.] *Ent.* June, p. 143.

Z —— Westmorland Coleoptera. *Nat.* July, pp. 239–242 ; Oct., pp. 327–328.

Z J. DAVY DEAN. Occurrence of *Physa gyrina* Say in Great Britain. *Journ. Conch.* Aug., p. 127.

Z L. S. DEAR. Long-eared Owl laying twice in same nest. *Brit. Birds.* June, p. 30.

Z ANTHONY W. N. DISNEY. *Lycæna arion* in North Cornwall. *Ent.* Sept., p. 216.

Z ANNIE DIXON. Protozoa. Report on Gatherings from a Pond at Lawnhurst, Didsbury, from 14th March to 12th Sept., 1918. *Lanc. & C. Nat.* Sept., pp. 74–81.

Z —— W. LEACH, H. BENDORF, and J. G. KITCHEN. Manchester Microscopical Society [Report], *loc. cit.* Aug., pp. 61–63.

B H. N. DIXON. Mosses collected on Deception Island, South Shetlands, by Mr. James C. Robins [Abs.]. *Journ Bot.* July, p. 200.

B LEONARD DOBBIN. On the presence of Formic Acid in the Stinging Hairs of the Nettle. *Proc. Roy. Soc. Edinb.* Vol. XXXIX., pt. II., pp. 137–142.

Z HORACE DONISTHORPE. Wasps attacking Flies. *Irish Nat.* Sept., p. 107.

Z —— A New County Record for *Zeugophora flavicollis* Marsh. *Ent. Rec.* Oct., pp. 185–186.

Z —— The Myrmecophilous Lady-bird, *Coccinella distincta* Fold., its Life History and Association with Ants, *loc. cit.* Dec., pp. 214–222.

Z —— Further localities for *Platypus cylindrus* F. *Ent. Mo. Mag.* Sept., p. 232.

B H. DOWNES. *Juncus pygmæus* Rich. *Journ. Bot.* Sept., p. 260.

B G. CLARIDGE DRUCE. The Botanical Society and Exchange Club of the British Isles. Report for 1918. *Rep. Bot. Soc.* Vol. V., pt. III., pp. 267–271.

Z —— Plant Notes, etc., for 1918 (mostly New Plants to the British Isles), *loc. cit.*, pp. 272–318.

B —— Notes on Publications, New Books, etc., 1917–18, *loc. cit* , pp. 319–349.

B —— Obituaries. [J. E. Bagnall, William Brack Boyd, Edward Fry, Joseph John Geoke, Reginald Philip Gregory, Edward Walter Hunnybun, Ernest David Marquand, T. W. Martyn, William Frederick Millar, Bishop John Mitchinson], *loc. cit.*, pp. 349–365.

B —— New County and Other Records, *loc. cit.*, pp. 365–412.

B —— The Dates of Publication of Curtis's " Flora Londinensis," *loc. cit.*, pp. 412–414.

B —— Additions to the Berkshire Flora, *loc. cit.*, pp. 443–480.

B —— Additions to the Berkshire Flora. *Proc. Ashmolean N. Hist. Soc.* for 1918, pp. 21–58.

B MARTIN C. DUCHESNE. Afforestation : Its Practice and Science. *Rep. Brit. Assoc.* for 1918, pp. 68–75.

Z JAMES DUNCAN. *Colias edusa* near Aberdeen. *Ent.* Oct., p. 235.

B T. A. DYMES. Notes on the Life-history of the Yellow Flag (*Iris Pseudacorus* Linn.), with special reference to the seeds and seedlings during their first year. [Abs.]. *Journ. Bot.* Aug., pp. 231–232.

Z F. W. EDWARDS. *Gnophomyia tripudians* Bergroth : A New British Fly. *Ent. Mo. Mag.* Aug., pp. 176–177.

Z L. A. CURTIS EDWARDS. Continental Jays in Oxfordshire and Sussex. *Brit. Birds.* Sept., p. 107.

B ROSE EGERTON. See W. S. LAVEROCK.

Z J. STEELE ELLIOTT. Grey Wagtails nesting at a distance from water. *Brit. Birds.* Aug., p. 81.

Z —— Hobby in Shropshire and Worcestershire, *loc. cit.*, p. 84.

Z —— Large Numbers of Bramblings in Worcestershire, *loc. cit.* Dec., pp. 194–195.

Z GEORGE ELLISON. The Nest of the Bank Vole. *Proc. and Trans. Liverpool Biol. Soc.* Vol. XXXIII., pp. 65–66.

Z —— Note on a White Orkney Vole, *Microtus orcadensis, v. alba.*, *loc. cit.*, p. 67.

Z G. W. ELLISON. Bank Vole (*Evotomys glareolus*, Schr.) nesting above ground. *Lanc. & C. Nat.* Nov., pp. 124–125.

Z RICHARD ELMHIRST. *Orthocladius* Spp. breeding in the sea. *Scot. Nat.* Nov., pp. 193–194.

Z L. G. ESSON. *Zygæna achilleæ* in Argyllshire. *Ent.* Aug., p. 189.

Z —— *Colias edusa* in Scotland, *loc. cit.* Nov., p. 259.

Z   WILLIAM EVANS. *Boarmia gemmaria* in the Forth Area. *Scot. Nat.* Nov., pp. 199–200.

Z   J. COSSOR EWART. Telegony. *Nature.* Nov 6, pp. 216–217.

Z   R. W. THOMAS EWART. Capercaillie in Montrose. *Scot. Nat.* Sept., p. 156.

Z   WM. FALCONER. The Spiders of Yorkshire. *Nat.* July, pp. 235–238; Aug., pp. 267–270; Oct., pp. 323–326; Nov., pp. 365–368; Dec., pp. 400–403.

Z   —— New and Rare British Spiders, *loc. cit.* Sept., pp. 295–302.

Z   —— Additions to the ' Spiders of Wicken,' *loc. cit.* Nov., p. 356.

B Z   —— · Plant Galls from the Scarborough District, *loc. cit.* Dec., pp. 392–393.

B Z   —— See W. E. L. WATTAM.

Z   H. H. FARWIG. Late Nesting of Linnets in Surrey. *Brit. Birds.* Dec., p. 195.

Z   —— Grey Wagtails breeding in Kent and Sussex, *loc. cit.*, p. 196.

Z   —— Greenshanks in Surrey, *loc. cit.*, p. 198.

Z   ANDERSON FERGUSSON. *Aspidiphorus orbiculatus* Gyll., in Scotland. *Scot. Nat.* Nov., p. 200.

Z   —— *Staphylinus cæsareus*, Ceder. in Main Argyll, *loc. cit.*

Z   —— *Halyzia* 16-*guttata*, L., and *Coccinella conglobata*, L., in Main Argyll, *loc. cit.*, p. 201.

Z   —— Additions to the List of Scottish Coleoptera, *loc. cit.* Sept., pp. 167–169.

Z   J. DIGBY FIRTH. See W. E. L. WATTAM.

Z   EPHRAIM FISHER. Ornithology [Report]. *Ann. Rep. Huddersfield Nat. etc. Soc.*, 1918–1919, pp. 13–15.

Z   N. H. FITZHERBERT. Ornithological Record of Derbyshire, 1918. *Journ. Derby. Arch. etc. Soc.* Vol. XLI., pp. 170–178.

P   H. J. FLEURE. Correspondence [Dolmen in Guernsey : quotes letter from Col. Guérin]. *Man.* Sept., pp. 130–132.

Z   H. D. FORD. *Aspilates ochrearia* in Cumberland. *Ent.* July, pp. 167–168.

Z   —— *Zephyrus quercus*, var. *bellus*, *loc. cit.* Oct., p. 236.

Z   W. J. FORDHAM. William Ernest Sharp, 1856–1919 [Obituary]. *Nat.* Aug., pp. 274–275.

Z   —— See W. E. L. WATTAM.

Z   H. E. FORREST. Nightingales in Shropshire. *Nat.* Aug., pp. 277–278.

Z ·   —— Hoopoe in Shropshire. *Brit. Birds.* July, p. 57.

Z   —— Shifting of Breeding-grounds by Terns, *loc. cit.*, pp. 61–62.

Z   —— Bar-tailed Godwits in Shropshire, *loc. cit.*, p. 165.

Z   —— Little Owl in Montgomeryshire, *loc. cit.*, p. 196.

Z   —— [N. E. FORREST, error.] Little Owl Breeding in Shropshire and Radnorshire, *loc. cit.* June, p. 30.

Z   R. FORTUNE. Herons nesting in Nidderdale. *Nat.* Nov., p. 371.

Z   —— Black-necked Grebe in Washburn Dale, *loc. cit.*

Z   —— See E. A. WOODRUFFE-PEACOCK.

Z   NEVIN H. FOSTER. Early Arrival of Redwings and Fieldfares. *Irish Nat.* Sept., p. 107.

Z   —— A List of the Myriapoda of Ulster. *Ann. & Mag. Nat. Hist.* Dec., pp. 395–407.

Z   W. W. F[OWLER]. [William E. Sharp, Obituary]. *Ent. Mo. Mag.* Nov., p. 263.

Z   HILDERIC FRIEND. *Sparganophilus :* A British Oligochæt. *Nature.* July 31, p. 426.

Z   —— Luminous Worms, *loc. cit.* Aug. 7, p. 446; Nov. 27, pp. 334–335.

Z   —— British Well-worms, *loc. cit.* Sept. 4, p. 5.

Z   —— A New British Enchytræid Worm, *loc. cit.* Oct. 30, p. 174.

Z   F. W. FROHAWK. *Nisoniades tages* Imbibing its Excretion. *Ent.* Sept., pp. 212–213.

Z   —— Variation of *Limenitis sibylla* in the New Forest, *loc. cit.*, p. 213.

Z   —— Variation of *Dryas paphia*, *loc. cit.*, pp. 235–236.

P Z   MARIAN FROST. Guide to the Worthing Museum and Art Gallery. pp. 1–16.

Z   GREEVZ FYSHER. See W. E. L. WATTAM.

Z   —— See T. SHEPPARD.

Z   J. GARDNER. *Plusia moneta*, F. at Hart, Co. Durham. *Ent.* June, p. 138.

B   L. V. LESTER GARLAND. New County Records [Botanical] for Argyle. *Journ. Bot.* Nov., p. 322.

Z   D. G. Garnett. *Agrotis præcox* in Westmorland. *Ent.* Nov., p. 257.

Z   Marjory Garnett. Coloration of the Soft Parts of some Birds. *Brit. Birds.* July, p. 62–63.

Z   Walter Garstang. Songs of the Birds. *Nat.* June, pp. 195–198.

Z   —— More Songs of the Birds, *loc. cit.* July, pp. 231–234. Sept., p. 281.

Z   —— Sea-fishery Investigations and the Balance of Life. *Nature.* Sept., 18, pp. 48–49.

Z   L. R. A. Gatehouse. American Blue-winged Teal in Anglesey. *Brit. Birds.* Aug., p. 85.

B Z   J. Brontë Gatenby. The Identification of Intracellular Structures. *Journ. Roy. Micro. Soc.* June, pp. 93–118.

Z   —— Notes on the Bionomics, Embryology, and Anatomy of Certain Hymenoptera Parasitica, especially of *Microgaster connexus* (Nees). *Journ. Linn. Soc. (Zool.).* No. 224, pp. 387–416.

Z   A. Giebeler. *Ino globulariæ.* *Ent.* Dec., p. 278.

Z   E. Leonard Gill. Crows, Rooks and Starlings *versus* Kestrels and Peregrine Falcons. *Brit. Birds.* June, pp. 23–25.

Z   —— Bird Notes from the Hancock Museum. *Vasculum.* Dec., pp. 156–157.

Z   Frederick Gillett. *Euproctis chrysorrhœa* on *Hippophaë rhamnoides*. *Ent.* Sept., pp. 215–216.

Z   C. T. Gimingham. Some Coleoptera taken in Hertfordshire in 1918. *Ent. Mo. Mag.* July, pp. 157–158.

Z   —— *Platyrrhinus latirostris*, F. at Long Ashton, Somerset, *loc. cit.*, p. 158.

Z   —— Some Coleoptera taken in Somersetshire, *loc. cit.* Aug., pp. 179–180—a correction, Sept., pp. 207–208.

Z   Hugh S. Gladstone. Kite in Kent in 1822. *Brit. Birds.* Aug., p. 84.

Z   —— A Naturalist's Calendar, kept by Sir William Jardine, Bart., LL.D., F.R.S., F.R.S.E., etc., at Jardine Hall, Dumfriesshire, from January 1 to May 31, 1829. *Trans. Dumfries. & Gall. Nat. Hist. & Ant. Soc.* Vol. VI., pp. 88–124.

Z   —— Observations on Carrion-crows. *Scot. Nat.* Sept., p. 166.

Z   —— Hawfinch nesting in Dumfriesshire, *loc. cit.*, p. 171.

Z   —— Albino Spotted Flycatcher, *loc. cit.* Nov., p. 195.

P   E. H. Goddard. Plans of Wiltshire Earthworks. *Wilts. Arch. & Nat. Hist. Mag.* No. CXXX. p. 352.

P   —— Romano-British Interments at Broad Town, *loc. cit.*, pp. 353–354.

P   —— Sarsens at Avebury, broken up, 1799, *loc. cit.*, p. 354.

P   —— Identification of Wiltshire Barrows, *loc. cit.*

P   —— Aldbourne, Flint Celts, *loc. cit.*, p. 355.

P   —— Bronze Implements found in Wiltshire not previously recorded, *loc. cit.*, pp. 359–360.

B Z   —— Lydiard Millicent Natural History Notes. Kentish Plover in Wilts ? Little Owl at Netherstreet. Rare plants, *loc. cit.*, pp. 364–365.

B   M. J. Godfery. The Problem of the British Marsh Orchids. *Journ. Bot.* June, pp. 137–142.

Z   W. Godfrey. *Spilosoma urticæ* in the Isle of Wight. *Ent.* June, p. 138.

Z   J. G. M. Gordon. The Lepidoptera of Wigtownshire. *Trans. Dumfries. & Gall. Nat. Hist. & Ant. Soc.* Vol. VI., pp. 156–167.

B   W. Balfour Gourlay. *Vaccinium intermedium* Ruthe. *Journ. Bot.* Nov., p. 322.

B   —— George Stephen West, M.A., D.Sc., F.L.S. (1876–1919), [Obituary], *loc. cit.* Oct., pp. 283–284.

B   —— and G. M. Vevers. *Vaccinium intermedium* Ruthe, *loc. cit.* Sept., pp. 259–260.

Z   Jane Gowan. The Kentish Glory Moth on Deeside. *Scot. Nat.* Nov., p. 200.

Z   Geo. Grace. Protection Coloration of Birds and Eggs. *Nature.* Aug. 7., p. 446.

Z   —— *Calosoma inquisitor* at Coniston. *Nat.* Sept., p. 292.

P   G. S. Graham-Smith. Anvil Stones : with Special Reference to those from Skelmuir, Aberdeenshire. *Proc. Prehist. Soc. E. Anglia.* Vol. III., pt. I., pp. 33.

Z   —— Further Observations on the Habits and Parasites of Common Flies. *Parasitology.* Oct., pp. 347–384.

Z C. GREATOREX. Some Ornithological Notes from Shetland. *Brit. Birds.* Nov., pp. 158–159.

Z WM. HARRY GREENAWAY. The Fulmar Petrel at Foula Isle during Winter. *Scot. Nat.* Sept., p. 170.

Z W. D. W. GREENHAM and C. B. LOWE. Entomological Section [Report]. *Rep. Marlborough Coll. Nat. Hist. Soc.* No. 67, pp. 28–34.

B T. GREENLEES and T. K. HOLDEN. The Flora of Bolton. *Lanc. & C. Nat.* Dec., pp. 153–158.

Z THOMAS GREER. Variation in *Diaphora mendica* in Ireland. *Ent. Rec.* Aug., p. 168.

Z —— Lepidoptera from East Tyrone. *Irish Nat.* Oct., pp. 118–119.

Z H. G. GREGORY. *Polygonia c-album* near Salisbury. *Ent. Rec.* Dec., p. 226.

Z PERCY H. GRIMSHAW. The Collection and Preservation of Diptera. *Scot. Nat.* Sept., pp. 151–156.

Z ERNEST GRINDEY. Gannets in Derbyshire. *Brit. Birds.* July, p. 59.

Z A. J. GROVE. The anatomy of the head and mouth parts of *Psylla mali*, the Apple-sucker, with some remarks on the function of the labium. *Parasitology.* Oct., pp. 456–488.

B W. B. GROVE. Mycological Notes IV. *Journ. Bot.* Aug., pp. 206–210.

B JAMES GROVES. *Tolypella glomerata* Leonh. in the Isle of Wight. *Journ. Bot.* July, p. 197.

B —— and G. B. BULLOCK-WEBSTER. New Variety of *Tolypella glomerata*, *loc. cit.* Aug., pp. 224–225.

P G. F. B. DE GRUCHY [Secretary]. Exploration of the Palæolithic Site known as La Cotte de St. Brelade, Jersey. Report of the Committee. *Rep. Brit. Assoc. for* 1918, p. 42.

P T. W. M. GUÉRIN. Notes on the Discovery of a Human Figure Sculptured on a Capstone of the Dolmen of Déhus, Guernsey [Abs.]. *Man.* Oct., p. 157.
See also under H. J. FLEURE.

B H. B. GUPPY. Plant-Distribution from the Standpoint of an Idealist. *Journ. Linn. Soc. (Bot.).* No. 299, pp. 439–472.

Z DONALD GUTHRIE. Some Bird Notes from South Uist. *Scot. Nat.* Sept., pp. 145–150, and Nov., pp. 187–192.

Z W. GYNGELL. Common Wild Birds of the Scarborough District. *Nat.* Aug., pp. 263–266 ; Oct., pp. 333–336.

Z —— Sounds that resemble the Songs and Calls of Birds, *loc. cit.* Sept., pp. 311–312.

Z —— [and] E. P. BUTTERFIELD. Former status of the Starling, *loc. cit.* June, p. 215.

Z F. H. HAINES. Argynnids in Dorsetshire. *Ent.* Oct., p. 237.

Z —— Orthoptera in Derbyshire, *loc. cit.*, pp. 238–239.

Z J. N. HALBERT. *Planorbis corneus* in Co. Dublin. *Irish Nat.* Nov., pp. 135–136.

Z —— See W. F. JOHNSON.

Z H. O. HALFORD. Notes from Godalming. *Ent.* Dec., p. 278.

Z ALBERT ERNEST HALL. Harmful and Useful Birds. *Nat.* July, p. 246.

Z —— [Note on Martyn's ' Figures of Non Descript. Shells '], *loc. cit.* Aug., p. 279.

Z H. M. HALLETT. Aculeate Hymenoptera in the Channel Islands. *Ent. Mo. Mag.* Nov., p. 262.

B Z W. HALLIDAY. Nature in the Isle of Lewis. *Animal World.* Aug., pp. 91–92.

B P S. H. HAMER (Secretary). The National Trust for Places of Historic Interest or Natural Beauty. Report, 1918–1919, 52 pp.

Z A. H. HAMM. Observations on the Horse Bot-Fly, *Gastrophilus equi*, F. *Ent. Mo. Mag.* Oct., pp. 229–230.

P SOREN HANSEN. On Posthumous Deformation of Fossil Human Skulls. *Man.* Aug., pp. 121–124. See notice in *Nat.* Sept., pp. 283–284.

Z J. RUDGE HARDING. Willow-tit in Ross-shire. *Brit. Birds.* Dec., p. 195.

Z —— Spotted Crake in Ross-shire, *loc. cit.*, pp. 197–198.

Z J. A. HARGREAVES. See W. E. L. WATTAM.

Z   S. F. Harmer. Progress of the Natural History Museum. *Nature.* Dec. 4
     p. 353.

P   H. S. Harrison. The Ascent of Man : A Handbook to the Cases illustrating
     the Structure of Man and the Great Apes. *Horniman Museum
     Handbook* No. 13. 74 pp.

Z   James M. Harrison. Iceland Gulls in the Orkneys. *Brit. Birds.* July,
     p. 62.

Z   —— Long-tailed Duck feeding on Grain [with note by F. C. R. Jourdain],
     loc. cit. Aug., pp. 85–86.

B   J. W. H. Harrison. Stray Notes on Plants. *Vasculum.* July, pp. 115–117.

Z   —— The Moth and the Candle, *loc. cit.* Dec., pp. 172–175.

Z   —— Studies in the Hybrid Bistoninae. III. The Stimulus of Heterozygosis.
     Journ. Genetics. Sept., pp. 259–265. IV. Concerning the Sex and
     related problems. Dec., pp. 1–38.

     —— A Preliminary Study of the effects of administering ethyl alcohol to the
     Lepidopterous insect *Selenia bilunaria*, with particular reference to
     the offspring, *loc. cit.*, pp. 39–52.

B Z   —— See R. S. Bagnall.

Z   —— R. S. Bagnall and J. E. Hull. Notes and Records. *Vasculum.*
     July, pp. 140–142.

Z   E. Hartert. Serins in Sussex. *Brit. Birds.* June, p. 26.

Z   —— See W. J. Williams.

Z   Ernst Hartert. See H. F. Witherby.

B   J. Hartshorn. See W. E. L. Wattam.

Z   B. S. Harwood. *Sarothrus areolatus* Htg. bred. *Ent. Mo. Mag.* Dec.,
     p. 280.

Z   P. Harwood. *Chaetocnema sahlbergi* Gyll. in Sussex, *loc. cit.*

Z   Maud D. Haviland and Frances Pitt. The Selection of *Helix nemoralis* by
     the Song-thrush (*Turdus musicus*). *Ann. & Mag. Nat. Hist.* June,
     pp. 525–531.

P   F. N. Haward. The Origin of the " Rostro-carinate implements " and other
     chipped flints from the Basement Beds of East Anglia. *Proc.
     Prehist. Soc. E. Anglia.* Vol. III. pt. I., pp. 118–146.

Z   H. C. Hayward. *Hesperia malvæ* in Derbyshire. *Ent.* July, p. 168.

Z   —— *Sesia asiliformis* feeding in the Wood of Birch in Company with *S.
     culiciformis*, *loc. cit.*, p. 190.

Z   —— Some Notes on Collecting Lepidoptera or Repton, 1918, with some
     records from other parts of the County. *Journ. Derby. Arch.* etc.
     Soc. Vol. XLI., pp. 179–182.

Z   F. W. Headley. Great Tit laying in an open nest. *Brit. Birds.* July,
     pp. 56–57.

Z   —— Adult Cuckoo killing nestling birds [with note by F. C. R. Jourdain],
     loc. cit., p. 57.

Z   E. W. Hendy. Field Notes on Nesting Kingfishers. *Brit. Birds.* June,
     pp. 28–29.

B Z   W. A. Herdman. The Marine Biological Station at Port Erin, being the
     Thirty-second Annual Report of the Liverpool Marine Biology
     Committee. *Proc. and Trans. Liverp. Biol. Soc.* Vol. XXXIII.
     pp. 25–38.

Z   —— An Address on Some Periodic Changes in Nature. Appendix A [to
     above Report], *loc. cit.*, pp. 39–50.

Z   —— Report on the Investigations carried on during 1918 in connection
     with the Lancashire Sea Fisheries Laboratory at the University of
     Liverpool, and the Sea-fish Hatchery at Piel, near Barrow, *loc. cit.*,
     pp. 71–84.

B Z   —— Andrew Scott and H. Mabel Lewis. An Intensive Study of the Marine
     Plankton around the South end of the Isle of Man. Part XI. *loc. cit.*
     pp. 95–105.

Z   T. F. Hewer. Odonata in Bristol District. *Ent.* Aug., p. 191.

P   H. Dixon Hewitt. Notes on Some Flint-chipping Sites at Risby, Suffolk.
     Proc. Prehist. Soc. E. Anglia. Vol. III., pt. I., pp. 67–72.

Z   A. Hibbert-Ware. Food of Little Owl. *Essex Nat.* Vol. XIX., pt. I.,
     p. 26.

B   W. P. HIERN. Eleventh Report of the Botany Committee. *Rep. and Trans. Devon. Assoc.* Vol. LI., pp. 114–129.

B   —— Clavis to Devonian Sedges. *Rep. Bot. Soc.* Vol. V., pt. III., pp. 414–416.

Z   IAN G. W. HILL. *Colias edusa* and its var. *helice* in the Edinburgh District. *Scot. Nat.* Nov., p. 199.

B   T. G. HILL. The Water Economy of Maritime Plants. *Sci. Progr.* July, pp. 60–80.

B   H. E. HILTON. Observations on Capillitia of Mycetozoa. *Journ. Quekett. Micro. Club.* No. 84, pp. 5–12.

Z   FRANK HIND. Birds of Skegness and District. *Trans. Lincs. Nat. Union for* 1918. pp. 134–142.

Z   EDWARD HINDLE. Sex Inheritance in *Pediculus humanus* var. *corporis.* *Journ. Genetics.* Sept., pp. 267–277.

Z   MARTIN A. C. HINTON. The Field Mouse of Foula. *Scot. Nat.* Nov., pp. 177–181.

Z   STANLEY HIRST. On Two new Parasitic Mites (*Myocoptes hintoni* [from Exeter] and *Psoroptes natalensis.* *Ann. & Mag. Nat. Hist.* June, p. 524.

Z   HAROLD HODGE. *Colias edusa,* etc., in West Cornwall. *Ent.* Nov., p. 260.

P   T. V. HODGSON. Memorandum of Flint Implements found on Dartmoor. *Rep. and Trans. Devon. Assoc.* Vol. LI., pp. 175–176.

B   H. S. HOLDEN. A Suggested Scheme for the Investigation of Marine Bacteria. *Journ. Marine Biol. Assoc.* July, pp. 136–140.

B   T. K. HOLDEN. See T. GREENLEES.

B   H. J. HOLME, G. E. BAKER, E. M. WOOD. Botany [Records]. *Proc. Liverpool Nat. F. Club* for 1918, p. 38.

Z   ARTHUR T. HOPWOOD. On Some Spurious Records of Southport Non-Marine Mollusca. *Lanc. & C. Nat.* Dec., pp. 168–169.

B   —— Intergrowth of Oak and Sycamore, *loc. cit.,* pp. 169–170.

Z   M. A. HORSFALL. Osprey in Yorkshire. *Brit. Birds.* Dec., pp. 196–197.

B   A. R. HORWOOD. Fourth Report of the Flora Committee, 1916–19. *Trans. Leicester Lit. & Phil. Soc.* Vol. XX., pp. 76–78.

Z   T. B. HOUGH [late]. Notes Regarding Bird Life in the Stewartry. *Trans. Dumfries. & Gall. Nat. Hist. & Ant. Soc.* Vol. VI., pp. 48–66.

Z   H. ELIOT HOWARD. Behaviour of a Cuckoo. *Nature.* July 31, p. 426.

B   W. O. HOWARTH. *Festuca rubra* near Cardiff : a Taxonomic, morphological and anatomical study of three sub-varieties of *Festuca rubra,* L. subsp. *eu-rubra* Hack., var. *genuina* Hack., growing near Cardiff, S. Wales. *New. Phy.* Nov., pp. 263–286.

P Z   HENRY HOYLE HOWORTH. Presidential Address [to Museums Association]. *Mus. Journ.* Aug., pp. 17–30.

Z   H. C. HUGGINS. Notes on Kentish Mollusca. *Journ. Conch.* Aug., pp. 104–105.

Z   J. E. HULL. Demodex and Follicular Mange. *Vasculum.* July, pp. 125–127.

Z   —— The Wolf [Wolf-spider] of the Shingle, *loc. cit.* Dec., pp. 178–181.

Z   —— See J. W. HARRISON.

Z   GEO. R. HUMPHREYS. Dotterel in Co. Dublin. *Brit. Birds.* July, p. 61.

Z   DOUGLAS G. HUNTER. Black-tailed Godwit in Forfarshire. *Scot. Nat.* Nov., p. 198.

Z   G. E. HUTCHINSON. *Notonecta halophila,* Edw. in Cornwall. *Ent. Mo. Mag.* Nov., p. 261.

Z   J. S. HUXLEY. Note on the Drumming of Woodpeckers. *Brit. Birds.* July, pp. 40–41.

Z   —— Some points in the Sexual Habits of the Little Grebe, with a note on the occurrence of vocal duets in birds, *loc. cit.* Oct., pp. 155–158.

B Z   A. D. IMMS. Grain Pests and their Investigation. *Nature.* June 26, pp. 325–326.

B   W. INGHAM. See W. E. L. WATTAM.

Z   COLLINGWOOD INGRAM. Incubation during the laying period. *Brit. Birds* July, p. 64.

Z   —— Down Tracts in Nestling Birds, *loc. cit.,* pp. 78–79.

Z   Geoffrey C. S. Ingram. On the Breeding of the Lesser Redpole in Glamorganshire. *Brit. Birds.* Oct., p. 136.

Z   J. J. The Fisheries and Scientific Research. *Nature.* July 17, pp. 385–386.

B   A. B. Jackson and A. J. Wilmott. *Barbarea rivularis* in Britain. *Journ. Bot.* Nov., pp. 304–306.

Z   Annie C. Jackson. See H. F. Witherby.

Z   Dorothy J. Jackson. Further Notes on Aphides collected principally in the Scottish Islands. *Scot. Nat.* Sept., pp. 157–165.

Z   J. Wilfrid Jackson. The Bristly Millipede at Saltwick Bay, near Whitby. *Nat.* July, pp. 243–244.

Z   —— 'Shell-Pockets' on Sand-Dunes on the Wirral Coast, Cheshire; and Notes on Ancient Land Surfaces. *Lanc. & C. Nat.* July, pp. 9–14, and Aug., pp. 39–44.

Z   —— and J. G. Kitchen. *Planorbis dilatatus* and *Physa heterostropha* in the River Tame, at Dukinfield, Cheshire, *loc. cit.* Nov., pp. 131–132.

P   A. W. Jamieson. Some Sussex implements. *Proc. Prehist. Soc. E. Anglia.* Vol. III., pt. I., pp. 108–110.

B   T. A. Jefferies. Natural Transformations in the Vegetation of Blackstone Edge. *Trans. Rochdale Lit. & Sci. Soc.* Vol. XIII. pp. 18–24.

B   Gertrude Jekyll. Pollination of *Viscum album.* *Journ. Bot.* Oct., p. 286.

Z   H. C. Jidden-Fisher. The Insects of East Grinstead District. *Ent.* Nov., p. 256.

Z   T. J. Joce. Swallows. *Rep. and Trans. Devon Assoc.* Vol. LI., p. 63.

B   J. W. Haigh Johnson. Fungi [Report]. *Ann. Rep. Huddersfield. Nat.* etc. *Soc.* 1918–1919, p. 19.

Z   —— See W. E. L. Wattam.

Z   W. F. Johnson. *Athous hirtus,* Herbst, a correction. *Irish Nat.* June, p. 80.

Z   —— *Ploiaria culiciformis* in Co. Armagh [with note by J. N. Halbert], *loc. cit.* July, p. 91.

Z   —— *Rhyssa persuasoria* in the counties of Down and Fermanagh, *loc. cit.* Oct., pp. 115–118.

Z   —— Entomological Notes for 1919, *loc. cit.* Nov., pp. 127–129.

Z   —— Irish Hymenoptera Aculeata in 1919, *loc. cit.* Nov., pp. 132–133.

B   Walter Johnson. The Jew's Ear Fungus (*Hirneola auricula-judæ,* Fr.). *Nat.* July, pp. 225–230; Aug., pp. 255–258; Sept., pp. 287–290; Oct., pp. 319–322.

Z   Jas. Johnstone. The Dietetic Value of Sprats and other Clupeoid Fishes. *Proc. and Trans. Liverp. Biol. Soc.* Vol. XXXIII., pp. 106–133.

Z   —— The Probable Error of a Bacteriological Analysis, *loc. cit.,* pp. 134–155.

Z   —— Pearl-like Concretions in Tripe, *loc. cit.,* pp. 156–158.

Z   K. H. Jones and A. S. Kennard. Notes on the non-Marine Mollusca observed in East Ross and the Orkney and Shetland Islands. *Proc. Malac. Soc.* Oct., pp. 146–152.

Z   R. Jones. See W. E. L. Wattam.

Z   Richard W. Jones. Velvet-Scoters in Summer off Carnarvonshire. *Brit. Birds.* Sept., p. 111.

P   T. A. Jones. Liverpool Geological Society [Report]. *Lanc. & C. Nat.* Dec., p. 174.

Z   Francis C. R. Jourdain. Ornithological Report, 1918. *Proc. Ashmolean N. Hist. Soc.* for 1918, pp. 11–13.

Z   —— Red-throated Divers in Derbyshire and Leicestershire [with note by W. H. Barrow]. *Brit. Birds.* July, p. 60.

Z   —— Large Clutch of Eggs of Little Grebe, *loc. cit.* Oct., p. 142.

Z   —— See James M. Harrison.

Z   —— See F. W. Headley.

Z   —— See A. Mayall.

Z   —— See W. H. Mullens.

Z   —— See J. H. Owen.

Z   —— See B. Van de Weyer.

Z   —— See H. F. Witherby.

Z   Norman H. Joy. Fierce Attack on a Cuckoo by a Meadow-Pipit. *Brit. Birds.* Oct., pp. 138–139.

Z D. KEILIN. On the life-history and larval anatomy of *Melinda cognata* Meigen (Diptera Calliphorinae) parasitic in the snail *Helicella (Heliomanes) virgata* da Costa, with an account of the other Diptera living upon Molluscs. *Parasitology.* Oct., pp. 430–455.

Z —— and G. H. F. NUTTALL. Hermaphroditism and other Abnormalities in *Pediculus humanus, loc. cit.*, pp. 279–328.

P H. G. O. KENDALL. Windmill Hill, Avebury, and Grime's Graves, Cores and Choppers. *Proc. Prehist. Soc. E. Anglia.* Vol. III., pt. I., pp. 104–108.

Z A. S. KENNARD. See K. H. JONES.

P Z —— and B. B. WOODWARD. Report on the Mollusca [Grime's Graves], *loc. cit.*, pp. 91–92.

Z —— —— On *Helix revelata*, Britt. auctt. (*non* Férussac *nec* Michaud), and the validity of Bellamy's name of *Helix subvirescens* in lieu of it for the British Mollusc. *Proc. Malac. Soc.* Oct., pp. 133–136.

Z —— —— On the Generic Names for the two British Ellobiidæ [*Olim auriculidæ*], *Myosotis*, Drapenaud (= *Denticulatus*, Montagu) and *bidentata*, Montagu) *loc. cit.*, pp. 136–139.

Z BART KENNEDY. Insects I have met. *Animal World.* July, p. 79.

Z —— My Window Sill [Birds], *loc. cit.* Dec., pp. 135–136.

Z JAMES H. KEYS. Coleoptera at the Lizard, Cornwall. *Ent. Mo. Mag.* Nov., pp. 259–260.

P H. KIDNER. Recent Discovery of an Unrecorded Type of Circular Earthwork in the New Forest [Abs.]. *Man.* Oct., p. 158.

Z E. BOLTON KING. *Euvanessa antiopa* in Warwickshire. *Ent.* Nov., pp. 260–261.

Z CHARLES KIRK. Great Spotted Woodpecker in Argyll. *Scot. Nat.* Nov., p. 185.

Z —— Hawfinch in Dumbartonshire, *loc. cit.*, p. 194.

Z D. J. BALFOUR KIRKE. Nesting of the Pied Flycatcher and Garden Warbler in Ross-shire, *loc. cit.*

Z MAUD KIRKWOOD. Black-tailed Godwits in Co. Mayo. *Irish Nat.* Sept., p. 108.

Z J. G. KITCHEN. See ANNIE DIXON.

Z —— See J. WILFRID JACKSON.

Z F. LAING. A Note on Four British Coccids. *Ent. Mo. Mag.* Oct., pp. 233–234.

Z —— Two species of British Aphides, *loc. cit.*, Dec., pp. 272–274.

Z —— Insects damaging lead, *loc. cit.*, pp. 278–279.

Z W. D. LANG. Old Age and Extinction in Fossils. *Proc. Geol. Assoc.* Vol. XXX., pt. III., pp. 102–113.

Z E. RAY LANKESTER. The Foundation of Biological Sciences. *Nature.* Nov. 6, pp. 198–201.

B J. LARDER. Size of specimens of *Acer campestre. Nat.* Nov., p. 372.

B C. E. LARTER. *Hypericum humifusum. Journ. Bot.* Oct., p. 287.

B WM. S. LAVEROCK. Presidential Address. ['The Collecting of Flowering Plants and Ferns and the Making of a Herbarium.'] *Proc. Liverp. Nat. F. Club. for* 1918, pp. 10–19.

B —— E. REID, ROSE EGERTON. The Field Meetings of 1918 [Report], *loc. cit.*, pp. 20–37.

Z A. K. LAWSON. Vitrea and Pyramidula Destroyed by Ants. *Journ. Conch.* Aug., p. 127.

P NINA F. LAYARD. The Mundford Pebble Industry. *Proc. Prehist. Soc. E. Anglia.* Vol. III., pt. I., pp. 150–157.

P —— Flint Tools showing well-defined finger-grips. *Proc. Suffolk Inst. Arch. & Nat Hist.* Vol. XVII., pt. I., pp. 1–12.

Z —— Remains of a Fossil Lion at Ipswich. *Nature.* Dec. 25, p. 413.

B Z W. LEACH. Manchester Microscopical Society [Reports]. *Lanc. & C. Nat.* Nov., p. 133.

Z —— See ANNIE DIXON.

Z MARIE V. LEBOUR. Feeding Habits of Some Young Fish. *Journ. Marine Biol. Assoc.* July, pp. 9–21.

Z —— The Food of Post-Larval Fish, *loc. cit.*, pp. 22–47

z MARIE V. LEBOUR. The Young of the Gobiidæ from the Neighbourhood of Plymouth, *loc. cit.*, pp. 48–80.

z —— Further Notes on the Young Gobiidæ from the Neighbourhood of Plymouth, *loc. cit.*, pp. 146–148.

B A. LEE. See E. WARHURST.

B JOHN R. LEE. Some Clydesdale Sphagna. *Glasgow Nat.* Sept., pp. 71–72.

B F. ARNOLD LEES. The Nightmare of Names in the bed of Roses. *Nat.* June, pp. 211–213.

B —— *Cypripedium Calceolus*: Earliest Record, *loc. cit.* Oct., p. 341 ; also Dec., p. 399.

B —— The Distribution of *Gentiana verna, loc. cit.* Dec., pp. 390–391.

z H. MAXWELL LEFROY. See K. M. SMITH.

z W. HAROLD LEIGH-SHARPE. The Genus *Lernaeopoda*. Including a description of *L. mustelicola* n.sp., remarks on *L. galei* and further observations on *L. scyllicola*. *Parasitology*. Oct., pp. 256–266.

z G. B. C. LEMON. *Silpha atrata* L., with Abnormal Antennæ. *Ent. Rec.* Oct., p. 185.

z —— Notes on Coccinellidae, *loc. cit.*, pp 213–214.

z H. MABEL LEWIS. See W. A. HERDMAN.

z STANLEY LEWIS. Introduction of Red Grouse into Somerset. *Brit. Birds.* Aug., p. 86.

z —— Rock-Dove absent from Cheddar Cliffs, *loc. cit.*, p. 111.

z T. LEWIS. Young Buzzard takes a Shower-bath. *Brit. Birds.* Oct., pp. 140–141.

z L. LINZELL. See A. BACOT.

B GULIELMA LISTER. A Three-spurred form of the Larger Butterfly Orchis (*Habenaria chlorantha* Bab. var. *tricalcarata* Helmsley). *Essex Nat.* Vol. XIX., pt. I., p. 22.

B —— Mycetozoa found during the Selby Foray. *Trans. Brit. Mycol. Soc.* Sept., pp. 88–91.

B J. E. LITTLE. Notes on Bedfordshire Plants. *Journ. Bot.* Nov., pp. 306–312.

z S. H. LONG. Osprey in the Norfolk Broads. *Brit. Birds.* July, p. 58.

z W. ARTHUR LONG. *Catocala nupta* ob. *Ent.* Nov., p. 261.

z C. B. LOWE. See W. D. W. GREENHAM.

z LEWIS R. W. LOYD. Little Owl in South Devon. *Brit. Birds.* Nov., p. 164.

z W. J. LUCAS. Notes on British Orthoptera in 1918. *Ent.* Aug., pp. 171–174.

z —— *Agriades corydon* in the New Forest, *loc. cit.* Oct., p. 237.

z —— *Pararge megœra*, Linn., *loc. cit.*

z —— *Vespa crabro, loc. cit.*, p. 239.

z —— Orthoptera in Captivity, *loc. cit.* Nov., pp. 249–250.

z —— Preserving Orthoptera, *loc. cit.*, pp. 250–252.

z —— Dorset Orthoptera, *loc. cit.* Dec., p. 280.

z —— The Odonata of the Lancashire and Cheshire District, *Lanc. & C. Nat.* Aug., pp. 55–60.

z G. T. LYLE. Contributions to our knowledge of the British Braconidæ. *Ent.* June, pp. 134–136 ; July, pp. 149–155 ; Aug., pp. 178–181.

z D. MACDONALD. Wigeon nesting in Ross-shire. *Scot. Nat.* Sept., p. 171.

z —— The Whooper Swan in Ross-shire in June, *loc. cit.*, Nov., p. 196.

z —— Whooper in Ross-shire in June. *Brit. Birds.* Oct., p. 141.

P CHARLES MCINTOSH. Note on a Stone Cist found at Dalguise. *Trans. Perth Soc. Nat. Sci.* Vol. VI., pt. v., p. 206.

z [W. C.] M'INTOSH. The Fisheries and the International Council. *Nature.* July 3, pp. 355–358 ; July 10, pp. 376–378.

z —— Sea-Fishery Investigations and the Balance of Life, *loc. cit.*, Sept. 18, p. 49.

z T. THORNTON MACKEITH. The Nightjar in Renfrewshire. *Scot. Nat.* Sept., p. 166.

P THOMAS MCLAREN. Note on a Stone Cist found at Kildinny, near Forteviot, November, 1917. *Trans. Perth. Soc. Nat. Sci.* Vol. VI., pt. v., pp. 201–203.

P —— Bronze Age Burial Urns and other Remains found at Sheriffton, near Scone, December, 1917, *loc. cit.*, pp. 203–205.

z M. C. MCLEOD. *Catocala fraxini* [near Eastcote]. *Ent.* Aug., p. 189.

z —— *Colias edusa* in Surrey., *loc. cit.*, Oct., p. 235.

Z   A. HOLTE MACPHERSON. Wild Hybrid between House-sparrow and Tree-sparrow. *Brit. Birds.* Dec., p. 199.

Z   WM. PERCY MAIL. Haunts of the Black-headed Gull. *Vasculum.* July, pp. 97–101.

Z   G. W. MALCOLM. Unusual Nesting-place of Grey Wagtail. *Scot. Nat.* Nov., p. 195.

B   M. MALONE. See W. E. L. WATTAM.

Z   WM. MANSBRIDGE. Lancashire and Cheshire Entomological Society [Report]. *Ent.*, June, p. 144 ; *Lancs. & Ches. Nat.* July, pp. 26–27.

Z   —— Lepidoptera. Report of the Recorder for 1918. *Lanc. & C. Nat.* Aug., pp. 44–47.

P   R. R. MARETT. Recent Archæological Discoveries in the Channel Islands.— (1) La Cotte de St. Brelade [Abs.]. *Man.* Oct., p. 157.

Z   A. W. MARRIAGE. Multiple Nests of Blackbird. *Brit. Birds.* Sept., pp. 108–109.

P   J. G. MARSDEN. Cone Cultures at the Land's End. *Proc. Prehist. Soc. E. Anglia.* Vol. III., pt. I., pp. 59–66.

B   E. S. MARSHALL. Notes on Somerset Plants for 1918. *Journ. Bot.* June, pp. 147–154 ; July, pp. 175–181.

B   —— *Barbarea rivularis* in England, *loc. cit.*, Aug., pp. 211–212.

B   —— *Verbascum thapsiforme* as a British Plant, *loc. cit.*, Sept., pp. 257–258.

Z   JAMES M'L, MARSHALL. Woodcock and Young. *Scot. Nat.* Nov., p. 198.

B   F. A. MASON. See W. E. L. WATTAM.

Z   G. W. MASON. Entomology. [Report.] *Trans. Lincs. Nat. Union* for 1918, p. 113.

Z   W. W. MASON. Jackdaw's Unusual Nesting Site. *Nat.* Oct., p. 318.

Z   HERBERT MASSEY. J. J. Lister on Hodgkinson's Record of *P. ægon* in Tutt's "British Butterflies." *Ent.* June, p. 137.

Z   —— Late Third Brood of Swallows. *Brit. Birds.* Nov., p. 161.

Z   —— Weights of Cuckoos' Eggs, *loc. cit.*, Dec., p. 198.

Z   GERVASE F. MATHEW. Notes on Butterflies. *Ent.* Oct., pp. 227–228.

Z   —— *Chœrocampa nerii* at Dovercourt, *loc. cit.*, p. 237.

Z   —— *Issoria (Argynnis) lathonia* and *Colias edusa* at Folkestone and Dover, *loc. cit.* Nov., pp. 259–260.

P   J. L. MAXIM. Discovery of a Bloomery at Birches, Healey. *Trans. Rochdale Lit. & Sci. Soc.* Vol. XIII., pp. 94–99.

P   —— Querns and other Corn-grinding Stones recently found in the Rochdale District, *loc. cit.*, pp. 100–102.

B Z   E. KELLY MAXWELL. The Amateur Microscopist during Wartime. *Journ. Quekett Micro. Club.* No. 85, pp. 63–72.

Z   HERBERT MAXWELL. Antlers. *Trans. Dumfries. & Gall. Nat. Hist. & Ant. Soc.* Vol. VI., pp. 12–21.

Z   A. MAYALL. Abnormal Clutches of Chaffinch's Eggs. With note by F. C. R. Jourdain. *Brit. Birds.* Aug., pp. 80–81.

Z   C. M. MAYOR. *Colias edusa var. pallida (helice).* *Ent.* Nov., p. 260.

Z   —— Eugonia polychloros in Devon, *loc. cit.*, p. 261.

Z   D. H. MEARES. Great Crested Grebes nesting in Kent. *Brit. Birds.* July, pp. 5 and 60.

Z   W. S. MEDLICOTT. Wood-Lark in North Lincolnshire, *loc. cit.* June, p. 26.

Z   —— Goshawk in Lincolnshire, *loc. cit.* Nov., p. 164.

Z   R. MEINERTZHAGEN. A Preliminary Study of the Relation between Geographical Distribution and Migration, with special reference to the Palæarctic Region. *Ibis.* July, pp. 379–392.

Z   C. MELLOWS. *Agriades corydon* in the New Forest. *Ent.* Nov., p. 262.

Z   —— *Vespa crabro*, *loc. cit.*, pp. 262–263.

B   J. COSMO MELVILL. Teratology in *Papaver orientale*. *Journ. Bot.* Aug., p. 226.

Z   JAMES MENZIES. Note on a Rare Myxomycete. *Linbladia effusa* Rost. *Trans. and Proc. Perth Soc. N. Sci.* Vol. VII., pt. I., p. 1.

Z   —— A List of the Discomycetes of Perthshire, *loc. cit.*, pp. 2–27.

Z   EDWARD MEYRICK. Habits of *Pancalia leuwenhoekella*. *Ent. Mo. Mag.* July, p. 160.

B   F. E. MILSOM. See W. E. L. WATTAM.

Z   R. S. Mitford. *Hylotrupes bajulus* near Weybridge. *Ent. Rec.* Dec., pp. 222–223.

Z   C. B. Moffat. The Rev. Charles William Benson, M.A., LL.D. [Obituary]. *Irish Nat.* June, pp. 73–78.

Z   —— *Leucophasia sinapis* in Co. Wexford, *loc. cit.* Sept., p. 106.

P   J. Reid Moir. The Antiquity of Man. *Nature.* Nov. 27, p. 335.

P   —— A Piece of Carved Chalk from Suffolk. *Man.* Dec., pp. 183–186.

P   —— A few notes on the sub-crag flint implements. *Proc. Prehist. Soc. E. Anglia.* Vol. III., pt. I., pp. 158–161.

B   Horace W. Monckton. The Flora of the Bagshot District. *Journ. Bot.* Sept., pp. 251–257.

Z   H. J. Moon. Turtle-Dove breeding in North Lancashire. *Brit. Birds.* Aug., p. 86.

Z   F. D. Morice. *Tenthredella flavicornis* F. at Lichfield. *Ent. Mo. Mag.* June, pp. 133–135.

Z   —— *Lygaeonematus wesmaeli* Tischb., a hitherto unrecorded British Sawfly (from Yorkshire), *loc. cit.* Sept., pp. 204–206.

Z   B. Morley. Yorkshire Entomology [in 1919]. *Nat.* Dec., pp. 407–408.

Z   —— See W. E. L. Wattam.

Z   Claude Morley. *Ocypus cyaneus* Payk. in Suffolk. *Ent. Mo. Mag.* July, p. 159.

Z   —— A few insects in the New Forest, *loc. cit.* Sept., pp. 208–209.

Z   —— *Caratomus megacephalus*, Fab. *Ent.* Aug., pp. 186–187.

Z   —— Historical Note : Nicholas Gwyn, *loc. cit.* Oct., p. 233.

Z   —— Stilpnid Ichneumons, *loc. cit.*, pp. 233–234.

Z   —— Dragonfly at Light, *loc. cit.* Nov., p. 255.

Z   Gordon Morrison. *Manduca atropos* in Co. Durham, *loc. cit.* Aug., p. 189.

B Z   Charles Mosley. Natural History Report. *Ann. Rep. Huddersfield Nat.* etc. *Soc.*, 1918–1919, pp. 4–7.

Z   —— Mammalia [Report], *loc. cit.*, p. 13.

Z   —— Entomology [Report], *loc. cit.*, pp. 15–18.

Z   W. H. Mullens. The Ruff—an early record. *Brit. Birds.* June, pp. 13–23.

Z   —— H. Kirke Swann and F. C. R. Jourdain. A Geographical Bibliography of British Ornithology. Part I., Nov. 7, pp. 1–96.

Z   J. F. Musham. Conchology [Report]. *Trans. Lincs. Nat. Union* for 1918, pp. 112–113.

Z   —— William Denison Roebuck, M.Sc., F.L.S. [Obituary], *loc. cit.*, pp. 116–117.

B   L. F. Newman and G. Walworth. A Preliminary Note on the Ecology of Part of the South Lincolnshire Coast. *Journ. Ecology.* Nov., pp. 204–216.

Z   Alfred Newstead. Curator and Librarian's Report. *Chester Soc. Nat. Sci.*, etc. 48*th Rep.*, pp. 13–15.

Z   —— New Varieties of British Lepidoptera from Cheshire. *Ent.* Oct., pp. 226–227.

B   R. Newstead. Some Scale-insects (Coccidæ) found in the Isle of Man. *Proc. and Trans. Liverp. Biol. Soc.* Vol. XXXIII., pp. 63–64.

Z   A. H. Newton. Notes on Odonata collected in North Wales in June and July, 1918. *Ent.* July, pp. 155–157.

Z   J. B. Nichols. Wild Hybrid between House Sparrow and Tree Sparrow. *Brit. Birds.* Oct., p. 130.

Z   C. Nicholson. *Pararge megœra*, and *Vespa crabro* in Essex. *Ent.* Nov., p. 262.

Z   —— Is *Arctia caja* habitually a day-flier ? *Ent. Rec.* June, p. 111.

Z   —— *Hibernia defoliaria* in January and February, *loc. cit.*

Z   —— *Pararge megœra* in Essex and Gloucestershire, *loc. cit.* Nov., p. 206.

Z   G. W. Nicholson. *Oxytelus insecatus* Gr. in ants' nests. *Ent. Mo. Mag.* July, p. 136.

Z   —— Note on the occurrence of *Lamprinus saginatus* Gr. with ants, *loc. cit.*, pp. 136–137.

Z   —— *Melanophila acuminata* de G. at a fire in June, *loc. cit.* July, pp. 156–157.

Z   G. W. NICHOLSON. *Atheta inhabilis* Kr. and *A. valida* Kr. in Berkshire, *loc. cit.* Sept., p. 207.

B   JAMES NOONAN. *Vaccinium Myrtillus* on Raths. *Irish Nat.* Sept., p. 105.

Z   GEORGE H. F. NUTTALL. The Systematic Position, Synonymy and Iconography of *Pediculus humanus* and *Phthirus pubis. Parasitology.* Oct., pp. 329–346.

Z   —— Observations on the Biology of the Ixodidae. Part III. Dealing with the behaviour of the sexes in *Amblyomma hebraeum, Hyalomma aegyptium* and *Rhipicephalus bursa* when upon the host, *loc. cit.,* pp. 393–404.

Z   —— See D. KEILIN.

Z   CHAS. OLDHAM. Lesser Shrew (*Sorex minutus*) in Cumberland. *Nat.* Sept., p. 292.

Z   —— Palmated Newt (*Molge palmata*) in the Lake District, *loc. cit.* Oct., p. 337.

Z   —— Diving Powers of Shoveler, *loc. cit.,* p. 110.

Z   —— See H. F. WITHERBY.

Z   H. ONSLOW. Wild Birds and Distasteful Insect Larvæ. *Nature.* Aug. 14, p. 464.

Z   —— The Inheritance of Wing Colour in Lepidoptera. I. *Abraxas grossulariata* var. *lutea* (Cockerell). *Journ. Genetics.* Sept., pp. 209–258. II. Melanism in *Tephrosia consonaria* (var. *nigra* Bankes). Dec., pp. 53–60.

Z   ARTHUR J. CAMPBELL ORDE. Fulmars nesting at Haskier. *Scot. Nat.* Sept., p. 166.

Z   J. H. ORTON. Sex-Phenomena in the Common Limpet (*Patella vulgata*). *Nature.* Dec. 11, pp. 373–374.

Z   J. H. OWEN. Birds covering their eggs at night during the laying period. *Brit. Birds.* June, p. 23.

Z   —— On the procuring of food by the male for the female among birds of prey, *loc. cit.,* p. 29.

Z   —— Large Clutch of Wren's Eggs (with note by F. C. R. Jourdain), *loc. cit.,* p. 82.

Z   —— Cuckoo's Eggs and Nestlings in 1919, *loc. cit.* Sept., p. 109.

Z   —— Some Habits of the Sparrow-hawk, *loc. cit.* Oct., pp. 114–124

B   M. NAST OWEN. The Skin Spot Disease of Potato Tubers. *Bull. Misc. Information.* Kew. No. 8, pp. 289–301.

Z   A. H. P. Obituary. The Late Frank Norgate. *Brit. Birds.* June, pp. 21–22.

Z   V. C. PAINE. An Aerial Combat [Sparrow-hawk and Rook]. *Rep. and Trans. Devon Assoc.* Vol. LI., pp. 63–64.

Z   C. K. PARKER. Lesser Black-backed Gull (*Larus fuscus affinis*) perching on wire. *Lanc. & C. Nat.* Nov., p. 125.

Z   W. H. PARKIN. Tree Creeper (*Certhia familiaris*). *Nat.* Oct., p. 336.

Z   —— See W. E. L. WATTAM.

B   DAVID PAUL. Presidential Address. On the Earlier Study of Fungi in Britain. *Trans. Brit. Mycol. Soc.* Sept., pp. 91–104.

B   ROBERT PAULSON and PERCY G. THOMPSON. Supplemental Report on the Lichens of Epping Forest. *Essex Nat.* Vol. XIX., Pt. I., pp. 27–30.

Z   DONALD PAYLER. Dwellers Underground. *Animal World.* Aug., pp. 89–91.

Z   J. H. PAYNE. Undocked Dogs. *Nat.* Sept., p. 311.

P   A. E. PEAKE. Excavations at Grime's Graves during 1917. *Proc. Prehist. Soc. E. Anglia.* Vol. III., Pt. I., pp. 73–92.

Z   E. J. PEARCE. *Adimonia oelandica* Boh. in Dorset. *Ent. Mo. Mag.* Sept., p. 207.

Z   —— Some Coleoptera taken on Dartmoor, *loc. cit.* Nov., p. 261.

B   WILLIAM HARRISON PEARSALL. The British Batrachia. *Rep. Bot. Soc.* Vol. V., Pt. III., pp. 428–441.

B   A. A. PEARSON. A New Mycena. *Trans. Brit. Mycol. Soc.* Sept., pp. 135–136.

B   —— See E. M. WAKEFIELD.

Z   DOUGLAS H. PEARSON. Dorset [Entomology of]. *Ent. Rec.* Nov., pp. 207–209.

B   WILLIAM HENRY PEARSON. Notes on Radnorshire Hepatics. *Journ. Bot.* July, pp. 193–195.

B   A. E. PECK. Fungus Foray at Helmsley (1919). *Nat.* Dec., pp. 396–399.

B   —— Thomas Hey [Obituary], *loc. cit.*, pp. 404–405.

Z   R. C. L. PERKINS. Note on a peculiarity in the burrows of *Halictus maculatus* Sm. *Ent. Mo. Mag.* July, pp. 160–161.

Z   —— *Andrena dorsata* K. and *A. similis* [Sm.] stylopised, *loc. cit.* Aug., p. 181.

B   T. PETCH. Further Notes on *Colus Gardneri* (Berk.) Fischer. *Trans. Brit. Mycol. Soc.* Sept., pp. 121–132.

B   G. H. PETHYBRIDGE. Notes on some Saprophytic species of Fungi, associated with diseased Potato Plants and Tubers. *Trans. Brit. Mycol. Soc.* Sept., pp. 104 *et seq.*

Z   WALTER PIERCE. *Limenitis sibylla* in Bucks. *Ent.* Oct., p. 237.

Z   —— *Polygona c-album*, *loc. cit.*

Z   O. G. PIKE. Probable Long-tailed Skua in Hertfordshire. *Brit Birds.* Oct., pp. 143–144.

Z   —— The Black-necked Grebe, *loc. cit.*, pp. 146–154.

Z   FRANCES PITT. The Little Owl. *Nat.* Nov., pp. 370–371.

Z   —— Little Owl breeding in Shropshire. *Brit. Birds.* Nov., p 163.

Z   —— See MAUD D. HAVILAND.

Z   GEO. T. PORRITT. *Choerocampa nerii* at Huddersfield. *Ent. Mo. Mag.* Oct., p. 232.

Z   —— *Crocallis elinguaria*, form *signatipennis*. *Ent.* Nov., p. 258.

Z   —— See E. P. BUTTERFIELD.

Z   M. PORTAL. Abnormal Late Hatching of Partridges. *Brit. Birds.* Nov., p. 166.

Z   J. PORTER. See T. SHEPPARD.

B   R. LLOYD PRAEGER. Clavaria argillacea [in Co. Carlow]. *Irish Nat.* June, p. 79.

B   —— Rev. Cosslett Herbert Waddell, B. D. [Obituary], *loc. cit.* Sept., p. 108.

B Z   —— Nathaniel Colgan. [Obituary], *loc. cit.* Nov., pp. 121–126.

B   —— See W. B. STEELE.

B   D. PRAIN. J. W. H. Trail, M.D., F.R.S. [Obituary]. *Journ. Bot.* Nov., pp. 318–320.

Z   H. PRESTON. Fossil Mammalia. *Trans. Lincs. Nat. Union* for 1918, p. 116.

B   H. W. PUGSLEY. Notes on British Euphrasias. I. *Journ. Bot.* July, pp. 169–175.

Z   W. RAMSDEN. Presidential Address on Surface Films. *Proc. and Trans. Liverp. Biol. Soc.* Vol. XXXIII., pp. 3–24.

Z   GILBERT H. RAYNOR. *Colias edusa* and *C. hyale* near Maldon. *Ent.* Oct., p. 235.

Z   —— *Abraxas grossulariata* ab. *exquisita* and ab. *pulchra*. *Ent. Rec.* Nov., pp. 205–206.

B   CARLETON REA. *Elatine Hydropiper* in Worcestershire. *Journ. Bot.* Nov., p. 323.

B   —— Amphlett, John, M.A., S.C.L., J.P. [Obituary]. *Rep. Bot. Soc.* Vol. V., pt. III., p. 349.

Z   R. A. S. REDMAYNE. *Nyssia lapponaria* in Inverness. *Ent.* June, p. 141.

B   E. REID. See W. S. LAVEROCK.

Z   —— See G. E. BAKER.

Z   PERCY C. REID. A Hunt for *Zygaena achilleae*. *Ent.* Aug. pp. 188–189.

Z   —— *Colias edusa* in Essex, *loc. cit.* Oct., p. 235.

B   A. B. RENDLE. The President's Address. Some Cases of Adaptation among Plants. *Journ. Quekett Micro. Club.* No. 84, pp. 23–28.

B Z   —— John Hopkinson, F.L.S., F.Z.S., F.G.S., F.R.M.S., Secretary of the Ray Society (1844–1919) [Obituary], *loc. cit.* No. 85, pp. 103–104.

B Z   —— George Stephen West, M.A., D.Sc., F.L.S. (1876–1919) [Obituary], *loc. cit.*, pp. 104–105.

Z   L. P. W. RENOUF. The Deceptiveness of Adult External Features as Guides to Relationship, as illustrated by Decapod Crustacea. *Glasgow Nat.* Sept., pp. 72–77.

B Z   JOHN RENWICK. David Gregorson—a Memorial Notice, *loc. cit.*, pp. 77–80.

B Z   —— Robert J. Bennett—a Memorial Notice, *loc. cit.*, p. 81.

Z   T. W. RICHARDS. Notes on Lepidoptera from Wales and Hereford. *Ent.* Aug., p. 190.

B H. J. Riddelsdell. *Simethis planifolia* Gren. & Godr. *Journ. Bot.* Oct., p. 285.

B —— Gloucestershire [Botanical] Notes, *loc. cit.*, pp. 350–353.

P William Ridgeway. An Irish Decorated, Socketed Bronze Axe. *Man.* Nov., pp. 161–164.

Z Leonora Jeffrey Rintoul and Evelyn V. Baxter. Report on Scottish Ornithology in 1918. *Scot. Nat.* July, pp. 99–144.

Z —— See Evelyn V. Baxter.

Z B. B. Riviere. Continental Jays in Norfolk. *Brit. Birds.* June, pp. 25–26.

Z Hugh G. Riviere. *Daphnis nerii* at Studland, Dorset. *Ent. Rec.* Nov., p. 207.

Z John Robertson. The Whinchat as Imitator. *Scot. Nat.* Nov., p. 195.

B Fred Robinson *Isoetes Hystrix* Durieu in Cornwall. *Journ. Bot.* Nov., p. 322.

Z H. W. Robinson. The Great Crested Grebe in Scotland. *Scot. Nat.* Nov., p. 197.

Z —— Sandwich Tern bred on Farne Islands recovered in Cumberland. *Brit. Birds.* Sept., p. 111.

Z —— Numbers of Swallow Broods in 1919, *loc. cit.* Nov., pp. 161–162.

Z —— Black-headed Gulls returning to their parent colony, *loc. cit.*, pp. 165–166.

Z W. D. Roebuck [late]. Census Authentications [Conchology]. *Journ. Conch.* Aug., pp. 101–102.

B I. M. Roper. Green, Charles Baylis [Obituary]. *Rep. Bot. Soc.* Vol. V. pt. III., p. 355.

Z H. Rowland-Brown. Hibernation of *Aglais urticæ*. *Ent.* June, p. 137.

Z —— Spring Emergencies in South Devon, *loc. cit.*, p. 139.

Z —— The Cotteswold Arion, *loc. cit.* Aug., pp. 174–178 ; a correction, Sept., p. 212.

Z —— *Anthrocera achilleæ*, Esper, in Scotland. Notes on its distribution and Variation, *loc. cit.* Oct., pp. 217–226.

Z —— A plea for Pioneer Work [in Entomology], *loc. cit.*, pp. 229–231.

Z —— *Erebia æthiops* at Arnside, *loc. cit.* Dec., pp. 265–267.

Z —— *Hippotion celerio* and other Lepidoptera in the Maidstone Museum, *loc. cit.*, p. 277.

Z —— Scarcity of *Aglais urticæ*, *loc. cit.*, pp. 277–278.

Z George Russell. Caterpillars of the Pale Mottled Willow Moth in Flax. *Scot. Nat.* Nov., p. 200.

Z George B. Ryle. Coleoptera of the Brighton District. *Ent. Mo. Mag.* Aug., pp. 178–179 ; a correction, Sept., p. 232.

Z —— *Odontaeus mobilicornis* Fab. in Wiltshire, *loc. cit.*, pp. 231–232.

B E. J. Salisbury. Botany. *Sci. Progr.* July, pp. 43–48 ; Oct., pp. 224–227.

B C. E. Salmon. Norfolk Notes [Botanical]. *Journ. Bot.* July, pp. 190–192.

B —— Argyle Records, *loc. cit.* Dec., p. 354.

B —— See Anthony Wallis.

Z E. U. Savage. Grey Wagtails nesting at a distance from water. *Brit. Birds.* July, p. 56.

Z R. F. Scharff. William Spotswood Green, C.B., M.A. [Obituary]. *Irish Nat.* July, pp. 81–84.

Z —— A New Irish Whale [*Mesoplodon mirus*], *loc. cit.*, pp. 130–131.

P Alex M. Scott. The Caledonian Camp, the Haer Cairns, and the Steed Stalls, in the Stormont. *Trans. and Proc. Perth Soc. N. Sci.* Vol. VII., pt. I., pp. 28–36.

Z Andrew Scott. On the Monthly Occurrence of Pelagic Fish Eggs in Port Erin Bay in 1918. *Proc. and Trans. Liverp. Biol. Soc.* Vol. XXXIII. pp. 85–94.

Z —— See W. A. Herdman.

B Z [D. J.] Scourfield. Pond Life Exhibition [Remarks on]. *Journ. Roy. Micro. Soc.* June, pp. 195–196.

B Reginald W. Scully. Some Stray Botanical Notes. *Irish Nat.* June, p. 80.

B —— *Eriophorum latifolium* in County Dublin, with some notes on the rarer County species, *loc. cit.* July, pp. 89–90.

Z   EDMUND SELOUS. Ornithological Observations and Reflections in Shetland. *Nat.* Aug., pp. 259–262 ; Nov., pp. 357–360 ; Dec., pp. 381–385.

Z   D. G. SEVASTOPULO. An Experiment on *Andrena fulva. Ent.* June, p. 138.

Z   E. P. and P. A. SHARP. *Sphinx pinastri* in Sussex. *Ent. Rec.* Aug., p. 168.

Z   W. G. SHELDON. The Variation of *Sarrothripus revayana*, Scopoli. *Ent.* June, p. 122–129.

B   —— The Re-discovery of *Anthrocera achilleæ* in Scotland, *loc. cit.* Sept., pp. 213–214.

Z   —— The Earlier Stages of *Peronea maccana*, Tr., *P. lipsiana*, Schiff, *P. rufana*, Schiff, and *P. schalleriana*, *loc. cit.* Nov., pp. 252–255 ; Dec., pp. 271–274.

Z   W. F. J. SHEPHEARD. Zoological Section [Report]. *Chester Soc. Nat. Sci.* etc. 48*th Rep.* pp. 18–21.

Z   C. E. SHEPHERD. On Otoliths. *Trans. Dumfries. & Gall. Nat. Hist. & Ant. Soc.* Vol. VI., pp. 21–27.

Z   T. SHEPPARD. The Hull Whaling Trade. *Mariner's Mirror.* Dec., pp. 162–178.

P   —— Dane's Dyke [British Earthwork]. *Trans. East R. Antiq. Soc.* Vol. XXII., pp. 33–42.

Z   —— Whaling Relics [Carved Tusks, etc.], *loc. cit.*, pp. 43–54.

B   —— Spring-gun enclosed in Oak Tree. *Nat.* June, p. 214.

Z   —— Bones of Bear from York, *loc. cit.* Sept., pp. 293–294.

B Z   —— Yorkshire Naturalists at Spurn [with contributions by E. W. Wade, J. Porter, C. A. Cheetham, Greevz Fysher, W. H. Burrell], *loc. cit.* Dec., pp. 386–389.

Z   ALFRED SICH. Notes on *Coleophora vibicella*, Hb. *Ent. Rec.* Aug., pp. 169–170.

Z   —— Notes on *Cemiostoma wailesella* and other Lepidoptera in Sussex in 1919, *loc. cit.* Nov., pp. 201–204.

B   [W. W. SIDDALL.] Botanical Section [Report]. *Chester Soc. Nat. Sci.* etc. 48*th Rep.*, p. 18.

Z   H. M. SILLAR. *Hyloicus pinastri* in Suffolk. *Ent.* Nov., p. 258.

Z   G. H. SIMPSON-HAYWARD. An Unusual Form of *Aplecta nebulosa*, *loc. cit.* Aug., pp. 189–190.

B   JAMES SMALL. The Origin and Development of the Compositæ. *New Phy.* May, pp. 129–176 ; July, pp. 201–234.

Z   F. W. SMALLEY. Mallard, Wigeon, and Lesser Black-backed Gull in North Uist. *Scot. Nat.* Sept., pp. 169–170.

P   G. W. SMALLWOOD. Potters' Tools of Slate. *Proc. Prehist. Soc. E. Anglia.* Vol. III., pt. I., pp. 115–118.

Z   H. DOUGLAS SMART. *Crocallis elinguaria signatipennis. Ent.* Dec., p. 278.

B Z   ARTHUR SMITH. The General Secretary's Report. *Trans. Lincs. Nat. Union* for 1918, pp. 101–104.

B   A. LORRAIN SMITH. New or Rare Microfungi. *Trans. Brit. Mycol. Soc.* Sept., pp. 149–158.

Z   [ELLIOT SMITH]. The Significance of the Cerebral Cortex. *Nature.* July 17, pp. 396–397.

Z   K. M. SMITH. A Comparative Study of Certain Sense-organs in the Antennæ and Palpi of Diptera. [Appendix by H. Maxwell Lefroy.] *Proc. Zool. Soc.* Sept., pp. 31–69.

P   REGINALD A. SMITH. Foreign Relations in the Neolithic Period. *Proc. Prehist. Soc. E. Anglia.* Vol. III., pt. I., pp. 14–32.

Z   THOMAS SMITH. *Diaphora mendica* in North Staffordshire. *Ent.* Sept., p. 216.

Z   CHAS. D. SOAR. British Hydracarina. *Vasculum.* July, pp. 107–112.

Z   —— *Hydracarina.* The Genus *Oxus* Kramer. *Journ. Quekett Micro. Club.* No. 84. pp. 29–34.

B Z   W. SOMERVILLE. Ear Cockles in Wheat. *Journ. Board Agric.* Dec., pp. 907–909.

Z   RICHARD SOUTH. Colour Variation of *Cacœcia cratægana*, Hb., in the New Forest. *Ent.* Aug., pp. 190–191.

Z   —— *Sphinx convolvuli* at Putney, *loc. cit.* Oct., p. 237.

Z   F. W. SOWERBY. *Eucris occulata* in Lincolnshire. *Ent.* June, p. 138.

Z EDWARD R. SPEYER. Wild Birds and Distasteful Insect Larvæ. *Nature.* Aug. 7, pp. 445–446.

Z HENRY SPEYER. *Papilio machaon* in Surrey. *Ent. Rec.* July, p. 130.

Z T. STAINFORTH. The Thomas Stather Collection of Lepidoptera. *Trans. Hull Sci. & F. N. Club.* Vol. IV., pt. VI., pp. 281–298.

Z —— The Bristly Millipede in North Lincs. *Nat.* July, p. 234.

Z —— Some Yorkshire Arthropods, *loc. cit.* Nov., pp. 361–362.

Z R. STANDEN. *Armadillidium pulchellum*, Zenker, near Whalley. *Lanc. & C. Nat.* Oct., p. 106.

Z —— The Hairworm (Gordius aquaticus, L.), *loc. cit.*, p. 111.

Z —— Entomological Notes from Dovedale, Derbyshire, *loc. cit.*, pp. 112–118, and Nov., pp. 121–123.

Z J. K. STANFORD. Pied Flycatcher in Suffolk in Spring. *Brit. Birds.* Aug., pp. 81–82.

Z —— Some Notes on the Wryneck, *loc. cit.*, pp. 82–83.

Z —— Pied Flycatchers on Migration in London, *loc. cit.*, p. 160.

Z —— Ornithological Notes from Suffolk, *loc. cit.*, pp. 170–172.

Z T. R. R. STEBBING. Alfred Merle Norman. 1831–1918 [Obituary]. *Proc. Roy. Soc.* B. 634, pp. xlvi–l.

B W. B. STEELE. *Viola stagnina* in Fermanagh. [With Note by R. Lloyd Praeger.] *Irish Nat.* July, p. 95.

Z A. W. STELFOX. *Pisidium parvulum* in Co. Antrim, *loc. cit.*, pp. 92–93.

P F. STEVENS. Skeleton found at Fargo, *Wilts. Arch. & Nat. Hist. Mag.* No. CXXX., p. 359.

B P Z [H. C. STEWARDSON]. Catalogue of the more important Papers, especially those referring to Local Scientific Investigations, published by the Corresponding Societies during the year ending May 31, 1918. *Rep. Brit. Assoc. for 1918*, pp. 90–103.

B ANNOT L. STONE and CHAS. F. THORNEWILL. Report of the Committee for 1918. *Proc. Ashmolean N. Hist. Soc. for 1918*, pp. 9–10.

Z H. F. STONEHAM. Meadow-pipits fiercely attacking Cuckoo. *Brit. Birds.* Nov., pp. 162–163.

Z E. A. C. STOWELL. Notes on Lepidoptera around Alton, Hants. *Ent.* Nov., pp. 258–259.

Z FRED J. STUBBS. Climbing of the Water Shrew. *Essex Nat.* Vol. XIX., pt. I., p. 21.

Z —— Occurrence of Trout in the River Roding, *loc. cit.*, pp. 25–26.

Z H. KIRKE SWANN. Peregrine Falcon attacking a boy. *Brit. Birds.* June, p. 31.

Z —— Probable Montagu's Harrier breeding near Sussex, *loc. cit.* Oct., p. 141.

Z —— A Synoptical List of the Occipitres (Diurnal Birds of Prey). Part I., July, pp. 1–38; Part II., Nov., pp. 39–74.

Z —— See W. H. MULLENS.

Z C. F. M. SWYNNERTON. Experiments and Observations bearing on the Explanation of Form and Colouring, 1908–1913. *Journ. Linn. Soc. (Zool.).* No. 224, pp. 203–385.

Z JOSEPH H. SYMES. Number of Eggs laid by a Marsh-Warbler. *Brit. Birds.* Nov., p. 160.

Z W. M. TATTERSALL. Crustacea. Notes on Lancashire and Cheshire Specimens. *Lanc. & C. Nat.* July, pp. 20–26.

Z —— Lancashire and Cheshire Mosquitoes, *loc. cit.* Sept., p. 69.

Z —— Rotifera, *loc. cit.*, p. 73.

Z —— Rare Lancashire and Cheshire Spiders, *loc. cit.* Oct., p. 120.

Z —— *Erysimum orientale*, Mill., at Prestwich, *loc. cit.* Dec., p. 170.

B BEATRICE TAYLOR. Sargant, Ethel, F.L.S. [Obituary]. *Rep. Bot. Soc.* Vol. V., pt. III., pp. 365–366.

Z R. H. S. TEBB. *Leucophasia sinapis* in Co. Wicklow. *Irish Nat.* July, p. 92.

Z GEORGE W. TEMPERLEY. Fulmar Petrels in Yorkshire in Summer. *Brit. Birds.* July, p. 59.

P A. G. THACKER. Anthropology. *Sci. Progr.* Oct., pp. 235–240.

Z FRED. V. THEOBALD. New and Little Known British Aphides. *Ent.* July, pp. 157–161.

Z   Fred. V. Theobald. Insects on the Sea Buckthorn, *loc. cit.* Aug., pp. 169–171.

Z   Alfred Thomas. Number of Eggs laid by Marsh-Warbler. *Brit. Birds.* Oct., p. 137.

B   E. N. Thomas [Secretary]. Plant Pathology. Report of the Committee. *Rep. Brit. Assoc. for* 1918, pp. 56–58.

Z   J. F. Thomas. Unusual Nesting Site of a Kestrel. *Brit. Birds.* Aug., p. 84.

Z   A. H. Thompson. *Hesperia malvæ* in Cheshire. *Ent.* Aug., p. 189.

B Z   —— Larval Food-plants of *Xanthœcia flavago, loc. cit.,* p. 256.

B   H. Stuart Thompson. Habitats of *Hypericum humifusum. Journ. Bot.* July, pp. 195–196.

B   —— Yew on Oak, *loc. cit.,* p. 197.

B   —— [*Hypericum humifusum*], *loc. cit.,* pp. 226–227.

B   —— *Carex montana* L., *loc. cit.* Oct., pp. 274–275.

B   —— *Galium erectum* in Somerset, *loc. cit.* Oct., pp. 286–287.

B   —— The Genus *Euphrasia* and *E. minima, loc. cit.* Dec., pp. 335–337.

Z   M. L. Thompson. See W. E. L. Wattam.

B   Percy Thompson. New Essex Lichen. *Essex Nat.* Vol. XIX., pt. i., p. 15.

B   —— Rare Essex Bryophytes, *loc. cit.,* p. 17.

B   Percy G. Thompson. See Robert Paulson.

Z   A. Landsborough Thomson. Pied Flycatcher in Aberdeenshire. *Scot. Nat.* Nov., p. 195.

B Z   J. A. T[homson], S.G., E.S.G., A.G., A.L.S., J.E. B. B. W[oodward], H.F.W., J. E. B., H[eron]-A[llen] and E[arland], A. B. R[endle]. Summary of Current Researches relating to Zoology and Botany (principally invertebrata and Cryptogamia), Microscopy, etc. *Journ. Roy. Micro. Soc.* June, pp. 127–186 ; Sept., pp. 229–299 ; Dec., pp. 329–386.

Z   H. J. Thonless. *Leptura rubra* L. in Norfolk. *Ent. Mo. Mag.* Aug., pp. 174–175.

B   Chas. F. Thornewill. See Annot L. Stone.

Z   N. F. Ticehurst. The Birds of Bardsey Island. *Brit. Birds.* July, pp. 42–51. Aug., pp. 86–75. Sept., pp. 101–106. Oct., pp. 129–134. Dec., pp. 182–193.

Z   —— Some Notes on the Wryneck, *loc. cit.,* pp. 82–83.

Z   —— Are Cuckoos ever reared by Greenfinches ? *loc. cit.* Oct., pp. 137–138.

Z   —— See H. F. Witherby.

Z   J. R. le B. Tomlin. New localities for *Hydrovatus clypealis* Sharp. *Ent. Mo. Mag.* July, p. 159.

Z   —— *Colpodes splendens* Morawitz, a Japanese Carabid in Berkshire, *loc. cit.*

Z   —— *Bagous lutulosus* in Glamorgan and Berks, *loc. cit.* Nov., p. 260.

Z   —— [J. R. U. B. Tomlin, in error]. William Ernest Sharp. An appreciation. *Lanc. & C. Nat.* Sept., pp. 65–67.

Z   A. E. Tonge. *C. polyodon* [at Worthing]. *Ent. Rec.* June, p. 110.

Z   Gladys M. Towsey. Birds near London. *Journ. Wild Birds Inves. Soc.* No. i., pp. 7–8.

Z   N. Tracy. The Drumming of Woodpeckers. *Brit. Birds.* Aug., p. 88.

B Z   Charles Taylor Trechmann. On a Bed of Interglacial Lœss and some Preglacial Fresh-water Clays on the Durham Coast [Abs.]. *Abs. Proc. Geol. Soc.,* pp. 22–25. *Ann. & Mag. Nat. Hist.* June, pp. 50–52 ; and *Phil. Mag.* Sept., pp. 425–426.

Z   Thomas H. C. Troubridge [Thomas H. H. Troubridge, in error]. Spoonbill in Hants. *Brit. Birds.* Aug., p. 85. Nov., p. 165.

Z   J. G. Tuck. The late Frank Norgate, *loc. cit.* July, p. 64.

Z   Emma L. Turner. The Bittern in the Norfolk Broads. "A Great Entail," *loc. cit.* June, pp. 5–12.

Z   —— Further Notes on the Bittern in the Norfolk Broads, *loc. cit.* July, pp. 34–36.

Z   Hy. J. Turner. The South London Entomological and Natural History Society [Reports]. *Ent.* June, pp. 141–143. July, p. 168. Aug., pp. 191–192. Nov., pp. 263–264 ; and *Ent. Mo. Mag.* June, pp. 141–143. July, pp. 162–163. Aug., pp. 182–183. Sept., pp. 209–210. Oct., pp. 235–236. Dec., p. 280.

Z   —— *Tortrix viridana* and others. *Ent. Rec.* June, p. 111.

Z   Hy. J. Turner. *Acalla reticulata*, Ström.=*contaminana*, Hüb.—Its History and its Variation, *loc. cit.* Aug., pp. 158–164.

Z   R. E. Turner. *Xiphydria prolongata* Geoffr. (= *dromedarius* Fabr.), bred from an artificial leg. *Ent. Mo. Mag.* July, p. 161.

B   W. B. Turrill. Female Flowers of *Plantago lanceolata*. *Journ. Bot.* July, p. 196.

B   —— Observations on the Perianth in *Ranunculus auricomus* and *Anemone coronaria*. *New Phy.* Oct., pp. 253–256.

Z   B. Van de Weyer. Pheasant breeding in Sparrow-Hawk's nest, with note by F. C. R. Jourdain. *Brit. Birds.* Aug., p. 87.

Z   H. J. Vaughan. Pied Flycatcher in South Wales, *loc. cit.* June, p. 27.

B   G. M. Vevers. See W. Balfour Gourlay.

B   Arthur E. Wade. The Flora of Aylestone and Narborough Bogs. *Trans. Leicester Lit. & Phil. Soc.* Vol. XX., pp. 20–46.

Z   E. W. Wade. See T. Sheppard.

B   Harold Wager. A Fluorescent Colouring Matter from *Leptonia incana*, Gill. *Trans. Brit. Mycol. Soc.* Sept., pp. 158–164.

B   E. M. Wakefield. The Selby Foray [and] Complete List of Fungi gathered during the Foray. *Trans. Brit. Mycol. Soc.* Sept., pp. 77–87.

B   —— New British Fungi, *loc. cit.*, pp. 132–134.

B   —— Charles Ogilvie Farquharson [Obituary], *loc. cit.*, pp. 236–237.

B   —— and A. A. Pearson. Additional Resupinate Hymenomycetes from the Weybridge District, *loc. cit.*, pp. 136–143.

B   —— See A. D. Cotton.

Z   J. J. Walker. Interim Report on Coleoptera. *Proc. Ashmolean N. Hist. Soc.* for 1918, p. 16.

Z   A. H. Wall. Hawk and Canaries. *Rep. Marlborough Coll. Nat. Hist. Soc.* No. 67, p. 39.

Z   W. Wallace. Anopheline Mosquitoes of Lincolnshire. *Trans. Lincs. Nat. Union for* 1918, pp. 118–119.

B   Anthony Wallis. [&] C. E. Salmon. Pembrokeshire and Carmarthenshire Plants. *Journ. Bot.* Dec., pp. 347–350.

Z   E. Arnold Wallis. One Meadow-pipit feeding two young Cuckoos. *Brit. Birds.* Nov., p. 163.

Z   H. H. Wallis. On the Aquatic Coleoptera, etc., of the Trent Valley in the Neighbourhood of Long Eaton. *Ent. Mo. Mag.* June, pp. 127–128.

Z   Geo. B. Walsh. Notes and Records of Coleoptera. *Vasculum.* July, pp. 142–144. Dec., pp. 182–184.

B   G. Walworth. See L. F. Newman.

B   Ethel Warhurst. Liverpool Botanic Gardens. *Lancs. & C. Nat.* Dec., pp. 171–173.

B   —— and A. Lee. Liverpool Botanical Society [Reports], *loc. cit.* Sept., pp. 83–87.

Z   G. W. Warner. *Colias hyale* in Leicester. *Ent.* Nov., p. 260.

P Z   S. Hazzledine Warren. The Dating of Surface Flint Implements and the Evidences of the Submerged Peat Surface. *Proc. Prehist. Soc. E. Anglia.* Vol. III., pt. I., pp. 94–104.

P   —— A Stone-axe Factory at Graig-Lwyd, Penmaenmawr. *Journ. Anthrop. Inst.* July, pp. 342–365.

Z   A. Joyce Watson. Occurrence of *Chirocephalus diaphanus* in Seavernake Forest. *Wilts. Arch. & Nat. Hist. Mag.* No. CXXX., p. 368.

Z   Hugh Watson. Notes on *Hygromia limbata* (Drap.). *Proc. Malacol. Soc.* Oct., pp. 120–132.

B   W. Watson. Habitats of Hypericum humifusum. *Journ. Bot.* Dec., pp. 353–354.

B   A. S. Watt. On the Causes of Failure of Natural Regeneration in British Oakwoods. *Journ. Ecology.* Nov., pp. 173–203.

B   Lawrence Watt. Notes on Plants from Banff and from Old Kilpatrick and on the Oaks in Erskine Policies. *Glasgow Nat.* Sept., pp. 65–70.

B Z   W. E. L. Wattam. Yorkshire Naturalists at Coxwold [With Contributions by Greevz Fysher, F. A. Mason, W. Ingham, M. L. Thompson, [W. J.] Fordham]. *Nat.* June, pp. 206–210.

B   W. E. L. Wattam. Yorkshire Naturalists at Ryhill [With Contributions by W. H. Parkin, J. Digby Firth, J. A. Hargreaves, H. H. Corbett, W. Falconer, B. Morley, [W. J.] Fordham and J. W. H. Johnson], *loc. cit.* Aug., pp. 271–273.

B   —— Yorkshire Naturalists at Hawes [With Contributions by R. Jones, C. A. Cheetham, [W.] Falconer, M. L. Thompson, J. Hartshorn, F. E. Milsom, F. A. Mason], *loc. cit.*, pp. 303–308.

B   —— Yorkshire Naturalists at Pateley Bridge [With Contributions by W. H. Burrell, M. Malone, E. P. Butterfield, C. A. Cheetham, R. Butterfield], *loc. cit.*, pp. 308–310.

B   —— Phanerogamic Botany [Report]. *Ann. Rep. Huddersfield Nat. etc. Soc.* 1918–1919, pp. 19–20.

Z   Gladys E. Webb. The Development of the Species of *Upogebia* from Plymouth Sound. *Journ. Marine Biol. Assoc.* July, pp. 81–135.

Z   F. D. Welch. Starling. *Nat.* Oct., p. 341.

Z   —— Combat between Hedge-Sparrow and House-Sparrow. *Brit. Birds.* Nov., p. 161.

B   George West. *Amphora inflexa*, a rare British Diatom. *Journ. Quekett Micro. Club.* No. 84, pp. 35–40.

Z   W. S. D. Westropp. *Colias edusa* in Co. Cork. *Irish Nat.* Oct., p. 120.

B   Harold J. Wheldon. Observations on the Fungi of the Lancashire and Cheshire Sand-Dunes. *Trans. Brit. Mycol. Soc.* Sept., pp. 143–148.

B   J. A. Wheldon. Notes on Braithwaite's *Spagnaceæ Exsiccatæ*. *Journ. Bot.* June, pp. 142–147.

Z   E. Whitley. *Eugonia polychloros* in Devon. *Ent.* Sept., p. 216.

Z   —— Aberrations of *Coccinella 7-punctula, loc. cit.* Oct., p. 238.

Z   Oscar Whittaker. A Rare Fairy Fly [*Mymar regalias*] in Cheshire. *Lanc. & C. Nat.* Nov., p. 144.

B   Henry J. Wilkinson. *Cypripedium Calceolus* L. *Nat.* Nov., p. 373.

B   J. W. Williams. *Anthidium manicatum*, Linn., in Worcestershire. *Ent.* Dec., p. 278.

Z   W. J. Williams. American Goshawk in Ireland [With Note by E. Hartert and H. F. Witherby]. *Brit. Birds.* June, p. 31.

Z   H. C. Williamson. The Common Tern and its Enemies. *Journ. Wild Bird Inves. Soc.* No. 1, pp. 3–6.

Z   [E. Willmott]. Pied Blackbird from Worley Place. *Essex Nat.* Vol. XIX., pt. 1., pp. 14–15.

B   A. J. Wilmott. See A. B. Jackson.

B   A. Wilson. West Yorkshire Botanical Notes. *Nat.* Nov., p. 369.

B   Malcolm Wilson. Some British Rust Fungi. *Journ. Bot.* June, pp. 161–163.

Z   R. Winckworth. *Loligo vulgaris*, Lam. in British Waters. *Journ. Conch.*, Aug., p. 127.

Z   H. F. Witherby. The Pied and White Wagtails. *Brit. Birds.* July, pp., 37–39.

Z   —— The ' British Birds ' Working Scheme. Progress for 1918, *loc. cit.* Sept., pp. 96–100.

Z   —— Swallow ringed in Yorkshire found in South Africa, *loc. cit.* Dec., p. 196.

Z   —— Ernst Hartert, Annie C. Jackson, F. C. R. Jourdain, C. Oldham, Norman F. Ticehurst. A Practical Handbook of British Birds. Part 3, June, pp. 129–208 ; Part 4, Sept., pp. 209–272 ; Part 5, Nov., pp. 273–336.

Z   —— See W. J. Williams.

B   E. M. Wood. See H. J. Holme.

B   T. W. Woodhead. Geology [Peat, etc.]. *Ann. Rep. Huddersfield Nat. etc. Soc.* 1918–1919, pp. 20–21.

B   E. Adrian Woodruffe-Peacock. Miss S. Catherine Stow. *Trans. Lincs. Nat. Union for 1918*, pp. 99–101.

B   —— Botanical Report, *loc. cit.*, pp. 104–111.

B   —— *Cytospora chrysosperma, loc. cit.*, p. 120.

Z   —— The Grey Dagger Moth, *loc. cit.*, p. 142.

Z   —— Undocked Dogs the Quicker. *Nat.* July, p. 246.

B   E. ADRIAN WOODRUFFE-PEACOCK. Do Leaves want Watering ? *loc. cit.*, p. 246.

Z   —— The Greenfinch's Nest, *loc. cit.*, p. 277.

Z   —— Two Phytophagous Chalcids, *loc. cit.*   Oct., pp. 329–330.

Z   —— Large Pike and Herons [With Note by R. Fortune], *loc. cit.*, pp. 340–341 ; also Dec., pp. 405–406.

B Z   —— The Witchery of Gilbert White. *Nat.*   June, pp. 199–205.

Z   —— Cocoons of the Horse Leech, *loc. cit.*   Aug., p. 278.

B Z   —— Whitethorn Seed Notes, *loc. cit.*, pp., 353–355.

B Z   —— Pond Frequenting Birds as Seed Carriers. *Journ. Wild Bird Inves. Soc.*   No. I, pp. 1–2.

B   —— *Hypericum humifusum.*   *Journ. Bot.*   Aug., p. 225.

Z   TOM W. WOODRUFFE-PEACOCK. *Vertigo pygmœa*, Drap. *Nat.*   July, p. 245.

Z   —— Freshwater Sponge, *loc. cit.*, p. 245.

P   A. SMITH WOODWARD. The Antiquity of Man. *Nature.*   Nov. 6, pp. 212–213 ; Nov. 27, p. 335.

P Z   B. B. WOODWARD. See A. S. KENNARD.

B   H. WORMOLD. The ' Brown Rot ' Diseases of Fruit Trees, with Special Reference to Two Biologic Forms of *Monilia cinerea*, Bon. *Ann. Bot.*   July, pp. 361–404.

P   R. HANSFORTH WORTH. Thirty-Eighth Report of the Barrow Committee, Kistvaens in the Erme Valley. *Rep. and Trans. Devon. Assoc.* Vol. LI., p. 79.

Z   L. H. BONAPARTE WYSE. *Leucophasia sinapis* in Co. Cork. *Irish Nat.* July, p. 92.

B   R. H. YAPP. The Fenland of East Anglia [Abs.]. *Ann. Rep. Proc. Belfast Nat. F. Club.*   1918–19, pp. 21–22.

Z   J. W. YERBURY. Seashore Diptera. *Journ. Marine Biol. Assoc.*   July, pp. 141–145.

Z   —— List of Diptera Hitherto Recorded from the County of Devon. *Rep. and Trans. Devon. Assoc.*   Vol. LI., pp. 222–252.

INDEX.

*Colloid Chemistry and its General and Industrial Applications.—
Third Report of the Committee consisting of* Professor F. G.
Donnan (*Chairman*), Professor W. C. McC. Lewis (*Secretary*),
Dr. E. Ardern,* Dr. E. F. Armstrong, Professor W. M.
Bayliss and the late Professor A. J. Brown, Mr. W. Clayton,*
Dr. C. H. Desch, Mr. W. Harrison, Mr. E. Hatschek, Professors
H. R. Proctor and W. Ramsden, Dr. E. J. Russell, Mr.
A. B. Searle,* Dr. S. A. Shorter, Dr. H. P. Stevens, and
Mr. H. B. Stocks.

INTRODUCTION.

THE plan already adopted in the two previous Reports of arranging
the subject matter under two heads, viz. : (1) classification according
to scientific subject; and (2) classification according to industrial
process, has been employed in the present Report.

The subjects dealt with under the first head in the accompanying
Report are :—

1. COLLOID CHEMISTRY OF SOAP, PART I.—SOLUTIONS. By
   Professor J. W. McBain.
2. ULTRAMICROSCOPY. By G. King, M.Sc., F.I.C.
3. SOLUBILITY OF GASES IN COLLOIDAL SOLUTIONS. By G. King,
   M.Sc., F.I.C.
4. ELECTRICAL CHARGE ON COLLOIDS. By J. A. Wilson.
5. IMBIBITION OF GELS, PART I. By J. A. Wilson.

The subjects dealt with under the second head are :—

6. IMBIBITION OF GELS, PART II.—INDUSTRIAL APPLICATIONS.
   By J. A. Wilson.
7. COLLOID PROBLEMS IN BREAD-MAKING. By R. Whymper.
8. COLLOID CHEMISTRY IN PHOTOGRAPHY. By Dr. R. E. Slade.
9. COLLODION IN PHOTOGRAPHY. By H. W. Greenwood.
10. CELLULOSE ESTERS. By Foster Sproxton, B.Sc., F.I.C.
11. COLLOID CHEMISTRY OF PETROLEUM. By Dr. A. E. Dunstan.
12. ASPHALT. By Clifford Richardson, M.Am.Soc., C.E., F.C.S.
13. VARNISHES, PAINTS, AND PIGMENTS. By Dr. R. S. Morrell.
14. CLAYS AND CLAY PRODUCTS. By A. B. Searle.

The Committee has again to express its sense of obligation to the
authors of the sections enumerated above.

A number of subjects yet remain to be considered. It is hoped
that these will be dealt with in the Fourth Report.

W. C. McC. Lewis.

* Names so marked are those of Assessors or Consultative Members, not
being Members of the Association.

COLLOIDAL CHEMISTRY OF SOAP.

PART I.—SOLUTIONS.

By J. W. McBain, *Leverhulme, Professor of Physical Chemistry,*
University of Bristol.

I.—*Brief Résumé.*

Recent investigations of aqueous soap solutions culminating in
1914, have revealed soaps as a prototype of a great class of colloids
remarkable alike for their theoretical interest and their industrial
importance. The substances belonging to this class have been defined
by McBain and Salmon[14] as " colloidal electrolytes."

Colloidal electrolytes are salts in which one of the ions has been
replaced by a heavily charged, heavily hydrated ionic micelle which
exhibits equivalent conductivity that is not only comparable with
that of a true ion but may even amount to several times that of the
simple ions from which it has been derived. In other words, this
ionic micelle is a typical but very highly charged colloidal particle
of very great conductivity.

The conductivity of such a colloidal electrolyte is quite comparable
with that of an ordinary electrolyte. On the other hand since the
ionic micelle exhibits only the osmotic effect characteristic of an
ordinary colloid, the total osmotic activity of the colloidal electrolyte
is correspondingly deficient and may be distinctly less than that of
a non-electrolyte. Thus high conductivity goes hand in hand with
only moderate osmotic effects.

Some of the very numerous substances which must be recognised
as belonging to this group are the protein and gelatine salts
(T. Brailsford Robertson's well-known alternative hypothesis, which
is to the effect that there are no ordinary ions present, but that the
protein salts ionise into two colloidal ions resembling ordinary slow
complex ions, appears to have been built up upon early E.M.F. data
which Pauli, Manabe, and Matuli have since shown to be erroneous),
dyes, such as Congo-red, indicators, the higher sulphonic acids and
hydrochlorides, tellurates and many inorganic substances; in fact,
most substances of high molecular weight or containing long carbon
chains which are capable of splitting off an ordinary ion.

The soaps are a particularly interesting case for investigation
in that their chemical formulæ are well ascertained, tautomerism
does not occur, true reversible reproducible equilibrium is established
in all solutions, and finally the definite transition from typical simple
electrolyte through colloidal electrolyte to neutral colloid may be
observed in all its stages. This transition from crystalloid to colloid
is exhibited not only in passing from salts of the lower to those of
the higher fatty acids, but may be demonstrated in any one of the
higher members merely upon change of temperature and concentration.

This striking and wholly unexpected combination of properties
on the part of the ionic micelle, was plausibly explained in 1913 by
the writer, on mechanical grounds. This conception is based upon
the consideration of the application of the principle contained in

Stokes' Law, that the frictional resistance to motion of a definite amount of substance depends directly upon its degree of subdivision. Thus, if say ten simple palmitate ions unite to form one single particle carrying ten electrical charges, the resistance to movement under the action of the electric current is considerably diminished and therefore one should expect greatly enhanced conductivity. This is, indeed, observed to a very appreciable extent in some cases, but it is partly off-set by the action of this enormous aggregation of electrical charges in condensing upon the particle large amounts of water and, indeed, any other constituent available in the solution such, for example, as undissociated colloidal soap. This also obviated the difficulties advanced by Bayliss[19] in proposing his tentative assumption of a mere aggregation of the anions of Congo-red.

This heavy hydration is held by many authorities to be a plausible explanation of the high viscosity frequently exhibited by substances of this class as well as the effect of varying conditions upon this viscosity, and it also accounts for the effects of temperature, for example, the high temperature coefficient of the conductivity.

The recognition of this class of colloid in spite of the assistance of isolated data for particular cases, was long delayed owing to the supposed irreconcilability of these properties and to their being ascribed to the presence of impurities and hydrolysis, &c. It will be seen that the results have now been experimentally established beyond reasonable doubt.

It is worth while noting that a colloidal electrolyte differs wholly in its behaviour from the class of substances represented by the dextrines which may be termed " semi-colloids." The semi-colloids are non-electrolytes exhibiting various degrees of osmotic activity, ranging from that of a typical crystalloid such as dextrose down to that of a colloid such as starch.

It is also worth while mentioning that cataphoresis has often been confused with conductivity. Most ions and charged colloidal particles and even coarse suspensions exhibit a velocity of cataphoresis of the same order of magnitude, but it is only in the case of the ions and a few selected cases of colloids that this has been regarded as being identical with conductivity. In dealing with colloids, in no case was a conductivity predicated greater than might be expected for a very slow-moving complex ion, whereas the theory of the ionic micelle predicts enhanced conductivity, and in the case of soap experiment shows that ionic micelle has to be recognised as being several times more mobile than the ions from which it is derived.

The writer incidentally considers that it has not yet been proven that there is any difference in kind between cataphoresis and ionic migration, except that in an ion the number of electrical charges is equal to the number of equivalents of substance in the ion. If so, electrical endosmosis would be a result of solvation. Quantitative data are being sought in order to test this point.

II.—*Constitution in Alcohol.*

In alcohol soaps exhibit a wholly different and much simpler behaviour. The soap here exists in the form of a simple unpolymerised

electrolyte in true solution, whereas in most aqueous solutions it is of course a colloidal electrolyte. The conductivity is only moderate, and the ebullioscopic measurement indicates that it is a rather weak electrolyte.

The great dissimilarity in the constitution of soaps in alcohol and aqueous solution is brought out strikingly in an observation of the writer. When alcohol is added to a clear aqueous solution of sodium oleate the oleate is immediately salted out as a transparent gel, although this readily dissolves again after a few minutes shaking.

There are, however, colloidal properties in alcoholic soaps which require further investigation. Thus, although Miss Laing[12] has proven that in solutions of potassium oleate in dry alcohol at boiling point there is no appreciable proportion of colloid present, yet sodium oleate solutions, which have not yet been carefully studied in anhydrous alcohol, are said to solidify to a gel on cooling; and this would appear to prove the presence of a large amount of colloid. Potassium oleate on the other hand solidifies to a white curd on cooling. Oleic acid itself in all concentrations of alcohol is a simple electrolyte only very slightly dissociated.

III.—*Hydrolysis, Hydrolysis-Alkalinity and Products of Hydrolysis.*

(a) *Hydrolysis-Alkalinity.*

Until quite recently, the extent of the degree of hydrolysis and the hydrolysis-alkalinity of soap solutions has been a moot point, the estimates ranging practically from neutrality up to nearly complete hydrolysis. This was due to the difficulty of finding a satisfactory method of investigation, one which should not destroy the soap solutions that were being subjected to measurement.

The two methods introduced by McBain and Martin[24] and McBain and Bolam[25], that of E.M.F. and rate of catalysis have sufficed to establish that the alkalinity of soap solutions is very small, being of the order of magnitude of $0 \cdot 001$ N free OH' for most concentrations of soap. The E.M.F. method is of doubtful application where unsaturated compounds are present, as in the case of all commercial soaps, and the catalytic method is only applicable in dilute solutions at high temperatures.

The hydrolysis-alkalinity of soap solutions depends upon the concentration, the temperatures, the nature of the soap, and upon its state of aggregation.

Taking first the effect of concentration; in extreme dilution hydrolysis is very appreciable, but once the concentration of soap approaches decinormal, the hydroxyl ion increases but slowly with future increase of concentration and passes through a flat maximum shortly before normal concentration is obtained.

The results obtained by E.M.F. in sodium and potassium soaps at 90° are given in the following table in which all concentrations are expressed in weight normality. Diffusion potential was not taken into account; a recalculation in which diffusion potential is allowed

for gives appreciably lower values for the alkalinity of the more concentrated solutions :—

Soap.		Sodium Palmitate.		Potassium Palmitate.	
—		OH′	% Hydrolysis.	OH′.	% Hydrolysis.
1·0 N	-	0·0020	0·20	0·0008 ·	0·08
0·75 N	-	0·0023	0·30	0·0024	0·31
0·5 N	-	0·0019	0·37	0·0032	0·65
0·1 N	-	0·0013	1·28	0·0013	1·25
0·05 N	-	0·0011	2·22	0·0010	2·02
0·02 N	-	—	—	0·0011	5·6
0·01	-	0·0007	6·6	0·0007	6·8

The hydrolysis of soap solutions above decinormal (3 per cent. soap solution) is only a fraction of 1 per cent., and it is not very different for sodium and potassium salts. The falling off in alkalinity in concentrated solution is probably quite real, the chief experimental error is in the opposite direction and the diminishing alkalinity is readily accounted for by the disappearance of the hydrolysable palmitate ion to form ionic micelle. The addition of sodium chloride also diminished the alkalinity, which in this case, however, passes through a minimum, since whenever a soap solution becomes heterogeneous, its alkalinity becomes distinctly increased. Further measurments show that the soap solution persists in being distinctly alkaline, even in presence of large excess of palmitic acid. Thus an excess of 10 per cent. palmitic acid reduces the alkalinity to two-fifths of that given in the table, and even in presence of 100 per cent. excess of palmitic acid the OH′ is still 0·00004 N. It is very important to note that even this slight degree of alkalinity precludes the existence of more than minute traces of free fatty acid in any soap solution, so that any solid product of hydrolysis can never be free fatty acid, but must always be an acid soap, intermediate in composition between neutral soap and $NaHP_2$, where P represents the fatty acid radical.

It is equally important to note that when the excess of alkali is added, it is not appreciably taken up by the soap present but remains almost entirely in the free condition, in other words, basic salts are not formed.

If various fatty acids are compared it is found that degree of hydrolysis increases rapidly as the homologous series is ascended.

Influence of temperature again is important. The hydrolysis-alkalinity decreases with lowering of temperature as, indeed, would have been expected.

Very few measurements of the hydrolysis-alkalinity of commercial soaps have been made, but the OH′ concentrations of solutions made from soaps which were finished neutral, are usually less than that of sodium palmitate.

(b) *Degree of Hydrolysis.*

Whilst the experimentally determined values of the hydrolysis alkalinity, that is, the concentration of hydroxyl ions, seems to be well established, it is a matter of opinion how to interpret these in

terms of hydrolysis. The writer regards the extent of hydrolysis as being identical with observed alkalinity, in this particular case, as in most cases. Bancroft, however, in the Second Report (1919, p. 15) has expressed his opinion that soaps are really greatly hydrolysed, but that the hydroxyl ion is almost completely adsorbed by undissociated sodium palmitate. Not to mention the difficulty in accounting for the disposal of the equivalent quantities of palmitic acid set free, this interpretation is diametrically opposed to experimental data, which show that any excess of hydroxyl ion added to a soap solution, remains free in the solution as such and is not adsorbed.

(c) *Products of Hydrolysis.*

Since all soap solutions are slightly alkaline, they must contain at least small amounts of products of hydrolysis. This has been observed from the time of Chevreul, who found that most soap solutions exhibit fine suspensions of acid soap of varying composition, varying between nearly neutral soap and a soap in which the alkali is deficient by any value up to one-third of the theoretical value for a neutral soap. The more dilute the solution, as Krafft and Stern[17] have found, the more nearly the composition of the suspensions approaches that of a sodium hydrogen soap ($NaHP_2$). McBain, Laing and Taylor find that acid soap formed in palmitate solutions at $90°$ has the composition $HP.2NaP$.

Whereas in aqueous solution even the presence of one complete equivalent excess of palmitic acid still leaves free OH' present, a concentration 40 times as great as that required for coloration of phenol phthalein, the alkali is so rapidly diminished by the addition of alcohol that titration can be carried out in 40 per cent. ethyl alcohol, although 60 or 80 per cent. solution is distinctly preferable.

(d) *Effect of Carbon Dioxide.*

Although the dissociation constants of the fatty acids are perhaps 40 times greater than that of carbonic acid, yet the comparative insolubility of the fatty acids and of the acid soaps often results in extensive decomposition of the soap by excess of carbon dioxide. Even alcoholic solutions are decomposed. As a rule, the result of the interaction is the visible separation of acid soaps, as Krafft,[17] Stern,[17] and Wiglow[17], and Fendler[22], and Kuhn[22] showed. In the case of soaps from such oils as olive oil, however, the solution remains clear. Repeated treatment with carbon dioxide removed progressively less of the fatty acid, although ultimately nearly all may be removed. The equilibria involved have not as yet received quantitative study.

The solubility of the fatty acids is slight, nevertheless they are sufficiently strong to make it impossible for them to exist even in such concentrations in the presence of more than the merest traces of alkali hydroxide. The theoretical necessity for the existence of minute traces of free acid in all soap solutions is substantiated by the observation of Krafft and Wiglow, that soap solutions yield appreciable concentrations of fatty acid when shaken out with toluene or petroleum

ether, solvents in which the concentration of a fully saturated solution would be very great. The fact that such extracts are far from saturation—concentration is further proof that the fatty acid in the aqueous layer cannot have reached its minute saturation value either.

IV.—*Evidence for, and Properties of, the Ionic Micelle.*

(a) *Conductivity.*

Soaps particularly in concentrated solutions all exhibit a high conductivity quite comparable with that of, say, sodium acetate. This conductivity must be a property of the soap, since as we have seen, there is very little free alkali present, and there is nothing else to which the conductivity can be ascribed. These points are well brought out in the following table from McBain and Martin's paper, which comprises the conductivity of sodium and potassium palmitate solutions divided in each case into their two components; that due to free hydroxide as indicated above and the remainder of the observed conductivity which has to be ascribed to the soap itself. The ratios of the conductivity of the soap to that of the corresponding acetate solutions are also given for comparison :—

Conc.	Sodium Palmitate.			Potassium Palmitate.		
	μ NaOH	μ NaP.	NaP/NaAc.	μ KOH.	μ KP.	KP/KAc.
1·0 N	- 1·10	83·6	0·644	0·46	123·7	0·699
0·75 N	- 1·65	85·8	0·619	1·87	126·0	0·685
0·5 N	- 2·1	87·4	0·568	3·8	123·2	0·627
0·3 N	- 2·7	84·3	0·499	—	—	—
0·2 N	- 3·0	79·4	0·444	3·8	107·2	0·492
0·1 N	- 7·0	75·5	0·387	7·5	99·5	0·421
0·05 N	- 12·2	76·4	0·368	12·1	98·7	0·396
0·02 N	- —	—	—	33·5	99·7	0·380
0·01 N	- 36·0	101·7	0·446	40·6	131·0	0·435

The form of the conductivity curve is remarkable, and is such as has hitherto been observed only for certain anomalous non-aqueous solutions. Both relatively and absolutely, the conductivity is at a minimum between 0·05 N and 0·1 N solution. Thereafter, instead of decreasing steadily with increasing concentration, the conductivity rises to a pronounced maximum in 0·75 N solution. The relative conductivity as compared with the corresponding acetate, has nearly doubled with this same increase of concentration. These unique relationships clearly establish that when such soap solutions increase in concentration, the palmitate ion is being replaced by some other, much better conductor of electricity, namely, the ionic micelle already referred to.

The experimental evidence for the reliability of these results rests upon a very painstaking study carried out during several years by McBain and Taylor,[2] in which all possible sources of error were carefully studied and eliminated in the special case of sodium palmitate.

This was necessary in order to place beyond controversy the fact that colloids could exhibit a proper conductivity that could not be explained away as being due to unknown impurities in accordance with universal custom hitherto. There has since accumulated a mass of corroborative evidence in the measurements carried out by Taylor,[2] Cornish,[3] Bowden,[4] Bunbury,[4] Martin,[8] and Laing[12] in collaboration with the writer; as also the measurements carried out by F. Goldsmidt and his co-workers,[6] Weissman[7] and Kurzman,[9] and by Reychler,[5] and by Arndt and Schiff.[10] (See the classified list of references appended.)

When the results for the potassium and sodium salts of all saturated fatty acids from acetic to behenic acid and likewise the oleates, are reviewed, the utmost regularity is observed in the gradual and regular transition from a typical curve of an electrolyte presented by sodium acetate through the appreciable deviations of the lower fatty acids (hexoates and caprate) to the laurate, in which a step out or maximum and minimum is first observed (in the case of sodium and potassium laurate respectively).

It must be borne in mind that some of these solutions are extremely viscous whilst others are quite fluid. This enormous alteration in viscosity appears to exert no effect upon the conductivity. Work at present being carried out by Miss Laing in collaboration with the writer shows that when the soap solution has been converted into a solid gel, its conductivity, vapour pressure and concentration of sodium ions are identical with that of the same solution in a state of fluid sol at the same temperature and concentration. This revolutionary observation appears to us to be of great importance for the theory of gels since here the constituent out of which the mechanical structure of the gel is built up has to be recognised as constituting one of the best conductors present or the colloidal ionic micelle has to move as freely through the stiff colloidal gel (where structural constituent must then be neutral soap) as through the fluid sol itself. This work and the conclusions to which it leads will be reported upon elsewhere.

The independence between conductivity and viscosity is further exemplified by the temperature coefficient of conductivity. In some cases the viscosity is diminished several hundred fold by rise of temperature and yet the temperature coefficient of conductivity is no greater than in others where the viscosity is not so markedly affected. The fact that the temperature coefficient is rather higher than that of an ordinary typical electrolyte is ascribed to hydration and hence increased mobility of the ionic micelle with rise of temperature.

Finally it should be noted that although potassium and sodium soaps exhibit a surprisingly close general resemblance, the typical relationships which have been described are rather more markedly exhibited by the solutions of potassium soaps which evidently contain rather greater quantities of ionic micelle.

(b) *Osmotic Pressure.*

It is, perhaps, hardly realised how difficult it is to obtain really reliable and unambiguous determinations of the osmotic activities

of colloids. The well-known osmometer method in which the membrane is not always strictly semipermeable involves a further very serious and not completely evaluated complication described in Donnan's approximate theory of membrane equilibria. Results of such measurements therefore are almost always subject to a certain ambiguity of interpretations. The classical methods of boiling point, lowering of vapour pressure, and freezing point lead to results which are beyond question, only provided that the essential conditions for the application of these methods are fulfilled.

The classical experiments of Krafft and of Smits using boiling point and vapour pressure respectively were completely vitiated by the unsuspectedly large amounts of dissolved air from which soap solutions cannot readily be freed and which develop a partial pressure quite comparable with the lowering of the vapour pressure of an ordinary crystalloid or electrolyte. To carry out a single vapour pressure measurement really requires several weeks of effort. The result then obtained is in agreement with those by the dew point method, to be described below. The osnometer data of Moore and Parker,[16] in the case of soaps were wholly erroneous on account of the effects of carbon dioxide.

The effect of dissolved air can be completely eliminated and accurate measurements of the vapour pressure made, by a modification of the dew-point method described by McBain and Salmon.

The freezing point method is applicable where solutions remain in sol condition at the freezing point and where ice separates out in crystals free from colloid. In the case of soap solutions the latter condition was attainable by inoculation, but very few soap solutions are liquid at 0° C. Determinations of all these have been published by McBain, Laing, and Titley.[13]

Thus the bulk of the available data for soap solutions are those obtained by the dew-point method which possesses an additional advantage in that it can be utilised at any temperature.

The results of the lowering of dew-point and freezing point agree in establishing the fact that soap solutions have a very real osmotic pressure of the same order of magnitude as that of a non-electrolyte such as sucrose. A continuous series of values for the osmotic activity is obtained, depending upon the concentration and position in the homologous series, which range from a fraction of that for non-dissociated crystalloid to that of a moderately dissociated electrolyte.

In discussing osmotic data, it is almost always essential to keep clearly in view the very important effect which hydration exerts in magnifying the apparent osmotic pressure more especially in concentrated solution. For instance, the apparent dissociation of electrolytes frequently exceeds 100 per cent. according to osmotic methods.

It is generally agreed that hydration is greatest at lower temperatures. In the case of the potassium salts of the lower fatty acids this enhanced osmotic effect is clearly visible at 0° as compared with the more normal results at 90°, but in spite of the magnification of osmotic data at lower temperatures the osmotic activity exhibited by soap

is very decidedly less at 0° than at 90°. Soap solutions are very much more colloidal at lower temperatures than at the boiling point, and the presence of colloid extends into comparatively dilute solutions at 0°.

In case of the higher soaps the relative osmotic activity is practically independent of the concentrations above 0·4 N at 0°, showing how complete the formation of colloid has been. Again, potassium octoate is distinctive in its behavoiur at 0° in that in lower concentrations it is a typical electrolyte whereas in extreme concentrations the relative osmotic activity rapidly diminishes.

(c) *Concentration and Composition of the Ionic Micelle.*

The evidence for the existence of the ionic micelle is based primarily upon comparison of conductivity and osmotic data. Briefly it amounts to this, that in concentrated solution of the higher soaps, the osmotic activity is often only about one half that required to explain the conductivity. The conductivity is, say, 2/3 of that of a salt like the acetate, whereas the osmotic pressure is, say, half that of a non-electrolyte such as sucrose, and yet both sets of data are fully trustworthy. The osmotic activity, therefore, corresponds with that of one ion only, and the other half of the current must be carried by an ion that is colloidal so as not to exhibit appreciable osmotic activity, and that nevertheless retains the sum total of the electrical charges of the ions from which it was derived. This is the ionic micelle.

The osmotic activity measures the total concentration of crystalloidal matter of all kinds. The conductivity measures the K · or Na · in addition to the carrier of the equivalent negative charges. To reconcile the experimental data, say in normal solution of potassium laurate at 0°, it is necessary to ascribe the whole of the observed osmotic activity to the potassium ion, and even when this is done, about half of the conductivity is still to be accounted for and must then necessarily be ascribed to colloid. It is notable that the equivalent conductivity thus ascribed to the ionic micelle of potassium laurate is about three times greater than the sum total of the conductivity which the separate laurate ions that are grouped in the micelle, would have exhibited if they had retained an independent existence. This, however, as the writer has indicated above, is only what would have been predicted from Stokes' Law, since the resistance offered to a particle increases directly with its diameter. When a number of small particles coalesce to form larger particles, the diameter of the larger particle does not increase by any means in the same ratio, whereas the electrical driving force will be directly proportional to the number of aggregated laurate ions if these latter have retained their equivalent electrical charges. Therefore for electrical forces the mobility of such a large particle is very great, whereas its diffusibility is only that of a colloid.

It is necessary to make some sort of assumption as to the mobility of the ionic micelle for a particular case in order to be able to evaluate the conductivity observed. A safe provisional rule is to ascribe to the ionic micelle, the minimum conductance which will serve to

reconcile the conductivity and osmotic effects. Of course, when it is possible to take hydration into account as it is hoped to do when certain measurements have been completed, the equivalent conductivity of the ionic micelle will then be ascertained.

On reviewing the results for all concentrations and temperatures, it is found necessary in concentrated solutions to ascribe to the ionic micelle a conductance which is equal to that of the potassium ion. This is by no means identical with the well-known and striking fact that the mobility of many mechanical suspensions as well as of colloids in the electric field is as great as that of an ordinary ion, since in the case of the ionic micelle of soap it is not merely the mobility but the equivalent conductivity that is so great.

There is a good deal of evidence for the conception that the mobility of the ionic micelle increases with concentration owing to diminishing hydration due to increasing amount of neutral colloid in the micelle. The most important considerations is, that otherwise it would follow from the principle of mass action that within a very narrow range of concentration the colloidal electrolyte would pass completely into undissociated form and no longer conduct.

The small but unmistakable difference between a potassium and a sodium palmitate solution is not readily accounted for if the ionic micelle is considered to be merely an aggregate of palmitate ions, and, therefore, necessarily identical in the two cases. There are a number of reasons for considering that the ionic micelle contains some of the colloidal undissociated sodium and potassium palmitates respectively, which would thus account for the difference in behaviour, since thus the micelles are no longer identical.

For these reasons the formula ascribed to the ionic micelle in a soap such as sodium palmitate is—

$$(NaP)x \cdot (P')n \cdot (H_2O)m.$$
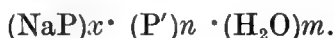
According to this formula, the composition of the micelle must alter continuously with change in concentration or temperature or upon the addition of salts. Thus in very concentrated solution or in presence of large amounts of another electrolyte such as sodium hydroxide or chloride, the soap must be nearly all colloid of approximately the composition—

$$(NaP)x \cdot (P')n \cdot (H_2O)m.$$
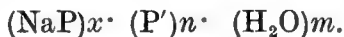
Again this formula accounts for the real difference, namely, in conductivity and osmotic behaviour, between solutions of potassium and sodium soaps, as was pointed out above.

With regard to the value of n, the number of negative charges on the micelle, it must be at least 10, and probably is very much greater. Thus the "molecular weight" of the ionic micelle must be at least of the order of magnitude of 3,000, although the true molecular weight of palmitate is only 255. In a similar way the enormous molecular weights ascribed to various substances which occur only in the colloidal form may well be derived from the aggregation of comparatively small molecules.

One solution may be cited to illustrate typical values of the concentrations arrived at for the respective constituents 0·6 N KOl at 0° — 18°, is found to contain 0·17 N potassium ion, not more than 0·01 of other crystalloidal matter, the remainder being entirely colloid, probably largely included in the ionic micelle and comprising 0·16—0·17 N aggregated oleate ion, with a total of 0·41—0·43 N aggregated neutral potassium oleate.

In many other cases, however, the limits of concentration of the constituents have not yet been so narrowly defined.

V.—*Physical Properties of Soap Solutions.*

Having discussed the general constitution of soap solutions, we shall now discuss in turn a number of their chief physical properties.

(a) *Viscosity.* (For references, *see* classified bibliography appended.)

As in the case of so many colloids the viscosity constitutes a prominent characteristic responding in typical fashion to alteration in experimental conditions. Whilst very readily measured and reproducible, the data do not lend themselves to quantitative interpretation and explanation. Nearly all our exact knowledge of this subject is due to the numerous and careful measurements of F. Goldschmidt[6] [7] and his collaborators, although Farrow carried out a series of exact measurements at 70° in Donnan's laboratory.

The viscosity increases with rise in concentration of the soap, at first gradually, then enormously. For instance, potassium oleate at 20° exhibits a viscosity of 1·19 for N/20, 1·87 for N/5, 8·02 for 0·4 N, and no less than 1573 for 0·6N, taking water as unity.

The effect of temperature in the case of less viscous soap solutions is practically that of the alteration of the fluidity of water.

Addition of hydroxide, chloride or carbonate at first lowers the viscosity, which passes through a minimum, and thereafter rises enormously. This was observed by Mayer, Schaeffer and Terroine,[26] Botazzi and Victorov[27] and Leimdörfer, Farrow,[28] Goldschmidt and Weissmann, and most carefully studied by Kurzmann.[9] The more concentrated the soap solutions, the more pronounced is the minimum, and the less the salt required to produce it.

The following data present typical cases of such minima produced by the addition of potassium hydroxide at 20° and at 90° to solution of 0·375 N, potassium laurate and 0·6 N potassium oleate respectively, the viscosity of water at 20° being taken as unity :—

At 20° the viscosity of the laurate is decreased from 1·96 to 1·57, that is, by 20 per cent. At 90° the decrease is from 0·604 to 0·522, being 14 per cent. At 20° the values for the oleate are 1573 to 728, by 54 per cent. At 90°, 3·80 to 3·01 by 21 per cent. These remarkable changes are produced by the addition of 0·5 N KOH to the laurate, but only 0·02N KOH to the oleate.

It is at once apparent that the lowering of the viscosity is dependent upon the presence of ionic micelle. There must be at least three primary effects at work in all these cases. The first, lowering is

dehydration through lowering of the reactivity (vapour pressure) of the water upon addition of the salt and its ions; for as McBain and Salmon have indicated, the heavily hydrated micelle must be extraordinarily responsive to such slight changes in the availability of the water. The second factor is formation of ionic micelle through the influence of the added potassium ion. The third, like the second, raises the viscosity, and consists in that unexplained change which, upon further addition of salt, tends to produce a jelly perhaps ten thousand times as viscous in the original soap. This last effect may well be due to the formation of neutral undissociated colloid and its subsequent linking up as explained in Section VI below. It is interesting to note that in accordance with all these views such high viscosities are more quickly obtained by adding further quantities of the soap itself, rather than by adding equivalent amounts of salt or alkali.

However, it must be concluded that the foregoing is merely a programme or working hypothesis for further experiment (a colloidal explanation used as a term of reproach), and quantitative work is called for in order to test whether or not the first two effects are due solely to the value of the total osmotic pressure and to the actual concentration of potassium ion respectively. Colloidal chemistry at present abounds in such problems and "explanations" which need to be replaced by precise and quantitative conceptions and measurements, otherwise such explanations do more harm than good.

In the case of concentrated soap solutions or those to which large amounts of electrolyte have been added, the temperature coefficient of viscosity becomes extremely great, so that the viscosity at $20°$ may be several hundred or thousand fold greater than at the boiling point.. Of course, the addition of these electrolytes tends to induce gelatinisation or, in the case of higher sodium soaps, salting out. Since salts effect the viscosity to different extents, depending upon the nature of the salt, the effect is ascribed to the anion. Obviously this requires further and quantitative elucidation.

The effect of position in the homologeous series is very marked. Even in rather dilute solution the sodium behenate (C_{22}) is highly viscous, whereas in the case of the laurate (C_{12}) only the most concentrated solution containing large amounts of electrolyte can really be termed viscous. A normal solution of potassium laurate at $20°$ and at $90°$ exhibits $8 \cdot 4$ and $2 \cdot 8$ times the viscosity of water, whereas a normal solution of potassium oleate is 1573 and $3 \cdot 80$ times as viscous as water at the same temperatures.

Potassium or sodium oleate (C_{18}) at room temperature is very much more viscous than say potassium myristate (C_{14}). It is probably much like the stearate (C_{18}) except that the effect of the double bonds is to render it liquid even at the freezing point. The effects of additions of an electrolyte is much more pronounced in the case of the oleate than the laurate, and again in concentrated soap solution as compared with dilute soap solution. In other words, it is evident that the typical effects of added electrolytes are to be attributed to their effect upon the ionic micelle and its development, concentration and composition.

Electrolytes with the common alkali ion must exert three chief effects upon a colloidal electrolyte. First the furthering of formation of ionic micelle if large quantities of simple ions are still present; secondly, driving back the dissociation with its corresponding alteration in the composition of the ionic micelle; and third, diminishing hydration of the ionic micelle.

The only mixtures which have been properly investigated are those of potassium laurate and potassium oleate. Addition of small quantities of potassium laurate to potassium oleate slightly increase the viscosity at 90°, whilst at 20°, the viscosity is greatly diminished, thus very much lowering the temperature coefficient. Such an addition must have partly the same effect of that of an electrolyte with the added complication of the possible formation of mixed ionic micelle. A mixture of equal quantities of potassium laurate and potassium oleate fairly closely resembles the pure laurate. A mixture of even relatively small amounts of laurate with the oleate causes the solution to respond to the addition of salts in a manner much more closely resembling the laurate than the original oleate.

An addition of equivalent quantities of various electrolytes the hydroxide has the greatest effect, followed by the chloride, and then the carbonate. Although if equimolar concentrations of carbonate and hydroxide are taken, the order of magnitude of the effects are the same.

It should be emphasised that other colloidal electrolytes exhibit the same typical behaviour in the addition of electrolytes as has been observed, for example, by Woudstra in the case of ferric hydroxide (chloride). According to Pauli this also is a case of a colloidal electrolyte.

An excellent example of an industrial application of this behaviour in the closely analogous case of sodium silicate is to be found in the use by Malcolmson (*Jour. Ind. and Eng. Chem.*, 1920, **12**, 174) of brine for diluting water glass whilst retaining its adhesive power. In this way a given volume of water glass could be increased by 25 per cent. without reducing its viscosity.

(b) *Density*.

Soap solutions are remarkable for their bulkiness. All soap solutions have approximately the same density as water of the temperature, and this even in extreme concentration. For example, the volume of a normal solution of sodium stearate is 31 per cent. greater than that of a solution of sodium acetate containing the same amount of water. Not only so, but we have here the almost unique case of the solution being in some cases less dense than either constituent.* For instance, a normal solution of sodium palmitate has a density equal to 0·997 of that of water of the same temperature (at 90°).

The potassium soap solutions are slightly denser than those of sodium. The ammonium soaps are somewhat lighter than the sodium soaps. The highest homologues of the fatty acids produce soap solutions of the least density. There is a steady increase as

* Compare the case of p. nitrotoluene in carbon bisulphide; Hyde, *Journ. Amer. Chem. Soc.*, 1912, **34**, 1507.

the homologous series is descended, with a definite break in the curve between the hexoate and the acetate, once again indicating the relatedness of the hexoate to the higher soaps. The appearance, washing power, density, and conductivity curve of potassium hexoate distinctly mark the beginning of that deviation from the behaviour of the acetate, which rapidly and regularly increases through the other members of the homologous series until it attains the typical character of the higher soaps.

It is worthy of note that the density of soap solutions does not by any means conform with the conception of electro-striction, although in these solutions there are almost as many ions and opposed electrical charges as in the case of a typical electrolyte.

(c) *Surface Tension.*

Some of the matter which might have been discussed under this heading will be referred to in paragraph 8 below, which deals with detergent action, and further, the rather extensive subject of soap films and bubbles will not be considered. Several years of further investigation would be required to answer all the questions which any chemist would at once ask with regard to the surface tension of soap solutions. The existing data are so isolated that it is still impossible to give a satisfactory broad survey of the subject. They refer, in a few instances to surface tension against air, but most of the experiments refer to surface tension at the interface with fatty or petroleum oils. In many cases the results are affected to an unknown extent by the carbon dioxide of the air, and frequently the composition of the soap is extremely ill defined, and in the experiments, for example, of Botazzi, the soap was rendered still more undefined by being submitted to a process of dialysis. Another factor which can interfere with the results is the rather ready oxidation of the unsaturated substance like the oleate. It is well worth while determining not merely the surface tension, but the actual amounts of adsorbed substance, and still further the actual composition of the " soap " in the meniscus; that is, the amounts of alkali, fatty acid, and hydrate water in the surface film. Lastly, an experimental difficulty which should be taken into account is the insolubility of the saturated fatty acids and of the acid soaps which separate out unless the solutions are kept hot.

Soap Solution—Air Interface.

Sodium oleate solutions in contact with the atmosphere constitute the only case in which the interfacial tension of the boundary between soap solution and a gaseous phase, has received any serious consideration. Plateau, Quinke, Marangoni, Lord Rayleigh, Milner, Hall, Harkins, Perrin, and Hillyer may be referred to in this connection, although some of the results refer to the chemical decomposition of the soap by carbon dioxide, oxidation, and hydrolysis. By far the most careful work is that of Harkins, Davies and Clark (*Jour. Am. Chem. Soc.*, 1917, **39**, 586), who, like Hillyer (*Ibid*, 913, **25**, 515), used the drop weight or drop number method. These authors found that when pure sodium oleate was added in increasing quantities to water

of surface tension 2·8 dynes per cm. at 20°, the surface tension falls at first with extreme rapidity to 24·29 dynes when 0·002 N sodium oleate has been added, but that further addition of sodium oleate causes only a slight and gradual increase of the surface tension to the value 27·20 dynes for decinormal solution. A 0·0001 N sodium oleate has a surface tension as low as 60·46 dynes and even when, to diminish hydrolysis, 0·0002 N NaOH had been added, the surface tension was still only 61·32 dynes. Similarly, a 0·008 N sodium oleate solution of surface tension 25·30 dynes still exhibits a surface tension of 31·79 dynes in presence of the comparatively large amount of sodium hydroxide 0·008 N. Harkin's conclusion is that the surface film consists largely of a single layer of sodium oleate molecules, with possibly singly and multiply charged oleate ions (ionic micelle), and hydroxyl and hydrogen ions, the film is thus entirely saturated even at low concentration of solution.

According to Hillyer the drop number of a decinormal rosin soap is the same of that of sodium oleate.

There is no recorded analysis of the composition of the substance actually adsorbed. Perrin's work in 1918 in this connection is again vitiated by the possibility that the observed effects were due to carbon dioxide.

It is worth while remembering that the power of foaming, like emulsification, is not always or entirely due to low surface tension; for instance, saponin solutions whose surface tensions are only slightly less than that of water, have a very great power of foaming on account of the formation of a solid or highly viscous film of adsorbed material in the surface.

Soap Solution—Oil Interface.

Here again the stalagometer or drop method has been almost invariably employed. An excellent and luminous investigation was published by Hillyer in 1903, and it was very unfortunate for this subject that his researches were not continued since his elucidations of the phenomena of surface tension and detergent action are still as convincing as ever. His papers are published in the *Journal of the American Chemical Society* which, as the writer knows from personal experience, has been inaccessible in many of the European Universities in spite of its present enormous circulation in America.

Hillyer found that in contradistinction to the behaviour of their surface tensions against air, the surface tension of sodium oleate solutions against paraffin oil (kerosene) continues to diminish rapidly and steadily with increasing concentration up to decinormal. Decinormal rosin soap like decinormal sodium oleate exhibits a surface tension which is only 5 or 6 per cent. of that of water. Decinormal sodium hydroxide has almost as great a surface tension against paraffin oil as pure water. Sodium oleate solution at 100° exhibited no marked difference in the form of curve connecting surface tension and concentration.

Hillyer also investigated the effect of excess of alkali and of acid, the former had but little effect upon any concentration of sodium oleate, whereas excess of oleic acid gradually and steadily removed

the lowering of surface tension, although even the acid oleate NaOl, HOl, causes a marked lowering of surface tension.

Finally Hillyer investigated solutions of sodium palmitate and stearate at 70° and 100°. The first traces of soap do not greatly lower the surface tension of the water but as the concentration passes through N/1000, the surface tension falls very rapidly at first and then above N/80 more gradually up to N/20 solution. This anomaly in very dilute solution was not further investigated, but was ascribed to hydrolysis. Rosin soap, whether at 100° or in the cold, does not differ markedly from sodium oleate, and is not very much affected by temperature. Both, however, exhibit a slightly developed inflexion in dilute solution like that of the stearate or palmitate.

Donnan and Potts (*Kolloid Zeitsch*, 1910, **7**, 208) measured the drop numbers of a nearly neutral paraffin oil against various concentrations of the sodium salts of the saturated fatty acids from the acetate up to the myristate (C_{14}). They found that the surface tension was lowered roughly in proportion to the concentration of the salts up to and including the heptoate (C_7). Decinormal solutions have a surface tension between 87 and 78 per cent. that of water. The octoate which appears as the lowest soap, has a much more marked effect, N/400 solution causing a lowering of 4 per cent. A N/400 myristate has a surface tension only 58 per cent. of that of water, and the curve connecting surface tension and concentration is only slightly curved. Donnan's earlier work (*Z. physikal Chem.*, 1899, **31**, 427), in which various fatty acids were dissolved in oils and their drop numbers determined against dilute alkali is explained by those results owing to the formation of soap in these experiments. This is invariably the case when commercial fatty oils come in contact wtih alkaline solutions, since they all contain more or less of free fatty acid, and it is only under these conditions that emulsification occurs.

Soap Solution—Benzene Interface.

Harkins, Davies, and Clark have further made exact measurements of the surface tension of sodium oleate against benzene. It falls extremely rapidly at first from the value for pure water, 35·03 dynes, through 9·00 dynes for 0·005 N, and 2·22 dynes for 0·014 N to 1·78 dynes for 0·1 N soap. It may be noticed this behaviour differs from both of those in the two interfaces already discussed. Briggs (*Jour. Phys. Chem.*, 1915, **19**, 210) has done experiments in which he measured instead the apparent amounts of sodium oleate removed from the aqueous layer, using a somewhat unsatisfactory and very incomplete method of analysis. He found a similar behaviour in that the amount removed increased only slightly in higher concentrations. Absolute results were not obtained. Briggs and Schmidt (*Ibid.*, 1915, **19**, 479) found that the optimum amount of soap for emulsification of the benzene is 1 per cent.; that is, three times the value given by Harkins, 0·01 N. Addition of 0·1 per cent. alkali favoured emulsification, but larger amounts interfered.

Shorter (*Jour. Soc. Dyer-Colourists*, **31**, 1915, 64; *ibid*, **32** (1916) 92), Shorter and Ellingworth (*Proc. Roy. Soc.*, 1916, A, **92**, 231),

showed first that potassium hydroxide has practically no effect upon the surface tension against benzene. Then he found that though the addition of oleic acid to benzene lowered the tension against water, it had no effect on the tension against soap solution, and he concluded that the surface activity of the hydrolysis alkali was only about one-fifth to one-fourth that of the undecomposed soap.

In all these cases again there is still the greatest uncertainty as to what it is that is adsorbed in the interface. In Spring's work, referred to in the section of " Detergent Action," he concluded that it was acid soap that was sorbed by lamp black, whereas filter paper sorbed alkali leaving acid soap in the solution.

Woodmansey (*Jour. Soc. Dyers Colourists,* **35**, 1901, 169) found that the amount of base sorbed from a solution of soap was greater than that of fatty acid, an acid soap remaining in the water.

(d) *The Optical Properties of Soap Solutions.*

Soap solutions constitute clear transparent solutions disturbed only by the suspended insoluble particles of acid soap resulting from hydrolysis, and exhibited in varying degrees by all soaps from caproate upwards. Here investigation (Darke and Salmon) shows that the clear liquid is not resolvable under the ultramicroscope, and that the ionic micelle is invisible in the cardioid ultramicroscope. The suspended particles of course exhibit Brownian movement, the purple ones being exceptionally active, the smallest green ones coming next. This Brownian movement serves as an excellent index to the viscosity of the liquid; it is scarcely detectable when the soap is highly viscous, and it is highly developed when most of the soap has been removed from solution as by the formation of curd. It is probable that with higher resolving power innumerable white particles would be visible, and that these constitute the particles of neutral colloid. We have detected them only when conditions of illumination, &c., were exceptionally good, just upon the limits of possible observation and they exhibited a degree of Brownian movement which corresponded to the fluidity of the medium.

Earlier investigations of the optical appearance of soap solutions whether microscopic or ultramicroscopic, have thus all had reference to the suspensions of insoluble acid soap produced in smaller or greater amounts by hydrolysis. The frequent turbidity of soap solutions has thus been an illusory proof of their colloidal nature. The insoluble acid soap may sediment out almost completely upon standing, leaving the bulk of the soap in prefectly clear transparent solution, or it may remain partly or wholly suspended in particles whose dimensions range all the way from coarse suspensions down to the smallest resolvable in the ultramicroscope. Every change of temperature or of concentration or of additions which affects hydrolysis will affect the amount of this product of hydrolysis. The acid soaps in suspension constitute an ordinary colloid.

It is now clear why Mayer, Schaeffer, and Terroine observed progressively more colloidal particles in any one soap by passing from alkaline to acid solution. Further it becomes plain why the viscosity of solutions of salts of the lower fatty acids up to the

valerianate is not markedly altered upon the addition of strong acid which liberates the soluble liquid fatty acid, although excess of base raises the viscosity. In the higher members of the homologous series, where insoluble acids and acid soaps are in question, addition of strong acids increases the viscosity appreciably through formation of colloidal acid soap; alkali does so also, although through formation of ionic micelle and neutral colloid. In accordance with this the higher soap solutions require a slight excess of alkali to clarify them completely by driving back hydrolysis. The authors named thought to identify the point of complete clarification with the point of minimum viscosity without carrying out the quantitative work and without a knowledge of the constitution of these solutions.

Lifschitz and Brandt have recently studied refractive index at 70°. They find that the refractive index varies linearly with the concentration; this held good also in the alcoholic true solutions. This undoubtedly shows that the molecular refraction is independent of whether the soap is present as crystalloid or colloid, and in the latter case is independent also of the actual constitution and of the degree of dispersion of such colloid. This conclusion was directly confirmed by the agreement of the values observed with those calculated from the atomic refractivities which, of course, have been obtained in the study of crystalloids.

(f) *Ultrafiltration and Dialysis.*

The behaviour to be expected in the ultra-filtration or dialysis of soap solutions is now clear on the basis of the theory which we have established. In the case of ultrafiltration the effect will depend largely upon which fatty acid is taken and the concentration of the soap. In dilute solution or solutions of the salts of the lower fatty acids, they will tend to pass through the filter unchanged. In more concentrated solutions of the higher soaps which contain very little crystalloid other than the alkali ions accompanying the ionic micelle, the membrane will retain practically all the soap. Intermediate cases will be particularly interesting as they will afford a direct quantitative measurement of the crystalloid matter present in such solutions.

The only published investigation is the brief note of Mayer, Schaeffer, and Terrione, already referred to, whose results were apparently in quantitative agreement with that here outlined. A systematic investigation is now in progress whose results likewise agree with prediction. Further it is readily shown that this may be made a general method for the study of hydration of colloids. It is only necessary to include in the solution to be filtered some readily analysed material which is not sorbed by the colloid and then to determine the increase in concentration of this reference substance which the filtrate exhibits. The amount of this increase affords a quantitative measurement of the total amount of water which was abstracted by the colloid from solution whether as adsorbed, or chemically combined water.

As regards dialysis, there are two points to be emphasized, the one theoretical, the other practical. In the first place the crystalloid present will be able to pass through the membrane so that in no case

will the membrane be quite impermeable to soap, and further, since dilute solutions in particular are appreciably hydrolysed, hydrolysis will be developed by the continued removal of the alkaline products of hydrolysis, leaving behind the insoluble or colloidal acid soap, the whole forming an instance of membrane hydrolysis. In the second place, however, one practical consideration vitiates the niceties of theoretical interpretation, that is that quite exceptional opportunities are afforded for decomposition of the soap by the carbonic acid from the air or from the large quantities of water employed. For instance, as we have seen Moore and Parker[16] obtained an osmotic pressure 50 times too small, and were unable to detect any alkali coming through ordinary parchment. Various other investigators, Rotondi, Botazzi, and Victoroff[27] found that acid soaps or possibly even free fatty acid remained behind on dialysis under such conditions. However, they, like Mayer, Schaeffer, and Terroine[26] found that as long as the solution was kept alkaline some of the soap did pass through the membrane.

VI.—*Solidification and Gelatinisation of Soap Solutions.*

The solidification and gelatinisation and salting out of soap solutions afford penomena of extraordinary interest which have not received adequate interpretation in the literature.

Upon cooling, or upon the addition of various substances white opaque curd may be formed, or again, there may be obtained typical gels, clear and transparent or ranging through various degrees of cloudiness. These two very distinct forms, white curd and clear gel have always hitherto been confused with each other and referred to indiscriminately. Hence there has been controversy as to whether they were colloidal at all.

Sodium oleate solutions as studied by Miss Laing present a particularly interesting case since here it is possible to obtain one and the same solution at any one temperature in the form of transparent sol, clear jelly, or white opaque curd. The conductivity of a given soap solution is independent of whether it is studied as sol or clear gel. Again the concentration of sodium ions present is also indentical in the two cases.

We are tending towards the opinion that in a gel there exist well-developed strings of long molecules forming an exceedingly fine filamentous structure which accounts for the elasticity of gels and also for the fact that they exhibit more or less clearly oriented properties, such, for instance, as the lenticular, fairly definitely oriented, form of bubbles generated within gels. This assumed structure is, however, not resolvable under the ultramicroscope, as is only to be expected. If we are able to obtain sufficiently definite evidence for this, it would appear that the same forces are in play here as account for the phenomena of crystalline liquids and liquid crystals. There is a great deal of circumstantial evidence for this. It would serve also to explain the incipient structure which most sols develop on standing and which is such as prevents for example, definite measurements of viscosity from being taken independent

of age and rate of shear. An illustration of this general and well-known behaviour is the case of sodium behenate, where occasionally a perfectly clear and apparently homogeneous solution can be divided by pouring into two test tubes, one of which will then be found to contain practically all of the behenate, the other containing almost only water.

Freundlich in his study of certain vanadium pentoxide sols which had been aged during many years, found that at boundaries or throughout the sol, whenever the sol was set in movement, it was anisotropic and exhibited all the behaviour of a crystalline liquid, a behaviour which is likewise now attributed to the presence of long molecules, both on physical and chemical grounds. This explanation therefore links up with Bose's theory of swarms of long molecules and with Vorländers' observations that long molecules are required for the formation of liquid crystals. Sandquist's bromophenanthrene-sulphonic acid is obviously a colloidal electrolyte, analogous to soap; and solutions can be obtained under certain conditions as crystalline liquids.

These strings of molecules which we contemplate may be microns or millimetres long, in other words, they consist of innumerable molecules placed length-wise and their formation would, of course, be ascribed to residual affinity. The very strong tendency of soaps to form such structures is proven by the manner in which they form filamentous and fibrous curds (see below). We investigated several sulphonates containing up to 10 carbon atoms, but as these are ring compounds, the development of ionic micelle was only very slight, and the solutions crystallised without tendency to form fibres.

The ultramicroscopic investigation of soap gels presents some difficulty, since it is difficult to know when it is a real gel that is being observed. Probably all of Zsigmondy and Bachmann's[29] work (like our own) refers to curds. A gel is probably optically clear apart from the particles which result from hydrolysis and which are, of course, devoid of motion on account of the high viscosity of the soap solution. Nevertheless in one undoubted instance, we observed and photographed a cloudy gel of sodium oleate which, in addition to curd fibres, contained an exceedingly fine and delicate filamentous network. The quartz surfaces in contact with these sodium oleate gels often exhibit indefinitely long and exceedingly fine filaments just on the limits of visibility and just capable of being photo-graphed with long exposures. Their regularity of form and texture is astounding. They simulate living matter in their appearance; they may take the form of a simple sine wave, or of regular waves with higher harmonic series superimposed on them. Any one part of such a filament is identical in form and structure with any other part. They are probably derived from originally straight just resolvable translucent tubes containing very regularly spaced whitish dots or lengths.

The beautiful pioneer observations of Zsigmondy and Bachmann on soap curds and our own studies, in which the cinematograph was combined with the ultramicroscope in order to follow the genesis

and life histroy of the various structural elements, have shown that soap curds consist invariably of fine fibres.

In the case of all sodium soaps these may be many centimetres in length, and may be straight or characteristically curled but they are never of greater thickness than about 1 micron. Most of them are of ultramicroscopic diameter, and the thicker ones may be always or usually merely parallel bundles of finer ones These fibres are often so fine that shorter or unattached fibres exhibit Brownian movement when the medium is not too viscous. These curd fibres constitute the only mechanical structural element of sodium soap curds and represent the stable condition of such curds even after the lapse of years.

The potassium soaps are somewhat more complicated and the phenomena require more time for development. Upon slow cooling, minute V-shaped twin fibres appear of about a few tenths of a micron in diameter, but never more than a few hundredths of a millimetre in length as compared with the inch-long fibres of sodium soaps. These twin fibres however, do not constitute the stable solid phase in potassium soap solutions for, on standing for a few days, they gradually become replaced by thin irregular leaflets or crystals less than 1 micron in thickness.

The curd fibres of sodium soaps may scarcely be called crystals, nevertheless they constitute a sufficiently definite phase to exhibit some of the properties of crystals. For instance, they appear to exhibit a definite solubility at each temperature. A curd of sodium soap therefore in general consists of a felt of curd fibres in which is enmeshed an aqueous liquid containing, according to the temperature, traces of soap and alkali, the product of hydrolysis, or perhaps large amounts of soap sol or gel depending upon age, previous history and, of course, temperature.

At the temperature of initial solidification only a few fibres are formed, the bulk of the soap remains in the solution which therefore exhibits a practically undiminished vapour pressure and conductivity. As the temperature is lowered, the solubility of the curd fibres rapidly diminishes until the enmeshed liquid consists chiefly of water and its vapour pressure and conductivity behave accordingly. Throughout this range of temperature the stable condition of the soap solution is the formation of the appropriate amount of curd fibres with enmeshed gel. The definite solubility of the curd fibres at any one temperature is evinced by the fact that the conductivity of a well-aged curd is approximately independent of the concentration of the original soap.

It follows from the above conception based upon direct measurements that different concentrations of any one soap cannot solidify at the same temperature, but this depends upon the temperature-conductivity curve of the curd fibres. The lower the concentration of the soap, the lower the temperature of initial solidification.

It is a remarkable fact to which Krafft and Wigelow[17] called attention, that there is a certain parallelism between the initial solidification temperature of a sodium soap solution and the melting point of the pure anhydrous fatty acid from which the soap is derived.

This is strictly true for only one concentration. Rather concentrated soap solutions have an initial solidification temperature which is very near the melting point of the fatty acid, dilute solutions of course fall much below it, whilst, as we have found, highly concentrated soap solutions come distinctly above it. No tenable explanation of this behaviour has been put forward. It is probable that the soap fibres consist of hydrated neutral colloid and that the slight alkalinity of the mother liquor is due to the particles of acid soaps resulting from hydrolysis and which we have so often referred to. There are particles in fact still visible under the ultramicroscope leading an independent existence in the wet curd, and it is amusing to watch a small particle performing violent Brownian movement up and down the length of a curd fibre from whose neighbourhood it often has difficulty in escaping. This form of Brownian movement has not previously been described. The coincidence with regard to melting points refers of course, only to sodium soaps.

It is worth while emphasizing that neither gelatinisation nor formation of curd is identical in kind with the process of attainment of equilibrium within the liquid sol. A true reversible equilibrium is attained from both sides within the sol whilst it is supersaturated with respect both to gelatinisation and formation of curd.

Several authors have observed that formation of curd is accompanied by the evolution of heat.

VII.—*The Effects of Additions.*

(a) *Excess of Fatty Acid.*

In the case at least of the saturated fatty acids, the addition of excess of acid results in the formation of acid sodium or potassium salts which are insoluble. These acid soaps probably do not correspond to any very definite chemical formula and as yet no complete chemical analysis of such a specimen has been obtained from which, for example, the amount of water in it could be deduced. As regards amount of alkali, their composition lies between that of the acid salt exemplified by the acid sodium acetate, $NaHAc_2$, and that of neutral soap. These acid soaps do not lie within the region investigated from the standpoint of the phase rule by Donnan and White,[19] who showed incidentally that possibly no pure acid sodium palmitate $NaHP_2$ exists.

In the case of sodium palmitate, we have found that addition of palmitic acid to the solution causes a change in the conductivity and also in the vapour pressure. Both conductivity and lowering of vapour pressure when plotted against the amount of acid added, are seen to diminish linearly to a small value at a point corresponding roughly to $HP \cdot 2NaP$. At this point the magnitude of the conductivity and lowering of vapour pressure although small is definite and independent of the original concentration of the sodium palmitate. This behaviour clearly points to the formation of the insoluble acid soap $HP \cdot 2NaP$, in equilibrium with its saturated solution. Further addition of palmitic acid causes both the lowering of vapour pressure and conductivity of the aqueous fluid to become still smaller, showing

that the acid soap becomes more and more insoluble as the amount of acid in it increases beyond the composition mentioned. These " solutions " appear as translucent pastes or jellies at 90°. On cooling silky fibrous crystals of acid soaps may separate. More dilute solutions froth very readily.

(b) *Excess of Sodium Hydroxide.*

Most of the phenomena occurring upon the addition of caustic soda or other electrolytes, which ultimately result in the quantitative salting out of the soap from the solution, will be discussed in Part II., which deals with the technical colloidal chemistry of soaps, their solutions, gels, and curds.

Rarely are homogenous solutions of soap to be met with during the process of manufacture, since these would be of prohibitively great viscosity. Usually, as has been found in this laboratory, the soap is in the form of an emulsion of two soap solutions, whether sol-sol, sol-gel, or sol-curded gel. The experimental evidence will be published elsewhere.

A number of theoretical points also must be reserved for forthcoming publications. However, it should be mentioned here that for the greater part these additions do not actually combine with the soap, but drive back its dissociation or displace the numerous equilibria involved.

VIII.—*Detergent Action of Soaps.*

The brilliant work of a number of such investigators as Hillyer (1903) and Spring (1908) has shown conclusively that the detergent power of soaps is due to their colloidal nature, and it is only influenced by their chemical constitution in so far as this affects their behaviour as colloids.

In commerce soaps are usually bought and sold upon their appearance and texture after assurances have been given with regard to their classification as curd soaps, tallow rosin soaps, cleanser soaps, &c. There are no universally accepted quantitative standards to which soaps are referred with regard to their washing power. It is convenient to give a list of all the cognate experimental methods hitherto published as having more or less bearing upon this question :—

    (1) Measurement of surface tension against air by capillary tubes or by drop members or by bubbling or by measuring the amount of froth produced under definite conditions.

    (2) The measurement of surface tension against oil or paraffin oil or benzene by drop numbers or measurement of emulsification.

    (3) Measurement against carbon or other powders by measuring rate of sedimentation or protective action in filtration.

    (4) Protective action as measured by gold numbers.

    (5) Direct washing experiments with specially soiled clothes under controlled conditions of true temperature and concentration. (*Zhukov and Shestakov, Chem. Ztg.*, 1911, **35**, 1027.)

It is, of course, impossible to give here a summary of the mass of information, chiefly of a technical character, with regard to the detergent action of various soaps and soap substitutes and their effect upon various textiles, but the chief theoretical results will be referred to.

Krafft has quite rightly insisted that the soaps must be in solution in order that such solutions should exhibit appreciable detergent action. Thus in cold water sodium palmitate and stearate are nearly insoluble, whilst the soluble oleate is an ideal detergent, and even the only moderately soluble laurate and rosinate wash fairly well. Hot water is necessary for the use of tallow soaps, which consist largely of palmitate and stearate; as Jackson (Cantor Lecturer) has pointed out, potassium soaps are more soluble in the cold than sodium soaps. We would add here the deduction from our measurements of the amount of ionic micelle present in various pure soap solutions, that the washing power which the oleates exhibit *par excellence* is connected with the fact that their colloidal nature persists well into dilute solutions; whilst the laurate in dilute solutions and more especially at higher temperatures is largely broken down to simple electrolyte. For this reason, stearate is suitable for washing at the boiling point, since at this temperature it is soluble and it contains much more colloid than the lower soaps. Such considerations are of importance, for example in the choice of a suitable soap for the washing of woollen goods in the cold.

Earlier theoretical writers, such as Chevreul, Berzelius, Persoz, von Bussy, Waren-Delarne, Knapp, Stiepel and Moride emphasized the emulsifying powers of soap solutions and of soap foam towards fats and fatty materials. The capacity of soap solutions to wet oily matter was also emphasized whilst the unknown amount of hydrolysis alkali was credited with powers of saponification.

Hirsch, in 1898, showed experimentally that fatty oils were not more readily emulsified than were various other organic liquids, and he demonstrated also that the large amount of oil emulsified was quite out of proportion to the small amount of soap present. This showed that the effects produced were ascribable to the soap itself and not to any alkali present.

Donnnan (*Zeitsch physical Chem.*, 1899, **31**, 42) showed experimentally that emulsification and lowering of surface tension went hand in hand in the case of solutions of different soaps. Plateau had pointed out that in general formation of solid surface films might be quite as effective as low surface tension in emulsification.

Hillyer (*Jour. Amer. Chem. Soc.*, 1903, **25**, 511, 524, and 1256) showed experimentally that the emulsifying properties of soap could not be attributed to hydrolysis alkali hydroxyl ions, nor did alkali possess the power of wetting oily matter that soap did. Hence both these factors in detergent action must be due to the soap itself. Of course, as Donnan had pointed out, the free fatty acid contained in all natural fats and fatty oils could be neutralised with formation of soap this would be quite different from saponification of glycerides which is an extremely slow reaction. Hillyer demonstrated again the parallelism

of low-surface tension and emulsification in the case of soaps, and incidentally showed that saponin emulsified through formation of solid surface film instead of through low surface tension. Hillyer also demonstrated the power of penetration into capillary interstices which is conferred upon soap solutions by their very low surface tension.

We have then experimental proof of the operation of two of the previously assumed factors of detergent action, emulsification (parallel to low surface tension), and wetting power, both ascribable to the undecomposed soap itself. The third suggested factor, the action of soap in making tissue and impurities less adhesive to one another was also put forward again by Hillyer, but the experimental evidence is entirely due to Spring (*Kolloid, Zeitsch*, 1909, **4**, 161; 1909, **6**, 11, 109, 164; *Arch. Sci. phys. nat.*, **28**, 569; **29**, 41, **36**, 80; *Rec. trav. chem.*, **29**, 1; *Bull. acad. Roy. Belg.*, 1909, 187, 949; 1911, **24**, 17). Goldschmidt has already postulated a protective action of the colloidal soap upon dirt particles, since Zsigmondy had shown that the gold number of sodium stearate (the minimum quantity to protect 10 c.c. of red gold sol from colour change upon addition of 1 c.c. of 10 per cent. sodium chloride solution) was 10 mg. at 60° and 0·01 mg. at 100°. The value for sodium oleate was 0·6 to 1 mg.

Spring pointed out that all previous workers had been imbued with the conception of dirt as being of a fatty or oily nature, or covered with a coating of such nature. In his experiments, therefore, he carefully eliminated all fatty matter, which leaves the detergent action of soap quite unimpaired. His striking and original experiments dealt with purified lamp black, silica, alumina, and iron oxide.

Spring's conception is that carbon promotes the hydrolysis of a soap solution and forms a stable non-adhesive sorption compound with the acid soap produced. Dirt upon a fabric he regards as being combined with the fabric in an analogous way. Cleansing by soap is simply the formation of a sorption compound dirt and soap in place of the sorption compound dirt and fabric by direct substitution. A more logical alternative is that of double decomposition in which two sorption compounds are formed, fabric + soap and dirt + soap. As a matter of fact it is often extremely difficult to remove soap from a fabric after the operation of washing. Spring points out that alcoholic solutions possess poor detergent power because hydrolysis is not so great, this is not true if the alcoholic soap is used in water. However, he found that whilst lamp black took up acid soap, ferric oxide, silicic acid, and cellulose take up soap containing an excess of alkali, so that his results in some cases might be more logically attributed to soap itself. The basic soaps of which he speaks do not in fact exist. The poor detergent action of alcoholic soaps on this view would simply be ascribable to the fact that in alcohol the soap contains only traces of colloid.

Spring's two chief experimental results, are that lamp black is carried through filter paper which, in the absence of soap, would retain it completely, and, secondly, that the rate of sedimentation is a function of the concentration of soap or, indeed, of alkali present. Lamp black sedimented as fast in 2 per cent. soap solution as in water, whereas in 1 per cent. solution it remains suspended for

months, and in $\frac{1}{2}$ per cent. solution for days. In the case of ferric oxide, the optimum concentration was $\frac{1}{2}$ per cent. soap, and in the case of potters' clay, 1/32 per cent. soap. Alumina showed remarkable periodic optima in 1/4, 1/8, and 1/16 per cent. soap, with a similar numerical periodicity of coagulation. Zhuknov and Shestakov found that in laundry practice the best results were obtained with $0\cdot2$ to $0\cdot4$ per cent. soap solution.

Donnan and Potts found a similar optimum concentration in the emulsification of paraffin oil by soap solutions in a concentration of N/300. They emphasize that the excess of soap in the interface which is a necessary corollary of the lowered surface tension and the viscous nature of the surface film must both contribute to the stability of the emulsions. Pickering (*Jour. Chem. Soc.*, III., 86) agrees both with Donnan and Spring, but considers also that oil and other substances are soluble in the presence of soap.

Jackson (*Jour. Soc. Arts*, **55**, 1101 and 1122 (1908), Cantor Lectures) called attention to the influence which soap exerts upon the state of subdivision of the dirt, and he observed under the microscope dirt particles and fibres of linen being brought first into oscillation and then completely loosened by a soap. This spontaneous action was best exhibited by an alkaline oleate. He also, pointed out that the presence of glycerine in soaps had only an unimportant influence upon their detergent action.

Shorter (*loc. cit*) adduces experiments from which he concluded that acid soaps exhibit no surface activity and that detergent action is due mainly to undecomposed soap. Excess of alkali enhances the detergent action. Developing the consideration of the electrical effects introduced by Donnan and Potts (*loc. cit*), he points out that the effect of the alkali is to increase the negative charge both on the particles of oil and dirt and on the surface to be cleansed, which tends to prevent coagulation and redeposition of those particles.

It emerges from all this discussion that there are a number of definite factors in detergent action; first, the necessity of having the soap in solution; second, power of emulsification which goes parallel with low surface tension and the formation of surface films; third, wetting power which like the last, is ascribable to the undecomposed soap itself; fourth, the action of soap in forming non-adhesive colloidal sorption compounds with tissue and impurities due sometimes to acid soap, but more often to soap itself and capable of remaining in stable suspension; fifth and lastly, it is an essential in all cases that the soap should be in colloidal form.

It is evident that comprehensive quantitative work is necessary to complete and co-ordinate the existing fragmentary work in any one case. Each of these factors are perfectly capable of simultaneous determination and quantitative evaluation.

IX.—*Retrospect.*

We have seen that all the phenomena of soap solutions point to the existence of a highly conducting heavily hydrated ionic micelle of the general formula $(NaP)_x \cdot (P')_n \cdot (H_2O)_m$; and that from

the scattered indications in the literature it is evident that many colloidal substances of great industrial importance must exhibit similar behaviour and be classed together with soaps as colloidal electrolytes.

This conception leads then to the following complete circle of continuous transitions between types of solutions and sols through each of the well-recognised intermediate types. For convenience, the circle is given as a table. The $+$ signs indicate the relative extent to which osmotic pressure and conductivity are exhibited by the various standard types. The table is intended to represent the natural sequence of transition in which consecutive pairs are much closer than non-consecutive pairs. Thus there is evidently a gradual transition from neutral colloid through semi-colloid (not through charged colloid or colloidal electrolyte) to ordinary crystalloid, that is, non-electrolyte. Some of these transitions may be realised in the case of soap solutions by merely altering the concentration or temperature or by passing from one member of the homologous series to another.

TABLE I.

Circle of Continuous Transition between Types of Solutions and Sols.

Group.	Examples.	Osmotic Activity.	Conductivity.
Crystalloid, non-electrolyte	Sucrose	$+ + +$	0
Semi colloid	Dextrines	$+$	0
Neutral colloid	Pauli's egg albumin	0	0
Charged colloid	Gold sol	$+$	$+$
Colloidal electrolyte	Soap, dyestuffs	$+ +$	$+ + +$
Crystalloidal electrolyte	KCl	$+ + +$	$+ + +$
Electrolyte	$HgCl_2$	$+ + +$	$+$
Crystalloidal non-electrolyte.	Sucrose	$+ + +$	0

The conception here presented of the constitution of soaps and their solutions, gels, curds, and sediments is only a preliminary sketch but its main outlines would appear to be authenticated by experiment.

SELECTED REFERENCES.

Free use has been made in the text of the various investigations carried out in this laboratory, a number of which have not yet appeared, and a few of which are not yet completed.

I.—*Conductivity.*

1. KAHLENBERG and SCHREINER. (Zeitschr. physikal. Chem. 1898, **27**, 552.)

Conductivity of dilute solutions of sodium oleate, potassium stearate and potassium palmitate. Freezing points of N/8 and N/16 sodium oleate. Boiling point experiments showing that the method was unreliable.

2. J. W. McBain and M. Taylor. (Zeitschr. physikal Chem. 1911, **76**, 179)

Very painstaking measurements of the conductivity of sodium palmitate in all concentrations, sodium, acetate, sodium hydroxide, sodium palmitate with excess of hydroxide and of palmitate acid; boiling point determinations showing that the method was inapplicable, to a vapour pressure measurement in which the vitiating influence of dissolved air was largely eliminated; salting out experiments and analyses of sediments.

3. J. W. McBain, E. C. V. Cornish, and R. C. Bowden. (' Trans. Chem. Soc.' 1912, **101**, 2042.)

Conductivity and densities, solidification, temperatures and appearance of sodium laurate and myristate at various temperatures.

4. R. C. Bowden. (' Trans. Chem. Soc.' 1911, **99**, 191.)

Conductivity of sodium stearate at 90°.

5. A. Reychler. (' Kolloid Zeitschr.' 1913, **12**, 277; 16th ' Vlaamsch Nat Gen. Congres, Leuven,' 1912, 69; ' 8th Intern. Congress App. Chem.' 1912, **22**, 221; ' Bull. Soc. chim. Belg.' 1913, **37**, 300; **27**, 217; **26**, 193, 485; **27**, 110, 113.)

Conductivity of sodium palmitate, cetyl sulphonic acid, sodium cetyl sulphonate, cetyl sulphonate of tri-ethyl cetyl ammonium, hydrochloride of di-ethyl cetyl amine, tri-ethyl cetyl ammonium iodide, sodium oleate. Boiling points of the frothing solutions of many of the above, unreliable. Sediments in sodium palmitate solutions, extractability of sodium oleate solutions by toluene.

6. F. Goldschmidt and L. Weissmann. (' Kolloid Zeitschr,' 1913, **12**, 18; ' Seifensieder Zeitung,' 1914, **41**, 337.)

Conductivity and viscosity of the ammonium soaps of the fatty acids of palm kernel oil at various temperatures and concentrations, with and without additions of ammonia and ammonium chloride.

7. F. Goldschmidt and L. Weissmann. (' Zeitschr. Elektrochem,' 1912, **18**, 380.)

Conductivity and viscosity of the potassium soaps of the fatty acids of palm kernel oil with and without additions of potassium hydroxide and potassium chloride.

8. H. M. Bunbury and H. E. Martin. (' Trans. Chem. Soc.' 1914, **105**, 417.)

Conductivity, density and appearance of potassium salts of the fatty from the acetate to the stearate at 90°.

9. J. Kurzmann. (' Kolloidchem. Beiheft.,' 1914, **5**, 427; Dissert. Karlsruhe. 1914.)

Conductivity and viscosity of potassium myristate, laurate and oleate and their mixtures with each other and with potassium hydroxide and potassium carbonate at various concentrations and temperatures.

10. K. Arndt and P. Schiff. (Kolloidchem. Beihefte. 1914, **6**, 201.)

Conductivity, viscosity sediments and solidification of sodium and potassium palmitates with and without added chloride.

11. H. Pick. (Seifenfabr. 1915, **35**, 255–7, 279–81, 301–5, 323–5.)

Probably a review of work chiefly from this laboratory.

12. M. E. Laing. (' Trans. Chem. Soc.' 1918, **113**, 435.)

Conductivity and boiling point measurements of dry alcoholic solutions of potassium oleate and of oleic acid.

13. McBain, M. E. Laing and Titley. ('Trans. Chem. Soc.' 1919, **115**, 1279.)

Conductivity, density, and freezing points of potassium, laurate, oleate, octoate, acetate and decoate and of sodium oleate and acetate. Formulation, concentration and mobility of the ionic micelle. Dew point measurements of ammonium laurate and palmitate and of potassium chloride, laurate, octoate, and oleate at 20°.

14. J. W. McBain and C. S. Salmon. ('Proc. Roy. Soc.' and 'Journ. Amer. Chem. Soc.,' March 1920.)

Dew point measurements of most of the sodium and potassium soaps down to the acetate at 90°. Formulation, concentration and mobility of the ionic micelle.

15. J. W. McBain, M. E. Laing and M. Taylor. (Not yet appeared.)

Conductivity, dew point and density of sodium palmitate solutions containing various additions of sodium hydroxide and of palmitic acid.

II.—*Molecular weight and Osmotic activity.*

1, 2, 5, 12, 13, 14, 15 above.

16. B. Moore and W. H. Parker. ('Amer. Journ. Physiol.', 1902, **7**, 261.)

B. Moore and H. E. Roaf. ('Kolloid Zeitschr.', 1913, **13**, 133.)
Osmometer experiments with soaps giving unreliable and erroneous results, in some cases of the wrong order of magnitude.

17. F. Krafft and A. Stern, H. Wiglow, A. Strutz. ('Ber.', 1894, **27**, 1747, 1755; 1895, **28**, 2,566; 1896, **29**, 1,328; 1899, **32**, 1584.)

Ebullioscopic measurements of the sodium and potassium salts of most of the fatty acids in water and in alcohol, in most cases vitiated by the partial pressure of air in the soap bubbles. Salting out. Extractability with toluene. Appearance and sediments Effect of carbon dioxide.

18. A. Smits. ('Zeitschr. physikal. Chem.', 1902, **39**, 385; 1903, **45**, 608.)
Tensimetric determinations of sodium palmitate vitiated by partial pressure of air present.

19. W. M. Bayliss. ('Proc. Roy. Soc.', 1910, B, **81**, 269.)

F. G. Donnan and A. B. Harris. ('Trans. Chem. Soc.', 1912, **99**, 1,554.)
Conductivity and osmometer measurements with Congo-red.

III.—*Hydrolysis.*

20. J. Lewkowitsch. ('Zeitschr. Angew Chem.', 1907, **20**, 951.)

The hydrolysis of soap solutions could never be complete since it was impossible to extract all the fatty acid by shaking with another solvent.

21. D. Holde. ('Zeitschr. Elektrochem.', 1910, **16**, 436.)

Extraction of aqueous alcoholic soap solutions by petroleum ether
See 2 above.

22. G. Fendler and O. Kuhn. ('Zeitsher. angew. Chem.', 1907, **22**, 107.)
Effect of carbon dioxide upon various soaps. *See* 17 above.

23. F. Goldschmidt. ('Chemiker. Zeitung.', 1904, 302.)
Hydrolysis in aqueous alcoholic solutions of soap

24. J. W. McBain and H. E. Martin. ('Trans. Chem. Soc.', 1914, **105**, 957.)

Determinations of hydrolysis alkali by hydrogen electrode in sodium hydroxide, sodium and potassium palmitates, potassium myristate to acetate, with and without excess of palmitic acid or alkali or sodium chloride at various temperatures and concentrations and some commercial soaps at 90°.

25. J. W. McBain and T. R. Bolam. ('Trans. Chem. Soc.', 1918, **113**, 825.)

Determination of hydrolysis-alkali by rate of catalysis of nitrosotriacetonamine in sodium acetate and palmitate with and without added sodium hydroxide and of potassium palmitate at various temperatures and concentrations.

IV.—*Viscosity.*

6, 7, 9, 10, above.

26. A. Mayer, G. Schaeffer and E. F. Terroine. ('Compt. rend.', 1908, **146**, 484.)

Viscosity, appearance, sediments, dialysis of sodium oleate, from the caproate to the stearate and oleate, with excess and deficiency of alkali. Ultramicroscopic particles.

27. F. Botazzi and Victoroff. ('Atti. R. Accad. Lincei.', 1910, **19**, i., 659.)

Surface tension, viscosity and appearance of a dialysed Marseilles soap of unknown composition, with and without addition of alkali.

28. F. D. Farrow. ('Trans. Chem. Soc.', 1912, **101**, 347.)

Viscosity of sodium palmitate solutions at 70° in various concentrations with and without additions of palmitic acid, sodium hydroxide, sodium chloride or potassium chloride.

V.—*Optical.*

29. R. Zsigmondy and W. Bachmann. ('Kolloid. Zeitschr., 1913, **11**, 145.)

Ultramicroscopic observations of the formation of curd in aqueous and alcoholic solutions of sodium and potassium oleate, stearate and palmitate, also solubilities and sediments and solidification temperatures.

30. J. Lifschitz and J. Brandt. ('Kolloid-Zeitsch.', 1918, **22**, 133.)

Refractive index of sodium stearate, palmitate and oleate solutions and of potassium palmitate in various concentrations at 70°.

[Note.—The subject of soap manufacture will be dealt with by Prof. J. W. McBain and Mr. Ernest Walls in the 4th Report of the Committee.—W. C. McC.L.]

ULTRA-MICROSCOPY : DEGREE OF DISPERSION— MEASUREMENT WITH THE ULTRA MICROSCOPE.

By George King, M.Sc., F.I.C.

The physical and chemical phenomena observed in colloidal solutions, depend upon four factors : the *concentration* of the disperse phase, its *size, shape,* and *internal structure.* A knowledge of these factors is desirable in order that the phenomena observed in any particular investigation may be interpreted in terms of the energy distribution in the system.

(a) *Concentration of Disperse Phase.*

The concentration is often determined by the evaporation of the dispersion medium—this method is satisfactory only in the absence

of non-volatile substances of molecular dimensions. The concentration of the disperse phase in hydrosols is most conveniently determined by ultra-filtration through collodion membranes. The method used by Zsigmondy[57] gives very accurate results and is less complicated than the Bechold[2] method. By suitably varying the composition of the collodion (Schoep[35]), a rough grading of a non-uniform disperse phase is rendered possible, those colloidal particles above a definite size only, remaining on the filter. Certain physico-chemical methods, such as colorimetric determinations have been successfully used to determine concentration and Marc[34] has developed the interferometer for technical use, whilst Mecklenburg[25], and more recently, Tolman[48], have used the intensity of the light scattered by a colloidal solution (Tyndall Beam[31]).

(b) Size, Shape, and Internal Structure.

The size of the particle more than any other property determines the behaviour of a particular solution, and its estimation is therefore of considerable importance.

According to Ostwald the dimension is best expressed as the degree of dispersion, or, the ratio of the absolute surface of an individual particle of the disperse phase, to the volume of that particle. von Weimarn[50], however, has pointed out that this ratio alone does not sufficiently determine the properties of the system, on account of the difference in the internal structure of two otherwise similar particles. Moreover the shape and hence the value of the ratio, depends upon the method used for the preparation of the colloid (Svedberg, Gans[12]). Since nothing definite is known of the internal structure and shape of the particle under particular experimental conditions, it is more usual to express the size in terms of the linear dimensions of the particle. The method used for this determination depends upon (1) the physical condition of the continuous phase and the size of the particle, (a) microscopic, (b) sub-microscopic. Suspensions containing particles as large as $10 \mu = \cdot01$ mm. exhibit colloidal properties—e.g., Brownian movement, cataphoresis—and for the purpose of classification, such particles are called microns[58]. The lower limit for the micron is conventionally fixed at $\cdot2 \mu$ and corresponds with the limit of microscopic visibility as determined by Johnston Stoney[19]. Sub-microns are detected by means of the ultra-microscope, and range from $\cdot2 \mu$ to the lower limit $3 \mu\mu = \cdot000003$ mm. observed by King[20] using the Zsigmondy[59] immersion ultra-microscope.

I.—Continuous Phase Solid.

(a) Microns.

Owing to the absence of Brownian movement, direct microscopic measurements can be made. Tinker[47] using an oil immersion fluorite objective, and improved illumination, was able to measure the particles in a semi-permeable membrane up to the theoretical limit of microscopic visibility.

(b) *Sub-Microns.*

Two methods each depending upon the application of the Tyndall effect are available, but they cannot be applied to the non-transparent solid solutions encountered in metallurgical practice.

1. *Rayleigh's Formula.*—It is well known that an incident beam of light falling upon particles, small compared with the wave length of light, is not reflected but scattered and polarized. The intensity of the scattered light varies according to the Rayleigh[32] formula $I_s = Anr^6/\lambda^4$ where A is a factor which depends upon the experimental conditions, " n " is the number of particles of radius " r " scattering light of wave length " λ " With a particular solution where "n " is constant and the intensity of the scattered light may be measured by means of a spectrograph and " r " found by substitution in the Rayleigh formula. The method is considered by Henri[15] to be very sensitive and to give exact results with hydrosols. But it is neither so convenient nor so rapid as the method elaborated by Zsigmondy[59] in his investigation of gold ruby glass and for which, in collaboration with Siedentopf[36] the first ultra-microscope was designed.

2. *Ultra-Microscopy.*—Zsigmondy observed the scattered light in a microscope set orthogonally to the illuminating beam. The particle then looks like a planet, self-luminous in a dark field. The light observed is sometimes coloured[60] and appears as diffraction rings, the size and colour of which is independent of the size of the particle, but depends upon the numerical aperture of the objective and condenser, and the intensity of the illuminating beam. The light used must be of high specific intensity, and the illumination system such that in the solid under examination, a layer of known thickness, and less than the depth of vision in the microscope, is illuminated, by a beam of known width. Suitable apparatus made by Zeiss[56] and Reichert[33] is known as the Zsigmondy " slit " ultra-microscope, and the method of manipulation has been well described by Heimstädt.[14]

The solid solution to be examined is cut so as to form at parallelopiped some 3 mm. thick, with two carefully polished faces at right angles. This is placed on a special microscope stage so' that the illuminated layer can be observed at any point in the parallelopiped. By direct counting it is then possible to determine the number of particles " N " in unit volume of the solid, containing a mass " M " of the disperse phase of density D. By substitution in the formula $L = \sqrt[3]{\dfrac{M}{DN}}$ the linear dimension of the particle, considered as a cube,is calculated (*c.f.* Wiegner[54]). Errors may be introduced in the determination of density and mass. It is usual to assume that the density is constant and independent of the size of the particle, and accordingly the ordinary density of the substance in microscopic condition is used. For metal sols in solid solution the error, if any, is negligible (Cholodny[7]), but for oxides, hydroxides, and emulsions this assumption may introduce an error making he estimated size too large (Wintgen[55], Büchner[4]). The determination of mass per

unit volume in solid solutions gives the total mass of submicrons and also amicrons* present. Since the amicrons are not included in the number of particles counted, their mass should be subtrated in order to obtain the true concentration of the submicrons. For solid solutions there is no satisfactory method of correcting for this error, but with hydrosols it is possible to separate the amicrons by ultra-filtration, and it is found that the error they introduce does not affect the order of the magnitude, and is seldom more than 50 per cent. The more recent ultra-microscopes cannot be used for the examination of solid substances.

<p style="text-align:center">II.—Continuous Phase Liquid.</p>

(a) *Microns.*

1. *Filtration.*—For the rapid grading of microns filtration through special earthenware filters is convenient, this was the method of Linder and Picton[22] (1892). Bechold[2] extended the method by preparing a series of graduated filter papers which are standardised for known solutions, and used under pressure in a special apparatus.

2. *Stokes' Law.*—The viscous resistance of spheres of radius " r " density " ρ " moving with uniform velocity " v " in a medium of density ρ^1 and viscosity η is given by Stokes' Law $F = 6\pi\eta r v$. This Law was first applied to colloidal solutions by Barus and Schneider (1891) to measure the radius of particles precipitated by electrolytes. Since the force acting is gravity " g " the equation becomes—

$$r = \sqrt{\frac{g}{2} \frac{\eta v}{\rho - \rho^1}},$$

It was shown by Perrin[30] that Brownian Movement does not interfere with the mean velocity of the particle in colloidal suspensions and solutions so that, assuming they are spheres, we have a ready means of determining accurately their size from diameters of 1 mm. to and beyond the microscopic limit.

(b) *Sub-Microns.*

Methods may be classified as (1) ultra-filtration; (2) ultra-microscopy; application of single reflecting condensers, using (3) Stokes' Law; and (4) density of distribution; (5) double reflecting condensers for direct measurement; (6) improved ultra-microscopy; application of (7) light absorption; (8) Rayleigh Formula; (9) diffusion constant.

1. *Ultra-Filtration.*—Bechold's method (referred to under Microns) may be used but the prepared papers, owing to the negative charge in the capillaries, cause adsorption and precipitation of the positive colloid.

2. *Ultra - Microscopy.* — The Zsigmondy - Siedentopf apparatus (described above) was adapted for the examination of liquids and gases by introducing an observation cell. This cell—improved by

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* The term amicron is used for particles which cannot be detected in the ultra-microscope.

Thomae[46]—is provided with quartz windows at right angles and held in a special clip so that the condenser and water immersion objective are directly opposite the windows. On account of the Brownian Movement it is more difficult with liquids than with solids to count the particles in a known illuminated volume. The solution should be so diluted, that at the same time, about four particles only are visible and ten momentary counts are made. The solution in the cell is changed ten times, counted each time, and the mean of the 100 counts is found. A control observation should be made with a different dilution. With experience the total time required for a determination is about 30 minutes, and it is recommended by King[20], that for rapid work, momentary illumination by means of a pendulum be used, and the momentary observations recorded on a typewriter. With this apparatus hydrosol submicrons of 2μ to $5\mu\mu$ can be measured, provided the refractive index* of the particle is sufficiently different from that of the water—a proviso which applies to all ultra-microscopic observations.

Shortly · after the introduction of the " slit " ultra-microscope other systems of dark ground illumination, well known to earlier English microscopists, were used for the examination of colloidal solutions. These systems differ from the Zsigmondy arrangement in that the illuminating beam is coaxial with the microscope, and is brought to a focus in the colloidal solution by means of a special substage condenser, provided with a central stop, so that the solution is illuminated from all sides by rays comprised within the apertural zone of $1 \cdot 0$ to $1 \cdot 3$ NA. It is evident that the value of the apertural zone need not be greater than the refractive index of the medium, *e.g.*, for hydrosols $1 \cdot 33$. The earlier condensers of this type are of historical interest only, as also are the Cotton and Mouton[3], Scarpa[34], and modified Abbe condensers used with centre stop objectives of high numerical aperture. Reference should be made to the useful summary by Burton[5].

The substage ultra-condensers suitable for colloidal solutions or the observation of living and unstained bacteria may be classified as (1) single reflecting, Reichert and the Zeiss Paraboloid[37]; (2) double reflecting, Leitz[23]-Jentzsch[18]; and Zeiss Cardioid[38].

1. *Single Reflecting Condensers.*—The Reichert and so-called paraboloid† although different in principle have both the same uses and limitations. They are suitable only for comparatively coarse hydrosols, and cannot be used for direct quantitative estimations, since both show spherical aberration and in the paraboloid the images produced by the different zones vary in size. Consequently the intensity of illumination decreases from the centre of the field. Owing

* The intensity of scattered light

$$I_s \propto \left[\frac{\mu_1^2}{\mu^2} - 1 \right]^2$$

where μ is the refractive index of medium, and μ_1 of the submicron.

† Siedentopf modified the shape of the true Paraboloid of Wenham (1584) and so made the technical reproduction of the condenser cheaper and less tedious.

to the fact that both of these condensers do not focus at a sharp point, but rather build up a patch or fleck of light, no difficulty is experienced in the centering of the condenser, and although the thickness of the microscope slide or observation chamber may be varied slightly (0·3 mm.) this does not seriously interfere with the observation. The illumination used is either an inverted incandescent gas mantle or a Nernst lamp. For the rapid examination of a series of solutions the paraboloid is not so convenient as the Zsigmondy arrangement since the microscope setting must be distrubed after each reading and the slide and cover glass most carefully cleaned. For use with the Reichert instrument a small observation cell has been constructed through which a series of solutions may be passed.

3. *Stokes' Law.*—Although useless for direct quantitative work, Reichert and Paraboloid ultra-microscope when arranged horizontally, are well suited for the purpose of determining the radius of sub-microns from their speed of settlement by the application of Stokes' Law (*see above under* Microns). The method cannot conveniently be used for submicrons of less than $20\mu\mu$ diameter since calculation shows that gold particles of this size take seven hours to fall $0·1$ mm. By super-imposing on gravity an electrical field, the speed of settlement is increased and Burton[6] by this means has applied the Law to the " weighing of the particles " (*c.f. also* Westgren[53]).

4. *Density of Distribution.*—These instruments may also be used to count the number of particles n and n° of radius r density ρ in equilibrium at heights o and h. The radius is then calculated from Perrins[31] equation (*c.f. also* Oden[28])—

$$\log \frac{n°}{n} = (\rho - \rho_1) \frac{h \ r^3 \ 10^{17}}{1·9.}$$

2. *Double Reflecting Condensers.*—The Siedentopf Cardioid[38] and Jentzsch[18] are practically free from spherical aberration and are almost aplanatic. By means of a micrometer eye-piece the area of an illuminated layer of fixed depth—slightly moe than the depth of vision—is determined and the volume calculated. This volume cannot be varied as in the slit or immersion ultra-microscope (*see later*), and the determintions are not so reliable. On the other hand owing to the increased intensity of the illumination it is possible to measure particles which are too small for counting in the slit ultra-microscope, but which can be counted in the new Zsigmondy[61] immersion apparatus. The Jentzsch or Cardioid are particularly suitable for the examination of thread-like bacteria of microscopic length but ultra-microscopic breadth. Such bodies in the slit or immersion ultra-microscope appear as points of light. Siedentopf[39] showed that this was due to illumination from one side only, and that the appearance of such bodies depends on the azimuth of the incident light. Owing to the fact that the illumination in all reflecting condensers is concentric, the complete structure of such bodies is revealed. Since the rays are brought to a sharp focus it is essential that double reflecting condensers should be accurately centred and that the special slide, cell, and cover be " ultra-microscopically clean."

The need for the careful adjustment of these parts after each observation renders the cardioid instrument troublesome for the rapid quantitative examination of a series of sols. This has been recognised by Siedentopf[10] who describes an accessory objective for preliminary qualitative investigations. Jentzsch[23] has designed an ultra-condenser specially suitable for the purpose, the cell of which is provided with inlet and outlet for hydrosols, and if desired with electrical and other fittings. Mention should also be made of the Leitz-Ignatowski[23] Universal condenser, which may be used for both ordinary and dark ground illumination, but the dark field is not so perfect as in the Jentzsch condenser.

The determination of the size of metal hydrosols less than $5\mu\mu$ diameter is best made by the "nuclei" method. It was found that very small particles, $5\mu\mu$ or less (amicrons), which cannot be counted in the slit ultra-microscope, but which give to the field a uniform illumination, can be seen by means of the cardioid condenser. For the determination of their size an indirect method due to Zsigmondy[62] is recommended. It is known that a gold sol prepared under standard conditions by reduction with phosphorus, gives a colloidal solution which contains amicrons only—so small as to be beyond the range of the "slit" ultra-microscope. If some of this solution (containing 5 mgs. of gold per 100 c.c.) be added to a colloidal gold solution which is being prepared by other methods, e.g., formaldehyde or hydrazin) the small amicrons function as nuclei around which the gold present in the second solution builds up, forming a larger particle. Doerinckel and Menz[26] observed that the size of the particles in the resulting solutions depend upon the number of "nuclei" added. Thus, knowing the amount of gold in the nuclear solution it is possible to calculate the size of the amicron nuclei. Good agreement was found within the range of the slit ultra-microscope and the method has been frequently used by Svedberg and Zsigmondy to determine the size of amicrons. Recently Zsigmondy has succeeded in increasing the intensity of the orthogonal illumination in a system similar to the "slit" ultra-microscope so that particles between $5\mu\mu$ and $3\mu\mu$ may now be counted as submicrons. The diameters of such particles as calculated by the "nuclei" method, from observations in the slit ultra-microscope, and as directly observed in the new immersion ultra-microscope agree.

6. *Improved Ultra-Microscopy.*—Other conditions being equal the brightness of the particles seen in the ultra-microscope depends upon the product of the square of the numerical aperture of the condenser and objective. In the slit ultra-microscope it was found necessary[36] to use a condenser of small aperture (·30) and objective of ·75 NA. By using a condenser and objective of high numerical aperture, 1·05 N.A. set orthogonally, Zsigmondy[61] has very considerably increased the intensity of the illumination and the corresponding intensity of the scattered light, so that smaller particles can be detected and measured. Since the focal distance of such a lens system is only 6 mm., it was found necessary to cut away a portion of the front lens and mounting, in order to be able to bring both

objective and condenser near enough to enable the point of focus of the condenser to be observed through the objective set at right angles. The hydrosol to be examined is introduced into the small space between the front lens of condenser and objective, in fact a drop will remain suspended in this position, so that the path of the beam through the solution can be seen. For convenience a small ebonite cup is used to hold the drop in position and this cup is so arranged that fresh hydrosol or water can be rapidly brought into the field by opening a spring clip. The instrument is thus an immersion ultra-microscope with a dark field superior to any other. The illumination is by sunlight or arc lamp and the dimensions of the beam are accurately defined by a special micrometer slit and eye-piece so that particles of gold of $3\mu\mu$ are countable and smaller particles are visible. This apparatus is by far the best for the examination of hydrosols* and no difficulty is experienced when using viscous or jelly-like hydrosols. It has the disadvantage that, unlike the Cardioid or Jentzsch thread-like structures and bacteria are observed as points of light. Observations with this instrument have been reported by Zsigmondy[59]; Zsigmondy and Bachmann[61], and King[20].

7. *Application of Light Absorption.*—The investigations of Garnett[13] and Mie[27] should be referred to but so far insufficient is known of the fundamental relationship to enable the size of the particle to be determined by this means (*c.f.* however Voigt[49], Svedberg, Kruyt[21].

8. *Rayleigh's Formula.*—*See under* Solid Continuous Phase, (b) 1.

9. *Diffusion Constant.*—The Sutherland[42] Einstein[11] equation will be discussed later under gas as continuous phase. The equation has been applied to measurements obtained in highly disperse liquid systems *c.f.* Dabrowski[10], Svedberg, but the method is too tedious to be of practical value.

III.—*Continuous Phase-Gas (Smokes).*

(a) *Microns.*

On account of the rapid Brownian Movement direct microscopic observation is impossible, unless the particles are collected on a microscope slide. Stokes' Law may be applied to gases up to the microscopic limit.

(b) *Sub-Microns.*

The methods are classified as (1) Rayleigh's Law (*see above*, (b) 1); (2) Stokes' Law; (3) Diffusion constant; (4) Ultra-microscopy; (5) Oscillation in alternating field.

2. *Stokes' Law.*—The formula may be used for particles greater than 10^{-4} cms. diameter, granules below this size are comparable with the mean free path of the molecules of the gas and Cunningham's[9] correction factor must be applied. For particles 10^{-4} cms. the rate

* In 1914, when the author was associated with Zsigmondy in the development of this instrument, only aqueous solutions could be used, since the cement used for fastening the front lens was not chemically resistant. The instrument is now made by R. Winkel, Göttingen.

of fall in air, due to gravity is about 11 cms. per hour, but convection currents interfere with the observation in highly disperse smokes.

3. *Diffusion Constant.*—The value of the diffusion constant D in Sutherland's[42] formula $D = \dfrac{RT}{N} \dfrac{1}{3\pi\eta d}$ (where $R = 8\cdot316 \times 10^7$ $N = 606 \times 10^{23}$) may be substituted in the Einstein equation $x = \sqrt{2Dt}$ for the displacement x due to Brownian Movement. This gives a simplified equation $d = 4\cdot7 \times 10^{-11} t/x^2$ for calculating the diameter d of a particle in air as continuous phase at room temperature (20° C.) when the mean displacement x in time t is measured. This displacement was recorded photographically by De Broglie[3], but such records are reliable only if a photographic plate moving at a known rate be used. Owing to the impossibility of excluding convection effects this method is not recommended for measuring the size of granules in highly disperse gaseous systems.

4. *Ultra-Microscopy.*—Smokes are conveniently observed in the Jentzsch or " slit " ultra-microscope, but not in any of the others. Precaution should be taken to prevent disturbance due to convection and heating by using a heat filter (ferrous-ammonium sulphate solution 1 : 5) in the path of the beam.

5. *Oscillation in an Alternating Electric Field.*—If the particles are charged, they will move in an electric field, according to Stokes' Law, with a uniform mean velocity.

$$V = \frac{Xe}{3 \, \pi \, \eta \, d}$$

where X is the field in volts per centimetre and e the electronic charge (*c.f.* Hevesy[16]). By measuring the velocity and the field the diameter of any individual particle is determined. This method has recently been applied by Wells and Gerke[52] to the examination of smokes. By means of a rotating commutator the particles were caused to oscillate, and their motion was observed, either photographically or by means of a micrometer eye-piece, in a modified Zsigmondy ultra-microscope constructed for this purpose. Since the motion due to convection was perpendicular to the oscillation a zig-zag line was obtained. It was thus possible to measure the oscillation amplitude which, multiplied by the frequency of the field reversal gave the velocity. By varying the field and determining the corresponding velocities good agreement was obtained for the diameter of a tobacco smoke particle ($2\cdot73 \times 10^{-5}$ cms.). The method can be used for the measurement of the size of individual particles in a non-uniform disperse phase. This represents a distinct step forward. von Weimarn proposes to construct an ultra-microscope with quartz or fluorite lens and to use ultra-violet light, which by reason of its short wave length will increase the intensity of scattered light. With such an instrument, using the photographic plate method of Wells and Gerke[52], it should be possible to make still further progress into the realms bordering on the molecular.

Bibliography.

[1] Barus and Schneider, *Zeit. f. Phys. Chem.*, **8**, 278 (1891).

[2] Bechold, *Zeit. f. Phys. Chem.*, **60**, 257 (1907).

[3] de Broglie, *Le Radium*, **6**, 203 (1909).

[4] Büchner, *Proc. Akad Wetenschap*, **18**, 170; *J. C. S.*, 108 II., **749** (1915).

[5] Burton, *Phys. Properties of Coll. Solutions*, Longman, p. 45 (1916).

[6] Burton, *Proc. Roy. Soc.*, **95**, 480 (1919).

[7] Cholodny, *Koll. Zeit.*, 340 (1907).

[8] Cotton and Mouton, *Jour. Chim. Phys.*, **4**, 365 (1908) and *Compt. Rend.*, **136**, 1,657 (1903).

[9] Cunningham, *Proc. Roy. Soc.*, 83a, 357 (1910).

[10] Dabrowski, *Acad. of Cracow* (*vide* Henri) (1912).

[11] Einstein, *Ann der Phys.*, **21**, 756 (1906).

[12] Gans, *Phys. Zeit.*, **13**, 1,185 (1912).

[13] Garnett, *Phil. Trans.*, **203**, 385 (1904); **205**, 237 (1906).

[14] Heimstadt, *Handbuch der Mikroskop Technik V. Mikrokosmos*, p. 24 (1915).

[15] Henri, *Trans. Farad. Soc.*, p. 52 (1913).

[16] Hevesy, *Koll. Zeit.*, **21**, 129 (1917).

[17] Ignatowski, *Zeit. f. Wiss. Mik., Technik*, XXV., 1.

[18] Jentzsch, *Phys. Zeitsch*, XI., 993 (1910).

[19] Johnston Stoney, *Jour. Roy. Mic. Soc.*, p. 564 (1903).

[20] King, *Jour. Soc. Chem. Ind.*, **38**, Trans. 4, (1919).

[21] Kruyt, *J. Chem. Soc.*, II., 452 (1917)

[22] Linder and Picton, *Jour. Chem. Soc.*, **61**, 137 (1892).

[23] Leitz, *Wetzlar*, 44D., p. 12.

[24] Marc, *Zeit. f. Chem. Ind. Koll.*, **14**, 181.

[25] Mecklenburg, *Koll. Chem. Beiheft*, **5**, 375 and *Chem. Abs.*, 2,974 (1914).

[26] Menz, *Zeit. Phys. Chem.*, **66**, 132 (1909).

[27] Mie, *Ann der Phys.*, **25**, 337 (1908).

[28] Odén, *Koll. Zeit.*, 18, 33, *Chem. Abs.*, **10**, 2,429 (1916).

[29] Ostwald-Fischer, *Handbook of Coll. Chem.*, Churchill, p. 27 (1915).

[30] Perrin-Hammick, " Atoms," Constable, p. 99 (1916).

[31] Perrin, *Bull Soc. Fr. Phys.*, **3**, 155 (1909).

[32] Rayleigh, *Phil. Mag.*, 375 (1871) and (1889).

[33] C. Reichert, Optical Works, Vienna.

[34] Scarpa, *Ann. Chim. Applicata*, 3, 295, *Chem. Abs.*, 3, 395 (1915).

[35] Schoep, *Bull. Soc. Ch. Belg.*, **24**, 354 (1910).

[36] Siedentopf and Zsigmondy, *Drudes Ann der Phys.* (4), 10, p. 1 (1903).

[37] Siedentopf, *Zeit. Wiss. Miks.*, **24**, 13 (1907).

[38] Siedentopf, *Verhd. Deut. Phys. Ges.*, **12**, 6 (1910).

[39] Siedentopf, *Jour. Roy. Micr. Soc.*, 573 (1903).

[40] Siedentopf, *Zeit. Chem. In Koll.*, **12**, 68, *Chem. Abs.*, **7**, 2,872.

[41] Strutt, *Proc. Roy. Soc.*, 95a, 155 (1918).

[42] Sutherland, *Phil. Mag.*, **9**, 781 (1905).

[43] Svedberg, *Zeit. Chem. Ind. Koll.*, **9**, 418 (1915).

[44] Svedberg, *Zeit. Chem. Ind. Koll.*, **6**, 318 (1909).

[45] Svedberg, *Zeit. Phys. Chem.*, 76, 145.

[46] Thomae, *Jour. Pract. Chemie.*, 80, 390.

[47] Tinker, *Proc. Roy. Soc.*, 92a, 359 (1916).

[48] Tolman, *Jour. A. Chem. Soc.*, p. 297 (1919).

[49] Voigt, *Koll. Zeit.*, 15, 1914, *Chem. Abs.*, 406 (1915).

[50] von Weimarn, *J. Russ. Phy. Ch. Soc.*, **46**, 96, *Chem. Abs.*, **9**, 2,832 (1905).

[51] Weimarn, *Zeit. Chem. Ind. Koll.*, **2**, 175.

[52] Wells and Gerke, *Jour. Am. Ch. Soc.*, 41, 312 (1919).

[53] Westgren, *Zeit. Anorg. Allg. Chem.*, 93, 231, *Chem. Abs.*, 10, 551 (1915).

[54] Wiegner, *Koll. Chem. Beihefte*, 213 (1911).

[55] Wintgen, *Jour. Chem. Soc., Abs.* II., 251 (1915).

[56] Zeiss, *Jena. Mikro*, 308 and 306.

[57] Zsigmondy, *Ber.* **45**, 579 (1912).

[58] Zsigmondy, *Zur. Erkenntnis der Koll*, p. 87 (1905).

[59] Zsigmondy, *Physik Zeits.*, 14, 975 (1913.

[60] Zsigmondy-Spear, *Chemistry of Coll.*, *Wiley*, p. 117 (1917).

[61] Zsigmondy, *Kolloid Zeits.*, **14**, 281 (1914).

[62] Zsigmondy, *Zeit f. Phys. Chem.*, **56**, 65 (1906).

THE SOLUBILITY, RATE OF ABSORPTION AND OF EVOLUTION OF GASES, AS INFLUENCED BY COLLOIDS, WITH SPECIAL REFERENCE TO PHYSIOLOGY AND BREWING.

By George King, M.Sc., F.I.C.

Although the conditions governing the equilibrium in the simple gas-liquid and gas-solid systems, have received a considerable amount of attention both theoretically[28] and experimentally[24] no satisfactory explanation has yet been given of the mechanism of the process[58]. It is not surprising, therefore, to find that comparatively little systematic work has been done on the more intricate problem of gas distribution in colloidal solutions, although it is with such solutions that many of the problems of biology, brewing, botany, sanitation, and agriculture are chiefly concerned. For the purpose of this report the experimental investigation will be considered under :—(1) gas solubility in colloidal solutions; (2) rate of evolution (effervescence); (3) rate of solution (aeration).

I.—Solubility.

The well-known Ostwald* co-efficient of solubility gives the ratio of the concentration of the gas in the liquid and gaseous phase $C_l/C_g = \lambda$ (solubility) and is an alternative expression of Henry's Law. This law, as Findlay[15], [16], [17], has shown, is valid for solution of the common gases in water and aqueous solutions of salts and non-electrolytes, at 25° C., and over a pressure range of 250 mm. to 1,400 mm. of mercury. Cassuto[10], working between one and ten atmospheres, and, more recently, Sander[49], at pressures up to 160 atmospheres observed that, at high pressures, the solubility (λ) decreased slightly with increasing pressure.

At such pressures (2,500 lbs. per square inch) the gas concentration in the liquid phase is high, and the observed departure from Henry's Law is presumably closely connected with the compressibility of the concentrated gas solution (c.f. Ritzel[48]). Biologists often express solubility in terms of Bunsen's coefficient of absorption—the quantity of gas in cubic centimetres at N.T.P. which is absorbed by unit volume of the liquid at 760 mm. pressure. This coefficient has not proved so satisfactory as the solubility coefficient of Ostwald, and Sackur and Stern[50] propose to refer the gas absorbed to unit mass of the liquid. Stern[55] has recently determined the solubility and absorption co-efficients for solutions of carbon-dioxide in aliphatic alcohols, and found that Henry's Law held good to within one per cent., and that the solubility coefficient was constant over a much wider range of pressures than the modified Bunsen coefficient (c.f. also Drucker[12]).

(a) Physiological Aspect.

The difficulties which physiologists have encountered in explaining the transport of oxygen and carbon-dioxide by blood have been

* In a paper published in 1877 (Hufner Wied Ann, 1633) it is suggested that instead of employing Bunsen's coefficient, solubility should be expressed as " das Verhältnis des adsorbierenden Flüssigkeits volumens zum absorbierten gas Volumen " (Drucker).

discussed by Professor W. M. Bayliss in the Second Report of Colloidal Chemistry, 1918, p. 151. The study of colloidal chemistry has thrown new light upon the problem of the abnormal solubility of these gases in blood. Findlay and Harby[14] (1908) suggested that the effect is due, partly to absorption on the surface of the disperse phase, and partly to ordinary chemical combination. The accepted view was that absorption of oxygen by blood was due to the formation of a compound of oxygen with the hæmoglobin; and this view has recently received the support of Barcroft[4]. According to Peters[44]—on the basis of the iron content of hæmoglobin—one molecule of hæmoglobin combines with one molecule of oxygen, but the total quantity of oxygen absorbed, for one gram of iron, varies according to the nature of the blood, being 280 c.c. to 401 c.c. for pig's blood and 320 c.c. to 468 c.c. for dog's blood. This considerable variation must be due to the difference in the chemical or physical nature of the hæmoglobin and not to any great extent to variation in the plasma, for the total amount absorbed by centrifuged plasma of dog's blood is only $12 \cdot 6$ c.c., whereas the variation in total gas absorbed is 40 c.c. to 70 c.c. In order to investigate the influence of colloidal hæmoglobin on the solubility, Findlay[15–21] and co-workers carried out a series of systematic investigations on the influence of colloids and fine suspensions on the gas solubility. The specific effect due to the colloid was investigated by studying the influence of concentration and pressure on the solubility coefficient. The following are typical of the 17 colloids used :—ferric hydroxide, gelatin, starch, ox blood, hæmoglobin, peptone. Carbon-dioxide and nitrous oxide were used, and it was found that, for both gases, at pressures below atmospheric, the solubility (λ) is sometimes less, sometimes more, than in water, it always falls with increasing pressure, and passing through a minimum, rises again slightly.

The pressure at which the minimum occurs is practically constant for all concentrations of a particular sol, but varies for different solutions. It has been tentatively suggested by Findlay and Creighton[16] that this unique behaviour is due to gas solubility in the disperse phase and since the solubility then no longer follows Henry's Law, it must be assumed that gas polymerises in this phase. For the upward trend of the curve the increased solubility can be satisfactorily interpreted in terms of the absorption law. $C^4_2/C_1 = \text{constant}$ (e.g., serum albumen, charcoal, suspensions).

It is well known that the solubility of gases in salt solutions decreases with increasing salt concentration[24]—provided there is no chemical reaction. About 20 aqueous solutions of non-electrolytes have been studied, and in three only, Quinol, Resorcinol[60], and Aniline[16] is the solubility of carbon-dioxide greater than in water. These exceptions are no doubt due to chemical combination of the solute with the gas.

In considering the influence of the concentration of colloids on solubility, distinction must be drawn between suspensoids and emulsoids. In physiological and technical chemistry, suspensoids occur but seldom. Geffcken[24] and Findlay[16] have both found that the solubility (λ) of carbon-dioxide in arsenious sulphide sols, is less than in water, and the latter has observed that it is constant for

all pressures, furthermore the hydrogen[17] solubility in silver hydrosol was indistinguishable from that in water. Very different is the influence of emulsion colloids on the gas solubility, which is often considerably influenced by the concentration of the disperse phase, and especially is this so with serum albumen, egg albumen, starch, and dextrin, in which the solubility of carbon-dioxide[15], nitrous oxide[20], and hydrogen[17] is less than in water. Thus the behaviour is comparable with that of the non-electrolytes and salt solutions, and in fact provided adsorption and chemical combination do not interfere, *collodidal solutions in general diminish the gas solubility.* Thus gelatine, glycogen, ferric hydroxide, ox blood and serum diminish the solubility of nitrous oxide[16] at atmospheric pressure, so also hydrogen in gelatine, nitrogen in blood and serum, and carbon monoxide in serum[15] have a lower solubility than the corresponding gas in water.

For each system at and above a definite pressure there is evidence of adsorption, this adsorption effect being greatest for egg and serum albumen and least for gelatine and starch. Very different, however, is the behaviour of a gas which is known to act chemically with the disperse phase in solution. In such systems as carbon-dioxide in methyl orange[17], hæmoglobin, ox blood, and serum, and carbon-monoxide and oxygen in ox blood and serum[15] the gas solubility (λ) is very considerably increased, and within the range of the pressure investigated (to 1500 mm. of mercury) the solubility decreases with the pressure and the curves show no evidence of adsorption. Judging from solubility pressure measurements gelatine[19], ferric hydroxide[16], peptone[17], and propeptone[17], react chemically with carbon-dioxide and evidence from other sources confirms this view (*c.f.* Stocks[56], Luther[35]). These observations, therefore, support the chemical theory of oxygen and carbonmonoxide absorption by blood and clearly indicate that the absorption of carbon-dioxide must also be considered as in part due to chemical combination. This view of the carbon-dioxide solubility is supported by Harnack[26], Tissot[59], and more recently by Buckmaster[6] (*c.f. also* Boycott[7]). Mention should be made here, however, of the paper by Christiansen[11] and co-workers on the reciprocal solubility of these gases in blood in which the authors support the adsorption theory.

(b) *Botanical Aspect.*

The question of the assimilation of carbon-dioxide by plants has been shown by Willstätter[63] to depend upon the nature of the colloidal chlorophyll, and some experiments have shown that, whereas chlorophyll in organic solvents behaves as an electrolyte (*c.f.* Kremann[29]) and does not increase the gas solubility, the same chlorophyll in aqueous solution produces an hydrosol which absorbs much more gas " than other colloidal solutions."

(c) *General Application.*

(1) Wolman and Emslow[64] :—Chlorination of turbid river water.
(2) D. Berthelot[5] and R. Trannoy :—Absorbent power of dry and

moist earth with respect to chlorine gas.   (3) Swanson[57] and Hulett :—
Estimation of gases in effluents by partition between vacuum and
liquid—assumption that Henry's Law holds true for such solutions.

(d) *Application to Brewing.*

In the brewing industry it has long been recognised that a know-
ledge of the carbon-dioxide equilibrium is of the first importance in
connection with the sprakling quality, persistency of head and
palatability of beer.   After Langer[31] and Schultze's experiments it
was for a long time thought that the solubility of carbon-dioxide in
beer was greater than the solubility in a corresponding water-alcohol
solution.   Prior[46], modifying the method of investigation, considered
that increased absorption was due to presence of phosphates.   The
mean phosphate content of beer, however, is about ·07 per cent.;
insufficient to explain any considerable increase in carbon-dioxide
solubility.   The assumption that an ester of carbon-dioxide and
alcohol is formed, has been shown by Mohr[39] to be without foundation
as also was the suggested combination with proteids.   Mohr concluded
that the gas was merely held in suspension to an extent determined
by the viscosity.   On the basis of Langer's experimental data,
Emslander[13] and Freundlich, considered that the increased solubility
was due to *adsorption* of the gas by the colloids—chiefly dextrin and
albuminoids.   These colloids have each been found to lower the
solubility of carbon-dioxide in water; and alcohol also lowers the
solubility (Müller[40]).   It is not surprising therefore that Findlay and
Shen[18] found for wort and different grades of beer that, contrary to
the usually accepted view, the carbon-dioxide solubility was con-
siderably less than for water, and independent of pressure.   Increase
in alcohol content, with corresponding increase in total colloids,*
was shown, also, to decrease the solubility, and that even allowing for
the alcohol, the solubility was much less than in water.   It is evident
that the method used by the earlier investigators gave the total
amount of carbon-dioxide in a beer which was supersaturated to an
unknown degree.   Distinction must therefore be drawn between true
solubility, and degree of supersaturation, and to understand the
problem better the second aspect of gas equilibrium must be
considered, namely :—

II.—*Rate of Evolution of Gas.*

It is well known, although not always sufficiently well recognised,
that gas supersaturation readily occurs in aqueous salt solutions
(Lamplough[32]), and in ordinary brewing practice during fermentation
in cask, the beer becomes supersaturated.   It was shown by Findlay
and King[22] that certain colloidal solutions supersaturated with carbon-
dioxide remain quiescent, sometimes over a period of 20 minutes.
In this condition of metastability the solution is exceedingly sensitive,
a very slight mechancial shock being sufficient to cause gas evolution
(*c.f.* Young[65]).   Such quiescence only occurs if scrupulous care is
taken in the cleaning of the apparatus and filtration of the solutions

* According to Marc colloids are proportional to total solids present in
beers.

used (Veley[62]). The period of quiescence is followed by periodic evolution of gas, and the duration of the period is, perhaps, a measure of the rate of growth and number of gaseous nuclei formed in the liquid. No quiescence was observed with solutions of peptone and ferric hydroxide, but after an initial rapid effervescence the process went on as in the case of the other solutions. An important contribution to our knowledge of the law governing the rate of escape of gases from solution was made by Carlson[9] (1911), who passed an indifferent gas over a well-stirred solution of oxygen in *water*. This method of investigation (Bohr[8]) enables the results obtained, as Meyer[83] pointed out, to be interpreted in the light of the Nernst[42] diffusion theory. Perman[45] and later Steele[54] showed that the rate of removal of carbon-dioxide by a stream of air, was proportional to the concentration of the gas in solution. Both the apparatus employed and the method of interpreting results are well suited for the investigation of colloidal solutions. In the only investigation of this nature which has been reported, Findlay and King[22] used mechanical shaking and supersaturated solutions of carbon dioxide (saturated at two atmospheres and reduced to one). Experimental conditions so obtained are more in accordance with those encountered in brewing, and it was shown that the gas effervescence is considerably influenced by both the quantity and nature of the disperse phase.

Using water as a standard of reference the degree of supersaturation was proportional to the rate of evolution* and inversely proportional to the coefficient k in the equation—

Velocity = k × (degree of supersaturation).

By plotting the values of velocity coefficient against degree of supersaturation a means of representing distinctly the characteristic behaviour of colloids is obtained. Hugo Muller[41] observed that freshly carbonated water loses its gas more readily than a solution which has stood. This was not confirmed, for all observations on water k is almost constant and directly proportional to degree of supersaturation.

There is a much more rapid evolution of gas initially from solutions of gelatine, peptone, ferric hydroxide, and agar, than from water solutions of potassium chloride, dextrin, starch and platinum sol (dilute), in fact, a ·05 per cent. gelatine sol effervesces as rapidly as a 3 per cent. dextrin or starch solution and a little more than half as rapidly as a ·7 per cent. peptone sol. Towards the end of the effervescence, *i.e.*, as the degree of supersaturation diminishes, in the gelatine and agar solutions, the velocity coefficient increases —the more so the greater the concentration. But there is a marked falling off in the value of the coefficient for solutions of dextrin, starch, peptone, ferric hydroxide and suspensions of charcoal. Such decrease is attributed to the slow outward diffusion of the gas dissolved in the disperse phase. With carbon the effect was observed *only* when the suspension was kept in contact with the gas for a

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* That the logarithmic law holds true for rate of evolution from supersaturated solutions is confirmed by calculations from the Pressure recovery curves of Patten and Mains[43] for carbonated water.

prolonged period. This slow outward diffusion from solid solution is the counterpart of the phenomenon observed when the initial taking up of the gas is followed by a period of very slow absorption. The same behaviour was observed by Lefebure[33] (1914) for the rate at which gases are taken up by celluloid, and a similar explanation is given. The comparatively slow saturation of beer containing high proportion of solids is, without doubt, to be interpreted as evidence of solution of the gas in the disperse phase, the rate of which is directly proportional to the rate of diffusion.

Practically nothing is known of the mechanism of this solution in the solid phase, and it is instructive to compare the phenomena observed by Findlay and King[22] with the results obtained by Anderson[1] who investigated the rate of elimination of water vapour from silicic acid gels[65].

Anderson showed that the rate depends upon the capillary structure of the gel and towards the end of the dehydration the rate is influenced by the same phenomena which is responsible for a section of the Van Bemmelen[61] curve. Anderson's results recalculated in terms of the velocity coefficient used by Findlay and King yield a curve of the same form as that obtained by the latter for starch, dextrin, and suspensions of charcoal. The influence in the Van Bemmelen and Anderson curve is considered to indicate solid solution of the gas in the walls of the capillaries (Zsigmondy[65]). The slow evolution of gas from solutions of starch, peptone, and dextrin is obviously of considerable importance in connection with the palatability and sparkling quality of beverages, and if, as it appears, this depends upon the slow diffusion outward from the disperse phase, every facility must be afforded, during carbonation, for the gas to dissolve in that phase. In fact the beer should, as Langer[30] has shown by practical tests, contain a high proportion of residual extracts, and be carbonated at low temperature. The assumption by Siegfried[52] of the formation of a carbamic acid which at the higher temperature of the palate gives free carbon dioxide would seem to be unnecessary. The formation of head is governed by the rate of effervescence, the increase being greater, the greater the concentration of colloids, within limits obviously determined by viscosity. More important, however, is the absence of substances tending to reduce the surface tension (Bau[3]), and according to Ihnen[27] under similar conditions those beers rich in dextrins retain the foam best.

When the complicated phenomena of gas solubility in colloidal solutions is better understood, it will no doubt be found that much of the conflicting evidence as to the effect of colloids will be explained, simply, as due to alteration in the physical condition of the particular colloid. It has long been known that gelatine solutions if repeatedly heated above 60° C. lose their power of jellying when cooled. The experiments of Menz[37], Ganett[25], von Schroeder[51], and more recently, Smith[53], have shown that both an irreversible change (decomposition?) and a reversible change gel sol takes place. At temperatures above 35° C. sol is stable, the gel form being stable below 15° C. When a solution is cooled between these two temperatures the change sol so gel does not take place at once but the equilibrium depends on

temperature and time during which the solute is heated, and on the rate of cooling of the solution. Such variations must be taken into account and carefully controlled, since it has been shown that the physical condition of both gelatine and starch has a very considerable effect on the rate of escape of gas from solution, in fact the determination of gas evolution provides a means of closely following the changes indicated above, and of investigating the nature and history of a particular gelatine. (For details, *see* Findlay and King[23], Part II.)

III.—*Rate of Solution.*

Except for incidental observations by Findlay and co-workers and by Geffcken, no investigation on this question appears to have been reported. A bibliography of work published on the rate of solution *in water* will be found under Papers 2, 9, 34, 38, 43, 47, 64.

BIBLIOGRAPHY.

[1] J. S. Anderson, *Zeit. f. Physikal Chem.*, 58/-, 191–228 (1914).
[2] Adeney & Becker, *Sci. Proc. Roy. Dubl. Soc.*, 15/-, 385 (1918).
[3] Bau, *Woch. f. Brau.*, 382 (1904).
[4] J. Barcroft (The respiratory function of blood) Cambridge, 320 pp. (1914).
[5] Berthelot & Trannay, *Compt. Rend.*, **168**, 121 (1919).
[6] Buckmaster *J. Physiol.*, **51**, 164 (1917).
[7] Boycott & Chisholm, *Bio. Chem. Jour.*, 523 (1910).
[8] Bohr, *Wied. Ann.*, **68**, 500 (1899).
[9] Carlson, *Jour. de Chem. Physique*, 9/-, 235 (1911).
[10] Cassuto, *Nuovo Chem.*, **6**, 5–20 (1903); J. C. S., abs. i., 161 (1904).
[11] Christiansen, Douglass & Haldane, *J. Physiol.* **48**, 244 (1914).
[12] Drucker, *J. C. S.*, abs. ii., 25 (1911).
[13] Emslander & Freundlich, *Zeit Physikal Chem.*, **49**, 317 (1904).
[14] Findlay & Harby, *Zeit. Chem. Ind. Koll.*, **3**, 169 (1908).
[15] Findlay & Creighton, *Bio. Chem. Jour.*, **5**, 304 (1910).
[16] Findlay, *Ibid, J. C. S. Trano*, **97**, 536, 555, 560 (1910).
[17] Findlay & Shen, *ibid*, **101**, 1459 (1912).
[18] Findlay & Shen, *ibid* **101**, 1313 (1912).
[19] Findlay & Williams, *ibid*, 103, 636 (1913).
[20] Findlay & Howell, *ibid*, 105, 291 (1914).
[21] Findlay, *Ibid, ibid*, 107, 282 (1915).
[22] Findlay & King, *ibid*, 103, 1170, 1175 (1913).
[23] Findlay, *Ibid, ibid*, 105, 1297 (1914).
[24] Geffcken *Zeit. Physikal Chem.*, **49**, 257 (1904).
[25] Garett, *Phil. Mag.*, **6**, 374 (1903).
[26] Harnack, *Zeit. Physikal Chem.*, **26**, 571 (1899).
[27] Ihnen, *J. Soc. Ch. Ind.*, 504, (1905).
[28] Jäger, *Chem. Abs.*, **10**, 551 (1916).
[29] Kremann, *J. C. S.* (i), 544 (1919).
[30] Langer, *Zeit. Brauwesen*, **27**, 307 (1904); *J.S.C.J.* 555 (1904).
[31] Langer & Schultze, *Zeit. Brams.*, **2**, 369 (1879); **6**, 329 (1883).
[32] Lamplough, *Proc. Camb. Phil. Soc.*, **14**, 591 (1908).
[33] Lefebure, *J. C. S., Trans.* 332 (1914).
[34] Luther & MacDougall, *J. C. S.*, ii., 361 (1908).
[35] Luther & Kasnjavi, *Zeit. Physikel Chem.* **46**, 170 (1905).
[36] Marc, *Zeit. F. Chem. Ind. Koll.*, **14**, 181.
[37] Menz., *Zeit. Physikel Chem.*, **66**, 129 (1909).
[38] J. Meyer, *Zeit f. Elektroch.*, 241 (1909)
[39] Mohr, *Woch f. Brau.*, 363 (1904).
[40] Muller, *Wied Ann*, **37**, 24 (1889).
[41] H. Müller, *J. C. S.*, 23, 37 (1870).

[42] Nernst, *Zeit. Physikal Chem.*, 52 (1904).
[43] Patten & Mains, *J. Ind. Eng. Chem.*, 10, 279 (1918)
[44] Peters, *Jour. Physiol.* **44**, 131 (1912).
[45] Perman, *J. C. S.*, 67, 868, (1895); 73, 511 (1898).
[46] Prior, *J. Soc. C. Ind.*, 288 (1899).
[47] Roth, *Zeit. Elektroch.* (15), 328 (1909).
[48] Ritzell, *Zeit Physikal Chem.*, **60**, 319 (1907).
[49] Sander, *Zeit. Physikal Chem* (1912).
[50] Sackur & Stern, *Zeit. Elektroch.*, **18**, 641 (1912).
[51] von Schroeder, *Zeit. Physikal Chem.*, **45**, 75 (1903).
[52] Siegfried, *Zeit. Physiol. Chem.*, **44**, 451 (1905).
[53] Smith, *J. Am. Ch. Soc.*, 41, 145 (1919).
[54] Steele, *J. C. S.*, **83**, 1470 (1903).
[55] Stern, *Zeit. Physikal Chem.*, **71**, 468 (1912).
[56] Stocks, First Report on *Coll. Chemy. Brit. Assn.*, 73 (1917).
[57] Swanson & Hulett, *J. Am. Chem. Soc.*, **37**, 2490 (1915).
[58] Trautz, *Zeit. Anor. Chem.*, **106**, 149 (1919); *J. C. S.*, 137 (1919).
[59] Tissot, *Compt. Rend.*, **158**, 1923 (1914).
[60] Usher, *J. C. S. Trans*, **97**, 73 (1916).
[61] Van Bemmelen, *Zeit. f. Anor. Chem.*, **13**, 233 (1897).
[62] Veley, *Phil. Trans.*, 275 (1888); *Phil. Mag.*, 209 (1903).
[63] Wellstättër & Stoll, *Ber.*, **50**, 1791 (1917).
[64] Wolman & Emslow, *Ind. Eng. Chem.*, 209 (1919).
[65] Young, *J. Am. Chem. Soc.*, **33**, 48–1375 (1911).
[66] Zseigmondy & Spear, Chemistry of Colloids, p. 147 (1917).

THE ELECTRICAL CHARGE ON COLLOIDS.

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The origin of the electrical charge on colloids is still a matter of uncertainty, although it is possible that the charges may not always arise from the same cause. It has been common practice for writers to shelve the question by assuming that a sufficiently satisfactory answer is given by Coehn's empirical law that " a substance of higher dielectric constant charges itself positively when it comes in contact with a substance of smaller dielectric constant." Even if the statement of this law were true, it would not constitute an explanation since it tells nothing regarding the manner of bringing about the charging.

Taylor has suggested the possibility of the charge arising from the colloid surface being more impermeable to certain ions than to others. He found that a membrane of aluminum hydroxide is formed by the interaction of aluminum salts and ammonia which is permeable to hydrion, but impermeable to hydroxidion, even a large E.M.F. failing to drive hydroxide ions across such a film. Thus, when alumina is suspended in water, the hydrion dissolves in or diffuses into it, leaving the hydroxidion at the surface, and the particles become positively charged. This will also explain the alteration of the charge on albumen by acids and alkalis, if it may be assumed that hydrogen and hydroxide ions are equally soluble (or diffusible) in albumen. In such a case the concentration of either ion in the albumen would vary directly as its concentration in the solution This explains why albumen possesses no charge in neutral solution

and why the positive charge increases with increasing acidity and the negative charge with increasing alkalinity. However, Taylor admits that these explanations are not necessarily correct, because they fit the facts and even suggests that adsorption, rather than differential diffusion, is responsible for the charge.

The great majority of opinion regarding the origin of the charge seems to be included in the following possibilities; that the charge results from the selective adsorption of ions at the surface of the colloid; that it is due to the ionization of foreign substances incorporated in the surface of the particles; or to the ionization of the colloid itself.

In his recent book, Burton gives an interesting discussion of the subject. In hydrosols of the oxidizable metals, the particles are positively charged, while in hydrosols of the non-oxidizable metals, the particles are negatively charged. Hardy explained this on the assumption that the charge is due to a reaction between the metal and water at the moment of formation of the hydrosol. In the case of the oxidizable metals, ionizable hydroxides are formed; in the case of the non-oxidizable metals, ionizable hydrides. The particles, however, are so comparatively large that the greater portion of the metal compound has the properties of matter in mass and ionization takes place only at the surface of the particles. Burton has confirmed this view by experiments with methyl and ethyl alcohols, which have easily replaceable OH groups. He was unsuccessful in repeated attempts to prepare alcosols of platinum, silver, and gold, but succeeded in preparing alcosols of the oxidizable metals. On the other hand, when platinum, gold, and silver wires were sparked under ethyl malonate, which has a replaceable hydrogen, very stable sols were obtained, in which the particles were negatively charged; but sols of the oxidizable metals could not be obtained with ethyl malonate. Burton's own view is that the charges on the particles are due, in the case of the Bredig metal sols, to the ionization of a layer of hydroxide or hydride on the surface of the particles. Duclaux believes that the charge always arises from the dissociation of a portion of extraneous substance retained by the particles from the reacting media and he has shown that there is always a trace of $FeCl_3$ in $Fe(OH)^3$ sols.

Zsigmondy admits that in special cases the charge is due to the ionization of the colloid itself, for example, where an ionizable substance has molecules so large as to give it the properties of a colloid. He objects to the idea of the formation of chemical compounds in all cases on the ground that it would entail " the inclusion in the category of chemical compounds of a large number of badly defined bodies, and load chemistry with much useless ballast." He prefers explaining such reactions on the basis of adsorption of ions. The acceptance of Langmuir's explanation of adsorption as a chemical phenomenon would reduce Zsigmondy's objection to one of terminology.

Among the more recent papers dealing with colloidal metallic hydroxides may be mentioned that of Pauli and Matula, who confirm Duclaux's belief concerning colloidal $Fe(OH)_3$. Kimura regards the charge as due to simple ionization of the hydroxide, the extent of which is determined by a balancing of the forces causing ionization

and the attractive forces acting between the positively charged particles and the hydroxide ions. But Powis produced negatively charged colloidal ferric hydroxide by adding a sol of the common type to a dilute solution of sodium hydroxide. He considered the change in sign of the charge to be due to the adsorption of hydroxidion.

No reliable method has yet been devised for determining the absolute value of the electrical charge on a colloidal particle. From Burton's data, Lewis made an extremely rough calculation of 8×10^{-5} electrostatic units for a platinum particle. Powis calculated a value of about 2×10^{-7} for a coarse silver particle. Upon the assumption of the existence of the Helmholtz double-layer at the surface of colloidal particles, the difference of potential between the disperse phase and the medium has been calculated. The voltages found lie almost entirely between $-0\cdot07$ and $+0\cdot07$. The results of Ellis and of Powis for oil emulsions indicate that for a stable emulsion the absolute value of this voltage must be greater than $0\cdot03$. Between the values $-0\cdot03$ and $+0\cdot03$, complete coagulation occurs, but at a velocity apparently independent of the voltage.

Wilson has shown that the electrical charge on the surface of colloidal particle must cause an unequal distribution of ions between the surface layer of solution surrounding the particles and the bulk of solution, which would, in turn, result in a difference of potential between the two phases. A very important conclusion of this work is that the addition of an electrolyte to a sol, provided no chemical changes follow, must result in a lowering of the absolute value for the potential difference between the two phases, even though there may be no change in the magnitude of the electrical charge on the colloid itself. This is considered to be the explanation of the precipitation of suspensoids by addition of salt.

This subject is so closely allied to that of electrical endosmose that the report compiled by Briggs (Second Report, p. 26) should be consulted.

BIBLIOGRAPHY.

EFANS, H. T., and EASTLACK, H. E. ' The Electrical Synthesis of Colloids ' J. Am. Chem. Soc.,' **37**, 2667 (1915).

BURTON, E. F. ' The Physical Properties of Colloidal Solutions ' (Longmans, Green & Co.).

ELLIS, R. ' Properties of Oil Emulsions ' : I Electric Charge; ' Z. Physik. Chem.' **78**, 321. II. Stability and Size of the Globules, *ibid.*, **80**, 597. III. Coagulation by Means of Colloidal Solutions, *ibid.*, **89**, 145 (1914).

FRENKEL, J. ' The Surface Electric Double-Layer of Solid and Liquid Bodies.' ' Phil. Mag.' **33**, 297 (1917).

GLIXELLI, S. ' The Electrical Transference of Gels.' ' Kolloid-Z.' **13**, 194.

HARDY, W. B., and HARVEY, H. W. ' Surface Electric Charge of Living Cells.' ' Proc. Royal Soc.' London, **84**, 217.

HARDY, W. B. ' Differences in Electrical Potential within the Living Cell.' ' J. Physiol.' **47**, 108.

VON HEVESY, G. ' The Charge and Dimensions of Ions and Dispersoids,' ' Kolloid-Z.' **21**, 129 (1917).

KIMURA, M. ' Nature of the Double Layer in Colloidal Particles,' ' Memoirs Coll. Science,' and ' Eng. Kyoto Imp. Univ.', **5**, 201.

KRUYT, H. R. 'Current-Potential of Electrolytic Solutions.' 'Verslag. Akad. Wetenschappen,' **23**, 252 (1914).

'Electrical Charges and the Limiting Values for Colloids,' *ibid.*, **23**, 260 (1914).

'Current Potentials and Colloid Stability.' 'Kolloid-Z.', **22**, 81 (1918).

LEWIS, W. C. McC. 'A System of Physical Chemistry.' (Longmans, Green & Co.)

PAULI, W., and MATULA, J. 'A Physicochemical Analysis of Colloidal Ferric Hydroxide.' 'Kolloid-Z.' **21**, 49 (1917).

POWIS, F. 'The Relation between the Stability of an Oil Emulsion and the Potential Difference at the Oil-Water Surface Boundary and the Coagulation of Colloidal Suspensions.' 'Z. physik. Chem.' **89**, 186 (1914).

'The Influence of Time on the Potential Difference at the Surface of Oil Particles Suspended in Water,' *Ibid.*, **89**, 179 (1914).

'Negative Colloidal Ferric Hydroxide.' 'J. Chem. Soc.' **107**, 818 (1915).

'Transference of Electricity by Colloidal Particles.' 'Trans. Faraday Soc.', (1915).

'The Coagulation of Colloidal Arsenious Sulfide by Electrolytes and its Relation to the Potential Difference at the Surface of the Particles.' 'J. Chem. Soc.', **109**, 734 (1916).

SHORTER, S. A. 'The Capillary Layer as the Seat of Chemical Reactions.' 'J. Soc. Dyers & Colourists,' **34**, 136 (1918).

TAYLOR, W. W. 'The Chemistry of Colloids' (Longmans, Green & Co.).

THOMAS, A. W., and GARARD, I. D. 'The Fallacy of Determining Electrical Charge of Colloids by Capillarity.' 'J. Am. Chem. Soc.' **40**, 101 (1918).

WILSON, J. A. 'Theory of Colloids.' 'J. Am. Chem. Soc.' **38**, 1982 (1916).

ZSIGMONDY, R. 'Theoretical and Applied Colloid Chemistry.' (John Wiley & Sons. 1917)

IMBIBITION OF GELS—PART I.

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One of the commonest methods of demonstrating what is meant by imbibition is to immerse a thin sheet of ordinary gelatin in water. In less than an hour the gelatin will be found to have become much swollen by absorbing, or *imbibing* water. After the first hour, the rate of swelling noticeably decreases, and the volume papers to approach a definite limit. The amount of water taken up can be determined by weighing the gelatin before and after swelling. The absorbed water behaves much as though it were dissolved in the gelatin, and it can be removed by washing the gelatin with absolute alcohol. As the water is removed, the volume of the gelatin diminishes, approaching its volume before swelling. During imbibition heat is evolved, which has often been referred to as heat of swelling. Probably this is responsible for the repeated statement that the application of heat will repress the swelling of gels. On the contrary rise of temperature causes greater swelling, which seems to indicate that the liberation of heat is not due to swelling, but to some other cause, which the writer believes to be chemical combination between the gelatin and a small portion of the absorbed water. Procter has shown that the degree of swelling is also dependent upon the previous history of the gelatin. He prepared three solutions containing 5 per cent., 10 per cent., and 20 per cent. respectively of gelatin and allowed them to set. He then dried the jellies and, after weighing, allowed them to soak in water for seven days. The sample with the

greatest volume at time of setting absorbed 14·6 times its weight of water, the second sample, 7·7; and the sample with smallest setting volume only 5·8 times its weight of water.

Gelatin swells to a much greater extent in dilute acid solutions than in water. The parts of solution absorbed by one part of a certain sample of gelatin, at 18°, were in pure water 8, in 0·006 N HCl 42, in 0·05 N HCl 28, in 0·3 N HCl only 17. Stronger solutions of the acid caused the gelatin to soften and finally dissolve. This action appears to be independent of the swelling phenomenon, and at higher temperatures becomes more marked, even with lesser concentrations of acid. By plotting the amount of swelling against the concentration of acid up to 0·3 N, a curve with a maximum is obtained, the explanation of which has given rise to several theories, which will be treated later.

The most extensive investigation of the hydrochloric acid gelatin equilibrium seems to have been made by Procter, who showed that the concentration of free acid is always less in the solution absorbed by the gelatin than in the remaining external solution, and that the sum of the amounts of free acid in both these solutions is less than the amount in the solution before the introduction of the gelatin. He attributes this difference to chemical combination between the gelatin and some of the acid and regards the product as a hydrolyzable, but highly ionizable chloride of gelatin. This agrees with the electrometric determinations of Manabe and Matula, who found that in certain hydrochloric acid solutions of gelatin nearly all hydrogen ions were bound by the protein and nearly all chloride ions were free. They examined acid solutions of serum albumin similarly and regarded the behaviour of both proteins as that of weak bases forming hydrolyzable salts.

Gelatin swells likewise in solutions of other acids, but not to the same extent as in HCl, nor does the point of maximum occur at exactly the same concentration of either total acid or hydrogen ion. With strong acids the maximum in the swelling curve is very pronounced, becoming less so with weaker acids. With acetic acid the maximum is hardly reached at concentrations so great as to cause solution of the gelatin. Extremely weak acids like boric produce very little swelling.

If sodium chloride, or other neutral salt, be added to acid-swollen gelatin, the latter contracts and gives up the solution it had absorbed to an extent depending upon the concentration of added salt. If the solution be saturated with salt, the gelatin shrinks to a horny mass. Fischer found that even non-electrolytes, such as sugars, produce this repression of swelling, although not nearly to the same extent as salts. Repression is produced by the acid itself when present in concentrations greater than that required to produce maximum swelling.

Experiments dealing with the swelling of gelatin in alkalis are generally not so satisfactory as those with acids, because of the more powerful solvent action of alkalis on the swollen gelatin. Nevertheless, by using only very dilute solutions at low temperatures, sufficient data have been collected to show that alkaline swelling is of the same

general nature as acid swelling. The swelling increases with increasing concentration of alkali to a maximum and then falls until such concentration is reached that the gelatin softens and dissolves. Swelling is repressed, here too, by addition of neutral salts.

While neutral salts are capable of repressing the swelling of gelatin in acids and alkalis, it must not be overlooked that they are also capable of producing swelling. Procter found that, with increasing concentrations of sodium chloride, gelatin swells to a maximum and then contracts steadily until the solution is saturated. The swelling was not so marked as in the case of acids, however, the gelatin taking up a maximum of only about 17 times its weight of water as compared to three times this amount with HCl. He also noted that the gelatin removes some of the salt from solution, suggesting combination, but that the addition of HCl again liberates this salt and causes the salt to become more concentrated in the external solution than in the solution absorbed.

Loeb has done some work on neutral salts that should be mentioned here. In each of a series of experiments he placed two grams of finely powdered gelatin into a cylindrical funnel, the powder being held in the cylinder by a circular piece of filter paper. One sample was perfused six times in succession with 25 c c. of distilled water and the amount of swelling noted, which was taken as the height in millimetres to which the gelatin rose in the cylinder; this sample was taken as the standard. Another sample was perfused twice with 25 c.c. of M/8 NaCl, and then four times with 25 c.c. of distilled water; the swelling here was several hundred per cent. greater than that of the standard. Still another sample was perfused six times in succession with 25 c.c. of M/8 NaCl; it did not swell to any greater extent than the standard. Loeb attributes these results to a chemical combination between the gelatin and salt, a highly ionizable sodium gelatinate being formed. In the third experiment, much swelling was prevented by the excess of salt present; when this was washed away, as in the second experiment, the gelatin swelled to a much greater extent than in pure water. He confirmed this view by showing that, when placed in an electric field, gelatin which has been treated with NaCl migrates to the anode. A sample first perfused with calcium chloride solution and then with distilled water showed very little more swelling than the standard. He accounts for this by saying that the calcium gelatinate formed is only very slightly ionizable.

Collagen, fibrin, and other proteins behave much like gelatin when immersed in solutions of acids, alkalis, or salts, and are probably subject to the same general laws. Other examples of imbibition are the swelling of agar-agar, gums, and cellulose in water and the swelling of rubber in organic solvents. It would take volumes to mention all of the work done on this subject.

Numerous attempts have been made to explain the molecular mechanism of imbibition, particular attention being paid to the explanation of the peculiar swelling curve. Most of these, however, have been guilty of drawing largely upon the imagination for something that would agree qualitatively with the experimental data

without regard for lack of grounds for the assumptions involved, Several of these still survive because, while they cannot be proved, they are not of a nature to be easily disproved. It will probably be sufficient to outline two of the more recent ones.

Perhaps it would be unfair to consider Fischer's theory of imbibition as attempting to give an explanation of the molecular mechanism of the phenomenon. He regards gelatin as a substance capable of existing in different degrees of association or polymerisation; in other words, the particles of gelatin may vary greatly in size, dependent upon conditions to which they are subjected. Thus rise of temperature or increase in concentration of acid or alkali, causes the particles to become smaller in size, the change being reversible. The particles are assumed to be capable of becoming most heavily hydrated, that is, of absorbing most water, when they have a medium diameter. The particles of neutral gelatin are large and capable of absorbing comparatively little water. Increasing the concentration of acid decreases the size of the particles, making them capable of absorbing more water, and the gelatin swells until the size of particle most readily hydrated is reached. As the particles become still smaller, the swelling becomes less until finally the particles become so small that the gelatin apparently goes into solution.

The theory of Tolman and Stearn assumes that, because of their amphoteric nature, protein colloids have marked tendencies to adsorb hydrion from acid solutions and hydroxidion from alkaline ones. In a solution of a strong acid, the adsorbed hydrogen ions, together with a corresponding number of anions, form a " double layer " on the walls of the pockets or pores in the interior of the gel, and this leads to swelling and imbibition of water by electrostatic repulsion. The addition of neutral salt or excess of strong acid to such a swollen colloid will furnish ions in the interior of the pockets which will tend to arrange themselves so as to neutralise the electrical fields of the adsorbed layer and thus being about a reduction of the swelling. The addition of a neutral salt to the acid solution tends to neutralise the electrical field of the adsorbed acid, making it easier for more acid to get to the surface of the pockets, thus leading to increased adsorption. Polyvalent ions are more effective than univalent ions in reducing swelling because, while taking up no more room than univalent ions, they are twice as effective in neutralising an existing electrical field.

In contrast to these stands the theory originated by Procter and developed by him in collaboration with his pupils. By very extensive investigations with gelatin and aqueous solutions of acids and salts, he succeeded in finding quantitative relationships between several of the variable factors involved. Once a foothold was gained in the form of an equation, it was found possible to make big advances merely by an application of mathematics. Procter built up his theory from experimental data; more recently J. A. and W. H. Wilson worked in the opposite direction by purely mathematical reasoning from the assumption of the existence of a certain hypothetical substance and calculated what results Procter should have found experimentally.

All of their calculated curves coincided completely with Proctor's experimental ones, and for this reason the writer considers the essential part of the theory as proved. The importance of the subject warrants our giving a review of the mathematical deductions, which include an adequate explanation of Procter's theory.

Consider the purely hypothetical substance G which is a colloid jelly completely permeable to water and all dissolved electrolytes, is elastic and under all conditions under consideration follows Hooke's law, and which combines chemically with the positive, but not the negative ion of the electrolyte MN, according to the equation—

$$[G] \times [M^+] = K[GM^+] \tag{1}$$

In other words, the compound GMN is completely ionized into GM^+ and N^-. The brackets indicate that concentration is meant and all concentrations are in moles per litre. The electrolyte MN is also considered totally ionized.

Now take one millimole of G and immerse it in an aqueous solution of MN. The solution penetrates G, which thereupon combines with some of the positive ions, removing them from solution, and consequently the solution within the jelly will have a greater concentration of N^- than of M^+, while in the external solution $[M^+]$ is necessarily equal to $[N^-]$. The solution thus becomes separated into two phases, that within and that surrounding the jelly, and the ions of one phase must finally reach equilibrium with those of the other phase.

At equilibrium, in the external solution, let—

$$x = [M^+] = [N^-]$$

and in the jelly phase let—

$$y = [M^+]$$

and—

$$z = [GM^+]$$

whence—

$$[N^-] = y + z.$$

The relation existing between the concentrations of diffusible ions of the two phases at equilibrium can be derived from the consideration of the transfer of an infinitesimally small amount, dn moles, of M^+ and N^- from the outer solution to the jelly phase, in which case, since no work is done—

$$dnRT \log x/y + dnRT \log x/(y + z) = 0,$$

whence—

$$x^2 = y(y + z). \tag{2}$$

But in this equation, the product of *equals* is equated to the product of *unequals*, from which it follows that the sum of those unequals is greater than the sum of the equals, or—

$$2y + z > 2x.$$

This is a mathematical proof that the concentration of diffusible ions of the jelly phase is greater than that of the external solution, and makes possible the derivation of a second equation involving e,

which is defined as the excess of concentration of diffusible ions of the jelly phase over that of the external solution—

$$2x + e = 2y + z. \tag{3}$$

Since $[N^-]$ is greater in the jelly than in the surrounding solution, the negative ions of the colloid compound will tend to diffuse outward into the external solution, but this they cannot do without dragging their colloid cations with them. On the other hand, the cohesive forces of the elastic jelly will resist this outward pull, the quantitative measure of which is e, and according to Hooke's law—

$$e = CV \tag{4}$$

where C is a constant and V the increase in volume in cubic centimeters of one millimole of the colloid.

Now, since we have taken unit quantity of the substance G—

$$[G] + [GM^+] = 1/(V + a)$$

or—

$$[G] = 1/(V + a) - z \tag{5}$$

where a is, not the initial volume of the colloid, but the free space within the jelly before swelling through which the ions may pass. For our hypothetical substance, we may consider the limiting case where the value of a is zero, in which case, from (1) and (5) we get—

$$(1/V - z)y = Kz \tag{6}$$

and from (2) and (3)—

$$z = e + 2\sqrt{ey}$$

or—

$$z = CV + 2\sqrt{CVy} \tag{7}$$

From (6) and (7)—

$$V(K + y)(CV + 2\sqrt{CVy}) - y = 0 \tag{8}$$

where the only variables are V and y. Now we have only to know the values for the two constants, K and C, to plot the curve for any variable in terms of any other value. For example, by giving y a definite value, we can calculate V from (8). Knowing y and V, we can calculate z from (7) and with y and z, we can calculate x from (2), while e is obtained directly from (4).

If any values for K and C be substituted, the resulting relations will be found to be of the same general nature as are obtained with any proteins in acid solutions, but values of K and C for gelatin have been determined. By means of the hydrogen electrode, Procter and Wilson found the value $K = 1 \cdot 5 \times 10^{-4}$ for gelatin and HCl, while their experimental value for C at $18°$ was 3×10^{-4}. The whole series of curves for these values of the gelatin constants has been plotted, and appears in the *Journal of the American Leather Chemists' Association* for 1918, pages 184 and 185, which should be consulted. Procter's experimental determinations are included in the same figures, and it will be seen that better agreement could not be obtained if the curves were drawn from his data.

Since, theoretically, the calculated curves should not coincide absolutely with the experimental ones for gelatin and HCl, it is interesting to note why no appreciable discrepancies were found.

That the quantity a, defined as the free space within the unswollen jelly, has a measurable value for gelatin is suggested by the fact that the volume of the swollen jelly is slightly less than the sum of the volumes of the gelatin and absorbed water before swelling. But the values for V are generally so much greater than the total volume of the gelatin before swelling that any error due to neglecting a would be insignificant. Appreciable errors might have resulted from the assumption of total ionization of the electrolyte, were it not for the fact that the rates of change of the slopes of the curves are greatest for concentrations of HCl less than $0·01$ N.

In order to apply the theory to weak acids, it is necessary to include the equation which defines the ionization constant of the acid. Procter and Wilson have derived equations which explain the action of salt in repressing the swelling of gelatin in acid solutions; for these and others dealing with polybasic acids, reference should be made to the original papers. At the moment of writing no evidence incompatible with the theory has been discovered. E. A. and H. T. Graham stated that the theory would not account for the repression of swelling by sugars, but Wilson pointed out that the difference in molal fugacity of the sugar in the two phases due to the differences in ion concentrations is sufficient to account for the repressing action of sugar.

The possibilities of gain to pure science fully justify the undertaking of the enormous amount of work still to be done on the theoretical side of imbibition and the problem appears to lie as much in the field of the mathematician as in that of the chemist.

Recent Publications on Imbibition of Gels.

(See also bibliography at end of Part II.)

ARISZ, L. 'The Sol and Gel State of Gelatin Solutions.' I. Gelatinising. II. Swelling. 'Koll. Beihefte,' **7**, 1 (1915). Size of gelatin particles and attractions between them probably decrease during swelling. Swelling and solution are considered two stages of the same process, indefinite swelling amounting to solution.

BENNETT, H. G. 'The Swelling of Gelatin.' 'J. Soc. Leather Trades Chem.', **2**, 40 (1918). Polemical.

FISCHER, MARTIN H. EDEMA and NEPHRITIS. (J. Wiley & Sons, 1915.) On Hydration and "Solution" in Gelatin. 'Science,' **42**, 223 (1915). A warning against the general adoption of the view that the "solution" of a protein represents but the extreme of that which in a lesser degree is called swelling. Hydration or swelling is to be regarded as a change through which the protein enters into physicochemical combination with water; "solution" as one which can be most easily understood as the expression of an increase in the degree of dispersion of the colloid.

FISCHER, M. H. and SYKES, A. 'The influence of Non-electrolytes on the Swelling of Protein.' 'Kolloid-Z.' **14**, 215. Non-electrolytes decrease the degree of swelling of protein the more so the greater the concentration. The conclusion is drawn that the phenomenon is one of adsorption rather than osmosis.
'The Non-acid and Non-alkaline Hydration of Proteins.' 'Kolloid-Z.' **16**, 129 (1915). In the swelling of gelatin by urea, there are two changes, one towards increasing hydration, the other towards increasing degree of dispersion.

FISCHER, M. H. and HOOKER, M.O. 'On the Swelling of Gelatin in Polybasic Acids and their Salts.' 'J. Am. Chem. Soc.' **40**, 272 (1918).

FISCHER, M. H., and BENZINGER, M. ' On the Swelling of Fibrin in Polybasic Acids and their Salts.' ' J. Am, Chem. Soc.' **40**, 292 (1918).

FISCHER, M. H., and COFFMAN, W. D. ' On the Liquefaction or " Solution " of Gelatin in Polybasic Acids and their Salts.' ' J. Am. Chem. Soc.' **40**, 303 (1918).

GRAHAM, E. A., and GRAHAM, H. T. ' Retardation by Sugars of Diffusion of Acids in Gels.' ' J. Am. Chem. Soc.' **40**, 1900 (1918).

HATSCHEK, EMIL. ' Viscosity and Hydration of Colloidal Solutions.' ' Biochem. J.' **10**, 336 (1916).
' An Analysis of the Theory of Gels as Systems of Two Liquid Phases.' ' Chem. News,' **116**, 167 (1917).

KATZ, J. R. ' Micella are not necessary for the Explanation of Uncomplicated Swelling.' ' Z. Physiol. Chem.' **96**, 255 (1916). The facts are fully covered by the theory of a solid solution of water in the swellling substance, and in Nägelli's theory *micella* can be replaced by *molecules*.
' The Laws of Swelling. The Swelling in Water without Complications.' ' Kolloidchem. Beihefte,' **9**, 1 (1917).

LENK, EMIL. ' Importance of Electrolytes for Swelling Processes " : A. The Action of the Individual Electrolytes. B. Combinations of Electrolytes.' ' Biochem. Z.' **73**, 15 and 58 (1916). The conclusion is drawn that the antagonistic actions of ions must be due to colloidal phenomena and not to osmotic pressures.

LOEB, JACQUES. ' Ionization of Proteins and Antagonistic Salt Action.' ' J. Biological Chem.' **33**, 531 (1918).
' The Stoichiometrical Character of the Action of Neutral Salts upon the Swelling of Gelatin.' *Ibid.* **34**, 77 (1918).

MANABE, K., and MATULA, J. ' Physical Changes in the States of Colloids.' XV. Electrochemical Investigations of Acid Albumin. ' Biochem. Z.' **52**, 369 (1913). The work involved the electrometric determinations of the hydrion and chloridion concentrations of hydrochloric acid solutions of serum albumen and of gelatin.

OSTWALD, WOLFGANG. ' Importance of Electrolytes for Swelling Processes.' ' Biochem. Z.' **77**, 329 (1916).

PAULI, W., and HIRSCHFELD, M. ' Alteration in the Physical Conditions of Colloids.' XVIII. Protein Salts of Different Acids. ' Biochem. Z.' **62**, 245. Equal concentrations of protein bind less of a weak than of a stronger acid.

PROCTER, H. R. ' On the Action of Dilute Acids and Salt Solutions upon Gelatin. ' Kolloidchem. Beihefte,' 1911 ; ' J. Am. Leather Chem. Assn.' **6**, 270 (1911. This paper contains a great deal of valuable experimental data.
' The Equilibrium of Dilute Hydrochloric Acid and Gelatin.' ' J. Chem. Soc.' **105**, 313 (1914).
' The Combination of Acids and Hide Substance.' London ' Collegium,' 1915. A paper dealing with the subject in a more popular style.
' The Swelling of Gelatin.' ' J. Soc. Leather Trades Chem.' **2**, 73 (1918). A reply to Bennett's paper of the same title.

PROCTER, H. R., and WILSON, J. A. ' The Acid-Gelatin Equilibrium.' ' J. Chem. Soc.' **109**, 307 (1916).
' The Swelling of Colloid Jellies.' ' J. Am. Leather Chem. Assn.' **11**, 399 (1916).

RINGER, W. E. ' Further Studies on Pekelharing's Pepsin.' ' Z. Physiol. Chem.' **95**, 195 (1915). The action of pepsin and swelling of protein are closely related.
' Further researches upon Pure Pepsin.' ' Proc. Akad. Wetenschappen,' **18**, 738 (1915). The point of maximum swelling of protein does not occur at the same hydrion concentration with different acids.
' The Importance of the Condition of the Substrate in the Action of Pepsin.' ' Kolloid-Z.' **19**, 253 (1916).

ROBERTSON, T. B. ' The Physical Chemistry of the Proteins.' (Longmans. Green & Co., 1918.)

ROSENBOHM, E. ' The Heat of Swelling of Colloids.' ' Kolloidchem. Beihefte,' **6**, 177 (1914). The swelling of gelatin appears to be divided into two phases, the first where a small amount of water is taken up and all the heat of swelling is evident, and the second 'where a large amount of water is taken up and no heat evolved.

TOLMAN, R.C., and STEARN, A.E . ' The Molecular Mechanism of Colloidal Behaviour.' I. The swelling of Fibrin in Acids. ' J. Am. Chem. Soc, **40**, 264 (1918).

WILSON, J. A. ' Retardation by Sugars of Diffusion of Acids in Gels.' ' J. Ams Chem. Soc.' **41**, 358 (1919). A reply to the statement by E. A. and H. T. Graham (*see* above) that Procter's theory cannot account for the repression of the swelling of acid-swollen gelatin by sugar.

WILSON, J. A., and WILSON, W. H. ' Colloidal Phenomena and the Adsorption Formula.' ' J. Am. Chem. Soc.' **40**, 886 (1918). A further mathematical development of Procter's theory of the swelling of colloid jellies and its relation to other branches of colloid chemistry.

WOLFF, L. K., and BÜCHNER, E. H. ' The Behaviour of Jellies towards Liquid, and their Vapours.' ' Verslag. K. Akad. Wetenschappen,' **21**, 988 (1912) and **22**, 1323 (1914). It is contended that von Schroeder's observation that the amount of water in gelatin swollen in liquid water decreases when the gelatin is placed in water vapour rests upon a defective method of experimentation.

IMBIBITION OF GELS. PART II.—INDUSTRIAL APPLICATIONS.

By JOHN ARTHUR WILSON, *Chief Chemist, A. F. Gallun & Sons Co., Milwaukee.*

Imbibition plays a most important role in the manufacture of leather, paper, textiles, and many other colloidal products, but few cases are generally known of applications of theory to manufacturing conditions. In view of the fact that Procter's theory grew from an investigation of the process of pickling hides, it is not surprising that what applications of it have so far been published have been connected with the *leather industry*, especially since many of the formulas are of recent derivation. A general survey of applications, to the leather industry has been given by Procter in the First Report pp. 5–20, and need not be repeated here. Wilson and Kern used the theory to explain causes for certain discrepancies in tannin analyses made by the hide-powder method, which is widely employed both in Europe and America. One direct outcome of the theory of imbibition is the Procter–Wilson theory of vegetable tanning, which like its parent theory is largely mathematical in character. The equations forming part of the theory enable one to regulate the astringency of the tannins, their rate of diffusion into the hide, and the degree of plumping of the hide fibres, by simple alterations of the concentrations of electrolytes in the tan liquors.

Bovard, in pointing out the importance of imbibition to the *manufacture of paper*, claims that the character of the sheet is largely determined by the degree of hydration and length of the fibres. He noted that cellulose swells more rapidly in alkaline than in neutral or acid solutions, and since the rosin size used in paper manufacture is alkaline, he reasoned that the hydration of the stock would be

promoted if the size were added to the beater before the alum. In an actual test, working in this order, the paper tested $1\frac{1}{2}$ points higher than that made by putting the alum into the beater first.

Photographic workers sometimes experience a most annoying reticulation of the surfaces of negatives, particularly when fixing or washing during hot weather. The wet gelatin layer becomes finely wrinkled or corrugated, the network of puckers forming a pattern. Sheppard and Elliott have found two causes for this difficulty. When sheets of gelatin swell or contract slowly, they undergo a change in volume but not in shape. On the other hand, the gelatine on a negative must undergo a change in shape, since one face of it is held to the plate. So long as the amount and rate of swelling are not great, no trouble is experienced, but when the swelling is excessive, due to higher temperatures or chemicals in the fixing bath, the upper surface of the gelatin will have an area so much greater than the lower surface as to cause puckering. The second cause is the presence in the solution with which the plate is treated of both swelling and contracting agents. Reticulation is readily produced by immersing the plate in a solution containing acetic and tannic acids, the former tending to swell or soften the gelatin, the latter to contract or harden it. Their rates of diffusion are determined by their effects upon the gelatin, and the result is a mosaic-like alternation of hardening and softening effects, the ridges being more swollen and the valleys contracted by tanning.

Judging from the literature available, the most extensive applications of imbibition have been in the field of *biology*. Almost without exception, this has been done by analogy, rather than by application of theory. Fischer's book, "Edema and Nephritis" is replete with analogies between inbibition of such proteins as gelatin, fibrin, and gluten, and that of things so widely different as muscles, eyes, nervous tissues, catgut, and living frogs. Loeb showed that the behaviour of dried pig's bladder in solutions of various salts very closely resembles that of powdered gelatin in the same solutions. Reference should also be made to Loeb's work on the fundulus egg, to Arnold's studies of the swelling of human muscle, and Traube's work on the swelling and germination of plant seeds.

Recent Publications Dealing with the Application, or Suggesting an Application, of the Subject of Imbibition of Gels.

(*See also* Bibliography at end of Part I.)

ARNOLD, R. 'The Swelling Capacity of Different Kinds of Muscle in Acid Solutions.' 'Kolloidchem. Beihefte,' 5, 511 (1914). The structure of human muscle is not a criterion of its relative swelling capacity.

BOVARD, W. M. 'Colloidal Chemistry in Papermaking.' 'Paper' 22, 11 (1918). Shows the importance of controlling hydration of the stock in making paper.

FISCHER, M. H. ':Relation between Chloride Retention, Edema, and "Acidosis." 'J. Am. Med. Assn.' 64, 325 (1915).
'The Classification and Treatment of the Nephritides.' 'The Journal-Lancet,' July 1, 1916.

FISCHER, M. H., and HOOKER, M. O. 'Trench Nephritis.' 'The Int. Assn. of Med. Museums,' Bulletin No. VII., May, 1918.
'Ternary Systems and the Behaviour of Protoplasm. 'Science, 48, 143 (1918). Many of the laws governing the hydration and dehydration of soaps

are identical with those governing the hydration and dehydration of certain proteins, which in turn show analogies to living cells.

HARRISON, W. 'Investigations on the Physical and Chemical 'Properties of Cotton. Report of 'The Nat. Assn. of Cotton Mfrs.', 1916.

LOEB, JACQUES. 'The Mechanism of the Diffusion of Electrolytes through the Membranes of Living Cells.' I. The Necessity of a General Salt Effect upor the Membrane as a Prerequisite for this Diffusion.' 'J. Biol. Chem.' **27**, 339 (1916). II. The Diffusion of Potassium Chloride out of the Egg of the Fundulus and the Relative Efficiency of Different Ions for the Salt Effect. *Ibid.*, 353. III. The Analogy of the Mechanism of the Diffusion for Acids and Potassium Salts. *Ibid.*, 363.
'The Similarity of the Action of Salts upon the Swelling of Animal Membranes and of Powdered Colloids. *Ibid.*, **31**, 343 (1917).

PROCTER, H. R. 'Recent Developments in Leather Chemistry. 'J. Royal Soc. Arts,' 1918.
'Colloid Chemistry of Tanning. 'First Report,' p. 5.

PROCTER, H. R., and BURTON, D. 'The Swelling of Gelatinous Tissues.' J. Soc. Chem. Ind.' **35**, 404 (1916). Treats the subject from the viewpoint of the leather chemist.

PROCTER, H. R. and WILSON, J. A. Theory of Vegetable Tanning. J. Chem. Soc.' **109**, 1327 (1916).

SHEPPARD, S. E., and ELLIOTT, F. A. 'The Reticulation of Gelatin.' J. Ind. and Eng. Chem.' **10**, 727 (1918). An application to photography.

TRAUBE, I., and KÖHLER, F. 'The Velocity of Formation and Solution and of Swelling of Gels.' 'Intern. Z. Biol.' **2**, 42 (1915). A discussion is given of the relation between experimental results and the following biological problems : narcosis, plasmolysis of cells, chemotaxis, muscular work, edema, and inflammations.

TRAUBE, I., and MARUSAWA, T. 'Swelling and Germination of Plant Seeds.' 'Intern. Z. Physik. Chem. Biol.' **2**, 370 (1915).

UPSON, F. W., and CALVIN, J. W. On the Colloidal Swelling of Wheat Gluten.' 'J. Am. Chem. Soc.' **37**, 1295 (1915).

WILSON, J. A. 'Theories of Leather Chemistry.' 'J. Am. Leather Chem. Assn.' **12**, 108 (1917).
'Theory of Tanning,' *ibid.*, **13**, 177 (1918).
'Theory and Practice of Leather Chemistry,' *ibid.*, **14**, 93 (1919).

WILSON, J. A., and KERN, E. J. 'The Non-Tannin Enigma.' 'J. Am. Leather Chem. Assn.' **13**, 429 (1918).

COLLOID PROBLEMS IN BREAD-MAKING.

By R. WHYMPER, *Chief Chemist to Messrs. Peek, Frean & Co., Ltd., Biscuit Manufacturers, London, and late Assistant Inspector of Bakeries, B.E.F., France.*

There is no manufacturer less aware of the chemical problems underlying his trade than the master baker. In spite of his ignorance, however, he is one of the most efficient members of society, in that he produces an excellent article with great regularity. This is, perhaps, less a matter of wonder when it is realised that the art of bread-making of a high order can be traced through the Chinese to about 2,000 years B.C., and is of course older than that, and that even to this day the majority of people can make a very passable loaf.

Such scientific work as has been done, in or for the bakery, has usually been undertaken for some specific material object, for advantage to the large manufacturer (yields, moisture retainers, effect of machinery) or for the protection of the consumer (sanitary conditions of manufacture, effect of alum, bleaching agents, &c.). There is comparatively

little published work available to show that the problems have been tackled for a scientific purpose, or for improvement of the process. Indeed, there has been no stimulus for the chemist, since, in the bakery the ability of a master baker to feel, taste and smell the ingredients is a more sure guide in the production of good bread than the knowledge obtained by use of test tubes and balance.

It will be observed that the two points of view, that of the producer and that of the consumer, are largely but not entirely sympathetic. The former, whether the small hand-baker or large manufacturer, demands the greatest yield from his ingredients, and the most attractive-looking product, but really is not greatly concerned about the flavour, provided he can sell his bread. The result is the exhibition loaf of perfect proportion, if somewhat insipid, or the water-laden and profitable loaf, both the outcome of scientific treatment. All the consumer asks to-day is that his bread shall be palatable, and be made in a cleanly manner and cheap, whilst he is not greatly interested in the amount of water present in his loaf. Such a loaf, to suit the consumer, can generally be assured by the hand-baker. It is therefore rather in the economic direction that any considerable amount of work has been done by the bakery chemist, who himself is only just beginning to realise the variety and complexity of the problems underlying the art. That he will, at a near date, assume supreme importance in the bakery, especially in the large machine or automatic bakery, is certain, but the time is not yet, for he does not know enough and, above all, the master baker is well aware of the fact. It is easy to analyse all the ingredients in use in the bakery, but it is less easy to determine how a combination of these ingredients will turn out as bread, since the mutual influence of one complex upon another is not known with any degree of certainty.

It is unfortunate also that so many technologists, having acquired a smattering of chemistry, pose as scientific experts, with the result that conflicting opinions, arising from the interpretation of inaccurate results, have injured the prestige of the chemist. On the other hand, the chemists have not been free from the fault of supplying, from a laboratory, advice which clearly indicates that they do not know the elements of bread-making.

Briefly put (for it must be assumed that the elementary principles of bread making are understood by the reader), bread is made from flour, yeast, water and salt, with occasionally milk, fat, malt extract, yeast salts, wheat germ, aerating chemicals, &c., according to the quality of bread required, English bread, farmhouse bread, milk bread, germ bread, malt bread, tin loaves, Viennese rolls, French rolls, &c.

With the addition of any new ingredient over the first four mentioned, fresh complications in the chemical changes during bread-making are introduced. Added to these must be considered the changes involved during fermentation and baking, and, one of the largest problems of all, during the change from freshness to staleness which the loaf undergoes with the passage of time. With the use of flour, yeast, water and salt alone, a mixing of dough, and the subsequent loaf, are of sufficient complexity, involving the saturation

and swelling of the starch granules (for future gelatinisation by heat), the production of hydrated forms of gluten, itself a complex proteid, biological changes during the growth of yeast, the occlusion of gas evolved during that growth, the action of the yeast enzymes on the carbo-hydrates, the working of the dough to secure suitable elasticity, proteolytic enzyme action on the gluten, the hardening action of salt on the gluten, gelatinisation and saturation of the starch-water system on baking, and the gradual changes, physical and chemical, during cooling and ageing which bring about eventual staleness. This is but a broad outline of the problems to be attacked when the barest necessities for the production of bread are used, and it is evident that the addition of milk or fat must increase the complication of the bread system.

I.—*Flour.*

The flour components of greatest importance are starch, gluten, mineral salts and enzymes.

(a) *Starch.*

Some space has already been devoted in Report I. (p. 46 *et seq.*) to the consideration of wheat starch and its behaviour in the presence of water at a temperature to cause gelatinisation. It is not intended to cover this ground again.

Wheat starch in the presence of a sufficiency of water will commence gelatinisation about 60° C., and every granule will be completely burst at about 65° C. Since the baking temperature of a bread oven is usually between 200° and 240° C., and the amount of water present in a normal dough is about 41 per cent., the baking lasting for one hour, it might be imagined that, all other factors removed, a great proportion of the starch granules to be found in bread-crumb would be gelatinised. Yet, as a rule, the starch granules inside a loaf show a mixture of those untouched by the damp heat and of those only slightly swollen, whilst a few only have undergone complete gelatinisa-tion. The reason for this divergence from theory may be found in the fact that a temperature seldom higher than 95° C. is reached within the loaf, and then only for a very short period, during the baking hour, though temperatures as high as 99·5° C. have been recorded. It is probable that the time factor is of some importance here. Further, the saturation of starch by water is reached when a mixture shows 41 per cent. of the latter, hence, after allowing for the absorption of water by gluten (which is not inconsiderable, being as much as 200 per cent. of the dry gluten of a strong flour and slightly less for weaker flour), there is an insufficiency of water present to allow complete gelatinisation of all the starch granules at the temperature of baking. The complexity of the colloid systems in a baked loaf is, therefore, considerable.

Were there no component other than starch to consider, such problems as the change in physical consistency from freshness to staleness of a loaf would be comparatively easy of solution. But there are so many underlying difficulties connected with dissolution by enzymic action on the one hand, and coagulation by electrolytes on the other, that the behaviour of the colloids in the loaf after

baking is less easy to interpret. It must not be forgotten, moreover, that gluten, so far as can be ascertained, hinders the deposition of starch from its jelly. At any rate, by the addition of extra gluten to flour-dough the resulting bread possesses better keeping qualities, which, from our recent researches (Whymper, R., "*The Conditions that Govern Staleness in Bread*," *Maclaren and Sons, Ltd.*, 1919) in the Army, have been shown to depend chiefly upon the rate of deposition of starch from its solutions with the passage of time. In other words, gluten acts as a protective colloid to starch in solution. Exception may be taken to such a generalisation as this, on the ground that it is the quality of the gluten that determines whether bread will keep well or quickly become stale. Snyder (*U.S. Dept. Agric., Bull.*, 101, 56, 1901) found that the addition of starch or the addition of gluten, the former up to 20 per cent. of the flour, was without material effect upon the size of the loaf, though the water-absorbing capacity of the starchy flour was reduced. We are of the opinion, however, that the conclusion reached by that worker and by Jago ("*The Technology of Bread-Making*," 1911, p. 305), that "the character rather than the quantity of the gluten content" governs the quality of bread, is too far-reaching, since they omitted to consider the keeping qualities of the loaf. Jago's figures for the viscometer readings of a similar experiment are not without interest (Table I.), for, though of a rough order, they give some indication of a previously observed fact, that, in the case of flours of high water-absorbing capacity, this power is retained but little diminished on being reduced to a uniform wet gluten-percentage level by the addition of wheat starch.

TABLE I.

Viscometer Determinations on Mixtures of Flour and Starch.   (Jago.)

	I. Spring American Patent.	II. Winter American Patent.	III. Second Class Winter American Bakers.	IV. Hungarian Patent.	V. English Wheat Patent.	VI. British Milled First Patent.	VII. British Milled Second Patent.
Original percentage of wet gluten	39.2	28.2	32.0	35.0	27.75	31.9	38.4
Water - absorbing power by viscometer	68.6	54.8	69.0	76.0	61.0	60.5	64.0
Viscometer readings on gluten being reduced by admixture of starch to—							
35 per cent.	65.0	—	—	—	—	—	—
30 „	62.7	—	—	71.3	—	60.0	63.0
25 „	62.0	55.5	66.0	70.7	59.5	—	—
20 „	61.4	55.4	62.0	66.0	57.5	57.5	58.5
Weight of starch added to 100 parts of flour to reduce gluten to 20 per cent.	96.0	41.0	60.0	75.0	38.75	59.5	92.0

With regard to starch solutions and starch pastes, some viscosities have already been given. (Report I., p. 47. *See* especially Samec, 1911, *Koll. Chem. Beihefte*, **3**, 123–160; 1912, *idem*, **4**, 132–174; 1913, *idem*, **5**, 141–210.) Elsewhere (" *The Conditions that Govern Staleness in Bread*," 1919) the present writer has observed that " the change in viscosity, with time, of starch paste follows a general rule in that, at first thinly viscous when first prepared (hot), it sets to a jelly, and later becomes thinly viscous again." Pure supsensoids change their viscosities (usually a decrease) in days, the more typical emulsoids showing an increase in viscosity much more quickly. It is assumed that, in starch paste, the latter changes are masked by the slower changes of the former. As a matter of fact, the curve (viscosity plotted against time) is of a distinct S shape, the changes being more marked the more concentrated the solution. The solutions worked with were prepared by digesting 8 grams of wheat starch (containing 10·8 per cent. water) in 180 c.cs. of water, and boiling till the clearest paste was obtained. Other figures of interest obtained with such a solution are shown in Table II., but should be compared with those in Table III. showing the same methods applied to Doughs and Bread-crumb, since, in every case, it is clear that the principle under-lying changes in starch paste need not necessarily be reproduced in bread-crumb, in which the phases are so differently distributed.

Table II.

Starch Paste.

—	—	Apparent Moisture, per cent.	Soluble Extract* on Bone-dry Solids per cent.	Colorimetric Value of Extract with standard Iodine solution.
I	After 30 minutes cooling	95·90	[19·15]	1·31 blue
II	,, 6 hours ,,	95·95	[12·74]	1·05 ,,
III	,, 24 ,, ,,	95·92	[7·62]	0·12 ,,
IV	,, 48 ,, ,,	95·97	[19·5]	{ 0·07 ,, 0·04 red
V	,, 72 ,, ,,	96·84	[16·55]	{ 0·04 blue 0·05 red
VI	,, 96 ,, ,,	95·95	[17·26]	{ 0·08 blue 0·21 red
VII	,, 144 ,, ,,	95·99	[33·91]†	{ 0·34 blue 0·20 red

* These values are unreliable, owing to magnification of experimental error by calculation.

† After 150 hours a still higher figure was obtained.

Table III.

Doughs and Bread-Crumb.

—	Loaf weights in grams.	Apparent Moisture per cent.	Soluble Extract on Bone-dry Solids per cent.	Colorimetric Value of Extract with standard Iodine solution.
Dough after mixing	—	41·21	10·07	$\left\{\begin{array}{l}0\cdot21\text{ red}\\0\cdot05\text{ yellow}\end{array}\right.$
Dough after proving	—	41·10	9·76	Nil.
Bread 6 hours old -	$\left\{\begin{array}{l}\text{Original }997\\6\text{ hours }973\end{array}\right\}$	41·42	12·84	$\left\{\begin{array}{l}1\cdot10\text{ blue}\\2\cdot50\text{ red}\end{array}\right.$
Bread 70 hours old-	$\left\{\begin{array}{l}\text{Original }995\\6\text{ hours }972\\24\text{ hours }964\\48\text{ hours }952\\70\text{ hours }945\end{array}\right\}$	41·35	15·31	$\left\{\begin{array}{l}0\cdot15\text{ blue}\\0\cdot25\text{ red}\end{array}\right.$

Among other conclusions reached by this research were :—

1. The loss of water during cooling and drying-out of a loaf is not responsible for staleness.

2. During the process of becoming stale, there is a fall in soluble extract obtained from the crumb, followed, after a time, apparently independent of staleness, by a rise. The soluble starch in bread-crumb (as shown by the iodine colouration) drops rapidly between 6 hours' and 24 hours' cooling.

3. Investigation shows that a similar fall and rise of soluble extract is to be seen in starch pastes, whilst the iodine colouration follows the same rule observed in bread-crumb.

4. Staleness may be attributed to—

(i) Deposition of solid starch in the crumb of bread, starting between 6 hours and 24 hours' cooling period—

(*a*) By change of temperature ;

(*b*) Accelerated by the presence of solid starch particles already existing in the crumb.

(ii) Partial polymerisation of starch, independent of the deposition already stated, which tends to crumble the gelatinous nature of the bread-crumb when fresh.

The statement in 4 (i) (*b*) is open to dispute, for we have no actual proof that the deposition of solid starch from solution is accelerated by the presence of other solid particles. This work is even now in progress. On the other hand, the statement made in para. 4 (ii) is, we believe, substantially correct, for the following reasons, which must be taken in conjunction with the figures given in Table II showing the soluble extracts from starch paste decreasing with time, (especially as indicated by the iodine colouration) :—

If we take the suspension of starch granules in water as the starting point of our consideration, it is seen to be a coarsely disperse system,

or a crude suspension of colloid matter in a dispersion medium, increasing in uniformity of distribution as the subdivision of the suspended particles is increased. Such a system allows almost complete separation of the dispersion means by filtration through an ordinary filter paper. The ordinary filter paper will hold back particles having a diameter greater than about 5μ, though, according to the method of preparation, filters can be obtained which will stop the passage of particles down to an approximate diameter of $2\ \mu$. According to E. F. Armstrong (Brit. Assocn., 1909), the smallest wheat starch granules vary from 3 to $5\ \mu$, and the largest from 30 to $35\ \mu$, so that the filtration of the water from the granules should be almost complete. This, in practice, has been observed to be the case.

The simplest form of starch has been given (Brown, H. T., and Morris, G. H., *Jour. Chem. Soc.*, **53**, 610, 1888) a molecular formula $[(C_{12}H_{20}O_{10})_{20}]_5$. Lobry de Bruyn and Wolff (*Rec. Trav. Chim. des Pays. Bas.*, **23**, 155, 1904) estimated the size of the starch molecule to be approximately $5\ \mu\mu$, as compared with hydrogen gas $0\cdot067$–$0\cdot159\ \mu\mu$, and water vapour $0\cdot113\ \mu\mu$. Soluble starch prepared by the action of ozone on common starch was examined by Friedenthal (*Physiol. Zentralbl.*, **12**, 849, 1899). who obtained a molecular weight of 9,450 as against 32,400 in the formula above. The product obtained in this way was clearly more highly dispersed than ordinary starch, a fact borne out by the definite depression of the freezing point of water containing it in contrast to a suspension of ordinary starch or to a starch paste.

Depression of Freezing Point of Soluble Starch.

Concentration per cent.	Depression of the Freezing Point.
$2\cdot5$	$0\cdot005$
$5\cdot0$	$0\cdot01$
$10\cdot0$	$0\cdot02$

On the figures of Lobry de Bruyn and Wolff, " if a cubic centimeter of dry starch could be subdivided into its molecules or dissolved in the ordinary sense of the word, the starch would present a total surface of several thousand square metres towards the solvent," and, in doing so, would pass from an average size of its individual particles of $20\ \mu$ through the value $0\cdot1\ \mu$, which represents the limit of microscopic visibility, to a value of $1\ \mu\mu$, a figure somewhat smaller than that of a particle hitherto observed with an ultra-microscope.

It is between the last two values that colloid chemistry has to deal, according to Zsigmondy's system of classification. (Zsigmondy, R., *Zur Erkenntnis der Kolloide*, XXII., Jena, 1905.)

With regard to filtration of colloids through various papers and diaphragms, a considerable amount of work has been done, the most interesting for the immediate purpose being that by Bechold (*Zeitsch. physik. Chem.*, **64**, 328, 1908). Bechold's results, from his experiments with the pores of filter papers, cannot be taken as absolutely accurate during prolonged filtration of colloid solutions,

since absorption effects are often observed, due to the action of the paper on the dispersed phase, and often lead to clogging of the pores of the filter. The results are, however, interesting, since they show that typical colloids, with particles having a diameter less than $0 \cdot 1 \mu$, are easily able to pass through all the filters tested.

Size of Pores in Filters.

Filter.	Average Size of Pores (Permeability to Water).
Ordinary thick filter paper - - -	$3 \cdot 3 \mu$
Filter paper, No. 556. (Schleicher and Schüll) - - - - - - -	$1 \cdot 7 \mu$
Filter paper, No. 602. (Extra hard, Schleicher and Schüll) - - - -	$0 \cdot 89\text{--}1 \cdot 3 \mu$

The degree of solubility is also of importance in this question of filtration, chiefly because the solubility of a substance is dependent upon its specific surface, or, in other words, the solubility rises greatly with the extreme subdivision. The jelly concentration of silicic acid was found by Graham (*Jour. Chem. Soc.*, 1864), in the very early days of colloid chemistry, to influence the maximum molecular solubility in excess of water. Thus he found that only two parts of 1 per cent. silicic acid jelly formed a molecular disperse solution in 10,000 parts of water, one part of a 5 per cent. jelly, and less of greater concentrations of jelly in the same amount of water. Concentrated jellies, therefore, are less disperse than the more dilute, and so have a lower molecular solubility. The low solubility of gelatinised starch will be appreciated when the figures shown in Tables II. and III. are compared.

The viscosity of starch solutions and separation of starch paste into two phases has already been pointed out in the First Report (p. 49), but, before passing to other points, it is as well to consider another aspect of true soluble starch. Some description of soluble starch has already been given in a previous Report (No. I., p. 51), but there is little doubt that confusion has arisen owing to its variable nature according to the method of its preparation.

Soluble Starch.—Soluble starch paper, prepared by the action of diastase or acids on starch paste or by heating dry starch in a suitable manner (Zalkowski, *Chem. Zeit.*, 1888, 1060), is soluble only to a very small extent—about $2 \cdot 3$ per cent.—in cold water, yet it is possible to obtain a preparation, by the action of sodium peroxide (Syniewski, *Ber.*, 1897, XXX., 2415) on starch suspended in water, that is soluble to the extent of some 12 per cent. in the cold. The latter seems quite a different compound to that prepared by Lintner's method with diastase.

The varied descriptions of the behaviour of soluble starch when placed in water, dissolved and subsequently cooled, would add weight to the conclusion that it was not always the same kind of soluble starch that was under consideration. It may, and usually does, form a thin opalescent paste, remaining fluid on cooling, however prepared, but it does not always revert to the insoluble form in time. Fouard (*Compt. Rend.*, 1908, **97**, 931–3) used a soluble starch that reverted

apparently in a comparatively short time, whilst the writer, in Table IV., has shown that, if there were any reversion at all after 144 hours, in the soluble starch that he employed, it was quite insignificant. There is undoubtedly a small quantity of true soluble starch formed in bread-making.

TABLE IV.

Soluble Starch. (From Messrs. Baird and Tatlock.)

—		Apparent Moisture per cent.	Soluble Extract* on Bone-dry Solids per cent.	Colorimetric Value.
I	After 30 minutes' cooling	95·97	[82·38]	1·78 blue
II	,, 6 hours' ,,	96·05	[84·05]	1·00 ,,
III	,, 24 ,, ,,	95·93	[77·64]	1·68 ,,
IV	,, 48 ,, ,,	96·04	[77·52]	1·95 ,,
V	,, 72 ,, ,,	95·94	[72·66]	1·90 ,,
VI	,, 96 ,, ,,	96·06	[89·34]	1·43 ,,
VII	,, 144 ,, ,,	96·69	[83·38]	1·33 ,,

* These figures are open to less error than those for starch paste, owing to the greater amount of soluble matter actually present.

(b) *Gluten.*

Wheat gluten has been briefly referred to in Report I. (p. 72), from which it may be gathered that the proteid is itself a mixture of coagulable albumin, glutin, and gliadin. These more or less distinguishable proteids have been again subdivided by Osborne and Voorhees (*Amer. Chem. Jour.*, 1893, " *The Proteids of the Wheat Kernel* ") into glutenin, gliadin, globulin, albumin, coagulum, proteose, and certain nitrogen compounds soluble in water in the following proportions, for flour milled from specified wheat :—

TABLE V.

Composition of Wheat Gluten. (Osborne and Voorhees.)

—	Spring Wheat.			Winter Wheat.		
	Nitrogen × 5·68 = Proteid.			Nitrogen × 5·68 = Proteid.		
Glutenin - -	0·8245	=	4·683	0·7346	=	4·173
Gliadin - -	0·6977	=	3·963	0·6884	=	3·910
Globulin - -	0·1148	=	0·624	0·1148	=	0·625
Albumin - -	0·0657	=	0·391	0·0603	=	0·359
Coagulum - -	0·0453	=	0·269	0·0379	=	0·223
Proteose - -	0·0341	=	0·213	0·0791	=	0·432
From water-washings of Gluten.	0·2239	=	1·272	0·1552	=	0·881
Total -	2·0050	=	11·415	1·8703	=	10·603
In meal - -	2·10	=	11·93	1·94	=	10·96

Gluten of wheat flour is therefore a variable colloid when met with in the bakery. It is upon the proportion of glutenin to gliadin and upon the amount and quality of salts present that the nature of the gluten of wheat flour depends. Unless these proportions are known (obtainable only by laborious effort in the laboratory), it is not possible for the chemist to predetermine the quality of the resulting bread. "Washing out" accompanied by baking trials are the speedier tests for the quality of a flour.

Glutenin is insoluble in water, saline solutions and dilute alcohol, soluble in dilute acids and alkalis, and reprecipitated from such solutions by neutralisation.

Gliadin is insoluble in absolute alcohol; soluble in dilute alcohol, (slightly in 90 per cent. and very soluble in 70–80 per cent. alcohol), from which it is precipitated by adding a large quantity of water or strong alcohol, especially in the presence of much salts. It is soluble in distilled water, forming an opalescent solution from which it is precipitated by addition of sodium chloride.

Globulin is soluble in sodium chloride solutions, precipitated therefrom by dilution or saturation with magnesium sulphate or ammonium sulphate, but not with sodium chloride. Partly precipitated by boiling, but not coagulated at temperatures below 100° C.

Albumin is precipitated from its solution by saturating with sodium chloride or magnesium sulphate. Coagulated at 52° C.

Coagulum and proteose are both probably formed during the extraction of the gluten with water. The former is precipitated by saturating its solution with sodium chloride, or by adding 20 per cent. of sodium chloride and acidulating with acetic acid. On concentrating this solution, the proteose is coagulated, leaving behind a proteid called coagulum, which has not been separated in a pure state.

The behaviour of wheat gluten under the influence of salts is, therefore, clearly the result of complex and mutual action among the various colloid components and electrolytes. Very little more can be said definitely.

Ostwald and Lüers [*Koll-Zeits.* **25**, 26–45, 82–90, 116–136, 177–196, 230–240 (1919); **26**, 66–67, (1920)] were evidently working on the colloid chemistry of bread at the same time as the present writer who, unfortunately, has not had an opportunity of seeing the complete papers. The general results obtained seem, however, to bear out the conclusions reached by us in 1918, and published in the *British Baker* in the following year. Ostwald and Lüers have found that the chief differences between flour, dough, and new and stale breads, are of a physical rather than of a chemical nature, and these workers have studied each material separately as a colloid, in much the same way as we have done. The viscosity of various mixtures of flour and water, containing as much as 20 per cent of flour, were made, and mixtures of wheat flour and water were compared against rye mixtures. It was found that rye mixtures became more viscous, whilst wheat mixtures became thinner, on standing, and that traces of acids greatly increased the viscosity of the wheat mixtures, whilst sodium chloride appeared to reduce it. One point of great importance was established, viz., that a flour of

poor baking quality invariably gave viscosity figures considerably below those of good flours.

The action of gliadin in gluten was also studied, and the viscosity of gliadin solutions was found to be increased by traces of acids and alkalis, and diminished by neutral salts. Further, the viscosity of gliadin solutions was greatly affected by change of temperature, and it is interesting to observe that the temperature at which the maximum viscosity was reached was also the temperature at which the best doughs and breads are produced.

The more recent work of Wood, on the action of acids and salts on gluten, has already been briefly outlined in the First Report, but his subsequent comments (Wood and Hardy, *Proc. Roy. Soc.*, 1909, B. 81, 38), made to bring the phenomena of the solubility of gluten into harmony with the ionisation theory, cause rather a strain upon the imagination. We may well believe that " the variations in coherence, elasticity and water content, observed in gluten extracted from different flours, are due rather to varying concentrations of acid and soluble salts in the natural surroundings of the gluten than to any intrinsic differences in the composition of the glutens themselves," but it is less easy to understand that the formation of aqueous solutions of gluten " is due to the development of electric charges round the particles of the proteid owing to chemical interaction between proteid, acid, or alkali, and water," and that the converse, " the tenacity, ductility and water-content of a solid mass of moist gluten depends upon the total or partial disappearance of these electric double layers (supposed to surround each particle of solute), and the reappearance of what is otherwise obscured by them, namely, the adhesion, or ' idio attraction ' as Graham called it, of the colloid particles for each other, which makes them cohere when they come together." This may be the explanation, but it does not help us largely in predetermining the quality of the bread from any particular flour, especially as the glutens were treated after washing out and not in their normal surroundings.

Weyl and Bischoff (Jago, " *The Technology of Bread-making* ") showed that a flour moistened with a 15 per cent. sodium chloride solution gave a dough that had lost its tenacity. Flour baked for several hours at 60° C. can also not be doughed. Both experiments have given rise to the theory that a ferment " myosin " is largely responsible for the ultimate production of gluten during doughing.

Distilled water dissolves a certain amount of gluten from flour, and leaves the dough sticky rather than springy. Soft alkaline water destroys the springiness of gluten by disintegration of the gluten, and by prevention of the coherence of its particles. Hard waters, especially those containing much sulphates, harden the gluten considerably. Chlorides generally are more gentle in their action and, up to a point, assist the water-absorbing and retaining power of gluten.

For other information concerning gluten and its components, see also :—

Jour. Amer. Chem. Soc., 263, Guess (1900); 1068, Snyder, and 1657, Chamberlain (1905); 8, Norton (1906); 74, Matthewson (1908); 1295, Upson & Calvin (1915).

Jour. Soc.Chem. Ind., 1417 Arpin (abs.) (1902); 368, Baker & Hulton (1908).
Canadian Dept. Agric., [57], 37, Shutt (1907).
Jour. Board Agric. Supp. 4, 29, Saunders (1910); 52, Hardy (1910).
Agric. Gaz., N.S. Wales, Guthrie (1896).
Compt. Rend., **123**, 755, Fleurent (1896); **132**, 1421, Fleurent (1901).
Agric. Expt. Stn., Arkansas, Bull 53, Teller (1898).
U.S. Dept. Agric., Bull 101, 56, Snyder (1901).
Zeit. anal. Chem., **44**, 516, Osborne & Harris (1903).
Jour. Agric. Soc., **2**, 1, Humphries & Biffen (1907).

(c) *Mineral Salts.*

The character of bread and, incidentally, the character of gluten are so intimately connected with the action of mineral salts (or other electrolytes) upon colloids, that reference should first be made to the collection of information to be found in the two previous Reports. The works of Wood and Hardy have already been referred to, and scattered through the literature on colloids may be found many other references to the influence of various salts upon wheat gluten, the chemical composition and original nature of which is seldom specified.

The " maturing of gluten " is often mentioned in technical works, and means little else than modifying a " short " gluten to a condition by which it is capable of retaining the gas generated by the yeast. Kohman and Hoffman (*Jour. Ind. Eng. Chem.*, 1916, **8**, 781–9; 1917, **9**, 148–59) have made special claims for potassium bromate in this connection, and have employed it in a special yeast food which was used in the U.S.A. Army with great success. It should be noticed that the action of potassium bromate is more effective the higher the grade of flour used, both in modifying the gluten and in improving the colour and texture of the bread (*Rep. Conn. Agric. Expt. Stn., Bull*, 200, 1917). The addition of alum was but another attempt to modify a bad gluten, the hardening or coagulating effect of that chemical upon many colloids being well known (Odling, *Jour. Soc. Arts*, 1858). As a rule, alum was only employed on old or damp flour, in which the gluten had deteriorated due to the action of acetic or lactic acids. [*See also* the ancient use of sulphate of copper for improving flour. (Liebig, " *Letters on Chemistry* ").]

Deterioration with age of the physical qualities of gluten for bread-making is also well known (Whymper, R., " *Knowledge*," **36**, 85, 1913), whilst the maturing effect on flour gluten of time and bleaching materials should be considered. (*Rep. Local Govt. Board*, 1911, I. *and* II., N.S. 49, *Food Report* 12, by Hamill and Monier-Williams respectively.)

The nature of the water used in bread-making is of as much importance in securing quality as in brewing. It is, of course, the quality of the contained mineral salts in the water that determines the suitability or otherwise of any given source. The action of the salts is not only upon the starch and gluten colloid systems but also upon the degree and speed of development of yeast cells. The effect of soft and hard waters upon gluten has already been mentioned; that upon yeast nd fermentation can be found in any text-book (Reynolds Green, " *The Soluble Ferments and Fermentation*," &c.). It is not without interest in the latter connection that Müller

(*Ber. d. deut. chem. Gesell.*, **8,** 679, 1875) has found that diastase in the presence of CO_2 can act upon unboiled starch.

(d) *Enzymes.*

The colloid problems of enzymes have been reviewed in Report II., and but little can be added to this information so far as bread-making is concerned. Those enzymes that have to be considered are principally maltase, diastase, invertase, zymase, and certain proteolytic enzymes that are not easy to identify.

Of considerable interest to us is the future development of the work of Panzer (*Zeitsch. physiol. Chem.*, **93,** 316, 339, 1914) on the activation of carbohydrates. This worker found that dry lactose treated with dry hydrogen chloride and subsequently with ammonia acquires feeble amylolytic power. The same, he states, to be true of starches, dextrins, gum arabic, maltose, dextrose, lævulose, and galactose. (See also *Jour. Soc. Chem. Ind.*, 1908, 389, Ford and Guthrie.)

II.—*Yeast.*

The work of Professor Bayliss in Report II. (p. 117), surveying the existing knowledge of protoplasm, its nature and properties, and of enzymes that regulate the chemical reaction of the living organism, fully covers the experience of the bakery chemist so far as yeast is concerned. The problems are by no means simple, and involve the biological history of Saccharomyces cerevisiæ (*see* Lafar, " *Technical Mycology* ") in a variety of media (*see* Jago, " *The Technology of Bread-making,*" *Chap.* XI.), and the action of the enzymes contained in and produced by the yeast, as well as certain bacteria, such as the lactic, butyric, and acetic ferments. The auto-digestion of yeast is particularly interesting and important to the baker, who to-day uses, so largely, compressed yeast containing 70–75 per cent. of water In the Army in France the deterioration of yeast during the hot weather was studied, recourse to barms being frequent there in the summer months. Barms were in constant use in Gallipoli and Mesopotamia during the war.

The growth of " rope " (B mesentericus) and moulds should also be mentioned, since their presence must indicate that the bread had become a suitable medium for their propagation from internal changes, and by reason of suitable environmental conditions.

III. and IV.—*Water and Salt.*

As already indicated, the nature of the water is important in considering the quality of bread produced, and the action of various salts commonly found in water, and of sodium chloride in particular, upon the strength or quality of wheat gluten has already been outlined.

V. et seq.—*Fat, Milk, &c.*

The colloid nature of these ingredients has also been considered in previous reports. When used in conjunction with flour, water,

salt and yeast, in quite small quantities, their influence, both on the physical nature and on the keeping qualities of the bread (so far as staleness is concerned), is out of all apparent proportion, however, to the amount present. The present writer has already remarked elsewhere that :—

" The effect of freshness can be enormously increased and sustained for many days by the addition of small quantities of fat. With added fats, up to 3 lbs. to the sack of flour, to doughs made by the short, straight process, the colour is but little impaired, whereas the crust is shorter, the crumb sweeter and more palatable, and the effect of staleness is not appreciated for a much longer time than is the case with bread from the simple standard mixing. The use of half milk and half water, instead of all water, as liquor has a somewhat similar effect, and the bread produced from this mixing recalls the home-made farm loaf, which does not appear to change from the original state of freshness for a week or more, if kept in a cool, dry place."

The reason is not so far to seek, yet, up to the present, no figures have been obtained to demonstrate the protective influence of the colloid existence of these added ingredients upon starch solutions. There is little doubt, however, that they prevent the deposition of starch in much the same way as they oppose the setting of cements.

COLLOID CHEMISTRY IN PHOTOGRAPHY.

By R. E. SLADE, M.C., D.Sc., F.I.C., *Director of Research, British Photographic Research Association.*

Introduction.

Most photographic processes fall into one of the two following classes :—

(1) The substance sensitive to the light is eventually turned into the pigment.

(2) The substance sensitive to light is the support of a pigment already present.

In both classes of process the support of the sensitive substance or pigment is usually a dried gel, *e.g.*, collodion, gelatin, gum.

As an example of class (1) we will consider the ordinary commercial dry plate. The sensitive film consists of very small crystals of silver bromide, sometimes containing some iodide, supported in a dried gelatin gel. On exposure to light some of the grains become developable. On development—treating with a reducing agent, *e.g.*, alkaline hydroquinone—the gelatin swells and the developer diffuses into the gelatin, reaches the grains of silver bromide and reduces to metallic silver those previously made developable by light.

As an example of class 2 we will consider the carbon process. A gelatin solution containing a pigment such as finely divided carbon is coated on to a suitable support, dried, sensitised by immersion in a solution of ammonium bichromate and dried again. The print

is exposed behind a negative and is then developed by washing in warm water. Where the bichromated gelatin film has been exposed to light it has become insoluble, where it has not been exposed to light it dissolves in warm water and the pigment is washed away. Since the bottom of the film will have been protected from light by the pigment it will dissolve in water and all the gelatin film will come away from its support. It is therefore necessary to transfer the gelatin layer on to another support and to develop the image by washing away the pigment that was the bottom of the film.

Photographic Emulsions.

If, to a solution of potassium bromide we add an equivalent amount of a solution of silver nitrate, we obtain a precipitate of silver bromide which quickly collects in the form of large flocks and settles to the bottom of the vessel. If the precipitate in this state or after washing, is shaken up with a solution of gelatin containing a trace of potassium bromide, a colloidal solution of silver bromide is obtained[5]. Such solutions are usually termed emulsions, though, strictly speaking, this is a misnomer they are suspensions, in which gelatin acts as a protective colloid. If the silver bromide is allowed to stand for two or three days before treatment with the gelatin and bromide, it will not form the emulsion. The usual method of preparing emulsions is to precipitate the silver bromide in the presence of gelatin. The particles in the emulsion as soon as they are prepared, are very fine indeed, and such emulsions are sometimes called grainless. If the emulsion is washed free from dissolved salts at any time and kept at ordinary temperatures it becomes fairly stable, that is to say it changes fairly slowly or not at all. If there are dissolved salts present, which have a slight solvent action on the silver bromide, an increase in the size of grain takes place. The change is favoured by rise of temperature. The grains become more sensitive to light as they grow. This growing of the grains is called "ripening." Under suitable conditions the grains grow by capilliary forces until they are 2μ to 10μ diameter[13]. The smaller grains being more soluble than the larger, dissolve and make the solution supersaturated with respect to the larger ones which therefore grow. Usually crystals will only grow in this way until their diameter is about 2μ, because above this size the solubility no longer diminishes as the size increases. The crystals of silver bromide can certainly grow to 10μ diameter during ripening, and this may be due to the fact that such crystals are in the form of very thin plates (less than 1μ thick), or it may be caused by unequal heating and the presence of convection currents in the solution during ripening.

The chloride and bromide of silver crystallise in the cubic systems at all temperatures used in the preparation of photographic emulsions. The iodide crystallises in the hexagonal system below 146° C., and above that temperature in the cubic system. The three halides form solid solutions with each other, so that in an emulsion containing more than one halide, we have only one kind of mixed crystals present. Although in many plates several per cent. of silver iodide is present

with the silver bromide we only get crystals of the cubic system. A microscopic examination of ripened emulsion shows that the particles vary in size from $0\cdot4\mu$ to about 10μ, but emulsions used in practice do not often contain crystals larger than from 2–3μ. It is difficult or almost impossible, to determine the shape of particles smaller than $0\cdot8\mu$ in diameter, but all particles of this size and larger are found to be forms of the cubic system, principally hexagonal and triangular plate and tetrahedra. Since there is no sharp break in the properties of an emulsion at any particlar size of particle, it is probable that even the smallest particles are crystalline. There is no evidence of the existence of amorphous silver bromide. When crystals of silver chloride are grown from an ammoniacal solution, cubes are formed, if gelatin is present there seems to be a tendency for the 1.1.1 faces of the crystal to develop. The simple cube does not seem to be formed in the presence of gelatin.

When light shines on the silver halide a red coloration is produced. Luther showed that the halogen was liberated, and that the reaction did not take place if the pressure of halogen present exceeded a certain equilibrium value depending on the intensity of the light. These coloured halides are often called photohalides, they may also be prepared by subjecting a mixture of finely divided silver and silver halide to a high pressure. It was formerly considered that these photohalides consisted of silver subhalides. The work of Sichling, Lorenz und Eitel, and Lorenz und Hiege has, however, conclusively proved that these photohalides are colloidal solutions of silver in the halide. Sichling showed by E.M.F. measurements that if silver subbromide existed at all in the halides, it was only to a very small extent, and was only stable over a very short range of concentration. The existence of colloidal silver in the photobromide of whatever composition was definitely established. Lorenz and his co-workers showed that optically clear silver halides may be prepared by treating the fused salt with the halogen. When these optically clear crystals are exposed to light they darken, but remain at first optically clear, later the surface at which the beam enters becomes brown and the particles become visible in the ultra-microscope. The particles grow rapidly in the light, and will continue to grow if they are removed from the light and heated to 350° C. Heating without previous exposure to light does not produce these particles. The growth of these particles is accompanied by a diminution of coloration in the immediate neighbourhood. The effect is evidently due to the separation of colloidal silver in the metallic form. The analogy between these fogs and the metallic fogs formed in fused salts seems to be complete. It is probable that the latent image in the photographic plate consists of a colloidal solution of silver in the halide, and is the first stage of the formation of the photohalide. Many theories of the latent image have been put forward, among which may be mentioned that of H. S. Allen, who has suggested that the latent image is due to the loss of an electron by a molecule of the silver halide and the electron remains embedded in the gelatin. Under certain circumstances the electron may get back and the latent image may be destroyed. This, and many other theories have never been put to the proof. Some-

times it is difficult to devise experiments which will decide between various theories, all of which have been made as wide and indefinite as possible and consequently of little practical use. A deeper knowledge of the latent image can only be obtained by further experiments to test the various theories.

When silver bromide is precipitated by the addition of a soluble silver salt to potassium bromide in the presence of gelatin a colloidal solution is obtained. If the concentration of the silver salt added to the gelatin solution of potassium bromide is strong, the resulting colloidal solution appears blue by transmitted light[25], [26], [27]. Such emulsions are sensitive to red and even infra-red light. In the ordinary way emulsions are prepared from a dilute solution of silver nitrate, the emulsion thus obtained appears red by transmitted light. On ripening the colour changes to green. We do not know the size of the particles in the blue emulsion, they are probably very small. The red emulsion contains particles up to about $0 \cdot 1\mu$ in diameter, the green emulsion contains particles from $\cdot 5\mu$ upwards. In the blue and red emulsions the colours are quite well accounted for by the Rayleigh theory of the scattering of light by small particles, but as yet there is no explanation of the cutting off of the red end of the spectrum by the emulsions with particles of about the diameter of a wave-length of light. Keen and Porter have shown that a similar colour change takes place in a suspension of sulphur when the size of particle becomes a little greater than the wave-length of light.

BIBLIOGRAPHY.

Preparation of Emulsions.

[1] Abney, Sir William (Piper & Carter, London, 1883). 'Photography with Emulsions.'

[2] Eder, *Handbuch der Photographie*, Vol. III.

[3] *British Journal of Photography*, 275, 286, 300 (1917).

[4] Liesegang, R. E., *Zeit. Phys. Chem.*, **75**, 374 (1910). 'Uber die Reifung von Silberhaloidemulsionen.'

[5] Bancroft, Wilder D., *Jour. Phys. Chem.*, **14**, 12, 96, 201, 620 (1910). 'The Photographic Plate.' The Emulsion, Pt. I., II., III., IV.

> These papers give an excellent summary and extracts from the literature of the subject up to 1910.

[6] Luppo-Cramer, *Phot. Korr.*, **44**, 572 (1905).

Theories of Ripening and the Latent Image.

[7] 'Photohalides of Silver.' Carey Lea, *Sill. Am. Jour.*, **3, 33**, 349, 476 (1887); *Sill. Am. Jour.*, 3, **38**, 129, 237, 248 (1889).

[8] W. Ostwald, *Eder's Jahrbuch fur Photographie* (1897), 402.

[9] R. Luther, *Zeit. Phys. Chem.*, **30**, 628 (1899). 'Studien uber umkehrbare chemische Prozesse.'

[10] R. Luther, *Die Chemische Vorgange in der Photographie* (1899).

[11] Thiel, *Zeit. Anorg. Chem.*, **24**, 32 (1900). 'Formation of Mixed Crystals of the Halides of Silver.'

[12] Monkemeyer, *Jahr. Min. Beil.*, **22**, 1 (1906).

[13] Bellach, *Wilhelm Knapp, Halle* (1903). 'Struktur der Photographischen Negative.'

[14] Quincke, *Eder's Jahrbuch fur Photographie* (1903), 3.

[15] W. D. Bancroft, *Jour. Phys. Chem.*, **12**, 209, 318, 417 (1908); **13**, 1, 181, 269, 499, 538 (1909); **14**, 292 (1910). 'The Electrochemistry of Light.'

[16] Luppo-Cramer, (Theodor Steinkopf), Dresden (1908). 'Koloid-Chemie und Photographie.'

Several papers are published each year on this subject by Luppo-Cramer in the *Kolloid Zeitschrift*.

[17] Sheppard & Mees, *Zeit. f. wiss. Phot.*, **7**, 27 (1909). 'Theorie der Photographischen Prozesse: Reifen und der Photoelektrische Effekt.'

[18] A. P. H. Trivelli, *Zeit. f. wiss. Phot.*, **8**, 17 (1910). 'Beitrag zu einer Theorie des Reifungsprozesses der Silberhaloide.'

[19] Sichling, *Zeit. Phys. Chem.*, **77**, 1 (1911). 'Uber die Natur der Photochloride des Silbers und deren Lichtpotentiale.'

[20] W. D. Bancroft, *Jour. Phys. Chem.*, **15**, 313, 551 (1911); **16**, 27, 89 (1912). 'The Photographic Plate': The Latent Image, Pt. I., II., III., IV.

A summary of the literature up to that date.

[21] Allen, *The Photographic Journal*, **54**, 175. 'The Formation of the Latent Image.'

[22] Lorenz und Eitel, *Zeit. Anorg. Chem.*, **91**, 57 (1915). 'Uber Silbernebel in Silberchlorid und Silberbromid.'

[23] Lorenz und Hiege, *Zeit. Anorg. Chem.*, **92**, 27 (1915). 'Uber den Belichtungsvorgang in festen Silberchlorid und Silberbromid.'

[24] Krohn, *Phot. Jour.*, **58**, 179 (1918). 'The Mechanism of Development of the Image in a Dry Plate Negative.'

[25] Ritz, *Oeuvres de Ritz*, **4**, 80.

[26] Abney, *Phil. Trans.*, **171**, II., 653 (1880). 'The Photographic Method of Mapping the least Refrangible end of the Solar Spectrum.'

[27] Ritz, *Comptes Rendus*, **143**, 167 (1903). 'Sur la Photographie des Rayons infrarouge.'

References to the papers on optical properties of colloidal solutions are given under the next section.

Colloidal Silver and the Colour of Silver Deposits.

Colloidal solutions of silver in the finest state of division are yellowish brown in colour and as the size of the particles is increased the colour changes to ruby red, lilac, and blue. When these solutions are precipitated by the addition of an electrolyte a dark grey precipitate of silver is obtained. If a soluble silver salt is reduced by a powerful reducing agent such as alkaline pyrogallic acid, silver is precipitated in the black form, if a less powerful reducer such as pyrogallic acid alone, is used, a grey precipitate is obtained[1].

Any circumstance which tends to prevent the coalescence of the reduced silver such as the reduction of an insoluble salt, or the enclosure of the salt in gelatin, yields the dark modification. Collodion does not hinder the coalescence of the silver particles to nearly the same extent as gelatin[1].

The colour of the image obtained varies to some extent with the developer used. If gaslight paper is developed with an excess of bromide in the developer the first image becomes red, but appears black as the development proceeds. The products of oxidation of some developers stain the gelatin yellow or brown as these oxidation products are present in the greatest concentration at those parts of the plate which have been most exposed, the silver deposit in the negative appears to be stained through, though this is not the case.

[1] Luppo-Cramer, *Zeit. f. Chemie und Ind. der Kolloide*, **3**, 33, 135, 170 (1908). 'Uber das Silbergel in den photographischen Schichten.'

[2] Rayleigh, *Proc. Roy. Soc.*, **84**, 25 (1910). 'The Incidence of Light upon a Transparent Sphere of Dimensions comparable with the Wave-length.'

[3] Chapman Jones, *Phot. Jour.*, **51**, 159 (1911); **57**, 158 (1917). 'On the relationship between the size of particles and the colour of the image.'

[4] Nils Philbad, *Zeit. f. Chemie und Industrie der Kolloide*, **9**, 156 (1911). 'Zur Kenntnis der Lichtabsorption in Silberhydrosolen.'

[5] Swen Oden, *Zeit. Phys. Chem.*, **78**, 682 (1912). 'Beziehung zwischen Teilchengrösse und Stabilität disperser Systeme.'

[6] Keen and Porter, *Proc. Roy. Soc.*, **89**, 370 (1914). 'Diffraction of Light by Particles comparable with the Wave-length.'

[7] Paris, *Phil. Mag.*, **30**, 459 (1915). 'On the Polarisation of Light scattered by spherical metal particles of Dimensions comparable with the Wave-length.'

[8] Gans, *Ann. d. Physik*, **47**, 270 (1915). 'Uber die Form ultramikroskopischer Teilchen.'

[9] Liesegang, R. E., *Zeit. wiss. Phot.*, **14**, 343 (1915). 'Uber die Polychromie des Silbers.'

[10] E. F. Burton (Longmans, Green & Co., London) (1916). 'The Physical Properties of Colloidal Solutions.'

[11] Rayleigh, *Phil. Mag.*, **35**, 378 (1918). 'Scattering of Light.'

[12] W. D. Bancroft, *Jour. Phys. Chem.*, **22**, 601 (1918). 'Colour of Colloids.'

[13] Haas, *Ann. d. Physik.*, **57**, 7, 568 (1918). 'Die Beziehungserscheinungen, welche an einer grossen Anzahl unregelmässig verstreuter Offnüngen oder undurchlässigen Teilchen auftreten.'

Gelatin.

In the dry plate the gelatin is not only a protective colloid for the preparation of the emulsion, an adhesive substance for attaching the sensitive substance to the glass and a photochemical sensitiser of the silver halide, but it plays a most important part in development. Unless gelatin, or some other colloid, is present, strong reducing agents such as alkaline developers will reduce silver halides without previous exposure to light. In presence of gelatin, however, this reaction is extremely slow[1].

The oxidation products of developers usually tan the gelatin—make it less soluble so that an ordinary negative shows the pictures in relief. The parts containing most silver being lowest, and the clear gelatin projecting to the greatest height[5].

Sheppard and Elliot have given an explanation of the reticulation of the surface of photographic negatives. Their explanation is based on Procters' work on the effects of acids and alkalis on the swelling of gelatin[10].

The colloid chemistry of gelatin is discussed in detail by Procter in the First Report. The most important later work on the subject is that of C. R. Smith, who has studied the mutarotation of gelatin[11]. He showed that gelatin in solution may be in a sol form A, stable above 35° C., or a gel form B, stable below 15° C. Between these two temperatures the two forms eventually come into equilibrium and this causes mutarotation. Form B is much more viscous than form A, and a certain definite concentration of B is necessary to produce gelatinisation. The authors' experiments show that probably two molecules of the A form combine to form one molecule of the B form.

[1] W. Reinders & J. van Niewenburg. *Koll. Zeit.*, **10**, 36 (1912). 'Gelatine und andere Kolloide als Verzogerer bei der Reduktion von Chlorsilber.'

[2] Lumiere & Seyewitz, *Bull. Soc. Chim.*, iii., **35**, 14 (1906). 'Sur la composition de la gélatine insolubilisée spontanément dans l'obscurité.'

[3] Lumiere & Seyewitz, *Bull. Soc. Chim.*, iii., **33**, 1032 (1905). 'Sur la composition de la gélatine impregnée de bichromate de potassium insolubilisée par la lumière et sur la théorie de cette insolubilisation.'

[4] Lumiere & Seyewitz, *Bull. Soc. Chim.*, iii., **35**, 676 (1906). 'Action des alums et des sels d'alumines sur la gélatine.'

[5] Lumiere & Seyewitz, *Bull. Soc. Chim.*, iii., **35**, 377 (1906). 'Sur le phénomène de l'insolubilisation de la gélatine dans le développement et en particulier dans l'emploi des révélateurs à l'acide pyrogallique.'

[6] Lumiere & Seyewitz, *Bull. Soc. Chim.*, iv., **1**, 428 (1907) 'Sur l'insolubilisation de la gélatine par la quinoine.'

[7] Lumiere & Seyewitz, *Bull. Soc. Chim.*, iv., **3**, 743 (1908). 'Sur les phénomènes de la précipitation et de l'insolubilisation de la gélatine.'

[8] Procter, *Transactions of the Chemical Society*, **105**, 313 (1914). 'Equilibrium of dilute hydrochloric acid and gelatin.'

[9] Procter, *British Association, First Report on Colloid Chemistry and its Industrial Application*, 24 (1917). 'Colloid Chemistry of Tanning.'

[10] Sheppard & Elliot, *Brit. Jour. Phot.*, **65**, 480 (1918). 'The Reticulation of Gelatin.'

[11] C. R. Smith, *Jour. Am. Chem. Soc.*, **41**, 149 (1919). 'Mutarotation in gelatin and its significance in gelatinisation.'

Gum.

Gum is used in the gum bichromate process as the base of a pigment process just as gelatin is used in the carbon process, but a very thin layer of the colloid is used so no transfer is necessary. The gum is made insoluble by exposure to light after sensitisation with a bichromate. Gum arabic is generally used in this process. Starnes states that better results are obtained by the use of gum senegal.

Starnes has made some experiments from which he concludes that when bichromate is added to the gum it is at once rendered less soluble. On exposure to light it first becomes quite soluble and then, on further exposure more and more insoluble. Thus in the early stages of printing there is a reversal of the image. The phenomenon has, however, not been investigated quantatively.

Starnes, *The Photographic Journal*, **42**, 287 (1918). 'The gum-bichromate process with a new colloid.'

COLLODION IN PHOTOGRAPHY.

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The use of collodion has been coincident with the rise and progress of photography—by collodion is here meant a solution of pyroxylin in ether alcohol.

Any discussion of the colloid chemistry of collodion apart from other forms of nitro-cellulose is almost an impossibility, for although a great volume of work has been done, only a very small part of this may be claimed as appertaining solely to collodion.

The role of collodion in photography is similar to that of gelatine. It acts as a protective colloid, as a support, and also at times plays a part in the actual reactions. The greatest difference between collodion and gelatine is that collodion, except to a negligible degree, does not act as a sensitiser. Its insolubility in water, comparative

indifference to temperature, and its chemical inertia, are in marked contrast to gelatine, and are the main characters which render it of such importance as a support. Any discussion of the photo-chemical reactions involved in the preparation or utilisation of collodion plates or papers would be redundant, as they are of the same general character as occur in all photographic processes, and are dealt with elsewhere.

A very full account of the history and preparation of collodion and its application to photography will be found in Vol. 2 of Worden's "Nitro-cellulose Industry," 1911, pages 827 to 897, where copious references are given to both patents and literature.

Very little exact information exists as to the specific characteristics of photographic collodion. It is obvious that the character of any solution will depend upon the nature of the nitro-cellulose used, the solvents being under comparatively perfect control. The exact nature of the nitro-cellulose depends upon two factors, namely, the raw material used for nitration, and the constitution of the resultant nitro-cellulose, which latter naturally depends upon conditions and details of the nitration process. The permissible nitrogen content for the production of a photographic collodion lies somewhere between 11 per cent. and 12 per cent., and generally about 11·5 per cent. It is found that quite small variations of nitrogen content may involve large variations in physical character, such as, refractive index, viscosity, water compatibility, &c. Hence the mere nitrogen content, within the limits mentioned, does not in any way constitute a guide as to the suitability, or otherwise, of a nitro-cotton. Many attempts have been made to devise methods which would yield more positive results, but with comparatively small success; the behaviour of the nitrated fibre towards polarised light has been investigated by several workers, and a rough indication of the degree of nitration of the cotton can be obtained in this manner. The greatest value of the method so far is that it clearly differentiates, first, unnitrated fibres; and second, fibres of varying degrees of nitration. It is now realised that the usefulness of polarised light will be greatly extended when the results obtained by its use are correlated, not only with the nitrogen content, but with the physical properties, such as viscosity, refractive index, &c., which are of much greater significance than the nitrogen content in determining the properties of the collodion as far as its photographic utility is concerned.

The lack of knowledge as to the constitution of the nitro-cellulose molecule is a bar to more exact information regarding the behaviour of nitro-cellulose solutions, as collodion, in the various processes for for which it is used in photography. Results of an anomalous character are frequent in all investigations, and although there is now available a large volume of observations, they cannot yet be exactly correlated, nor is the information available which explains their occurrence. The effect of drying the cotton before nitration, and especially the temperature and treatment to which it is subjected before the actual nitration takes place, has a profound influence on the physical properties of the resultant nitro-cellulose. Further, both time and temperature modify the nitrated cotton. It would appear that all nitro-cottons undergo a process of denitration to a

greater or lesser degree, and that this denitration explains the anomalies that are so frequently met with in connection with the viscosity of collodions, and also with the behaviour of different films from one and the same solution. This decomposition is capable of acceleration not only by temperature but by many chemical agents; and also, probably takes place spontaneously at ordinary temperatures. The action of bromides in causing denitration is well known. One aspect of this phenomena is its importance in relation to the question of the keeping properties of collodion plates and papers, and of the permanence of the photographic image, whether negative or positive after the usual processes of development, fixation, &c., and in this direction much further investigation is called for.

CELLULOSE ESTERS.

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The technical problems which arise in any colloid industry naturally depend on the uses to which the finished material is put, and in view of the varied nature of the applications of such products as leather, glue, starch, explosives, and colloidal metals, it is not surprising that each industry is concerned with a somewhat different aspect of the chemistry and physics of highly disperse matter.

The industry of the cellulose esters has for its principal object the manufacture of a material of valuable mechanical properties. Its colour, transparency, surface, &c., though of great importance, would be of little moment if the material did not possess elasticity, tensile strength, and toughness at ordinary temperatures, and plasticity at higher temperatures. The object of the industry is the provision of a material combining these properties without prejudice to its adaptability for artistic and imitative effects.

It must be admitted that in the case of celluloid a high standard of technical excellence has been reached without the assistance of theories of the nature of the plastic material. The reproduction of material possessing the desired properties is accomplished only by strict control of the raw and semi-manufactured material, and close adherence to the conditions ascertained by experience.

A brief account of the manufacture of celluloid will be found in Thorpe's " Dictionary of Applied Chemistry,"[1] to which the reader is referred. The manufacture of plastic materials from acetyl cellulose is described by Worden.[2] The colloidal problems encountered in the two manufactures are very similar except in the case of the preparation of the starting materials, nitro-cellulose and acetyl cellulose. These will be considered separately.

Nitro-cellulose is made, as is well known, by the action of a mixture of sulphuric and nitric acids on cellulose, usually in the form either of cotton or paper. The two phases, solid cellulose and liquid acid mixture, persist throughout. It has been shown by Cross, Bevan, and Jenks, and by Hake and Lewis,[3] that there is an intermediate formation of sulphuric esters of cellulose, which are gradually converted

almost entirely into nitric esters. Leaving this complication out of account, however, it is evident that the reaction must proceed by the diffusion of the acid mixture through the fibre. The final degree of nitration reached depends principally on the composition of the acid mixture, and nitrocelluloses of all percentages of nitrogen from 10 to 13 occur in commerce. From what has been said it is evident that the nitration process is very complicated, and it is not to be wondered at that it has not been brought to mathematical expression, although a vast amount of data has been accumulated[4]. However, by the accurate control of the composition of the bath, the purity and humidity of the cellulose, the temperature and time of nitration, very uniform products are obtained, and, to take only one instance, that of the manufacture of guncotton, the accuracy of modern musketry and artillery practice proves how uniformly a two-phase reaction can be controlled, although involving diffusion through a membrance which itself alters during the reaction[5].

Acetyl-cellulose is prepared by the prolonged action of acetic anhydride, acetic acid and sulphuric acid on cellulose, but the process differs from nitration in that the product is soluble in the reaction mixture. In this case, therefore, the acids reach the cellulose by diffusion through a gel of acetyl cellulose in acetic acid. The final product of the reaction is homogeneous, at any rate down to ultra-microscopic limits, and the acetyl-cellulose is recovered in the solid state by precipitation with water. The acetylation of cellulose takes much longer than nitration, and up to the present the uniformity of the product is inferior to that of nitrocellulose. This is probably due partly to the difficulty of temperature control, and partly to the complications introduced by modifications designed to produce material soluble in special solvents[6].

The central point in the manufacture of celluloid and acetyl-cellulose plastic materials is undoubtedly the gelatinisation of the base. Nitrocellulose retains the form of the original cellulose, although harsher to the touch. When it is kneaded with camphor and alcohol it is converted into a transparent gel, and the remaining processes of the manufacture merely consist in manipulating the gel while it is slowly hardening through loss of alcohol and part of the camphor. It is rolled out into sheets, pressed into blocks, sliced on a planing machine, and finally polished if required. The treatment of acetyl-cellulose is similar in principle, although different solvents are employed.

During the evaporation of the volatile solvents from celluloid in its seasoning or drying stage, there is a gradual loss in weight, diminution in volume, and increase in specific gravity. It may be noted in passing that the specific gravity of celluloid is of particular interest to the manufacturer of celluloid articles, since he buys the material by weight, and, in effect, sells it by volume. The relation of the loss of weight to the loss of volume was investigated by the writer in relation to another technical problem. The extreme cases would be that of : (1) a sponge-like structure which could lose weight without (apparent) loss of volume in which case loss of weight/loss of volume $= \infty$; (2) a structure which could shrink in volume without loss of weight,

in which case loss of weight/loss of volume $= 0$. On *a priori* grounds, therefore, any value for this ratio (which is the apparent specific gravity of the alcohol and camphor lost) might be expected. It was found in some careful experiments on a certain variety of celluloid in the final stages of drying that the ratio varied only from $0\cdot82$ to $0\cdot91$, the mean value for 28 samples being $0\cdot87$. This is the specific gravity of a solution of camphor in alcohol containing 42 per cent. of camphor by weight. Although these experiments do not prove that the shrinkage in volume of celluloid while seasoning is exactly equal to the volume of camphor and alcohol lost, they show that the difference, if any, must be small.

There is no more fascinating branch of the technology of cellulose esters than the study of solvents. Hundreds of substances and mixtures of substances are known which have more or less marked solvent action on nitrocellulose or acetylcellulose, but the question of how to make a fair comparison between one solvent and another has never been completely worked out. The work was begun for nitrocellulose by the late F. Baker[7]. He came to the conclusion that the best solvent of a particular sample of nitrocellulose was the solvent which yielded the solution of lowest viscosity, and this is in agreement with manufacturing experience. It may be pointed out, however, that another method might be chosen, and that is to find the solvent yielding solutions which will bear the greatest dilution with an indifferent, miscible non-solvent, such as petroleum ether, before the cellulose ester is precipitated. This method is also in line with the technical valuation of solvents, and a rigorous comparison between the two methods would be most interesting. Petroleum ether has been suggested here as the indifferent liquid, because of the unexpected results sometimes obtained when mixtures of liquids act on cellulose esters. It has been long known that ethyl alcohol and ether are, separately, non-solvents of soluble nitro-cotton, but form a solvent when mixed. This mixture was investigated by Baker[8], and he concluded that the solvent power was exerted by a complex formed by the combination of ether and alcohol. This is a reasonable explanation of this particular case, but it does not explain the extraordinary effect sometimes produced on solvent power by the additions of quite small amounts of foreign substance. One instance which has long been known is the solvent power imparted to methyl alcohol by the presence of traces of acetone. A recent example of the technical application of the principle is Eng. Pats. 14,655 and 14,656[9], in which the addition of small quantities of substances such as nitro-toluenes, formanilide, &c., is employed to facilitate the solution of nitrocellulose in nitro-glycerine.

But, returning to the consideration of mixtures such as ether-alcohol where each constituent is present in bulk, it would be of interest to extend Baker's viscosity work on solvent power to such mixtures, and find what proportions of the constituents produced the best solvent mixture. Probably the records of such experiments exist, but they do not appear to have been published.—Index 9a. The results would be of value in a field somewhat remote from that of colloid chemistry, in view of the recent work of Bramley and others on binary

mixtures[10]. The viscosity/concentration curves of such mixtures usually show maxima, the positions of which vary with the temperature, and Bramley concludes that the viscosity of liquid mixtures as an independent test of compound formation in liquid mixtures is unsatisfactory. The writer suggests that the viscosity/concentration curves of mixtures of liquids containing a constant weight of a viscous colloid in solution might give more useful results. The viscosity of the solvent would be masked by the viscosity due to the colloid, dissolved, presumably, in a complex of the two liquids. It is certain that in many cases minima would be found, and it would be interesting to see whether these minima also shifted with change of temperature, and whether their position corresponded with molecular addition compounds. The method could be applied to all mixtures possessing solvent power for a viscous colloid, whether the constituents separately were solvents or not. For instance, mixtures of aliphatic alcohols and benzene hydrocarbons should be examined with nitrocellulose in solution, and mixtures of aliphatic alcohols and chlorinated paraffins with acetylcellulose in solution, and so on. Although the results obtained would be chiefly valuable as a contribution to the study of binary mixtures, they would also be useful data in the chemistry of emulsoid solutions[10a].

The same question was encountered in a different form some years ago by the writer in a purely technical investigation. It is well known that ethyl alcohol and toluene together form a solvent for nitrocellulose of low nitration. To find the composition of the best solvent, constant weights of nitrocellulose of imperfect solubility were treated with a constant volume of toluene and alcohol in different proportions and the amount of nitrocellulose dissolved by each mixture was estimated. The results showed that the optimum mixture corresponded closely with a mixture of three molecules of alcohol to one of toluene thus suggesting the existence of a complex $C_7H_8, 3C_2H_5OH$. Regarded from the purely colloid point of view, perhaps, the development of solvent power by relatively small additions of other substances to partial or non-solvents is the more interesting owing to its analogy to organised processes such as nutrition.

The nature of the solid camphor-nitrocellulose complex is still unknown. Schüpphaus[11] believes that there is some kind of chemical combination between camphor and nitrocellulose, chiefly on the ground that the heat of combustion of celluloid is less than that of its constituents. But this is only the complement of the fact that camphor and nitro-cellulose evolve heat when mixed, which cannot be regarded as evidence of chemical combination. The large changes in surface energy which must accompany gelatinisation would be sufficient to explain the thermal phenomena.

The optical rotation of an acetone solution of camphor and nitrocellulose is equal to the sum of the separate rotations of the camphor and the nitrocellulose, within the limits of experimental error[12].

Dubosc[13] expresses the following views on the constitution of celluloid. Celluloid is a "camphrogel" of nitrocellulose, camphor being the dispersion medium. In the process of gelatinisation

solid nitrocellulose absorbs camphor present in the liquid phase (camphor and alcohol), forming a mass of gel cells enclosing a pseudo-solution of nitrocellulose in camphor and alcohol. The subsequent rupture of the cells during kneading and loss of alcohol by evaporation, leads to an enormous increase in viscosity. There is, however, in the case of camphor celluloid, no separation of suspensoid or solid phase, such as may happen with some of the substitutes of camphor. This is shown by milkiness or opacity in the celluloid, by brittleness and lack of plasticity in the working. The plasticity of camphor celluloid on heating to about 80° C. is attributed to the formation of a liquid phase by fusion, diminishing internal friction.

Dubosc admits that experimental evidence in support of these views is meagre, but on the whole they appear reasonable. Exception might be taken to regarding camphor as the dispersion medium when it occupies only about one third of the bulk of the celluloid.

Speculations on the structure of cellulose esters and the plastic materials made from them inevitably lead to a consideration of the structure of cellulose itself. Cellulose has already been treated by other writers in these Reports[14], and a much-needed emphasis has been placed on its colloidal character. There are still chemists who write as if the elucidation of the structure of cellulose were a problem analogous to the determination of the formula of, let us say, brucine or dextrose. The underlying assumption appears to be that some day the suffix n in the formula $(C_6H_{10}O_5)$ n will be determined, and then, given a sufficiently large sheet of paper, the complete formula will be constructed. It must be admitted that this assumption has led to much interesting research[15].

It is only natural that chemists should attempt to assign definite molecular weights to the materials they handle. The tendency probably arises ultimately from the extraordinary developments of theoretical chemistry on the basis of Avogadro's hypothesis, culminating in the work of Vant' Hoff. It may not be out of place to point out that of all the materials that we wear, handle and consume, those to which the methods of Vant' Hoff can be applied are in a vast minority. Colloidal chemistry would be immensely simplified if it could be brought under systematic mathematical treatment, but in view of the vast differences in properties between the colloidal and crystalline states, it is unreasonable to expect to force colloids into the crystalline system. It is agreed on all hands that if cellulose and similar colloids have a definite molecule, its weight must be extremely large. But the larger the assembly of $(C_6H_{10}O_5)$ units becomes, the more difficult it is to imagine what forces would come into play to put a sudden stop to the process of aggregation. In view of the continuous nature of plant growth, it is not to be expected that there should be a definite limit to the size of the cellulose aggregate, any more than there is to the size of a honeycomb. The limit to the aggregation of C_6 units is probably set, not by internal chemical forces, but by external conditions such as atmospheric temperature and humidity, and the vital activity of the plant.—Index 14a. Wohler's synthesis of urea was hailed as breaking down the barrier between organic and inorganic chemistry, but crystalline substances like urea

are not typical of vital processes. It is always in a colloid medium that the phenomena of life are manifested[16]. Fischer's synthesis of the polypeptides comes much nearer to an imitation of vital processes, and there appears to be no chemical limit to the application of the reactions he employed.

A distinction should be drawn between efforts to determine the size of the cellulose molecule and experiments to determine the configuration of the $C_6H_{10}O_5$ units. It may be remarked in passing that there is no reason to suppose that all the $C_6H_{10}O_5$ units have the same configuration, and a quantitative conversion of cellulose to any particular sugar is unlikely[16a]. Pictet and Sarasin[17] by the dry vacuum distillation of cellulose obtained lævoglucosan, and Sarasin has suggested that cellulose is built up from this unit[18]. Owing to the drastic nature of the decomposition involved, the suggestion must be taken with some reserve, and Irvine has criticised it on other grounds[19]. It is interesting, however, to build up l-glucosan with the aid of tetrahedral carbon atom models, when it will be found that the carbon atoms 1, 2, 3, 6 and the oxygen atom are approximately in one plane, and the carbon atoms 6, 3, 4, 5 and the oxygen atoms 7 and 8 in another plane, approximately at right angles to the first plane (*see* formula in bibliography). The complex would therefore grow in three dimensions (an elementary detail which is sometimes overlooked), and groups and atoms would be brought into proximity in a way which would never be demonstrated by a diagram in two dimensions. One is tempted to speculate whether the insolubility of cellulose in water is due to this mechanical smothering of the hydroxyl groups by the growing complex, or even whether the hydroxyl groups really exist as such in solid cellulose[20]. Certainly any reaction which degrades the cellulose complex yields a product which will take up more water than the original cellulose, and even long-continued mechanical grinding will yield a sticky or pasty mass; from which it appears that increasing the surface of cellulose increases its solubility in water. On the other hand, the vulnerability of cellulose to esterification without profound hydrolysis militates against this view.

The chief contribution which the manufacture of celluloid makes to colloidal theory at the present time is in its insistence on the fundamental difference between emulsoids and suspensoids. There is a close connection between the viscosity of dilute solutions of cellulose esters, and the mechancial properties (or what is called in the rubber industry the nerve) of the solid product[21]. So far as the writer is aware, neither suspensoid nor crystalloid solutions possess any physical properties which can be correlated with the properties of the solid derived from them by evaporation. In crystalloid and suspensoid solutions we have free particles—ions, molecules, or aggregates—moving independently of each other in the dispersion medium. But in emulsoid solutions, as exemplified by cellulose esters, we find a manifestation, in a reduced degree, of the same forces that produce " nerve " in the solid form. Therefore the disperse phase cannot consist, at any rate entirely, of particles having independent existence in the dispersion medium. There

must be structure, and perhaps the most reasonable assumption is a form of network in three dimensions, the interstices being filled either with pure solvent or with solvent containing disperse phase in a lower state of aggregation. There are admittedly difficulties in such a hypothesis, e.g., in regard to the equilibrium between the two concentrations co-existing[22]. This view of the viscosity of nitrocellulose solutions draws a distinction between them and the highly viscous solutions of such crystalline substances as cane sugar in water, which may be regarded as owing their viscosity to the presence of large aggregates consisting of molecules of sugar surrounded by attracted water molecules. This suggests another contrast between the two. The attraction of sugar molecule for water molecules is regarded as being related to the high solvent power of water for sugar and also to the high viscosity of the solution. In the case of cellulose esters, as we have already seen, the best solvent is regarded as the one which gives solutions of least viscosity.

In view of these considerations, a study of the transition from emulsoid to suspensoid solutions would be of great interest. It is well known that an alcoholic solution of mastic, if poured into water, yields a suspensoid solution of mastic. In a similar way a dilute acetone solution of nitrocellulose on dilution with water yields a suspensoid solution of nitrocellulose. From some incomplete experiments it appears that the gradual addition of water to the acetone solution, keeping the concentration of nitrocellulose constant, produces a gradual rise in the viscosity of the solution, both absolutely and relatively to the viscosity of the solvent, until a maximum is reached, after which the viscosity falls until it almost coincides with that of the solvent. The Tyndall effect becomes marked close to the position of maximum viscosity. On the network view of the structure of nitrocellulose solutions these changes would mean a stiffening and probably a shrinkage of the network as the percentage of water in the solvent increases, and finally rupture into free particles, possessing a refractive index sufficiently different from that of the medium to show the Tyndall effect.

Reference should be made to the analogy developed in text-books of physics between viscous stress in liquids and shearing stress in a strained elastic solid, according to which a viscous liquid is regarded as able to exert a certain amount of shearing stress, but is continually breaking down under the stress. The equation developed is $\eta = n/\lambda$ in which η is the co-efficient of viscosity, n the co-efficient of rigidity, and $1/\lambda$ the time of relaxation of the medium, i.e., the time taken for the shear to disappear from the substance when no fresh shear s supplied to it. This is a much more illuminating way of regarding the viscosity of liquids than the analogy with gases, since the viscosity of liquids, unlike that of gases, diminishes with rise in temperature.[23] But it is particularly suggestive in the case of solutions of colloids like nitrocellulose and rubber which depend for their value on their mechanical properties[23a]. In the case of celluloid, in particular, we are dealing with what is at ordinary temperatures an elastic solid, and it would be most instructive to follow up its properties from dilute solution to the solid state, both with and without the addition of

camphor. The intermediate zone would offer considerable experimental difficulties, but the ground has already been broken by Trouton and Rankine[24].

A complete examination of celluloid from this point of view would include the viscosity in dilute solution, gradually increasing the concentration until determination of Trouton's coefficient of viscous traction became practicable, and finally the rigidity, bulk elasticity, and tensile strength as the celluloid gradually lost its solvent. In all cases temperature effects would have to be studied, and the comparison of rigidity and viscous traction at various temperatures would be of particular value in the case of finished celluloid, while the data as a whole could hardly fail to throw light on the problem of the structure of viscous colloids.

References.

[1] Vol. 1, p. 705 (1912 edition). The account given here is written by Schüpphaus.

[2] "Technology of Cellulose Esters," Worden, Vol. VIII.

[3] Cross, Bevan & Jenks, Ber., **34,** 2496; Hake & Lewis, *J.S.C.I.,* **24,** 374 (1905).

[4] *See,* for example, Lunge, *J. Amer. Chem. Soc.,* **23,** 527 (1901), and many others.

[5] Nathan, *J.S.C.I.,* **28,** 177 (1909). Note particularly (p. 185) the small variation in nitrogen percentage (from $12 \cdot 95$ per cent. to $13 \cdot 05$ per cent. in a year's work).

[6] *See* Miles, Eng. Pat. 19,330 of 1905, on the solubility changes induced by partial hydrolysis. An extensive list of patents is given by Worden, *J.S.C.I.,* **38,** 370 T (1919).

[7] Baker, *Trans. Chem. Soc.,* **103,** 1653 (1913). The viscosity of nitrocellulose in various solvents follows the empirical law $\eta = \eta_0 (1+ac)^k$, in which $\eta =$ viscosity of solution, $\eta_0 =$ viscosity of solvent, $c =$ concentration, a and k are constants depending on solute and solvent. The value of $\dfrac{d \log \eta}{d c}$ is convenient for comparing different solvents.

[8] Baker, *Trans. Chem. Soc.,* **101,** 1409 (1912). From the viscosity concentration curves given by mixtures of ethers and alcohols, it appears that dissociation of the latter takes place in such mixtures, but this dissociation is probably associated with the formation of an ether-alcohol complex, and to this complex the solvent power of these mixtures for nitrocellulose is due.

[9] Eng. Pats. 14,655 and 14,656 of 1915. (Rintoul, Cross, & Nobel's Explosives Co., Ltd.) The additions are of the order of 0.1 to 1 per cent., and enable the mixture of nitrocellulose and nitroglycerine to be gelatinised without raising the temperature.

[9a] Since this report was written, an important paper on the subject has been published by Gibson & McCall, J.S.C.I., **39,** 172–176 T, (1920). They find that the composition of the ether-alcohol mixture of optimum solvent power depends on the nitrogen content of the nitrocellulose.

[10] Bramley, *Trans. Chem. Soc.,* **109,** 469 (1916).

[10a] Price (*Trans. Chem. Soc.,* **115,** 1116 (1919)) has shown that the abnormality of the volatility of cordite made with equal properties of acetone and ethyl methyl ketone is not due to any abnormality of vapour pressure or density of the mixed solvents.

[11] Thorpe, *Dictionary of Applied Chemistry,* loc. cit.

[12] Unpublished observation. Only dilute solutions have been examined.

[13] Dubosc, *Le Caoutchouc et la Gutta-Percha,* 9803–9808 (1919).

[14] 1st Report, King 20–38, Chrystall 82–84.
2nd Report, Harrison 54–61.

[14a] Gibson makes the same suggestion in a recent paper. *Trans. Chem. Soc.* **117,** 482 (1920).

[15] *Cf.* Boeseken, v. den Berg & Kerstjens, *Rec. Trav. Chim. Pays Bas*, **35**, 320–345 (1916). Cellulose is regarded as $(C_6H_{12}O_6)n - (n-1) H_2O$, and the acetylation process is studied. As the molecule is hydrosed the acetyl number increases from $62 \cdot 5$ for cellulose triacetate to 77 for dextrose pentacetate. The molecule combines with $3n+2$ mols of acetic acid, giving a triacetate of molecular weight $(162n+18)+(3n+2)42$. The acetyl number $= \dfrac{1,000 (3n+2)}{48n+17}$ from which n can be calculated. Values of n as high as $47 \cdot 5$ were found.

[16] The viscous colloids are nearly all of vital origin. *Cf.* gelatin, rubber, casein, the gums, etc.

[16a] *See* in this connection Miss Cunningham, *Trans. Chem. Soc.*, **113**, 173–181 (1918).

[17] Pictet & Sarasin, *Compt. rend.*, **166**, 38–39, (1918).

[18] Sarasin, *Arch. Sci. Phys. Nat.*, IV., **46**, 5–32 (1918). Starch and cellulose are polymerides of 1-glucosan.
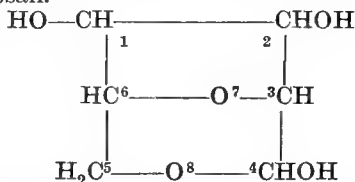
Polymers are found by the opening of the oxygen atom marked 8. This explains the presence of $2 \cdot 5$ dimethyl furane in the products of decomposition.

[19] Irvine, *Annual Reports (Chem. Soc.)*, 69 (1918). The glucosan-polymeride formula is criticised on the ground that it does not account for the particular trimethyl glucose obtained from methylated cellulose by hydrolysis. (*See* Denham & Woodhouse, *Trans. Chem. Soc.*, **111**, 244 (1917)).

[20] Compton (*J. Franklin Inst.*, **185**, 745–774 (1918)), concludes that in the solid state, atoms are so intimately intermingled that particular molecules cannot be said to have any real existence.

[21] Eng. Pat. 114, 304 (H. Dreyfus) states that the higher the viscosity of cellulose acetates, the greater the amount of plastifying or softening agents which can be incorporated, and the stronger the resulting material from every point of view.

[22] In this connection *see* Bayliss, 2nd Report of this Committee. ' Protoplasm and Cell Contents,' and ' The Nature and Permeability of the Cell Membrane.' pp. 117–137.

[23] J. D. van der Waals, jnr. (K. Akad Amsterdam, Proc. **21**, 5 pp., 743–755, 1919) points out this difference between gaseous and liquid friction. He attributes liquid friction to the forces exerted by the molecules on each other, whereas in gases it is due to the transfer of momentum from one layer to another.

[23a] The resistance to shearing stress gradually increases as solvent is removed, until finally an elastic solid results.

[24] Trouton, *Proc. Roy. Soc.* **77**, 426 (1906) and Trouton & Rankine, *Phil. Mag.* [6], **8**, 538 (1904). In the stretching of rods of highly viscous material the following relation is found :—

$$\frac{F}{A} \Big/ \frac{dv}{dx} = \lambda$$

Where F = stretching force.
   A = cross section of rod.
   v = velocity of given point of rod.
   x = distance of moving point from point of suspension.

From a chemical point of view it is unfortunate that Trouton worked with substances of so indeterminate a nature as pitch and shoemakers' wax. The use of a plastic material composed of a celluloid ester, a crystalline filler such as camphor, and alcohol, in a similar series of researches, would to some extent simplify the interpretation of the phenomena. We do not know yet why camphor is *par excellence* the solid solvent for nitrocellulose. It evidently possesses a somewhat unique combination of chemical and physical properties. Substitutes for camphor can only be accurately compared with it by some system of physical tests of the resulting material such as these. The explanation of its colloidal behaviour is not likely to be found until these experiments are done.

COLLOID CHEMISTRY OF PETROLEUM.

By Dr. A. E. Dunstan.

A considerable body of evidence is available concerning the colloidal nature of crude petroleum and some of its distillates. Crude oil of the paraffin base type (*i.e.*, oil which on distillation yields solid paraffins in the high boiling fractions) usually contains colloidal amorphous matter which only assumes the crystalline state after distillation. It is probable that the effect of heat in this case is physical, bringing about a change of state whereby the wax is no longer in the dispersed condition characteristic of the original oil. Such oils and their residues after the removal of benzine and kerosene are frequently highly viscous, particularly at low temperatures, owing to the " setting " of the paraffin sol. This phenomenon may be demonstrated by immersion of a suitable residual oil in ice for several hours when the mass appears to be quite solid. A sudden jerk or violent shaking will destroy the quasi-solid mass and a thick clotted semi-fluid material will be formed.

The black or dark coloured " asphaltic base " oils contain bituminous matter which is to be regarded as being derived from the petroleum hydrocarbons by oxidation (and sulphuration) and condensation.

These oils are optically heterogeneous, although in most cases the degree of dispersion is very high (*see* Holde, *Koll. Zeits.*, 1908, **3**, 270; Schneider and Just, *Zeit. f. Wiss. Mikrosk*, 1905, **22**, 561).

The colloidal asphalts may readily be coagulated by means of strong sulphuric acid (Schultz, *Petroleum*, **5**, 205, 446). The chemistry of the well known " acid treatment " has been investigated by B. T. Brooks and I. Humphrey (*Jour. Amer. Chem. Soc.*, 1918, **40**, 822). The usually accepted view that olefines are polymerised to tar and removed as sludge is erroneous, for pure olefines (up to the C_{16} member) do not give tars with acid up to 100 per cent. strength at 15° C. The formation of " acid tar " is probably a dual phenomenon—firstly, the acid coagulates the colloidal matter present in the oil; and, secondly, it brings about polymerisation of olefines and diolefines, sulphonation of aromatic derivatives, together with oxidation of primary materials and products.

Pyhälä (*Zeits. Chem. Ind. Koll.*, 1911, **9**, 209) considers that crude oils are sols of which the disperse phases are solid gels such as asphalt, together with liquid particles. Separation may be achieved by means of centrifuging or the addition of electrolytes. When the disperse phase exceeds 60 per cent. the phenomenon of gelatinisation makes its appearance.

In the discussion on a paper by Glazebrook, Higgins and Pannell (*Jour. Inst. Petr. Techn.*, 1915, **2**, 54 *et seq*) the writer brought forward the peculiar hysteresis effect in the viscosity of fuel oils and showed that by a suitable alteration in the previous history of a given oil, wide variations in its viscosity may be effected. The corresponding behaviour in aqueous gelatine sols is well known and affords an interesting parallel.

The colloidal asphaltic matter in crude oil and the yellow colouring matter in benzine, kerosene, and other distillates which is largely caused by " tar-fog " mechanically carried over, may be removed by coagulation (and solution) brought about by agitation with strong sulphuric acid. Direct adsorption on specific surfaces however, is equally effective. The writer has shown that floridin, Fuller's earth, and bauxite which, when freshly ignited, possess powerful adsorptive action, follow the well-known exponential adsorption rule :—

$Y/m = ac^{\frac{1}{n}}$. For example, using a $0 \cdot 25$ per cent. solution of a crude asphaltic base oil in benzine as a test liquid, constant values of n were obtained and the $Y/m - c$ curves were of the usual parabolic type.

The application of the adsorptive action of these substances in the refining of various distillates is well known and much of the theoretical side has been admirably expounded by Day and his co-workers (*Proc. Amer. Phil. Soc.*, 1897, 36, No. 154; *Trans. Petr. Congress, Paris*, 365; Gilpin and Cram, *U.S. Geol. Survey Bull.*, 365; Washington, 1908; and Gilpin and Bransky, *U.S. Geol. Survey Bull.*, 475, Washington, 1911. *See also* Richardson and McKenzie, *Amer. Jour. Sci.*, May 1910; Richardson and Wallace, *Jour. Soc. Chem. Ind.*, March 1912; Porter, *Bull.* 315 *U.S. Geol. Survey*, 1907).

In brief, the following conclusions were arrived at :—

(1) Fuller's earth tends to retain the unsaturated hydrocarbons and sulphur compounds in petroleum, thus exercising a selective action on the oil.

(2) When crude petroleum diffuses upwards through a column of Fuller's earth a fractionation of the oil occurs. The oil displaced by water from the earth at the top of the tube is lower in density than that from the bottom of the tube.

(3) The aromatic hydrocarbon in a mixture of a paraffin oil and benzene tends to collect in the lower end of the diffusion column.

Gilpin and Schnerberger (*Amer. Chem. Jour.*, 1913, **50**, 59) consider that the Fuller's earth behaves as a dialysing septum which allows paraffins and saturated hydrocarbons to pass freely but adsorbs bitumen, aromatic hydrocarbons, sulphur, and nitrogen compounds. The determining factor is surface. Similar views are propounded by Gurwitsch (*Petr.*, 1912, 8, 65) who ascribes Day's results not to capillarity but to specific surface adsorption. This author shows that floridin will adsorb solid paraffin from solution in petroleum spirit and benzol, but not from lubricating oil. An interesting observation was made by Herr (*Petr.*, 1909, **4**, 1284), who filtered Baku oil through fuller's earth and discovered that all the formolite forming compounds were removed, *i.e.*, the unsaturated compounds which react with formolin were adsorbed on the mineral gel.

It by no means follows that the compounds which are adsorbed can be recovered unchanged. Being possessed almost invariably of residual affinity the close contact afforded in the adsorbed layer promotes condensation and polymerisation, and thus Gurwitsch

(*Jour. Russ. Phys. Chem. Soc.*, 1915, **47**, 827) was able to show that floridin brings about active polymerisation when brought into contact with amylene and pinene, resulting in considerable rise in temperature. Curiously enough the same polymer, di-amylene, is produced both by sulphuric acid and by fuller's earth. Pinene, similarly, is converted after adsorption into sesqui and polyterpenes. Alumina behaves in the same way towards amylene, but is apparently without effect on pinene. The writer's experience has been that freshly ignited precipitated alumina is particularly effective as a decolourising agent for petroleum and its distillates and a series of experiments using a 0·25 per cent. solution of crude asphaltic oil in benzene showed the following order :—

Material (1 gram).	c.c. of coloured solution decolourised.
Alumina	60
Fuller's earth I.	30
Bauxite I.	30
Bauxite II.	25
Bauxite III.	20
Ignited peat	15
Bone charcoal	14
Bog iron ore	12
Fuller's earth II.	10
Ferric oxide	10
Ball clay	8
Fuller's earth III.	8·5
Fuller's earth IV.	4
China clay	2
Kieselguhr	2

The temperature at which the adsorbing surface exerts its specific effect is of some importance. Gilpin and Schnerberger (*Amer. Chem. Jour.*, 1913, **50**, 59) on passing Californian crude oil through fuller's earth found little fractionating effect at 20° C. but a satisfactory result at 70° C.

A peculiar observation made by the writer is of interest in this connection. Cold bauxite, which has been ignited and cooled in a vacuum desiccator was found to have lost its power of adsorbing sulphur derivatives from kerosene. When freshly heated (to 200° C.) its activity in this direction was regained. Heat appears to be evolved during active adsorption, thus a 20° C. rise in temperature was observed during the passage of 100 c.c. of kerosene through 50 grams of bauxite.

Amongst other effective materials may be mentioned Kambara earth (Kobayashi, *Jour. Ind Eng. Chem.*, 1912, **4**, 891), a mineral containing hydrated silica, which decolourises crude petroleum and adsorbs unsaturated hydrocarbons therefrom. Fibrous alumina has been recommended by Gawolowski (*Allg. Oesterr. Chem.*, 1908, **26**, 87), whilst animal charcoal and prussiate residues have long been employed for these purposes.

Naturally the degree of fineness of the adsorbent is important. The following case will illustrate this point :—

Bauxite Mesh.	Activity.
40/60 - - - - - - - -	1·0
60/80 - - - - - - - -	1·7

In point of fact the activity—all experimental conditions being the same—is approximately directly proportional to the mesh. A convenient method of demonstrating this point consists in treating a 100 c.c. of coloured solution (as e.g., that already mentioned) with varying weights of decolourant, matching the filtered resultant solution with the standard solution in a Nessler jar (C. c.cs.) and plotting $\dfrac{100\ C.}{\text{mass}}$ against C. Ordinates at $C = 50$ give directly the reciprocals of the masses required to remove 50 per cent. of the colour. Thus with floridin :—

Mesh.	Mass.
Passing 180 - - - - - - -	50/320
Passing 80 and retained on 180 - - - -	50/170
Passing 20 and retained on 30 - - - -	50/44
and with Bauxite	
Passing 80 - - - - - - -	50/175
Passing 60 and retained on 80· - - - -	50/80
Passing 20 and retained on 30 - - - -	50/30

Little can be said as to the relationship between the chemical composition of the material and its adsorbent properties. Apparently hydrated silica or alumina is effective after combined water is expelled, but no general statement can be made. Substances giving the same analytical figures may behave quite differently, and again, bodies of dissimilar chemical composition may be equally effective as decolourising agents.

The essential feature of all effective adsorption agents is development of surface, hence mineral gels containing water of combination which, on ignition, possess a characteristic structure are decidedly likely to possess decolourising and desulphurising properties.

Very characteristic is the behaviour of bauxite (say, 40/60 mesh) on being gently agitated with kerosene or benzene. Apparently a process of peptisation goes on, for a considerable amount of very finely divided material separates in suspension in the petroleum, and is sufficiently fine to pass readily through filter paper (see W. Bancroft, Vol. II. Report on Colloid Chemistry, 1918, page 2 et seq).

A highly important contribution to the application of colloid chemistry to industry was made by Clifford Richardson (" The Modern Asphalt Pavement " and reprint of a paper read before the St. Paul Engineering Society, 1917). This investigator showed that the durability of anasphalt pavement is directly connected with the fineness of the mineral aggregate, i.e., with the extent of the surface developed. The capacity factor of the surface energy is measured by the absolute surface displayed and the intensity factor by the particular surface tension of the materials employed. As a case in point, a

particular pavement laid in 1895, the surface of the aggregate was 44 square feet per lb. of material, whereas in a later one the surface was increased to 60 square feet per lb. The former pavement was unsatisfactory, and the latter was excellent.

Naturally occurring colloidal suspensions are found in Trinidad, where the asphalt contains 25 per cent. of finely divided mineral matter, but artificial mixtures of bitumen and dispersed clays can be made which may contain as much as 60 per cent. of mineral. The various asphalts (natural and artificial) possess different powers of retaining the disperse phase. Broadly speaking, asphaltic residues from crude oils are inferior in this respect to the natural bitumens.

Mixtures of 67 per cent. Bitumen and 33 per cent. Clay (introduced while wet) and maintained at 325° F. for 24 hours.

Source of Bitumen.	Colloidal Matter.		Sedimenta-tion.
	Before heating.	After heating.	
	per cent.	per cent.	per cent.
Trinidad residue - - - -	33·5	33·7	0·0
Badabin ,, - - - -	32·4	30·1	7·0
Mexican ,, - - - -	33·3	27·2	18·3
California ,, - - - -	31·8	23·8	25·2
Mid Continental residue - - -	—	—	—
Semi-paraffin ,, - - -	33·8	21·7	35·8

The temperature 325° F. is that at which is formed the film of bitumen which covers the mineral aggregate of a sheet asphalt pavement. It is striking that the Trinidad residual is so thoroughly differentiated from all the others, confirming the opinion based upon service tests in regard to the unique character of this material.

Although in actual refining operations the adsorptive properties of the materials described above have mainly been directed towards the removal of colour, yet considerable success has been achieved in connection with the equal important problem of desulphurisation. It by no means follows that an adsorbent is equally effective in removing colouring matters and sulphur derivatives. Usually this is not the case, and each material must be tested for its specific purpose. So far as the writer's experience goes, the sulphur compounds present in the lighter distillates are more readily adsorbed than those in the higher boiling fractions, although it is possible that in the latter case there is preferential adsorption of other substances, *e.g.*, unsaturated hydrocarbons. Whilst for example, floridin will desulphurise benzine quite readily, it has little effect on the sulphur compounds which occur in the lubricating oils derived from the same crude petroleum.

An interesting application of adsorption is to be seen in the method patented by Hall Motor Fuel, Ltd., for the purification of cracked

spirit. This material, as is well known, contains a considerable proportion of highly unsaturated hydrocarbons—olefines and diolefines —to the presence of which it owes its characteristic odour and its objectionable propensity towards resinification or "gumming." Although the reactive hydrocarbons can be removed by the agency of strong sulphuric acid, the operation is attended by serious loss, but by utilising the adsorbent capacity of floridin, the diolefines present are polymerised to high boiling products and a spirit free from objection is produced. The refining operation is best carried out with the spirit in the vapour state, under these conditions adsorption is followed by condensation and/or polymerisation.

Incidentally the sulphur derivatives present in many benzines and kerosenes may be removed in a precisely similar manner.

The problem of the breaking of persistent emulsions in refining operations is obviously one for treatment by the application of colloid chemistry. The soda wash which is employed to remove the traces of sulphuric acid in the refining of lubricating oils is a common source of this trouble and in some cases a practically permanent emulsion is formed. The sodium salts of naphthenic and sulphonated naphthenic acids are notable emulsifying agents and it is possible that hereinlies the cause of what is sometimes a serious difficulty. It is interesting to remember that sodium naphthenates are used very extensively as soap.

A recent patent by Southcombe and Wells brings out the novel point that a small amount (1 per cent.) of free fatty acid added to a mineral lubricating oil, not only replaces the usual blending fatty oil, but according as its molecular weight is low or high, yields a non-emulsifying or an emulsifying oil. It appears that the addition of the free fatty acid appreciably lowers the interfacial tension between the lubricating oil and the bearing.

Petroleum jelly or "vaseline" appears to be an emulsion of soft paraffins dispersed in heavy oils. The viscosity increases gradually with decreasing temperature until the gel state is attained, without, however, any separation of crystalline wax, but on being distilled, wax appears in the distillate. Various artificial jellies are on this market, being compounded of soft wax and heavy oil, these, on the contrary, are inclined to deposit crystalline matter on being cooled, and do not possess the salve-like nature of the natural product.* An apt comparison is in the different appearance of ice cream made with and without the addition of gelatine and in both cases—vaseline and ice cream— the presence of a protective colloid may be the explanation.

A peculiar illustration of the coagulation of a colloidal solution is seen in the action of flowers of sulphur on the yellow liquid which is produced by treatment of sulphur-containing distillates with sodium plumbite. There is a rapid flocculation and a dark brown precipitate appears.

* By this is meant the material which is obtained from a suitable crude oil by distilling off the lighter compounds and decolorising the residue (usually by filtration through fuller's earth).

The United States Navy Department and the Submarine Defense Association have developed a " colloidal fuel," and a summary of their report follows :—

" Pulverized coal can now be successfully held in suspension so that the colloidal liquid flows freely through the pipes pre-heaters, and burners of ships and power, heating and industrial plants equipped to burn fuel oil. Months after mixing, the composites show little or no deposits. A fixateur, which comprises about 1 per cent. or 20 lbs. per ton, acts to stabilise the particles of pulverised coal dispersed in the oil. In colloidal fuel every solid particle has its film of liquid hydro-carbon and a protective and peptizing colloid, itself combustible. These particles are in three classes as to dimensions—coarse, colloid, and molecular. By coarse is meant here the fineness of fifty million particles per cubic inch. The fixateur and fixated oil are readily made and may be shipped anywhere. The manufacture or distribution of the new fuels incorporating solid carbon in fixated oils involves no doubtful process or industrial problem. On burning, the combustion is so complete that with fair coal there is left no slag and very little ash, what there is being light as pumice and granular as sand.

" It is the property of colloidal fuel that without loss of efficiency per unit volume or change of oil storage or burning equipment it makes possible the conservation of at least 25 per cent. of the fuel oil now burned, or conversely with the oils now available in increases by 50 per cent. the world supply of fuel that is liquid. We may go further and state that a number of new fuels have been realised, each with varying percentages of oil and solid carbon. One useful composite, in the range of ordinary temperatures, is composed of about half coal and half oil. Another unctuous semi-liquid is nearly three-fourths coal and one-fourth oil. All the fuel pastes are mobile to sustained and easily applied pressure, and may thus be pumped, fed, and atomised in the combustion chamber. These semi-fluid composites will constitute the most compact and safest fuel for domestic and industrial use, and they will largely eliminate the smoke and ash nuisances of cities.

" For example, industrial colloidal fuel, grade No. 10, devised to use up some poor coal holding 25·5 per cent. ash, is composed of 61½ per cent. of pressure still oil, wax tailings, petroleum pitch and fixateur running 18,505 B.T.U. per lb. and 38½ per cent. of ' anthracite rice ' running 10,900 B.T.U. per lb. This grade contains 162,500 B.T.U. per gallon, and has 10·2 per cent. of ash. The fixated oil itself had 151,750 B.T.U. per gallon. In fuel value, therefore, the colloidal fuel of grade No. 10 is worth 7½ per cent. more per gallon than the oil from which it is made.

" If instead of ' anthracite rice ' very high in ash, a crude oil coke which is ashless had been employed, the colloidal fuel gallon would have contained 182,154 B.T.U., or roundly, 20 per cent. more than the oil base, and only quarter of 1 per cent. sulphur."

According to Wo. Ostwald ("A Handbook of Colloid Chemistry" page 103), petroleum oil fractions of high boiling point are to be classed as iso-colloids, *i.e.*, a category in which disperse phase and dispersion means possess the same (or analogous) chemical composition.

The ultra-microscopic examination of a number of mineral lubricating oils (Dunstan and Thole, *Jour. Inst. Petr. Tech.*, 1918, **4**, 191) has demonstrated that optical heterogenity exists, although, however, the degree of dispersion is exceedingly high. The same behaviour obtains for the fatty oils and it is possible that lubricating power is in some way connected with this iso-colloidal state.

Lubricating greases are examples of oil-water emulsions stabilised by soap. Commonly sodium soaps are used for motor greases and the proportions are lubricating oil (sp. gr. ·900–·910), 80 parts; stearin acid, 15 parts; and caustic soda, 2 parts. Part of the oil is mixed with the stearin acid and this is added to the soda in 40 per cent aqueous solution, with constant agitation. The remainder of the oil is then incorporated. Cheaper greases are compounded with lime soaps.

Acheson's oil-dag and aqua-dag are suspensoids of graphite in oil or water containing a protective colloid (tannin). Aqua-dag is made first, and the graphite is transferred from this to oil. The oil-dag contains about 15 per cent. of "deflocculated graphite" and is used in a dilute solution of lubricating oil (0·1 per cent. graphite) with beneficial results to the bearings, which gradually become coated with a "graphitoid" layer.

The colloidal graphite in oil-dag may be removed for analysis in two ways. Freundlich (*Chem. Zeit.*, 1916, **40**, 358) throws out the graphite by adding an electrolyte (acetic acid) to the benzol solution of the oil-dag whilst Holde (*Zeit. f. Elektrochem*, 1917, **23**, 116) adsorbs the graphite on recently ignited Fuller's earth in a Gooch crucible. A German proprietary material named "Kollag" appears to be similar to oil-dag.

The influence of colloidal bituminous matter which is mechanically carried over during distillation is frequently sufficient to prevent the easy separation of paraffin wax from that fraction known as "heavy oil and paraffin," and recourse is made to a sulphuric acid treatment before refrigeration. The paraffin scale is usually discoloured and contains a greater or less amount of uncrystallisable material which is removed by the process of "sweating," *i.e.*, fractional fusion. This operation serves to raise the melting point of the wax and also in part to purify it. Final decolourisation is effected by filtering the melted wax through Fuller's earth, bauxite, or prussiate charcoal.

THE COLLOIDAL STATE OF MATTER IN ITS RELATION TO THE ASPHALT INDUSTRY.

By CLIFFORD RICHARDSON, M.Am.Soc. C.E., F.C.S. (*Consulting Engineer, New York*).

The presence of mineral matter in a high state of sub-division in a system solid-liquid, the latter phase consisting of asphalt, reveals some interesting phenomena, connected with the relation of surfaces of solids and films of liquids, particularly where the mineral matter is sufficiently subdivided to exist in a colloidal state as regards the

bitumen. Owing to the viscosity of such a continuous phase the particles of mineral matter with which it is associated may be regarded as a colloidal state, although they may be of dimensions which would prevent their existence in such a state with a more mobile liquid, such as water. Clay and finely divided silica present such a relation to a highly viscous liquid, asphalt for instance, which may be regarded as a colloidal one. Attention was attracted to the subject in the course of a study of the native asphalt found in the Pitch Lake in the Island of Trinidad, British West Indies. This deposit is unique from a geophysical standpoint. It exists in a crater of an old mud spring on the West Coast of the island, and at a distance of about half a mile from the Gulf of Paria. Its surface was originally 138 feet above sea level. Borings which have been recently made show that the crude asphalt exists to a depth of more than 175 feet at the centre of the deposit, which consists of a bowl-shaped mass covering, originally, an area of 114 acres. Specimens taken at various points on the surface and at different depths show that it originates in an asphaltic petroleum, derived from oil sands occurring at considerable depth below the lake, with which a paste of mineral matter and water, originating in a mud spring, has become associated by the churning action of the natural gas accompanying the petroleum, on the release of the pressure to which it has been subjected as the oil approaches the surface. The material formed in this way is of highly uniform composition in all parts of the deposit, and consists of an emulsion of bitumen with a paste of clay and fine sand, and has the following composition :—

	Per Cent.
Bitumen - - - - - - - -	39
Mineral matter - - - - -	27
Water and gas, volatile at 100° C. - - -	29
Water of hydration of mineral matter - -	5
	100

The water, which on melting the asphalt under certain conditions can be separated therefrom, in a somewhat concentrated condition, has been found to contain in solution large amounts of sodium chloride and sulphate with a considerable amount of ammonium and ferrous sulphates, together with borates and a readily recognisable percentage of iodides. It also contains smaller amounts of potassium, calcium, and magnesia salts. It is plainly of thermal origin.

Refined Asphalt.

As it occurs in the deposit it is known as crude asphalt. As such it is submitted to a process of so-called refining at a temperature of 325° F., which removes the water and results in a material known as refined asphalt, which has the following composition :—

	Per Cent.
Bitumen - - - - - - -	57
Mineral matter - - - - - -	39
Water of hydration of clay - - - -	4
	100

In determining the percentage of bitumen in the refined material by means of solvents it is found that some of the mineral matter passes through the finest filters and is not removed from the solution on prolonged centrifuging. On examination under the ultramicroscope it is revealed that it consists of clay in a colloidal condition, originating in the mineral matter of the mud spring in which it existed in this state as regards the water with which it is associated, and which is introduced into the bituminous phase on the removal of the water on refining. The amount of mineral matter in the colloidal state depends on the concentration of the solution, that is to say, upon its viscosity, as shown by the following data :—

Characterisation of Solutions of Trinidad Asphalt (T.R.A.).

Per cent. concentration.	Specific gravity.	Specific gravity increase for 1 per cent. (T.R.A.).	Absolute Viscosity of Solution.	Viscosity difference for 1 per cent. (T.R.A.).
Solvent : Benzol -	0·876	—	0·00652	—
1 per cent. (T.R.A.)-	0·877	0·0010	0·00654	0·00002
2 ,, ,, -	0·879	0·0020	0·00687	0·00033
5 ,, ,, -	0·883	0·0013	0·00759	0·00024
10 ,, ,, -	0·889	0·0012	0·00961	0·00040
20 ,, ,, -	0·911	0·0022	0·01629	0·00067
30 ,, ,, -	0·930	0·0019	0·04198	0·00257
40 ,, ,, -	0·957	0·0027	0·09477	0·00521
50 ,, ,, -	1,012	0·0055	0·31800	0·02240
100 ,, ,, -	1·400	0·0076	—	—

Characterisation of Solutions of Trinidad Asphalt.

Per cent. Concentration.	Refined, per cent. Colloidal matter.	Refined, amount calculated per 1 per cent. (T.R.A.).
Solvent : Benzol—		
1 per cent. (T.R.A.) - - -	2·54	2·54
2 ,, ,, - - -	2·01	1·00
5 ,, ,, - - -	2·09	0·42
10 ,, ,, - - -	2·73	0·27
20 ,, ,, - - -	3·13	0·16
30 ,, ,, - - -	4·19	0·14
40 ,, ,, - - -	6·51	0·16
50 ,, ,, - - -	10·69	0·21
100 ,, ,, - - -	35·40	0·35

In dilute solution it appears that the amount of matter in a colloidal state is comparatively small, but with increased concentration, that is to say, with increased viscosity of the continuous phase, it

becomes progressively larger until in the refined asphalt itself all of the mineral matter, at ordinary temperatures, may be regarded as in a colloidal state. Trinidad asphalt appears, therefore, to be a material the components of which are in a state of equilibrium, and this accounts for its uniform composition. It is, therefore, a unique material, and it is to the large amount of surface energy developed by the highly divided mineral matter which it contains that the demonstrated industrial value of the asphalt is to be attributed.

The Introduction of Colloidal Clay into the purer forms of Bitumen.

In the light of the preceding facts the inference was drawn by the writer that clay in a colloidal state might be introduced in a similar manner, industrially, into the purer forms of asphalt, and into the residual asphalts prepared from petroleum. For this purpose, a paste of clay and water, in which the clay was in a colloidal state as regards the water, was emulsified with residual asphalts from various types of petroleum. The water was then driven off at high temperatures and it was found that the relation of the clay to the bitumen became a colloidal one. The proportions were so selected that the resulting material, after the removal of the water, should consist of 67 per cent. bitumen and 33 per cent. of clay. These materials were then maintained in a melted condition in tubes for 24 hours, at a temperature of 325° F. The sedimentation which ensued, with the reduction of the viscosity of the continuous phase at this high temperature, varied with the different residuals, and was as follows :—

Source.	Penetration.	Per cent. Colloidal Matter.		Per cent. Sedimentation.
		Before Subsidation.	After Subsidation.	
Trinidad Residual -	50	33·5	33·7	0·0
Bababui ,, -	48	32·4	30·1	7·0
Mexican ,, -	50	33·3	27·2	18·3
California ,, -	50	31·8	23·8	25·2
Mid-Continental Semi-Paraffin Residual.	51	33·8	21·7	35·8

It is apparent from the preceding data that the colloidal capacity, if it may be so designated, of the different materials is characteristic of the particular bitumen and of its viscosity at a definite temperature. The various bitumens are, in this way, very plainly differentiated.

Industrial Application.

Industrially these observations are of importance, especially in the construction of asphalt pavements, such as that laid on the Victoria Embankment in London. The mineral aggregate of this surface consists of fine sand, a filler for the voids in the sand,

Portland cement, and the mineral matter afforded by that present in the Trinidad lake asphalt cement which forms the cementing or binding material of the surface. Experience has shown that the stability of such a surface under heavy travel is dependent on the amount of surface energy developed by the mineral aggregate, that is to say, by the state of sub-division of the particles composing this aggregate. While this will depend upon the size of the sand particles and of those composing the filler, it is also contributed to by the highly developed surface of the colloidal components of Trinidad asphalt and to an extent which would be entirely lacking if the purer forms of bitumen were used with the aggregate, a fact which has been demonstrated by the difficulties which have been encountered in the construction of asphalt surfaces with the residual pitches, free from colloidal mineral matter, which have been met with in the past decade in England, and which have necessitated the employment of various expedients to overcome them.

The relation of surfaces of solids to films of liquids, especially when the surface is developed to such an extent as occurs in material in a colloidal state, has been demonstrated, therefore, to be a matter of supreme importance in carrying out successfully the construction of asphalt roadways to carry intense traffic.

[NOTE.—A more detailed account of the colloid chemistry of asphalt is given in the following paper : ' The Colloidal State of Matter in its Relation to the Asphalt Paving Industry,' C. Richardson, Minnesota Engineering Society, May, 1917. W. C. McC. L.]

VARNISHES, PAINTS AND PIGMENTS.

By R. S. MORRELL, M.A.Ph.D., F.I.C., *Chief Chemist, Mander Bros. Wolverhampton.*

In spite of the importance of the problems of surface it is surprising that the scientific study of the class of products comprising varnishes, paints and pigments, has been so much neglected.

The primary components in some form or other, dissolved in a suitable liquid or a finely ground pigment incorporated with a medium as in a paint, introduce a field of investigation of great practical importance and of absorbing interest. If the medium contains water, as in water paints, the properties of ordinary emulsions are prime factors of success. Problems of viscosity arise in varnishes, paints, dopes, and coatings containing cellulose esters; moreover polymerisation .of drying oils confers valuable properties on many varnishes and paints. The conditions of spreading on a surface depend on the physical properties of the components and of the mixtures. The changes on " drying " are essentially superficial, involving questions of adsorption, oxidation, and polymerisation, causing increases in viscosity. The permeability to water and the alteration in the appearance of films introduce the study of the properties of gels.

The resins in their many forms are typically colloid bodies, and their solutions show the properties of that class. The thickened oils

are considered by some to belong to the class of Isocolloids (Wo. Ostwald).

Drying Oils.

The drying oils used in varnishes and in paints in contact with water ought to behave like other vegetable oils in their power to yield emulsions, and the generalisations laid down in E. Hatschek's Report (B.A. Reports on Colloid Chemistry, **2**, 16), may be considered to apply. (*See also* " Modern Conceptions of Emulsions," W. Clayton, *J.S.C.I.*, **38**, 113, 1919.)

The drying oils seem to differ among themselves in their emulsifying power, although no drop number data are available. In the writer's opinion soya bean and linseed oils are superior to China wood oil; moreover, polymerised linseed oils emulsify better than raw linseed oil, but the emulsions are less stable. The properties of the emulsions with the soaps of the drying oils containing divalent metals are similar to those of other vegetable oils.

When a drying oil is thickened by heat out of contact with the air a marked increase in viscosity and modification of other physical and chemical properties are manifested. (The Chemistry of Linseed Oil, J.N. Friend, 1917, contains a full bibliography of the subject.)

Thickened linseed oil contains polymerised molecules, but there is also evidence of the shifting of the unsaturated linkages (Morrell, *J.S.C.I.*, **34**, 105, 1915). Such thickened oils are considered by Wo. Ostwald to belong to the Isocolloid class which includes petroleum, paraffin, liquid sulphur above 170° C.,and highly polymerised liquids. (Wo. Ostwald, " Handbook of Colloid Chemistry," 2nd Edit., p. 102.)

The Isocolloids are considered to be composed of one chemical substance; in other words the disperse phase and the continuous medium contain the same substance in different states. Their internal friction shows remarkably high temperature coefficients varying greatly with changing temperature. Comparison with the system styrol-metastyrol is, perhaps, the best in considering thickened drying oils (Lemoine, *Compt. Rend.*, **125**, 530, 1897, and **129**, 719, 1899). Seaton and Sawyer (*Jour. Ind. Eng. Chem.*, **8**, 490, 1916), in an investigation on the molecular weights of drying oils and their polymers have found that only in stearic acid as solvent were they able to obtain values of the molecular weights which were independent of the concentration of the solution or which showed absence of combination of solvent and solute.

In view of the complexity of composition of linseed oil with its varying amounts of mixed glycerides more reliable results may be expected from China wood oil.

C. J. Schumann (*Jour. Ind. Eng. Chem.*, **8**, 5, 1916) has investigated the changes which Tung oil undergoes on heating. The oil at first forms a simple polymeride and on further heating it sets to a stiff gel. Schapringer (*Chem. Zent. Blatt*, **2**, 1469, 1905), considers that the gelatinisation proceeds in two stages, the first progressive, and the latter instantaneous; a case of mesomorphic polymerisation. (Kronstein, *Ber.* **35**, 4150, 1902, and **49**, 722, 1916.)

Fahrion considers that the polymerisation of wood oil is not analogous to that of styrol. Polymerised styrol, on further heating, yields styrol, but not so in the case of wood oil (*Farb. Zeit.*, **17**, 25, 83, 1912, and *Ber.*, **49**, 11, 94, 1916). Schumann concludes that a dipolymerised glyceride is first formed which has the power of forming molecular complexes under favourable conditions, giving an insoluble colloidal mass, not, however, accompanied by any further loss of double linkages beyond those diappearing in the first stage of the change.

The presence of decomposition products from the oil prevents the gelation; rosin has the same effect. The solid gel is stated to be transformable into the dipolymer on heating with rosin or with the decomposition products of the oil. It is stated that if the decomposition products of linseed oil are removed while the oil is heated linseed oil will gel rapidly. Schumann concludes that the polymerisation is mesomorphic.

The writer (Morrell, *J.S.C.I.*, **37**, 181, 1918) can confirm the formation of the dipolymer with its subsequent gelation, but he wishes to lay stress on intramolecular changes occurring during the heating of other drying oils; thus Cyclolin or Polyolin (solid polymerised linseed oil) is difficult to saponify, insoluble in amyl alcohol and is considered by de Waele to be of a ring structure (*Jour. Ind. Eng. Chem.*, **19**, 1, 1917).

Krumbhaar states that the speed of polymerisation of Tung oil constitutes the greatest difference between it and linseed oil, and agrees with Fahrion that the polymerisation product is partially soluble in the unchanged oil. The viscosity increases with the amount of the polymer until saturation is reached, when the polymer is thrown out. (*Chem. Zeit.*, **40**, 937, 1916.)

This property of thickening is only markedly shown by the more highly unsaturated oils of the open chain series. Union of molecule with molecule undoubtedly occurs and the polymeride remains dissolved in the liquid oil with increasing viscosity until the fluid coagulates. In the writer's experience half the oil has been polymerised short of the point of setting, beyond that point the mass consists of a gel of the dipolymeride whose viscosity is influenced by the presence of specific substances as in the case of gelatine in water.

The problems of polymerisation and of thickening of drying oils are of the highest practical importance. Further investigation of the Polyolin of China wood oil would throw light on the properties of the thickened oils, especially in their emulsions in water and in other media.

The formation and properties of linoxyn, the oxidation product of linseed oil, are those of a gel, due to oxidation and not to heat, as in the polyolins or cyclolins (Annual Reports of the Society of Chemical Industry, 1916–18). In the manufacture of linoleum (A. de Waele, *Jour. Ind. and Eng. Chem.*, **9**, 1, 1917, and M. W. Jones, *J.S.C.I.*, 1919, **38**, 26) four oxidation products result of which linoxyn is one. These differ in degree of oxidation and linoxyn may be considered as solid oxidised linseed oil. It must again be noted that the degree of unsaturation plays an important part, because olein gives no linoxyn

substance, although it contains unsaturated groupings.   In oil varnishes the function of linoxyn is of paramount importance.

J. N. Friend (*Chem. Soc. Trans*, **111**, 162, 1917) has studied the effects of heat and of oxidation on linseed oil with reference to changes of density, viscosity, and coefficient of expansion.   The problem is complicated by the decomposition of peroxides with the loss of water carbon dioxide, and organic vapours.   There is an increase in volume up to the setting point of the oil, after which contraction ensues, and the expansion is dependent on the increase in weight.   The contraction suffered by the linoxyn explains the cracking of old paint.   The action of driers is bound up with the formation of peroxides.   (Ingle, *J.S.C.I.*, **36**, 319, 1917, and Morrell, *Chem. Soc. Trans.*, **113**, 111, 1918.)

From the writer's experience the peroxides undergo polymerisation passing from viscid oils to varnish films.   On exposure to air the peroxides undergo slow decomposition.   (Ingle, *J.S.C.I.*, 1913, **32**, and **38**, 101, 1919, and Salway, *Chem. Soc. Trans.*, **109**, 138, 1916.)

The gelatinisation of drying oils and oxidation is a problem of the greatest importance affecting the protective power of coatings on wood and on metal.   No doubt too much attention has been paid to the interpretation by changes due to modifications in composition or in orientation, but the distinctive and finer differences in the qualities of the coatings often find no explanation on strictly chemical grounds, and the investigator is driven to find some other cause. Many observers have noted the importance of the presence of the glyceryl radicle in the drying of an oil film although its presence has no marked effect on rate or amount of oxygen absorbed.

Primarily surface phenomena have to be studied and as yet no adequate help has been rendered by experience of other colloid systems.   The writer has experienced this difficulty for many years, and is of the opinion that much can be learnt by closer investigation on the lines of study of the properties of gels.   (Morrell, J.S.C.I., 1920, **39,** 153.)

In the changes occurring during the drying of oils attention must be paid to the surface action of the drier.   Driers like lead and manganese are in colloid solution, and according to Wenzel's Law the amount of chemical change in unit time is proportional to the absolute surface.   If it be granted that there is a large absolute surface in colloids many reactions will occur more rapidly and the phenomena of catalysis are especially marked in colloid systems.

Ostwald (" General Colloid Chemistry," p. 95) states that surface tension may be either raised or lowered by chemical action occurring in the two phases.   A lowering of the surface tension between two phases would accelerate the reaction.

To the best of the writer's knowledge no such measurements in reference to linseed oil have been published.   From his own experience from the measurement of the weights of drops in air by Morgan's method (*Amer. Chem. Journ.*, **33**, 1911), no change in surface tension of China wood oil before and after exposure could be observed.   Possibly the linoxyn was insoluble because it

was necessary to filter the oil from an insoluble skin before the weight of the drops of the exposed oil could be determined.

The surface tension of lead drying oil against air is, however, lower than that of linseed oil, from which it would follow that the lead soap would tend to accumulate on the surface, whereby its specific surface would be increased and consequently its chemical activity.

The whole subject requires further investigation, and it is much to be deplored that so little attention has been paid to it.

Varnishes.

In a paper on the viscosity of varnishes, Seaton, Probeck, and Sawyer (*Jour. Ind. Eng. Chem.*, **9**, 35, 1917) state that varnishes show two types of solutions, viz., true and colloidal; they may, under certain conditions, show the Tyndall effect.

The suspensoid and emulsoid classes differ in viscosity characteristics. The systems consist generally of three components, resin, oil, and thinner, *i.e.*, resin incorporated by heat with oil and thinner added. The variation in the viscosity of the emulsoid type with change in concentration is very great. Seaton shows that the viscosity temperature curves (determined by the Doolittle method) of the true solution type, containing soluble gum and low in polymerised oil, are curves whilst varnishes containing highly polymerised oils give straight lines; moreover, determinations of the viscosity of varnishes at various temperatures will give information as to the nature of the varnish solution.

Decrease in dispersion increases viscosity in emulsoid colloids and addition of thinner, increasing the dispersion of the polymerised components, will lower it.

If Seaton's view is correct the viscosity temperature curve before addition of the solvent would be a straight line, but after addition of the thinner it would be a curve. The examples given by Seaton are striking, but a number of variables define the viscosity of emulsoid colloids besides concentration, temperature, and degree of dispersity (Wo. Ostwald, *Trans. Faraday Soc.*, 1913, **9**, 34); especially there is to be considered solvate formation where the viscosity increases with the amount of dispersion medium taken up by the disperse phase. In view of the difficulty in deciding with accuracy the amount and even presence of polymerised oils in varnishes such a relationship as indicated by Seaton is of great value. Similar changes in viscosity during ageing are of importance because unless the viscosity approaches a constant value in a month's time the varnish may become unsuitable.

The importance of viscosity measurement in the standardisation of aeroplane dope and aircraft varnishes has been fully recognised as a determining factor for flow and freedom of working of these coatings. In view of the variety of composition of varnishes the volatility of the thinner as affecting the flow is of considerable practical importance.

Varnishes often contain a disperse phase associated with the continuous medium.

The application of Hatschek's formula :—

$$\eta^1 = \eta \; \frac{\sqrt[3]{\text{A.}}}{\sqrt[3]{\text{A} - 1.}}$$

$\eta =$ viscosity of the continuous phase.

$\eta^1 =$ viscosity of the emulsion phase.

$\text{A} =$ ration of total volume of the emulsoid to the volume of the continuous phase.

(*Zeit. Chem. Ind. Kolloid*, **11**, 284, 1912) would throw light on the relationship of resin, oil and thinner and also on the composition of the disperse phase, although the formula is stated to be inapplicable to organic solvents. Von Smoluchowski (*Koll. Zeitsch.*, **18**, 1910, 1916) does not consider the prospect of deducing such a formula likely to be successful.

The drying of varnish films is chemically an oxidation process accompanied by increase in weight, volume, and in viscosity during the formation of the colloid linoxyn. The rate of drying may at first be rapid, followed by a period of sweating or syneresis. After a time the sweating disappears; this is possibly a chemical process connected with movements in the combined oxygen of the peroxides primarily formed or to changes in the character of the preliminary linoxyn coating. Wolff (*Farben Zeit*, **24**, 1119, 1919) maintains that oxidation and polymerisation proceed at rates depending on the wave length of light to which a varnish is exposed.

It is to the linoxyn that the water-resisting power of varnishes is due. Recently work has been done in connection with the protection of metal and wood parts of aircraft under the auspices of the British Engineering Standards Association, and for the Materials Section of the Technical Department of the Air Ministry. Few resin or resin oil coatings are impervious to water; possibly Japan lacquer is the best.

From the writer's unpublished investigations the whiteness of a varnish layer when immersed in water is an emulsion of water in the resin oil mixing as continuous medium.

An emulsion would be formed if the emulsifying agent in this case, the resin or oil soap, forms a colloid solution in the non-aqueous solvent (Bancroft, *Jour. Phys. Chem.*, **17**, 501, 1913).

The best water-resisting coatings give an emulsion with difficulty, and although the layer may take up as much as 5 per cent. water, the varnish film will remain clear. The conditions are essentially dependent on the nature and concentration of the linoxyn surface layer and on the nature of the oil and rosin together with the electric charge on the metallic components present in the mixing. It must be pointed out that increased rate of drying of the oil is not sufficient to prevent emulsification.

The surface layer of a varnish is essentially semi-permeable to water, but not to salts contained therein, *e.g.*, NaCl, K_2SO_4, KCNS.

If plain wood be suitably varnished and placed in water absorption will proceed at a rate which varies with the nature of the coating. Professor Lang and the writer have found that for a high class article

the daily rate is $0\cdot0003$ grms. per sq. cm., and the rate appears to be the same either in a water-saturated atmosphere or when immersed in water. The absorption will continue without whiteness appearing until the wood is impregnated and the emulsion can form.

Similarly, gelatine under shellac will pull water through the film. If the film is in glass, cloudiness will appear at once in the absence of the absorbing undercoat, or if the layer is applied on an impervious surface. The milkiness disappears generally on drying the film. On continued immersion swelling ensues with the formation of blisters and detachment of the film.

Often the surface is ridged and shows numerous perforations as if the surface had been scratched and punctured, so that the water absorption on a glass plate becomes steady owing to complete saturation. The swelling must be due to the osmotic pressure of the colloid solution under the protecting layer of linoxyn compelling the compensating migration of water which forms the disperse phase of the emulsion.

It has been shown that normal solutions of $NaCl$, $MgCl_2$ and CaC_2l prevent the whiteness of an ordinary varnish film and reduce largely but do not prevent the passage of water through the film.

$N/2$ solutions of the above salts have nearly the same effect, and this is true for solutions of K_2SO_4 and $KCNS$. There are slight differences in behaviour due to the nature of the metal, so that although sodium and potassium salts show the same behaviour yet magnesium is slightly different to calcium, and that again different to aluminium in the form of chlorides in normal and half normal solutions. In .the case of calcium there is an indication of surface adsorption with the production of a surface bloom which can be rubbed off leaving a clear film. From the figures given by the Earl of Berkeley and Hartley (*Roy. Soc. Proc. A*, **92**, 477, 1916), it would appear that an osmotic pressure approaching $13\cdot5$ atmospheres is necessary to prevent the passage of water into a high-class ordinary outside varnish.

With the solutions of $N/20$ and $N/200$ of the above salts the water absorption increases largely, and attains its maximum in distilled water. The concentration of the linoxyn surface film together with polymerisation of the drying oil present appear to be factors deciding the impermeability; whereas the formation of the emulsion with the absorbed water depends on the nature of the emulsifying agent in the oil. (Morrell, *Jour. Oil and Col. Chemists' Assoc.*, **111**, 36, 1920.)

Sufficient has been given to show that in varnish films similar problems await solution as in ordinary emulsions, and the experience gained in researches on colloids in a water medium are of great value although the presence of non-aqueous solvents render many of the generalisations inapplicable.

Reference may be made here to some instances of application of knowledge gained by investigation of other colloid systems.

Bancroft (*Jour. Phys. Chem.*, **19**, 275, 1915) gives a number of instances of emulsions involving the use of varnish materials, *e.g.*, bronzing liquids in which the metal goes into the dineric surface.

Gelatine can be precipitated from a solution of glue by shaking with benzole, and rosin dissolved in dilute caustic alkali can be removed

by benzene (Winkleblech, *Z. Angew. Chem.*, **19**, 1953, 1906). Kerosene benzol, carbon disulphide, chloroform act similarly, but ether has no effect, and produces no emulsions. Such emulsions are noticeable in varnish analysis and are considered by Bancroft to be due to violent shaking, causing drops of the second liquid, which have the power of condensing colloid particles on the surface and coalescing to larger complexes, to form a rigid emulsion with water.

Colophony in the form of resinates behaves similarly to the soaps of fatty acids in forming emulsions. Among the more recent contributions to the subject may be mentioned the work of L. Paul (*Kolloid Zeits*, **21**, 176–91, 1917), who states that solutions of alkali and resin soaps behave like highly dispersed colloid systems. These colloidal soaps combine with basic dyes to form coloured rosin lakes and are characterised by the readiness with which they combine with petroleum hydrocarbons (*Z. angew. Chemie.*, **28**, Ref. 415, 387, 1915; and *Seifen Zeitung.*, **42**, 640, 659, 1915). The same author (*Kolloid Zeits.*, **21**, 148 and 191) finds that certain fractions of the distillate obtained by distilling a mixture of colophony with phenol or α and β napthol yield dyes with diazo and tetrazo-compounds.

Just as in the case of fats, fatty acids, soaps, and tannic acid, the surface tension of water is lowered by resins or resinates which may be considered to assume emulsoid or suspensoid properties in different dispersion media. J. Friedlander (*Z. phys. Chem.*, **38**, 430, 1901) showed in the solution of rosin in 1 per cent. alcohol how very slight are the changes in viscosity of a liquid when it takes up a suspensoid phase, and again a solution of rosin in alcohol containing a little water possesses a relatively high temperature coefficient (5–6 per cent. per degree temperature) against that of water, 2 per cent. (Hardy, *Z. phys. Chem.*, **33**, 328, 1900). Cohn (*Chem. Zeit*, **40**, 791, 1916) describes gel formation produced when colophony is treated with aqueous ammonia.

A. P. Laurie and Clerk Ranken (*Roy. Soc. Proc. A*, **94**, 53, 1917) describe the imbibition exhibited by some shellac derivatives. The solid which separates on cooling a solution of shellac in boiling sodium carbonate when immersed in water expands rapidly and ultimately disintegrates to a flocculent precipitate. At the maximum point of expansion the solid on immersion in a solution of sodium carbonate contracts, expanding again when transferred to water. It was found that the expansion was inversely proportional to the concentration of the salt solution. Since the shellac molecule is here considered to be permeable to salt solutions the mechanism of the expansion may be accounted for by the passage of the salt solution through the diaphragm, the soluble nucleus dissolving in the presence of the salt solution and the amount which can dissolve controlling the consequent osmotic pressure.

Shellac films from spirit solutions do not absorb normal salts from half normal solutions. The writer (*loc. cit.*) has found that the presence of salts, *e.g.*, N/2 K_2SO_4 or N/2 KCNS reduces the water absorption of the shellac film and no salt could be detected passing through it. The water absorption by shellac is much less than in the case of ordinary varnishes, but the effect is more permanent giving a cloudy

layer which does not clear on drying and becomes very granular with eventual loss of cohesion. Like varnish films shellac gives a semipermeable membrane and with half normal solutions of salts the absorption of water is practically inhibited.

In a varnish film such an equilibrium would leave the film clear, but in shellac there is a persistent cloudiness indicating that the film is becoming granular. The examination of the properties of shellac films is of interest in comparison with oil rosin films. In some respects there is much in common, but in the impregnated shellac film, water is probably the continuous medium. As in the case of oil varnishes a certain per cent. of water can be absorbed without opalescence appearing.

Natanson (*Z. phys. Chem.*, **38**, 690, 1901) has followed up Poisson's researches of 1829 in which it was stated that when a liquid is subjected to deformation a certain time is necessary for obtaining equilibrium, different for different liquids. Liquids have a very small relaxation value. For castor oil from G. de Metz's results the relaxation time is $0 \cdot 0031$ secs., and for solutions of tragacanth and collodion, values of the same order.

Reiger (*Physik Zeitschrift*, 8, 537, 1907; and *Annalen der Physik*, 4, 31, 51, 1910) has shown that fluid mixtures of rosin and turpentine have a possible elastic reaction by an oscillatory viscometric method provided due allowance be made for surface forces.

De Metz (*Comptes Rendus*, **136**, 604, 1903) has examined the very slow relaxation in the double refraction of a copal varnish induced by mechanical deformation caused by pressure or extension. The phenomenon of relaxation in a varnish lasts long enough to be observed in the fall in the double refraction. $T = \dfrac{t - t^1}{\log \Delta - \log \Delta^1}$, where log is to base e and $\eta = nT$ (Maxwell, *Phil. Mag.*, 4, 25, 129, 1868). T = time of relaxation, Δ and Δ^1 the difference of path of two rays at times t and t^1; η = coefficient of internal friction of the varnish and n = modulus of rigidity.

The modulus of rigidity of a liquid varnish calculated on the above formula is $n = 0 \cdot 12 \, \dfrac{\text{dyne}}{\text{cm}^2}$ at $22°$ C., and is of the same order as that of gelatine in water calculated by another method (*c.f.* Schwedoff, *Jour. d. phys.*, **8**, 341, 1889, and **9**, 34, 1890).

Paints and Pigments.

In the literature on oil paints the application of the principles of colloid chemistry is very scanty.

H. A. Gardner (*Jour. Ind. Eng. Chem.*, 8, 794, 1916) discussing the physical character of pigments and paints, points out that the opacity of pigments generally increases with fineness of division. As the refractive index of the vehicle approaches that of the pigment the opacity diminishes. Hence, in turpentine and in linseed oil the opacity will be less than in water as those media have higher refractive indices.

A lead paint will be opaque since its refractive index is greater than that of the oil, whilst a silica paint in turpentine or linseed oil will be practically transparent owing to close equality in the refractive indices of pigment and medium.

The opacity varies inversely with the amount of oil absorbed by the pigment, but the durability is improved by the presence of more oil.

The refractive indices of silica, barytes, zinc oxide, white lead, and zinc sulphide are $1 \cdot 55$, $1 \cdot 6$, $1 \cdot 9$, $2 \cdot 0$, and $2 \cdot 37$ respectively.

In lithopone a mixture of the components fails to give the same opacity as when prepared in contact. Between the limits of 28 per cent. and 38 per cent. zinc sulphide the covering power is best. It is probable that surface adsorption of the zinc sulphide by the barium sulphate occurs.

Rapidity of precipitation, strength of solution and temperature control, are factors which aid in the production of fine grained particles giving the greatest opacity.

The phenomenon of surface adsorption shown by certain lake bases in the presence of colouring matters is of interest and explains why the highly colloid pigments are often preferred. A measure of the degree of dispersion might be based on their colour.

Bingham and Green (*Am. Soc. Testing Materials*, 1919) distinguish between the viscosity of true liquids and the rigidity of plastic solids.

The application of the generalisations drawn from the study of other classes of colloid bodies to problems of the paint industry are referred to by Bancroft ("Theory of Emulsification," V., *Jour. Phy. Chem.*, **17**, 501, 1913). The use of sodium silicate to give an emulsion with linseed oil to prevent the paint from setting or hardening in the package has been known from 1865. Generally 2 per cent. water is the limit, although 4 per cent. may be employed to prevent settling, provided the emulsification of oil with water is assured. Instead of water as combining medium a rosin oil mixing may be employed. An alkaline water fluid is not desirable, and the addition of zinc oxide to the lead white is useful in maintaining the suspension in the linseed oil.

E. E. Ware and Christman (*Jour. Ind. Eng. Chem.*, **8**, 879, 1916) recommend that a non-aqueous protective colloid, *e.g.*, aluminium palmitate or oleate should be added to mixed paints to which small quantities of water have been added containing a protective colloid to prevent settling of the pigment.

The same authors have investigated the skinning, puttying, and livering of mixed paints. Livering is dependent on the acidity of the pigment, and in the case of enamels must be connected with the gelatinisation of the colloid resin due to reduction of its acidity. The coagulation depends on many factors of composition and the presence of foreig substances. Such gels would absorb oil and thinner with separation of the pigment. Skinning would seem to be caused by the oil acids acting on the pigment.

In an oil paint containing rosin the formation of resinates increases the viscosity and the further formation of zinc soap separating from the viscous solution of zinc resinates gives a gel occluding or adsorbing

the remaining oil (livering). Under suspensoid pigments the adhesive properties and cementing values of paint pigments apparently increase with approach to colloidal form. All paint pigments have colloidal properties. Gardner found in the clear oil upon the surface of specially prepared pigments which had stood for a year, the presence of pigment material showing Brownian movement on thinning with benzols. Experiments made with zinc oxide and with silica ground in linseed oil (thickened) gave even after thinning with four vols of benzole a cloudy fluid which yielded no clarification on centrifuging, but could be partially clarified by mixing the two fluids, a change probably due to electric neutralisation.

Paranitraniline red in oil is clear and slightly coloured, becoming bright red when benzole is added, a colour change common in the case of many suspensoid sols.

Prussian blue shows strongly Brownian movement and many particles of chrome green suspensoids are coloured crimson, orange, green, and blue in the ultramicroscope.

Carbon black (containing 90 per cent. carbon) probably adsorbs linseed oil as in the case of pigments. In the presence of strongly oxidised or boiled oils precipitation may occur on addition of benzene, which may be due to imbibition of the spirit comparable with the swelling of rubber in benzene or of gelatine with water. Again, if zinc oxide or lithopone be ground in alcohol and linseed oil added, the alcohol is displaced probably due to lowering of surface tension by introduction of the oil.

[An excellent summary of the properties and uses of carbon black is given by Perrott and Thiessen (J. *Ind. Eng. Chem.* **12**, 324, 1920,).]

Ayres (*J.S.C.I.*, **35**, 676, 1916) considers that "foots" from raw linseed oil can be removed easily by heating to 100° C. and centrifuging.

The presence of the mucilaginous material containing salts is considered by some to be highly detrimental not only in the manufacture but in the durability of many mixings and paint coatings.

Ware and Christman (*loc. cit.*) conclude that the use of emulsifying agents in paint grinding to prevent hard setting has not been satisfactorily explained. The emulsifying agent must exert no saponifying action on the oil, but the presence of metallic soaps in certain quantity retards the settling.

Hurst and Heaton state that the emulsification of the oil requires to be assisted by metallic salts such as zinc sulphate, manganese sulphate, and borax, &c.; moreover the addition of a minute proportion of tannic acid incorporated with the pigment prior to grinding with the oil causes the deflocculation of the pigment (Acheson, *J.S.C.I.*, **30**, 1426, 1911).

Cellulose varnishes have already been dealt with in previous reports under nitrocellulose, celluloid, and cellulose acetate.

A class of varnishes containing synthetic rosins on a phenolic trioxymethylene basis is of growing importance.

[Annual Reports of the Society of Chemical Industry (Paints, Pigments, Varnishes, and Resins, 1916, 17, and 18) and G. Matsumato, *Jour. Chem. Ind.*, Tokyo, **18**, 434, 1915; *J.S.C.I.*, 1104, 1915.]

The synthetic rosins may be soluble or insoluble in alcohol depending on the conditions of manufacture and show a great variety of chemical and physical properties. Their composition is complex (Lebach, *J.S.C.I.*, **32**, 559, 1913). The preliminary substance is a phenol alcohol, C_6H_4 (OH) CHOH.

Bakelite discovered by Baekeland, consists of soluble and insoluble forms. On stoving alcohol-soluble Bakelite at $140°-170°$ it passes to a hard insoluble layer or lacquer or to a solid of high chemical and mechanical resistance (Bakelite C, Resite).

Resite has been considered to be. derived from polymerisation products of $CH_2 : < \overline{} \cdot > = O$ (Wohl, *Ber.*, **45**, 2046, 1912).

The advantages of further study of such synthetic organic colloids seem to invite inquiry. Viscosity, gelation, relaxation effects, and dielectric properties are of importance, and, with the exception of the latter, await investigation.

There remains one important class of varnishes, viz., the black japans and black varnishes with carbon black as base.

In general, the pitch base blacks ought to show similar behaviour to the resin mixings or to resin mixings containing no oil with allowance for the nature of the pitch (asphaltum or resin or stearine).

The knowledge of their properties is in the hands of the craftsmen and owing to the complexity of the mixings is of the nature of a trade secret.

Those on a carbon black base involve the knowledge of the properties of carbon black in non-aqueous media. The suspensoid black in a high degree of fineness adsorbing the continuous medium is assisted by an emulsifying colloid forming a membrane around the particle of black.

The results obtained from attempts to produce liquid fuel from petroleum and coal dust are industrially applicable in this case, but the presence of resin and oil gives a more favourable medium for holding the carbon black in suspension.

From the brief summary of a very scanty literature it will be evident that although the main properties are conditioned by the chemical composition of the components, nevertheless the properties which decide between a high and low class of article or between suitability or unfitness are rather to be looked for in a comparison of relationship of phases and in changes of surface energy and adsorption.

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CLAYS AND CLAY PRODUCTS.

By A. B. Searle, *Consulting Chemist, Sheffield.*

The details of the structure of clays and clay products are to a large extent unknown. This is due to the variety of the materials commonly known as *clays*, to the complexity of the reactions which take place when the clays are moistened, dried, and heated, and to the extreme difficulty in studying the products of the reactions.

There are numerous definitions of the term *clay*, but most of them are incomplete. Some are so inclusive as to be applicable to any plastic material, others involve an erroneous assumption as to the manner in which the " alumina " and " silica " are combined, and no definition has yet been published which is entirely satisfactory. Until a better one is forthcoming the following is convenient, though by no means free from objection :—

> A clay is a naturally occurring earthy material, whose chief physical characteristic is its plasticity, and whose essential constituents are reported in an analysis of the substance to be " alumina," " silica " and " water."

This definition does not exclude those highly siliceous and plastic materials commonly known as " brick clays " though some of these are known to contain as much as 60 per cent. of materials of a non-plastic, sandy nature, which is certainly not of the nature of clay.

When a commercial sample of clay is mixed with an equal weight of water and allowed to stand for a few moments, and the liquid decanted through a sieve having 200 holes per linear inch, this treatment being repeated with fresh water until all the small particles have been removed, the residue will usually be devoid of plasticity and will not possess the properties of clay. Usually, it will resemble sand or a mixture of gravel, sand, and rock flour. When some clayey materials, such as some indurated clays, are subjected to this treatment, the whole of the plastic material is not removed, but on prolonged exposure to water, or better still, if the water is made slightly alkaline and boiled in contact with the clay for several hours, the material will be effectively separated into a coarser, sandy, non-plastic matter, along with the smallest non-plastic particles. By a suitable modification of the treatment just mentioned, a " clay " may be divided into a number of fractions, of which all those consisting of particles which will not pass through a No. 200 sieve are obviously not clay. The finer particles are sometimes designated " clay substance," but, though they contain the whole of the plastic material, they are not wholly " clay," as by careful elutriation or repeated sedimentation a further series of non-plastic and siliceous materials may be separated. Seger[1] suggested that the particles which were carried away by a stream of water flowing at the rate of $0 \cdot 18$ mm. per second should be regarded as " clay substance," but this fraction contains a considerable proportion of non-clayey material unless it is derived from a particularly pure clay, so that this use of the term " clay substance " should be abandoned.

The smallest particles which are obtained by elutriating the materials commonly known as clays are found to correspond more or less closely, on analysis, to a composition which may be represented by the formula $H_4Al_2Si_2O_9$. In some samples of Cornish clay and some kaolins, the composition is remarkably constant, but many highly plastic clays and most fireclays yield a product richer in silica and deficient in the elements of water. The constancy of the composition of the better qualities of white-burning clays has led to the supposition that there is in all clays an essential substance— *true clay, clayite,* or *pelinite*—on which all clayey mixtures depend for their chief properties. The existence of this " true clay " has

been so often assumed that there is a widespread impression that it really exists as a definite chemical compound, though it has never been satisfactorily isolated. A further objection to this belief in the existence of a single substance as the essential ingredient of all clays, is the fact that the composition and properties—especially the plasticity—of the elutriated product differs with the origin and nature of the " clay " from which it is obtained. A further objection is that all attempts to obtain a pure product by chemical means or to produce a synthetic clay have failed.

It has also been suggested by W. and D. Asch[2] that the essential ingredient of all clays is not to be expressed by a single substance, but by a large number of substances, each of which have a general similarity in composition, but differing from each other in the arrangement of the atoms in highly complex molecules. Thus, all clays possess properties corresponding to those of a series of insoluble acids and may, therefore, be regarded as alumino-silicic acids. W. and D. Asch[2] go still further and suggest that the essential substances in all clays are alumino-silicic acids, the atoms of which are arranged to form several ring-compounds united together, each ring containing six atoms of either silicon or aluminum together with the requisite number of oxygen and hydrogen atoms (the latter being in the form of hydroxyl groups) to form a saturated compound. In most cases, two or more hydrogen atoms are assumed to be replaced by those of sodium, potassium, calcium, magnesium, or iron.

Asch's theory has been worked out in great detail with regard to the available evidence, but for its ultimate proof it requires the synthetic production of clays of various compositions and this synthetical proof has not yet been accomplished. The synthesis of silica in the form of a hexagonal ring compound, Si_6O_{12}, by G. Martin[3] in 1913 lends some support to the suggested constitution of the purer clays. It has also been suggested that plastic clays are compounds of " alumina " and " silica " with organic groups. This theory does not appear to have been very fully investigated. It has the disadvantage of being largely inapplicable to the purer kaolins which are almost devoid of organic matter.

It was, at one time, thought that the plasticity of clays is due to the presence of bacteria and bacterial products, but this has not been proved, and appears to be improbable.

Although there is a considerable amount of evidence of the existence of one or more definite chemical compounds which are the essential ingredients of clays and, therefore, to be regarded as " true clay," there are numerous properties of clays which cannot be explained by any purely " chemical " theory, and of these the most important is the plasticity. There are also properties which are capable of other explanations, particularly those based on the colloidal nature of clays.

The existence of colloidal matter in clays was first established by Th. Schloesing[4] in 1872, but the most systematic exposition on the colloidal properties of clays is that of P. Rohland[5] in 1891, and more recently the possession of colloidal properties of clays has been generally recognised[6].

At the present time, the most probable theories of the constitution of clays are as follows :—

      (*a*) Clays are a mixture of adventitious minerals (such as sand) and one or more alumino-silicic acids, the latter being the true clays.

      (*b*) Clays are adventitious mixtures of an inorganic colloidal compound, or of several analogous compounds, and of inert minerals such as sand.

      (*c*) Clays are mixtures of alumina and silica or other simple mutually precipitated colloids with other non-colloidal minerals, such as sand.

A little consideration will show that (*a*) is not necessarily incompatible with either (*b*) or (*c*), as the two latter do not give any indication of the chemical composition of the colloidal matter. Moreover, (*c*) is not applicable to all clays, though it may be to some, so that present-day views of the constitution of clays may be reduced to regarding them as mixtures of non-clayey material (sand, &c.), with either (1) a complex compound possessing colloidal properties, or (2) a mixture of colloidal silica and alumina.

There is evidence in support of both these theories and no comprehensive combination of both of them has been published, yet neither theory alone explains all the facts, unless it is sufficient to regard the first theory as applying to some clays, whilst the second is more applicable to others.

It appears quite certain that commercially useful clays are not wholly colloidal in character; they rather resemble a mass of mineral particles, each covered with a film of colloidal matter. If the latter could be wholly separated, it would not possess all the properties which make a clay technically useful, and in this respect the application of the term *clay* to mixtures of sand and colloidal matter would appear to be justified. The laterite clays which are widely distributed in the tropics, are characterised by a large proportion of alumina and silica soluble in hydrochloric acid. The ratio of these two oxides is very variable, and seldom reaches 1 : 2 which is a conspicuous feature of the purer British clays. This great variation makes it more probable that the laterite clays are merely mixtures of colloidal silica and alumina; their other properties resemble those of such a mixture rather than those of typical clays, and the conclusions based on the results of elutriation may require to be received with caution.

The proportion of colloidal matter which can be definitely separated from clays is extremely small, being less than 3 per cent.[4] in the most highly plastic specimens.* Many investigators find it difficult to believe that so small a proportion can account for such great differences in the behaviour of lean and highly plastic clays, and have urged this as an argument against the plasticity of clays being due to the colloidal material present. On the other hand,

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* Ashley[7] has proposed to determine the relative amounts of colloids in clays by observing the amount of each required to just decolourise a standard solution of malachite green. This method, whilst useful for comparative purposes, gives no idea of the absolute amount of colloidal matter present.

there is a close similarity between the behaviour of many clays and that of a fine concrete composed of Portland cement and fine sand, the freshly-made concrete possessing a considerable amount of plasticity even when the total proportion of colloidal matter present is extremely small. A careful comparison of the structure and properties of such a concrete with those of a plastic clay gives a very clear idea of the possible nature of clay and especially of that of its most characteristic property—plasticity.

The properties of clays which are most closely allied to those of colloids or mixtures of colloids and inert materials differ according as the clays are respectively in the dry, pasty, or " slip " state. The pasty condition is produced by reducing the clay to a powder by grinding, and then mixing it mechanically with a suitable proportion of water. Some highly plastic clays occur naturally in the form of a stiff paste which may be softened by crushing between rolls so as to reduce to thin sheets and mixing this mechanically with water. Clay is converted into a slip or slurry by grinding or crushing it and then mixing with a sufficient quantity of water to keep the clay in suspension. The amount of clay which can be suspended in a given volume of water depends on the physical condition of the clay and the presence or absence of very small amounts of alkali, acids, or salts in the water.

The following properties of clay can be most satisfactorily explained by assuming the presence of colloidal matter :—

Water is absorbed by any clay in fairly definite proportions which appear to have some relation to its plasticity, the lean clays absorbing much less water than the more plastic ones.

When clay is completely dried without being excessively heated, it is highly hygroscopic and absorbs water readily—sometimes up to 15 per cent. of its weight—without becoming appreciably moist. It is, therefore, difficult to keep clay perfectly dry, and most specimens contain a considerable proportion of water which may, in some cases, cause the clay to be tough and plastic.

The hygroscopic nature of clay distinguishes it from silt and sand. When a piece of air-dried clay is placed in water, the latter enters into the pores, drives out the air, and lifts up the smallest particles of clay, disturbing the structure of the material so that a partial or complete breakdown or slaking occurs. The disruptive action of the water on the solid particles forming the clay mass may be attributed to a molecular attraction between the water and the clay whereby the water wets the surface of the latter and the resulting interposed film of water reduces the cohesion of the clay grains so that they separate easily. The absorption of water is accompanied by a slight rise in temperature, which though scarcely noticeable is characteristic. The amount of water absorbed varies greatly with different clays; in some cases, it is equal to 80 per cent. of the weight of the clay.

Rohland[5] suggests that this power of imbibing a definite amount of water is due to the colloids in the clay, and that as soon as the clay has absorbed a sufficient amount of water to convert its colloids into the form of a colloidal sol its ability to absorb water reaches a saturation point and ceases; this is proportional to the colloids

present, and probably, roughly to the plasticity of the clay. It may, however, be proportional to the capillary spaces between the clay particles.

In the manufacture of articles from clay paste, it will be found that each kind of clay requires a definite proportion of water for its efficient manipulation. If more is added it will become too weak, if less it will become too short. This water is known as "water of formation," and its amount has a theoretical as well as a practical importance, being closely related to plasticity. Unfortunately, there is no certain method of ascertaining the consistency of the clay paste, nor of ascertaining when the correct proportion of water has been added to a clay. The ordinary method consists in adding such a proportion of water that when the mixture is worked up into a paste it readily receives the impression of finger-prints, but does not adhere to the skin, the amount of water required being found by trial. This procedure is too rough for scientific purposes.

If water is added to a moderately plastic, dry clay in increasing quantities, the clay can at first be moulded with difficulty, then more easily, and later it may be moulded with the greatest facility. If the proportion of water is still further increased, the clay becomes sticky, then fluid, and it is eventually impossible to form it into any definite shape.

If the same experiment is repeated with a more plastic clay, using the same proportions of clay and water as before, it will be observed that it will adhere to the fingers and will allow of no further shaping unless its plasticity is diminished by adding non-plastic material or altering the proportions of clay and water.

An excessively lean clay, on the contrary, only acquires the desired plasticity when it has a very soft consistency, which does not allow it to remain in any given form, and it must, therefore, be rendered more plastic if it is desired that it should be shaped by hand. If the formation is done by mechanical means, in which the clay is subjected to much stronger pressure, less water must be added to the body in order to give it the required plasticity, and it will be expedient to make it of a stiffer consistency. Pressure, in this case, plays the same part as water in the plastic qualities of clays; the one can be partially replaced by the other, so that if the amount of pressure is increased the proportion of water should be diminished and *vice versâ*.

If a sufficient quantity of water is added to a clay to form a slip or slurry, the latter will have certain characteristics, according to the proportion of water and clay, to the nature of the clay and the purity of the water. If the proportion of water is very large and the particles of clay difficult to separate, they may fall to the bottom very soon after the mixing ceases, or the greater part of them may so fall, leaving only the smallest particles suspended in the water for many hours. With high grade clays, such slips have marked colloidal properties (see *Viscosity, Adsorption, &c.*).

Slips containing about an equal weight of water and clay are largely used in various branches of clay-working, for covering other clays of inferior quality when burned, and for making objects by the

process of casting. In the former case, the articles to be covered are immersed in the slip, and in the latter, the slip is poured into plaster moulds and allowed to remain for a short time, after which any superfluous slip is poured away. On allowing the mould to dry, the water is absorbed by the plaster, and the clay article may be removed in due course.

In both cases, it is necessary that the proportions of clay and water should be carefully adjusted, in order to obtain the best results. When a suitable mixture has been obtained, it will usually be sufficient to weigh exactly one pint of it accurately, and to dilute other mixings with a stronger slip or with water, until they reach the same weight per pint. The specific gravity of the slip may be determined with great exactness in a pycnometer, if desired, but this involves unnecessary trouble for most purposes.

Schwerin has found that water and alkalies in the clay slip may be removed by electro-osmosis by connecting the bottom of the tank containing the slip with the negative pole and the cover with the positive pole of a battery when, on passing a suitable electric current, the water and alkali will collect at the bottom, and the slip will become very stiff and apparently—though not actually—dry.

The *hygroscopicity* of dried clay is very marked, up to 20 per cent. of water being absorbed from a damp atmosphere by some clays. It does not necessarily prove the presence of colloidal gels, but if they were present such hygroscopicity would be anticipated.

Miscibility.—It is a remarkable fact that highly plastic clays, in addition to having a limited power of absorbing water, are incapable of forming a uniform mixture with less plastic clays. According to Rohland[5], this is due to the fact that when colloids in clay are coagulated they form gels which cannot be brought into solution by the addition of more water, and resist the absorption of water. They are also incapable of taking up anything from a second colloid. Hence, if the colloids are coagulated, as in very plastic clays, they will not absorb more than a certain amount of water, will not take up other plastic clays, and will not mix homogeneously with them. Many objectionable qualities of a highly plastic clay may be obviated by saturating it with water and then adding a suitable amount of non-plastic material. In this way, also, highly plastic clays gain the power to be mixed thoroughly with other plastic clays and with felspar, which forms coagulable colloid solutions.

Rohland has found that plastic clays in which there is only a small proportion of colloids, and these not coagulated, may be uniformly mixed with other similar clays.

Deflocculation.—Clays usually exist in large masses which are not readily affected by water, but smaller pieces may be broken down or "slaked," as just described, in a manner which is very similar to the deflocculation of colloidal gels. If a suitable electrolyte such as sodium hydroxide, carbonate or silicate, or baryta is added, the amount of suspended matter is increased, as with well-known colloids, and, if an acid is added to the suspension, the clay particles are rapidly precipitated like a coagulable gel. Clays are remarkably

sensitive to the action of electrolytes, a very small quantity of a solution of soda being capable of converting a clay-paste into a viscous fluid which, on the addition of just sufficient acid to neutralise the alkali, will again become solid. This behaviour bears a remarkably close resemblance to the action of electrolytes on the coagulation and deflocculation of colloids. Rohland[5] has suggested that the formation of a clay slip (sol) may be explained as due to the action of electrolytes on the colloids present in the " clay." With a negative sol in colloidal suspension, the most powerful factor in coagulation is the positive ion of the electrolyte added, the negative ion having but little influence. The power of different positive ions appears to be the same for those of the same valency, but divalent and trivalent ions are more powerful than monovalent ones. Thus, Foerster has shown that if a clay contains just enough calcium ions to keep the colloidal matter in the gel state, and sodium carbonate is added, the sodium will combine with the colloid clay so as to form the sol, the plasticity being reduced according to the completeness of the reaction, but if an excess of sodium ions is added they will recoagulate the colloid. This has been confirmed by experiments on the viscosity of clay slips by Mellor and others.

The addition of electrolytes to a clay body also affects some of the materials present. Thus, Schurecht[8] has found that the working properties of mixtures of graphite with sufficient plastic clay to act as a binder, such as are used in the manufacture of plumbago crucibles, are considerably improved by the addition of $0 \cdot 3$–$0 \cdot 4$ per cent. of sodium hydroxide or, in some cases, of hydrochloric acid, according to the nature of the colloidal matter present.

Kosmann[9] attributes the disintegration action of alkaline solutions on clays to the solution of a siliceous film on the particles which acts as a binder. This explanation scarcely seems to account for the great effect produced by so small a proportion of soda.

When clay is saturated with water and an electrolyte is then added, the adhesion of the particles is reduced, partly as a result of the osmotic pressure of the solution on the porous particles[10] which then act as a permeable diaphragm and force the water more strongly into the interior of the particles than would be the case if plain water were used. If the basicity or alkalinity of the solution is altered by the addition of an acid, the particles tend to coagulate and adhere to each other with the result that the mass becomes semi-solid.

When clay is suspended in a liquid having a higher coefficient of capillarity than water (*e.g.*, acids) the particles tend to precipitate, but in a liquid with a lower coefficient than water (*e.g.*, bases and alkalies), they tend to remain in suspension. This behaviour is attributed to the difference in the adhesion of the fluid particles of the liquid to the particles, the surface of the smallest particles being much greater in proportion to their weight than that of the larger ones.

Adolph Mayer has determined the limiting power of electrolytes which permit a fine clay (freed from carbonates and soluble salts by treatment with hydrochloric acid) still to be kept in suspension in water (100 grammes clay, 500 grammes water). The limits are :—ammonia, $2 \cdot 5$ per cent.; sulphuric, hydrochloric, and nitric acids

and the alkali salts of these acids, $0 \cdot 025$ per cent. Although $2 \cdot 5$ per cent. of ammonia caused precipitation in Mayer's experiments, a less amount favours deflocculation, or breaking up of the lump.

The "*fluidity*" of any clay slip depends chiefly on the proportion of water added, but it is largely affected by the presence or absence of very small proportions of electrolytes. According to Rohland[5], the addition of hydrochloric, nitric, sulphuric, acetic, or propionic acid increases the plasticity of the clay slip, apparently by coagulating the colloidal matter present. Solutions with an acid reaction such as sal-ammoniac, aluminium chloride, ferric chloride, and potassium bichromate behave similarly. Alkalies such as ammonia, caustic soda, caustic potash, lime-water, baryta, and basic salts, make the slip more fluid and reduce the plasticity of the material, but their behaviour depends on their concentration. The action of alkalies in reducing the viscosity, sometimes requires several days, and is accompanied by coagulation. An excess of alkali may cause a reversion of this action, the viscosity increasing again. The addition of salts usually decreases the osmotic pressure and increases the viscosity.

Acheson has patented the use of a solution of tannin and alkali to make a clay "fluid," and, followed by the addition of an acid—presumably by precipitating the colloid matter—to increase the plasticity of the clay.

The *viscosity* of clay suspensions, before and after the addition of various substances, can best be understood by assuming that it varies according to the condition, and proportion of the colloidal matter present. Mellor, Green, and Baugh[11] have arranged the substances likely to be present in, or added to, clays into five groups according to their action on the viscosity of the clay* :—

(1) Substances which first make the slip more fluid, while further additions stiffen the slip. Examples : sodium and potassium carbonates, fusion mixture, potassium sulphate, potassium bisulphate, potassium hydroxide, potassium nitrate, sodium sulphide, tannin and gallic acid.

(2) Small amounts thicken the slip; larger amounts make the slip more fluid. Examples : copper sulphate, dilute ammonia, and potassium aluminium sulphate.

(3) Substances which make the slip thinner : magnesium, mercury and sodium sulphates, sodium sulphite, sodium acetate, sodium chloride, sodium phosphate, ammonium gallate, hydrochloric acid, water-glass. It is just possible that some of these substances may have to be transferred to the first (or second group) if greater (or less) concentrations be tried than those employed by Mellor, Green, and Baugh.

(4) Substances which only stiffen the slip : grape sugar, humic acid, ammonium chloride, calcium chloride, calcium

* It should be noted that the slips used were not made from a single clay, but from a body mixture consisting of 16g. ball clay, 19g. China clay, 13g. Cornish stone, 20g. flint and 100 c.c. water. To this mixture, varying quantities of acid, alkali and salt—ranging from $0 \cdot 1$ to 6g. or $0 \cdot 1$ to 35 c.c.—were added. This may account for the difference between these results and those obtained by some Continental investigators.

sulphate, ammonium urate, aniline, ethylamine, methylamine. Here, again, some of these substances may have to be transferred to the second (or first) group if greater (or less) amounts than those mentioned are used.

(5) Substances which have no appreciable effect on the slip : *e.g.*, alcohol.

Rieke has stated that the most soluble substances increase the viscosity of the slip, but their effect may be neutralised by the addition of a solution of barium hydroxide. The most harmful sulphates according to the same investigator are those of calcium, aluminium, and the heavy metals. Alkali sulphates stiffen the slip when only 0·1 per cent. is present; larger proportions render it thinner until 1 per cent. is reached, after which they stiffen it again. Zinc and copper sulphates exhibit this phenomenon of variableness to a marked degree.

Bleininger found that the first addition of clay (up to 3 per cent.) decreased the viscosity of water on account of the deflocculation of the clay by dilution and the solution of the contained electrolytes. When, however, the addition of clay became so great that no further matter went into solution and the effect of the gel showed itself, the viscosity increased with each addition of clay. This negative viscosity is peculiarly characteristic of some clays.

The *size of the particles* of the purer clays is comparable with that of colloidal particles, but most clays contain so large a proportion of larger particles that it is almost impossible to isolate those which are colloidal, in an entirely satisfactory manner.

The *adsorptive power* of clays bears a striking similarity to that of colloids, or rather to that of a mass of inert material, the particles of which are covered with a film of colloidal matter which also fills some of the interstices. Thus, clays adsorb soluble dyestuffs, tannin, humus, oil, grease, salts,* &c.; and Hirsch and others have found that barium, lead, and aluminium salts are adsorbed more readily than those of lime and magnesia. Chlorides and nitrates are adsorbed more than sulphates, but alkali salts with the exception of the alkaline carbonates are not adsorbed. The behaviour of the alkaline carbonates may be explained by the almost invariable presence of calcium ions in clays, which react with the carbonate forming a precipitate of calcium carbonate, and so removing the carbonate ion from solution. Rohland[5] states that some clays which are only moderately plastic may, on the addition of alkali and certain salts, or through some chemical change, be made more adsorptive. The adsorptive power of clay is valuable in some industries, and it is on account of this power that if clay is mixed with neutral or slightly acid muddy solutions or emulsions, when the clay settles it will be found to leave a clear liquid. The adsorption of a clay is usually determined by noting the loss of colour of a dye solution such as malachite green, and comparing it with another similar solution to which a standard clay has been added.

* Many clays retain salts so tenaciously that it is impossible to wash them clean with plain water, but they can be removed by washing with a solution of a salt which is more readily absorbed by the clay.

If Olschewsky's suggestion that the particles of clay are porous is correct, the phenomena ascribed to adsorption may really be due to adsorption within the capillaries or pores.

The " scum " observable on some bricks is due to the salts adsorbed by the clay and carried to the surface during the drying of the bricks.

The *capillary phenomena* shown by many clays and soils may also be explained on the hypothesis that clays are colloidal in character.

The *porosity* of clays varies with the amount of water present, some stiff plastic pastes being quite impervious, though the same materials are porous when dry. This porosity appears to be associated with the capillary structure of many clays and whilst it is a property possessed by non-colloidal substances, it is a characteristic property of some colloids.

The *semi-permeability* of clays, like that of colloids, is a characteristic property, and although its nature is by no means well understood, it appears to confirm the presence of colloidal matter in clays.

When clays are made into semi-permeable " membranes," they behave according to their plasticity. The plastic clays effect a perfect separation between the colloid and crystalloid solutions and are truly semi-permeable, but very lean clays such as china clay are very irregular in their action. In some cases, the presence of a crystalloid may cause a sol to pass through a membrane, as when silicic acid is mixed with sodium chloride both will pass through. It is also stated by W. Ostwald[12] that fresh colloids (particularly silica) will pass through a membrane, but after keeping a few days they will not pass through. There is no connection between the rate of diffusion through the membrane and the molecular weight.

According to Rohland[5], plastic clays will allow ferric chloride and sugar (crystalloids) to diffuse, but not tannin (colloid). In emulsions of oil and water, plastic clays permit the (crystalloid) water to pass, but not the (colloid) oil. In alcoholic solutions of fat, such clays permit the alcohol to pass but not the fat. In aqueous rubber solutions, plastic clays prevent the rubber from diffusing, and in albumen, solutions the albumen is retained, both rubber and albumen being typical colloids. The diffusibility or speed at which the substances dialyse through the membrane depends upon their nature. Thus, water* which is a crystalloid, and electrolytes, *e.g.*, salts dissolved in it, diffuse rapidly, but colloids, such as ferric hydrate, hydrated silica, hydrated alumina, and most products of organic life such as starch, vegetable oils, and gelatin are either indiffusible or pass through with extreme slowness. Colours, on account of their complex composition, play a special part; they are retained by plastic clays, though these colours are crystalloid and not colloid. Berlin blue, potassium ferricyanide, aniline blue, sulphated triphenyl rosaniline, aniline red, carmine, malachite green, fluorescin, aurin, and other animal, vegetable, and tar colours, cannot diffuse through clay, and this, in spite of their crystalloid nature.

* Zschokke[11] suggests that plasticity is possessed by all substances composed of extremely minute particles with sufficient affinity for each other and with a power for combining with water.

The explanation of semi-permeable membranes most widely accepted at the present time is that of selective solubility, suggested by L'Hermite[13]. The membrane is permeable to those substances which dissolve in it but not to others.

As the semi-permeability of clays appears to be connected with the plasticity, any treatment which will increase the latter should increase the former. Rohland[5] has found this to be the case with some lean clays he has examined. Some of the phenomena occur whenever plastic clay is mixed with solutions, as the particles allow the crystalloids in the latter to pass through them, but retain the colloids on their surface. In this way, the adsorption of crystallised matter as well as colloidal matter occurs; but as the particles of clay are so minute the effects are scarcely distinguishable, and clays appear to be capable of absorbing both colloidal and crystalloidal substances.

The *permeability* of raw clays has been studied by Spring, who found that when such clays are confined so that they cannot expand, they will only absorb enough water to fill the pores. The amount absorbed varies from 3 per cent. with some fireclays to 25 per cent. with some sandy loams. When not confined in this manner, the extent to which the water can permeate a clay is dependent on the amount of non-plastic material it contains, and increases when sand or grog is added. The permeability of a fired clay is an important characteristic, and is described later.

The more permeable a clay, the more easily can it be dried and heated without damage, large pores being prefereable to small ones.

Wet clay in the form of a stiff-plastic paste is generally considered to be extremely impermeable, but, as already mentioned, this is only a relative property, as such a mass of clay, if left in water, will, in time, fall to pieces. Clay which has been suspended in water and allowed to settle is usually quite permeable, as are many natural clay deposits. It is only when the material has been " worked " or " pugged " that it becomes impermeable.

The *plasticity*[1] of clays is one of their most important properties. Plasticity may be defined as that property of a material which enables it to change its form without rupture, the new shape being retained when the deformatory force is removed. In other words, a material is said to be plastic when it can be kneaded or pressed into any desired shape, and remains in that shape when the kneading ceases or the pressure is removed; this alteration of shape being capable of being repeated indefinitely. It is a characteristic of many substances besides clays,* though clays possess it to the most marked degree. Ashley[7] has pointed out that very few people agree exactly with the conception of plasticity. Thus, a brickmaker terms a clay plastic when it works well in his machine, and is capable of being kneaded into a " good " paste, but a potter usually places more emphasis on the binding power of the clay, though he terms this its plasticity.

Although these definitions are sufficient for practical purposes, they are not entirely satisfactory, nor is there any explanation of the

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* The " possible plasticity " is that which can be developed under the best known conditions. For many purposes, it is not necessary to develop the plasticity of a clay to the utmost.

causes of plasticity which meets all the needs of the case. Plasticity varies with different samples and on different occasions, though no raw moist clays are entirely devoid of plasticity. Clays which are quite dry are not plastic, but become so when mixed with a suitable proportion of water so as to form a paste. Hence, the amount of plasticity developed is dependent on the proportion of water present.

Liquids other than water may be added to the clay to produce plasticity, but they must usually contain water, and even then, sometimes produce quite different characteristics. Thus, glycerine may be used, but it prevents the clay from drying, and Krupsay has pointed out that if plastic masses made from clay and glycerine and clay and water respectively be kneaded together the resulting mixture is non-plastic. Fatty liquids, such as oils, seem to make a more plastic body than with water, especially if the clay has been dried so as to take away from it the hygroscopic water, but alcohol, ether, and turpentine produce bodies with little or no plasticity.

The nature of the plastic product formed when liquids other than water are used is worth further study. In the case of an oil, the plastic mass is quite different from that produced with a liquid such as anhydrous nitric acid, anhydrous sulphuric acid, absolute alcohol and glycerine. Each of these fluids is soluble in water, and is, therefore, able to wet the hydrated clay grain with its attached water molecule and to separate the grains sufficiently to produce a plastic mass. In each case, the clay may be " dried " again and made plastic with any of the other fluids. According to R. F. MacMichael[15] only those liquids which " wet " the clay particles can produce plasticity. Water and fatty oils do this, but ether, gasoline, kerosene, engine oil, and similar fluids which do not " wet " the clay grains are either unable to penetrate between them and so do not develop plasticity in the clay or they form a film of such a nature around the clay grain as to prevent cohesion, so that the mass acts like sand and water, but there is no gradation or balancing of the forces, as is necessary in order to obtain true plasticity.

Plasticity also depends both on the nature of the fluid and that of the solid. Thus, while both water and oil wet quartz sand, water under suitable conditions will easily displace oil films from a mixture of sand and oil. On the other hand, both oil and water wet zinc oxide, but in this case the oil will readily displace the water films, forming paint or putty. The resulting mass, in this case, may be said to be oleated, in very much the same manner as clay is said to be hydrated. The same principle is employed commercially on a very large scale in the flotation of metal-bearing ores.

The possible plasticity[2] of clay or other substance cannot be developed by commercial methods of grinding unless the material is in a state which may be regarded as dormant plasticity. This has been regarded as an objection to the view that plasticity is due to the colloidal properties of clay, but the objection may be met by the difficulty of reducing some clays to so fine a state as is required to produce the requisite amount of colloidal matter.

Plasticity also varies with the presence of certain other substances; thus, the following soluble substances reduce the plasticity of clay :

ammonia, caustic soda, caustic potash, lime, sodium carbonate, potassium carbonate, borax, and water glass. They appear to do this by coagulating the colloidal portion of the clay, but their action may be prevented by the addition of a sufficient quantity of weak acid to neutralise the alkali in the clay.

The addition of certain organic acids as humus, or of gum, glue and starch confers a pseudo-plasticity on clay which is, however, quite different from true plasticity and makes the clay "sticky" rather than plastic.

The *stickiness* of certain clays (*e.g.*, London clay) is very pronounced, but must not be confused with true plasticity. Ashley[7] has stated that if the granular constituent is removed from a plastic body it loses plasticity and becomes sticky until the granular constituent is restored. This suggests that the practice of adding granular material of a non-plastic nature, so common among the users of London clay is based upon a sound principle. The stickiness of clay may be regarded as due to colloidal material which is not properly distributed throughout the inert granular mass.

Plasticity does not appear to be connected with the chemical composition, as clays which yield the same results on analysis may differ widely in plasticity, yet on heating above 415°–600° C. all clays lose their plasticity, and it cannot be restored. It is also a curious fact that the clays which are richest in " true clay " are seldom so plastic as those which are not so pure, so that any peculiar structure of the clay molecule can scarcely account for its plasticity, though several eminent investigators have laid stress on this suggested cause.

Several investigators have attributed the plasticity to the shape or size of the clay particles. Thus, Aron considered plasticity was due to the particles being spherical, but Zschokke, Biedermann, and Herzfeld dispute this, and attribute it to the presence of flat and laminated crystals,* a view early put forward by Johnson and Blake, and held later by Bourry[16], who stated that plasticity becomes greater in proportion as the grains diminish, and that all minerals if reduced to a sufficiently impalpable powder, will on the addition of a liquid produce bodies having a certain amount of plasticity.

According to Le Chatelier, the lamellar structure and the well-known capillary attraction are a sufficient cause of plasticity. He has shown that all plastic masses contain a large proportion of air by comparing their density with that of clay and water, and that in each plastic mass there are innumerable capillaries of not more than one three-thousandth of an inch in diameter. He concludes that the tension of the menisci between the water-surface and the air-surface in these capillaries explains the toughness of the plastic mass, as the capillary force prevents the mass from breaking up under pressure, but allows the minute particles to slip over each other, and yet adhere so strongly that the mass retains the new form when the pressure is removed. In other words, clay is plastic when sufficient water is

* The particles are so extremely minute that it is exceedingly difficult to ascertain their shape. Le Chatelier has noticed that if the material is disturbed when under the microscope, the crystalline form may be observed for a fraction of a second by polarised light if their symmetrical axis is perpendicular to the microscope axis. As soon as they are flat they are isotropic.

added to induce the cohesion to a point where it can readily be overcome by the pressure of the worker's hands, *i.e.*, to 1–3 lb. per square inch, so that it is a balancing of forces producing a peculiar combination of fluidity and rigidity in the mass of wet clay; under a light pressure it acts as a rigid body, under a heavier pressure, it acts as an imperfect fluid. The rigidity is attributed to friction between the clay grains, so that a mass of clay retains its form until acted on by a force sufficient to overcome this friction and produce distortion. The fluidity of the wet clay is due to the freedom of the individual particles to move over each other, after cohesion has been partially neutralised by the addition of water.

The theory that plasticity, instead of being a special property, is simply the result of molecular attraction, and that all bodies which are made up of laminated particles must become plastic when they are reduced to sufficiently impalpable powder has been confirmed by Vogt as regards mica, which is highly laminated, being made up of thin layers, and when reduced to an impalpable powder becomes distinctly plastic if water is added. The insistence laid by Bourry[16] on the laminated structure of the particles has been frequently overlooked, and the suggestion that, because burned clay may be ground equally fine and yet never become plastic his experiments are not conclusive, is irrelevent.

Seger[1], and independently Schumacher, consider plasticity to be due to molecular differences in the clay particles, and Bischof agrees with the latter in considering that clay has undergone great changes in density during deposition, and a kind of " felting " of the particles has resulted so that they adhere much more closely to each other than the quartz and other particles in which this felting process has not taken place.

Wolff has calculated the attraction of the particles of various substances to each other on the assumption that they are spherical. He finds that the mutual attraction of the clay particles is very high and that the ratio between their mutual attraction for each other and for water is much higher than for any other substances examined. He stated in confirmation of this theory that other substances can be made plastic, if they can be made sufficiently small, as by precipitation.* He also pointed out that the combined water in a clay particle increases the ratio considerably and is accompanied by an increase in plasticity not only in the clay, but in alumina and iron oxides. Zschokke confirmed this theory, and has shown that clay particles have a thicker film of water around them than particles of non-plastic materials such as sand.

It is extremely difficult to find satisfactory reasons for attributing the plasticity solely to the plate-like or lamellar structure of the particles or to purely mechanical or chemical characteristics in the atoms and molecules of the clay and water, though these are undoubtedly important. Nor has the effort of Le Chatelier to find the source of plasticity in the presence of small amounts of impurities proved really helpful. The smallness and shape of the particles appear to be important, as clay ground in a pan-mill is more plastic than when a ball-mill is used, as the former flattens out the material,

but this does not really affect the cause of plasticity. Grinding is not a cause of plasticity, though Johnson and Blake claim to have made a non-plastic china clay plastic by fine grinding.

It has been suggested by Olschewsky, who based his experiments on those of Daubée, that the water used has a chemical action, and that plasticity is due to the formation of a system of capillaries in the clay, a felt-like or spongy material being formed, and in this way, the clay particles are able to come into closer contact, owing to the production of a kind of gelatinous or colloidal film, but the presence of an alkali appears to be essential for this alteration to take place. Thus, Mellor found ground pottery, felspar, and Cornish stone become plastic on heating with water under pressure to a temperature of 300° C. for several days, but china clay and flint are scarcely affected. The finer a substance is ground the more complete is its reaction with water, because a small particle has a greater surface in proportion to the water than a coarse one. If the particles are sufficiently fine, water may, indeed, act in a similar manner to a caustic alkali; thus, very finely divided silica becomes colloidal when brought into contact with boiling water, just as coarser particles do when brought into contact with a boiling solution of caustic potash.

Koerner found that other substances (as alumina) become sufficiently finely divided in water, but their power of cohesion is lost on drying, and suggested that the plasticity may be brought about in a similar manner. This would explain why it is impossible to produce highly plastic clays from kaolin.

As many organic substances possess certain characteristics of plasticity, several suggestions have been made that these may be the cause of plasticity in clay. It is found, however, that there is no definite relation between the plasticity and the proportion of carbon in the clay, dark coloured clays, rich in carbonaceous matter, being no more plastic than lighter ones almost free from this material.

Several observers have suggested that bacteria produce plasticity, but Hecht and Gosmann have not found sufficient data to warrant this suggestion, especially as it has not been found possible to increase the plasticity of clay by inoculation.

Whenever plastic clay is subjected to pressure it tends to obey the laws of fluids, transmitting its pressure to all parts of its mass and flowing through an orifice through which it can escape, though it is far from being a perfect fluid. From this arises the modern conception of clay as a very viscous liquid in which every particle of solid matter is surrounded by a film of liquid, so that the particles are virtually in a state of suspension, and hence, that a plastic clay is, at any rate in part, in a colloidal condition.

As far back as 1872 Schloesing[1] suggested that the plasticity of clay was due to its colloidal nature, and claimed to have found an amorphous material of the same composition as kaolin which had all the characteristics of a colloid, and was termed by him *argile colloidale*. Very little notice was taken of this suggestion or of the

* This has more recently been confirmed by Cohn and Atterburg, who found that precipitated barium sulphate and calcium fluoride are both plastic when fresh.

allied work of other observers until 1896, when Rohland[6] investigated the subject further, and found indications that the colloidal nature of clay appeared likely to explain many of the facts noted in regard to plasticity.

The nature of the colloid material apparently existing in many clays has already been described. In attempting to explain plasticity as being due to these colloids, it is assumed that some or all of the pores of the clay are filled with a colloidal solution (gel) obtained by the partial hydrolysis of the clay, and that the larger the proportion of pores so filled, the fatter and more plastic will be the clay, provided the proper ratio of granular material to colloid gel is retained.

Rohland[5] and others have further shown that the addition of trifling amounts of electrolytes often produces great changes in the plasticity of a clay, and suggest that this characteristic of colloids is a strong argument in favour of the connection between the colloidal material in clay and plasticity. All electrolytes (such as acids) which yield hydrogen-ions on dissociation, increase the plasticity of clay, whilst those (such as alkalies) which yield hydroxyl-ions make a clay more fluid.

Plasticity is not, however, entirely due to the presence of colloidal matter in clays, though the effect of colloids in increasing plasticity cannot be denied. Hermann and others maintain that the presence of inorganic colloids in clay has never been conclusively proved. It should be noted that clay may be suspended in water and then precipitated or deflocculated indefinitely without impairing its plasticity. This is not usually the case with true mineral colloids, which usually set irreversibly and do not return to the colloidal condition. Moreover, the whole of any individual clay grain is not softened upon the addition of water. Repeated wetting and pugging does not materially alter the size of the grains or change their general outline or appearance. This would not be the case if the clay were softened and reduced to a homogeneous mass, wetted, and subsequently broken up with the formation of new grains when it was dried and ground. Whether wet or dry, under the microscope, the grains retain the appearance of a sharply-defined body.

Another difficulty has been pointed out by J. M. van Bemmelen, viz., the rapidity with which colloids lose their power of absorbing water. This suggests that clays of great geological age cannot contain active colloids produced when the clay was formed, though they may contain colloidal substances derived from adventitious materials—organic or otherwise—at a comparatively recent period. The fact that many highly plastic clays appear to be free from such extraneous colloids only increases the difficulty regarding the latter as the cause of plasticity. Other objections of equal or greater weight may be urged against any single theory yet published on the causes of plasticity so that much further work requires to be done.

Summarising the results of the numerous theories and experiments made, plasticity may be said to be due not to one, but to several causes, the chief of which are :—

(i) The nature of the molecules of " true clay " present.

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(ii) The extremely small size of the particles, their lamellar shape, large surface (due to their porosity), and (possibly) their fissile character. In such small particles, the phenomena of cohesion are quite different from those in larger particles.

(iii) The hydrolysing action of water on the particles and the probable production of inorganic colloid matter. If this is absent or neutralized by hydrogen-ions added purposely or occurring naturally or through fermentation of the organic matter in the clay, the plasticity will continue to increase until an excess of hydroxyl-ions is again produced; when the concentrations of hydroxyl-ions is large, the negatively charged clay particles will go into suspension. As the extent to which water can be dissociated is very limited, the plasticity of the clay can only be increased at so slow a rate that it is unlikely that slightly plastic clays (kaolin) can ever be made highly plastic by artificial means, though the increase in plasticity may be sufficient to show the nature of the reactions which take place.

(iv) The presence of organic colloid matter due to impurities in the clay, or added purposely, may still further increase the plasticity.

(v) The presence of minute quantities of soluble salts may exercise a pronounced effect on the plasticity. Their action has been mentioned under *Viscosity* (p. 121). Plasticity appears to be a resultant of several properties (see also *Cohesion, Adsorption, Tensile Strength, Binding Power*, &c.).

To increase plasticity.—The limits within which the plasticity of clay may be increased by the addition of soluble salts are very small, but there is such an abundance of naturally plastic clays that it is only where materials of exceptional purity are required that an increase in plasticity is desirable.

A small increase in plasticity may be obtained :—

(1) By increasing the hydrogen-ions in the material, by allowing the organic matter in the clay to decompose (ferment) and become acid, by adding weak acid, or by keeping the clay in intimate contact with fresh water by stirring the two together. This appears to hydrolyse the clay and forms colloid matter on the particles. It is important to have the particles of clay as small as possible in order to facilitate the hydrolysis. If water alone is used for this purpose, the clay must be allowed to stand until fermentation of the organic matter begins, and the mass reacts faintly acid. In any case, the time required for an appreciable increase in the plasticity may be several years. No addition of any electrolytes or substance other than plastic clay can increase the true plasticity of a paste chiefly composed of non-plastic materials. Many of the so-called " lean clays " are of this nature; they are rich in inert matter, but the proportion of colloidal matter in them is very small. Such clays can only be made more plastic by removing a large proportion of the inert matter naturally present in them or by

the addition of a highly plastic clay. The addition of electrolytes to such clays is only of value when their low plasticity is due to the clay gel present having become hardened or coagulated, but is still capable of being revived or deflocculated by means of an electrolytes or other simple treatment.

(2) By keeping the clay in a moist damp cellar. This is termed " ageing " or " souring."

(3) By the addition of colloids such as colloidal silica, alumina, or iron hydrate, hot starch, dextrin, tannin, rubber, sumach, inulin, caramel, gelatin, gum, glycogen, or various ferments and enzymes, the plasticity of the clay may be increased, but care must be taken to avoid confusion between true polasticity and the pseudo-plasticity caused by the addition of materials of an oily, gelatinous, or gummy nature.

Some very interesting experiments by Acheson and Ries on the effect of a 2 per cent. solution of tannin (gallotannic acid) on clay show that the addition of this substance notably increases the plasticity of clay, and at the same time apparently deflocculates it and breaks it up into finer particles. The tensile strength of the clay was nearly doubled.

In a later patent, Acheson first adds tannin and alkalies or ammonia and stirs the clay into a fluid state, and then by the addition of a suitable quantity of acid he coagulates the colloids and forms a stiff paste.

(4) By reducing a sufficient number of particles to so minute a state that they assume colloidal properties in the presence of water. Thus, by very prolonged grinding with water many hard clays[1] become appreciably more plastic.

The softer materials become exceedingly smooth and plastic; the harder ones yield less readily to the treatment, but still develop marked pasticity, very similar to that of normal clay heavily overloaded with sand or grog.

By selecting the materials and method of grinding, many degrees of plasticity may be obtained, from that of a very smooth plastic clay to that of a very short sandy clay, indicating that the difference is one of degree and not of kind, the essential characteristics being that the clay or other materials shall occur in a state of very fine subdivision, and that their surfaces are readily wetted by water. The chief practical difficulty lies in grinding sufficiently fine, as the smallest particle that can be seen under the microscope does not by any means represent the limit towards which the grinding should proceed. The plasticity produced by artificial grinding depends on the size and shape of the particles, and only indirectly on the material of which the plastic mass is formed.

To reduce plasticity.—(1) Hydroxyl-ions may be added and the temperature raised (direct reduction). (2) Non-plastic material may be added so as to spread the plasticity over a larger volume of material

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[1] Plastic material has been formed in this way from slate, plaster moulds, iron ore, ashes, lava, limestone, sandstone, burned brick, silica, mica, felspar, and even glass.

(indirect reduction or dilution). (3) The material may be heated to 200° C. or other suitable temperature.

For the first method, any basic material, either organic or inorganic, may be used though lime water is the cheapest. If lime is too weak in hydroxyl-ions, caustic soda may be used, as may any salt composed of a strong base and a weak acid, such as sodium (or potassium) phosphates or silicates, all of which readily hydrolyse and yield hydroxyl-ions, though the cation constituent of the salt may exercise a considerable effect. Thus, borax reduces the influence of the hydroxyl-ions and potassium carbonate increases it, yet both are salts composed of a strong base and a weak acid. The concentration of the alkaline or basic material added is also of importance, and it may be necessary to render sulphates and other soluble salts insoluble by the addition of baryta, as suggested by Weber. Certain clays, as Weber has shown, act in precisely the reverse manner. These are free from sulphates, and appear to be rich in colloidal matter.

Certain clays containing organic acids of a fatty nature are saponified on treatment with alkali, and the soap so produced increases, instead of diminishing, the plasticity, owing to the coagulation effected.

The reduction of plasticity by raising the temperature considerably is described later. A comparatively small rise in temperature produced by the action of mechanical stirrers—will reduce the plasticity of clay if free hydroxyl-ions are present.

The addition of non-plastic material, such as sand or grog, effects a reduction of the plasticity in an entirely different manner, by separating the clay particles from each other. It thus reduces the strength of the material, but by diminishing the shrinkage, it enables the clay to be used in a manner which would, otherwise, have been impossible, and the strength is seldom reduced sufficiently to make any notable difference to the user of the material. The proportion of non-plastic material to be added depends on the size of its grains and on the binding power of the clay. As the latter is closely connected with its plasticity, it will usually be found that the more plastic the clay, the larger the proportion of non-plastic material which may be used.

Some sands are quite useless for this purpose, so that great care is needed in their selection. For some clays, chalk, flints, or grog is preferable to sand.

The *measurement of plasticity* is a problem which has not yet been satisfactorily solved, probably for the reason that plasticity is the result of the united action of several forces some of which may not, as yet, have been recognised as important. Early attempts to measure plasticity usually resulted in only measuring one or more of these forces. Thus, Bischof added sand until the mixture was so soft that it could be rubbed away between his finger and thumb. Bischof's figures are, however, a measure of the binding power of the clay, but not of its plasticity. Measurements of tensile strength, viscosity, the amount of water required to produce a mass of given consistency, the consistency, or the depth to which a Vicat needle will penetrate, Sokoloff's slaking test and other single characteristics are all useful in their way, but they fail to include all the properties involved in the

use of the term " plasticity." Zschokke, who has examined the subject very fully, considers that the percentage of extensibility multiplied by the tensile strength of a freshly moulded clay cylinder of standard size (60 mm. high by 30 mm. diameter) is a coefficient of the plasticity. Modifications of this method have given excellent results in the hands of several experimenters in different countries and with a very large variety of clays. Grout considers that plasticity is proportional to the product of (a) the load required to sink a Vacat needle to a definite depth in a mass of clay; and (b) the deformation of the clay under stress, which he measures by the increase in area of a clay cylinder produced by a load which just causes cracks to appear. Both Zschokke and Grout[17] really consider plasticity to be measured by the product of the deformability and force resisting deformation, though they differ in the manner in which they measure these forces. More recently, Ashley[7] has adopted the same general idea as to the forces involved, but has assumed that the force-resisting deformation is exerted by the colloids in the clay. He, therefore, regards the plasticity of clay to be measured by the ratio :—

$$\frac{\text{Relative colloids} \times \text{the shrinkage of the clay}}{\text{Jackson-Purdy surface factor.}}$$

The term " relative colloids " is explained in the section on *Adsorption*.

As the ratio of the surface factor to shrinkage is approximately constant, Ashley concludes that the plasticity of the clay is directly proportional to the colloids present. The objection to this conclusion is that it appears unlikely, from other considerations, that the whole of the plasticity is due to the colloidal matter.

Rohland[5], also assuming that the colloidal matter in the clay is the chief factor of the plasticity, has suggested that the ratio obtained by dividing the coagulable colloids by the non-coagulable material is a measure of the plasticity. He ascertains it is by measuring the amount of water required to make the clay into the consistency of a good modelling paste, and argues that this is a measure of the colloids because as soon as sufficient water is present to dissolve the coagulable colloids, a saturation point is reached and no more water can be absorbed without the clay losing its stiffness.

Stormer has stated that plasticity may be judged by the following characteristics :—

(1) The proportion of water (absorption) which must be added to the clay to make a good modelling paste. This is not always reliable.

(2) The " feel " of the paste when rubbed between the finger and thumb (*binding power*).

(3) The behaviour of the paste when rolled up into a " sausage " (*toughness*).

(4) The adhesiveness of the clay (*adhesion*).

(5) Twisting a piece of clay into a spiral and noting its behaviour (*torsion*).

(6) Noting the length of the threads, produced by expressing the clay from a vertical pug mill, before they break off by their own weight (*tensile strength* and *extensibility*).

(7) Forming balls of clay and pressing them until the edges crack (*crushing strength*).

(8) Bending cylinders of clay into a ring (*bending moment*).

None of these characteristics taken alone can give a measure of the plasticity of a clay, though several of them are closely related to each other. The most reliable measure of plasticity appears to be that devised by Zschokke (p. 133) or by Rosenow, who multiplies Zschokke's figure by the percentage of water added to the dry clay to make it into a workable paste, *i.e.*, by Rohland's figure (p. 133).

The *binding power* of a clay is the property it possesses of uniting with non-plastic material and water to form a uniform plastic paste, and is consequently closely related to the plasticity. This absorption of non-plastic material with the spread of plasticity throughout the whole mass has been attributed to the power of the saturated colloids (gels) to retain the non-colloidal particles in a state of pseudo-solution. Other colloids are known to possess the property of preventing insoluble matter from settling, and this is, in some senses, a parallel case. The binding power of a clay may be determined by measuring the tensile strength of mixtures of clay with varying amounts of sand, but a skilled clayworker can tell by the " feel " whether such mixtures are strong enough to be useful. In order to determine how much lean clay or non-plastic material can be added to a clay without unduly destroying its value for moulding into shape, Bischof's test may be used. In this, the two materials are mixed in various proportions and the same measured quantity of water is added to each. The pastes are then rolled into small balls as equal in size as possible, and allowed to dry. They are then rubbed gently between the finger and thumb, or with a small " camel hair " brush. The mixture which just resists the action of rubbing may be taken as the standard. Some authorities make up balls of mixture in this way and then notice to what length a cylinder can be rolled from each without cracking.

Clays with a high binding power are known technically as " fat " clays ; " lean " clays are deficient in binding power.

Some writers appear to consider that binding power and plasticity are synonymous ; this is by no means the case, as a clay may be very plastic and yet not be able to bind much non-plastic material into a uniform plastic paste. At the same time, there is clearly some relationship between these two properties of clays.

The *dehydration of clays* is accompanied by changes which are remarkably similar to those which occur in the dehydration of colloidal gels. The most important of these changes is the shrinkage or contraction of the mass, the production of a hard material which— if the dehydration is accomplished by heat—may result in the production of a material comparable to an irreversible gel.

Plastic clays, like colloidal gels, shrink greatly when dehydrated and possess both a drying-shrinkage and a kiln-shrinkage. By mixing

an inert substance, such as sand, with a true colloid the shrinkage is lessened, and the cohesion of the dried colloid, including its adhesion to inert substances, are the causes of the increased mechanical strength of many such mixtures. This is another characteristic common to colloids and to all plastic clays.

As there is no wholly reliable method of measuring plasticity (*see* p. 124), it is not possible to state precisely what relationship exists between the plasticity and the shrinkage of clays. Speaking broadly, the most plastic clays shrink more than those which are less plastic, but this is not invariably the case. For instance, the Lias clays usually shrink less than would be expected from their plasticity.

When articles made of plastic clay are dried under suitable conditions, they contract equally in all directions, the contraction in volume being almost three times the linear shrinkage. Excessively plastic clays crack, or twist, when dried and many moderately plastic clays will do so if dried irregularly or too rapidly.

When water is added to a dry clay, it is first absorbed by the pores, but, when these are filled, any further supply of water appears to cause a separation of the particles from each other so that the volume of clay is increased, though not in proportion to the water added. The amount of water which can be absorbed in this manner differs greatly with different clays. The stage at which the clay contains the maximum quantity of water without loss of shape is also the point of maximum plasticity; it is said to be the "point of saturation of the coagulated colloids (gels) in the clay." If some of this water is removed, the volume of the mass begins to diminish and contraction occurs. This contraction or shrinkage is chiefly, but not entirely, due to the removal of water from the clay by evaporation at the ordinary temperature (air-shrinkage), at a somewhat higher temperature in the dryer (dryer-shrinkage), or during the burning (kiln-shrinkage).

As all coagulated colloids (gels) which are saturated with water shrink when the water is removed, some investigators consider that the shrinkage of clay may be due in part to this cause.

The more general idea (which states facts rather than explains them) is that, as the water is removed, any which remains draws the clay particles together into a smaller and denser mass.

The amount of shrinkage appears to depend partly upon the rate at which the clay is dried, for if this operation is performed rapidly the shrinkage will be less, the clay particles not having time to move over each other so freely as when the drying is slower. When drying a strong, porous clay, the water first evaporates from the surface and is replaced by capillary action from the interior, the mass contracting by the same amount as the water diminishes. All the pores remain filled with water until the rate of evaporation exceeds the rate at which the pores will transmit water. This point occurs, when the clay particles move so much less freely on each other that the rate of evaporation exceeds that of the contraction. After the first stage of surface-drying, the exterior loses water more rapidly than the interior; in the second stage the pores are no longer filled with

water at their outer ends and begin to form spaces in the clay, these spaces being filled with air and water vapour. Contraction still occurs throughout this second stage until the substance is so far solidified that the individual particles can no longer slip over each other at all. The third stage is then reached in which capillary action and shrinkage cease entirely. Evaporation now takes place entirely within the mass, and spaces are formed exactly corresponding to the water lost. That shrinkage ceases before the clay is completely deprived of water is shown by Aron and Brogniart to be characteristic of many, but not of all, clays. Aron supposed that the clay shrinks until the particles are practically in contact with each other, so that any further water which may be driven off does not make any notable difference in the volume of the clay; but supporters of the colloid theory argue that the heat used in drying really cause the colloid particles to shrivel, thus reducing their surface and increasing their density. Aron has further shown that the " pore space " is constant for each kind of clay, and is independent of the amount of water of formation added to the clay, though this last statement is only true of the purer clays.

If, now, the pastes made with varying amounts of water of formation are subjected to exactly the same conditions of drying, the rate is not proportional to the water added, but is slower in proportion for those with less water. It takes, approximately, the proportional time in the first two stages of drying, but the more solid the mass the longer it takes to eliminate the last portions of the water. It follows also from this, that want of uniformity in the substance of a mass of clay, such as must exist in bricks made by hand, and in a less degree in those made in a press or die, causes a corresponding want of uniformity in the shrinkage and the rate of drying in different parts of it. This is one cause of the warping or twisting of bricks in drying.

In the second stage of drying, all clays lose water more rapidly on the outside than on the inside, the angles and arrises in their turn drying more rapidly than the faces. The consequence of the greater shrinkage of the outer layer is a frequent cause of cracking, and it is, therefore, necessary to pursue this stage with great caution and to effect the drying with air already heavily charged with moisture. It is also essential, for this reason, to avoid the excessive prominence of any part of a complex-shaped article, and it is advisable to follow any projections on the exterior with hollows on the interior, so as to maintain an approximately regular thickness of material throughout. The frogs or indents on both sides of a common brick are serviceable in drying for the same reason. In a re-pressed brick, they serve the additional purpose of rendering the consistency more even throughout.

For objects of reasonable size, the rate of drying is approximately proportional to the ratio of surface to volume. Objects of large size, however, take much longer to dry, and require the application of considerable heat to complete the removal of all the water of manufacture from the interior. Many large goods made of fireclay and stoneware clay require extremely careful treatment, and have to be kept in a heated atmosphere for several days after the moisture

has apparently been completely removed. Disastrous results have frequently been known to occur in the steaming operations in the kiln for want of sufficient care in this particular. Manufacturers frequently adopt a very wise precaution in having such goods stamped with the date of making, and in holding their workmen responsible if they are rendered unsound by being burned before the lapse of a stated period of drying.

In order that the goods may not twist or warp when drying, it is essential that they should shrink very little. This means that only a limited proportion of plastic clay can be used in the material, although some is necessary to bind the particles together and to give it the general characteristics of "clay." With a carefully compounded mixture, the contraction of the paste prior to entering the kiln should not exceed 1 inch in 16 (or $\frac{3}{4}$ inch per foot). If it does so, more non-plastic material must be added. The lower the contraction, the better the chance of the ware coming "true" out of the drying rooms; hence, as much non-shrinking material as possible should be used in the clay mixtures, so as to keep the contraction at a minimum. The addition of a non-plastic material to a clay enables less water of formation to be used, and so reduces the shrinkage, but Aron has shown that if the amount of water is kept the same as for the clay alone, the addition of non-plastic material will increase the contraction which occurs on drying, until a certain point (that of maximum density) is reached. After this, the more " grog " added to the clay the less will it shrink, and the greater will be the porosity. The nature of the non-plastic material added will also affect the shrinkage to some extent, and will exercise a considerable influence on the amount of water which must be mixed with the clay. Thus, a porous, burned clay will absorb more water than will sand. Provided the non-plastic material is of a nature suitable to the clay (this must be determined by actual experiment), it may be added in any desired proportion so long as it does not too seriously reduce the strength of the mass, as it will do if more is added than the binding power of the clay can accommodate.

The porosity of a dehydrated clay appears to be due to the capillary structure of the material.

Other properties which clays in the plastic state possess in common with colloids are :—

Unctuousness, or a smooth, almost greasy, "feel," is a characteristic of some clays, a few being so oleaginous that they may be saponified by treatment with caustic alkali, the plasticity being thereby increased. In most cases, however, such treatment makes the clay more fluid.

Toughness, or cohesion, is closely allied with (1) *extensibility*, or the ability of clay to stretch when pulled, which is measured by ascertaining the fullest extent to which a clay test-piece of a given size will stretch without breaking; (2) *torsion*, or the extent to which a piece of clay can be twisted, which is measured by clamping one end of a bar of clay as rigidly as possible and rotating the other slowly by means of a screw, counting the number of complete revolutions which can be made before the bar breaks; (3) *bending moment*, or

the angle through which a bar of clay can be bent without rupture; (4) *elasticity*, or the extent to which a piece of clay can be stretched and yet return to its original length when the tension is removed. Many plastic clays show slight elasticity, though it is usually too small to be measurable.

The *tensile strength* of a clay is its resistance to torsion or to being pulled apart. The non-plastic materials influence its strength inversely as the diameter of their grains, so that fine-grained clays will usually be the strongest, though an excess of very fine or very coarse grains will cause the clay to break prematurely. In support of the theory that the grains of clay interlock to some extent, Ries found that mixtures of two clays can be made which have a higher tensile strength than either clay taken separately. This fact has long been known by the makers of crucibles for steel-melting in this country, as many as four different clays being sometimes used to produce a sufficiently strong crucible. The tensile strength of the clay has, in fact, an important bearing on its resistance to accidents in the process of manufacture, particularly from the commencement of drying to that of firing. It has sometimes been stated that the tensile strength of a clay enables it to carry a large quantity of non-plastic material, but this is rather confusing the effect with the cause. It is the binding power of the clay which enables it to carry such a large quantity of added material and still retain a sufficiently high tensile strength. Olschewsky has proved that there is no direct relationship between the binding power of a clay and its tensile strength when dry. It was at one time thought that the tensile strength of clays is proportional to the plasticity, but this is only true, if at all, when the pieces are tested in the moist (plastic) state. If air-dried, the definite relationship ceases.

The tensile strength of dried raw clays depends on the proportions of the grains of different sizes. Equal-sized grains cannot be packed into a dense mass. An excessive proportion of the finest clay particles or a large percentage of sand grains (0·5–1·0mm.) weakens the strength of an air-dried clay.

Fissility—or capability of being split up into thin slabs or flat pieces, or even into flakes or foliations—is characteristic of many indurated clays, especially of shales. If the splitting can be effected so as to form plates of extreme thinness, the material is said to be *laminated*; if the tendency to split is strongest in the direction of bedding, the material is termed *shaley*; if this tendency is strongly marked in any other direction it is said to be *fissile*, as are slates and certain limestones and sandstones.

Sectility, or capability of being easily cut, is a characteristic of clays which occur in a plastic condition, such as ball clays and many surface clays. This property often serves as a means of distinguishing " clays " from other minerals, though the " clays " so found may be too impure to be of any commercial value. Anyone constantly engaged in examining clays soon learns to recognise some varieties by their sectility and by the slightly glossy appearance of the freshly-cut surfaces, though these cannot be clearly described.

The effects of age on a clay paste are similar to those on colloidal gels, provided the conditions of storage (including the low temperature and a sufficiently humid atmosphere) are favourable. The plasticity of the clay is slightly increased and the colloidal properties are more marked.

From the foregoing, there appears to be a close parallelism between the more important properties of plastic clays and those of other colloids, but the question still remains as to whether these colloidal properties are due to the nature of an essential constituent of clays (clay substance) or to other colloidal substances which may be present.

Other Colloids in Clay.

The most important colloidal substances which are known definitely to exist in some clays are :—

Colloidal silica, which may exist in the form of a silica hydrogel, with or without occluded silica hydrosol, the latter being confined to any liquid portions of the clay paste, but distributed more or less uniformly throughout the clay slip. Various mineral forms of silica which are hydrogels are known; they possess the anticipated properties of inorganic gels and their nature is fairly well known. A small percentage of colloidal silica may be extracted by boiling some clays with water, with or without the addition of a little sodium carbonate, but in no clay of commercial importance is the amount of silica obtainable so large as to account for the whole of the plasticity of such clays, though it may partly do so.

Wolfgang Ostwald[19] holds the view that silicic acid sols are hydrated *emulsoids*, *i.e.*, the dispersed silicic acid is a liquid and not a solid. Like other known emulsoids, its viscosity is high and rises very rapidly after a given concentration whilst in suspensoid sols the increase of viscosity is steady throughout and the viscosity is very low. He also states that the silicic acid gel differs from the better known organic emulsoids in possessing little elasticity.

Attempts to increase the plasticity of sand by mixing it with silica hydrosol, and coagulating the latter, do not produce a material at all closely resembling plastic clay. Such a mixture, when dry, is deficient in strength and even in its most plastic state it is inferior to clay for modelling purposes. A still more striking difference between such a mixture and a clay is that when both are dried at 105° C. and afterwards mixed with water, the clay forms a plastic paste with all its original modelling power restored, but the silica-sand mixture is not " workable " or truly plastic.

Silica gels have a peculiar property of varying according to their age as well as according to the mode of formation. When a clay is soured, the proportion of silica gel tends to increase, but if soured too long the silica gels grow together forming larger particles and cause a diminution in the plasticity of the mass, the vapour pressure in the old gel being greater than in the new one.

A. Cushmann[18] found that the addition of dried colloidal silica gel to a dry clay, the mixture being afterwards made into a paste

with water, increased in binding power and shrinkage, but not in plasticity, whilst colloidal alumina, when similarly treated, increased in plasticity but not in binding power and shrinkage. A mixture of colloidal silica and alumina, made by adding a solution of water-glass to one of alum, increased both the binding power and the plasticity of a clay. Grout repeated this experiment, but obtained a negative result.

On drying the silica gel, the volume decreases to a characteristic point (Van Bemmelen's[19] " transition point "), after which it remains constant. On further dehydration, the clear gel becomes turbid and eventually chalk white, but becomes clear again when the water content is reduced to less than one molecule. The addition of water causes little change in volume. At low red heat the gel is completely dehydrated and strong ignition prevents the gel from again taking up water. Any salts present cause the gel to lose its power of taking up water more rapidly. Salts are also favourable to local fusion and destruction of the gel. These characteristics are equally characteristic of plastic clays.

Colloidal alumina, of which a small and variable proportion occurs in many clays, especially those known as *laterites*. Colloidal alumina has a very curious and variable effect on the clay. In some cases, it behaves as an acid easily combining with bases to form salts, whilst under other conditions it acts as a base combing with acids. It is soluble in both acids and alkalies forming different compounds in each case. The gel has a particularly low water content, having only about $2\frac{1}{2}$ per cent. more than is required to form the hydrate, a fact which is very unusual, as most mineral gels have a very large proportion of free water which can be removed on drying. In most of its physical properties, such as shrinkage, swelling power, &c., it is similar to silica.

Mixtures of sand and alumina hydrosol, treated so as to coagulate the latter are somewhat plastic, but would not be acceptable to clayworkers as a substitute for plastic clay.

Colloidal Ferric Hydroxide has been isolated in very small quantities from some ferruginous clays, but clearly cannot be an important cause of plasticity in clays which are almost free from iron compounds. Like silica, ferric hydroxide sols increase in viscosity with the concentration. The surface tension of ferric hydroxide sol is the same as that of water and the electric conductivity was observed by Malfitano to be 200×10^{-6}.

Colloidal ferric hydroxide particles carry a positive charge, *i.e.,* that opposite to that of colloidal silica, so that the two substances mutually precipitate each other. This accounts for some siliceous minerals and clays having a thin coating of iron which also partially fills the interstices. A very peculiar fact with regard to colloidal ferric hydroxide is that it is entirely free from the inky taste which is so characteristic of ordinary iron compounds, and it has no reaction with potassium ferrocyanide (the Prussian blue test which is the most characteristic test for iron), whereas yellowish china clays when treated with ferrocyanide become whiter owing to the Prussian blue

colour being complementary to the yellow tinge caused by iron compounds in the clay. The proportion of water present in colloidal ferric hydroxide is very variable; it parts with water on drying in a similar manner to silica though it is much more irregular. The variable proportion of water probably explains the variable colour to different clays containing iron compounds.

Various *colloidal silicates* have been found in small quantities in some clays, though absent in many highly plastic clays. The only exception to this is a possible " silicate of alumina " or more correctly " alumino-silicic acid " (or series of such acids), which appears to be an essential constituent of clays and may be the origin of the colloidal substance to which they are supposed to owe their value.

Colloidal organic matter, chiefly humus, may play an important part in giving to clays their characteristic properties, but as some well-known highly plastic clays are almost devoid of carbonaceous matter, the latter cannot be the chief cause of their plasticity. Moreover, the addition of certain organic colloids to feebly plastic clays does not increase their true plasticity, though it may increase the cohesion and stickiness of the particles.

From the foregoing it may be assumed that the characteristic properties of clays are not due to colloidal silica, alumina, ferric hydroxide or organic matter, or to colloidal silicates of the alkalies or alkaline earth metals, though when any or all of these are present they may slightly increase the plasticity, or otherwise modify the properties of the clay.

To what Colloidal Matter do Clays owe their Character ?

It has been suggested in the foregoing pages that the more important properties of clays may be due to the presence of colloidal matter in them. It has been shown that colloidal matter does exist in clays and that many of the characteristic properties of clays are equally characteristic of colloidal gels, though none of the latter possess all the properties of a valuable plastic clay. The question therefore arises, as to whether the so-called colloidal properties of clays are merely coincidental with the composition of clays, or whether plastic clays contain some substance or substances not hitherto identified as a colloidal gel.

There can be no doubt that most plastic clays contain a large proportion of non-plastic and non-clayey material and may be regarded as diluted clays. Some of the most highly plastic clays, on the contrary, consist of such small particles that the non-clayey matter cannot be satisfactorily separated. On the one hand, attempts to separate an ideal " clay substance " by chemical or mechanical methods have resulted in a material which is almost devoid of plasticity, and on the other, attempts to show that clays are essentially colloidal have not satisfactorily produced any definite colloidal substance which can be regarded as clay. We are, therefore, compelled to realise that many of the more important properties of clays are due to the colloidal nature of the material, though clays are not wholly

colloidal, and that this essential colloid has not been identified. In so far as it does exist, it appears to be (a) a colloidal alumino-silicic acid widely distributed through a mass of inert granular material of the same chemical composition or even of an entirely different one, such as sand, the whole being comparable to a freshly-made concrete, but differing from the latter in requiring heat to " set " it. Alternatively, (b) the colloidal material may be a mixture of colloidal silica and colloidal alumina precipitated simultaneously from the sol state in such a manner as to appear to be a definite chemical compound. This possibility can only be confirmed or disproved by a large amount of experimental work which is not yet completed. The chief difficulty in accepting the alternative hypothesis is that, if it were correct, it should be possible to isolate relatively large quantities of colloidal silica and alumina from such highly plastic and relatively pure clays as the ball clays, but this has not been accomplished. This may be rendered difficult or impossible by the mutually coagulated silica and alumina gels behaving as a compound in which the silica and alumina have so great an affinity for each other that they cannot be separated by means applicable to the isolation of the simpler gels. Finally, (c) as almost any material may, by suitable treatment, be converted into the colloidal state, the characteristic properties of plastic clays may be independent of the chemical composition of the colloidal matter present in them. If this were the case, any mineral substance which could be converted into colloids by the natural agencies to which clays had been subject would be possible sources of clay. This explanation has the great advantage of explaining the small proportion of colloidal matter present in even the most plastic clays, as if such colloidal matter were the result of age-long grinding of minute rock particles under water, it is only natural to suppose that the product of such action would be grains of the original rock surrounded by a film of colloidal material. If, on the contrary, clays are produced by mixing colloidal matter (formed separately) with non-plastic grains, it is most likely that considerable quantities of such wholly colloidal matter would be found in small pockets or fissures in clay beds. This does not appear to be the case.

If any or all of these three alternatives were correct they would explain many of the known properties of clays. Both silica and alumina gels readily become irreversible ; even when prepared under the most favourable conditions they are much less " manageable " than many other colloids. Hence, it is only to be expected, that if a complex gel containing both silica and alumina in intimate admixture or even in a state of combination would be extremely difficult to isolate in an approximately pure state.

The conditions, under which clays are formed from felspar and other silicates, are so varied, that they do not throw much light on the nature of clays. China clays in Cornwall appear to have been formed by the action of water and acid vapours on granite at a high temperature, the felspar present being decomposed into soluble potassium silicate which has, presumably, been removed in solution, insoluble quartz and clay, the clay being readily separated from other

detritus by a slowly moving stream of water. This explanation—though widely accepted—is by no means satisfactory, and if it is assumed to account sufficiently for the low degree of plasticity of china clay it does not explain the high plasticity of Devonshire ball clays not many miles away, unless the latter are presumed to have no connection with the Cornish clays. Rohland[5], on the contrary, attributed the low plasticity of some kaolins to the colloidal clay having been largely removed in the sol state.

So far as can be ascertained at present, the colloidal material to which clays appear to owe their characteristic properties has been produced by the very prolonged action of water on rocks, chiefly those composed of one or more alumino-silicic acids, the precise nature and origin of which is still uncertain, though represented roughly by the formula $H_4Al_2Si_2O_9$ in the case of Cornish china clays, Dorset and Devonshire ball clays, and possibly of other less pure clays. This colloidal constituent of clays appears to be a very finely divided, solid cellular substance* which can absorb water like a sponge and thereby form a kind of jelly which retains the water by capillary attraction and only permits it to evaporate very slowly at the ordinary atmospheric temperature. Consequently, the proportion of water present in a clay paste of given consistency is a rough measure of the colloidal matter present and when precisely similar clays are compared, it may also be a measure of the plasticity of the paste; the latter property is partly due, however, to the size of the grains coated by the colloidal material and by the thickness and other physical characteristics of the colloidal coating and the extent to which it penetrates any pores or interstices in the granular material. If such a colloidal material were isolated, it would apparently be peptised by a dilute solution of alkali, or by lime water, and recoagulated by strong acids. On heating, it would first contract greatly and simultaneously part with a considerable part of its absorbed water. On further heating up to 500° C. or above, it would be decomposed with the evolution of water and the formation of an irreversible material largely colloidal in character, though different in many ways from most well-known colloids. On further heating, it might undergo other changes, the nature of which can only be summarised as including polymerisation, but one product which under favourable conditions may be expected to be formed is crystalline sillimanite, $Al_2O_3Si\,O_2$. When heated with caustic alkalies, bases, and most metallic oxides, the colloid would probably form mixtures of the corresponding silicates and aluminates. The colloid alone would probably be highly resistant to heat, but in the presence of a metallic oxide (other than alumina) it would fuse more readily.

It is comparatively easy to obtain colloidal silica from calcined, and therefore irreversible, or from the crystalline forms of silica, but the process is so drastic (including the fusion of the material with sodium carbonate) that it appears inapplicable to clay, as the latter is completely decomposed during the heating with the flux and the final colloidal product is merely a mixture of colloidal alumina and

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* It need not, of course, consist of any one chemical compound; even if it consisted chiefly of one such compound, it would seldom, if ever, be pure.

silica which does not possess the properties of the original clay. Various other attempts to synthezise " true clay " have been equally unsuccessful.

It is generally considered that one of the most important sources of clays is the mineral felspar which can be obtained in the form of pure crystals, should form a good starting point. The felspar to which certain clays are commonly attributed is orthoclase, which has the formula $K_2Al_2Si_6O_{16}$. This is a minimum formula and may be more correctly represented by $K_{\times}Al_{\times}Si_{\times 3}O_{8\times}$. Keeping the simple form, the decomposition may be represented by—

$$K_2Al_2Si_6O_{16} + 3H_2O = 2K_2O + H_4Al_2Si_2O_9 + 4SiO_2,$$

which assumes that the felspar is a potassium alumino-silicate with china clay as the corresponding acid, but containing much less silica. Fireclays, on the contrary, even after such limited purification as can be effected, often contain more silica, and indicate a slightly different equation and they appear to have an entirely different origin both chemically as well as geologically. Unfortunately, this decomposition of felspar has never produced clay when carried out in the laboratory. Water produces under great pressure a white body which has little or no plasticity, which may be clay, but cannot be identified as such.

Another attempted synthesis was that of Pukall,[20] who decomposed a pure china clay with sodium chloride and treated the product with carbonic acid. It then yielded a salt deficient in silica and soda and corresponding to $2Na_2O\ 4H_2O\ 6Al_2O_3\ 10SiO_2$. The same salt when treated with a strong acid (HCl) dissolves and from the solution ammonia precipitates a material which Pukall mistook for synthesized clay, but which contains rather more hydrogen and oxygen and dehydrates readily at 350° C. instead of 500° C. Apparently, an isomer of clay had been formed.

At present, it seems quite impossible to be certain of the composition of the substance or substances to which clays owe their chief characteristics. The reporter favours the view (p. 125) that age-long grinding has produced a film of colloidal matter on grains of non-plastic material as probably accounting for most clays, but he also believes that no single cause can account for the formation of all kinds of clay, and that other explanations, such as those given on p. 126 *seq.*, may be equally correct in some cases. The fact that the finer particles of all plastic clays correspond more or less closely to the formula $H_2Al_2Si_2O_9$ does not necessarily invalidate the theory that clays are simply a product of intensely ground rocks, as there are many clays which do not correspond to the formula just mentioned and some of these which correspond to it more closely are only feebly plastic. The similarity in composition of materials regarded as clays may, possibly, be merely a coincidence due to the predominating proportion of alumino-silicious rocks in the material of which the earth's crust consists.

Whatever its nature and origin, it is now fairly well established that many of the properties of clays are closely connected with the colloidal matter present, such matter being in the form of a film of

colloidal gel surrounding particles which are of a non-plastic or colloidally inert nature; in some cases, they may be rich both in alumina and silica—as in china clays and ball clays—whilst in others they may be almost wholly silicious, as in fireclays and many brick earths.

The particular kind and amount of gelatinous matter present, the size and shape of the grains of non-colloidal material, and the relative proportions of large and small grains are important factors in determining the various physical properties of clays, particularly their binding power, compressive strength, tensile strength, and air shrinkage.

Some Technical Uses of the Colloidal Properties of Clays.

Although the nature of the materials to which clays apparently owe their colloidal properties is unknown, great use is made of the properties in various industries, as will be seen from the following notes :—

In *purifying clays* and similar substances, the suspension of the material in water, followed by a process of elutriation or sedimentation, whereby the coarser impurities are removed whilst the partially purified clay is carried off in suspension, has been in use since ancient times. More recently—especially in America—electrolytes, such as sodium carbonate, caustic soda, &c., have been added to the water employed, so as to ensure a maximum amount of clay being held in suspension in a minimum quantity of water. The use of such electrolytes also ensures a sharper separation of the inert sandy material present. The suspension is run off into suitable vessels, the added alkali neutralised by the addition of sulphuric acid; the clay is allowed to deposit and afterwards removed and dried. The separation of the impurities is due to the fact that whenever a charged colloid particle in suspension meets another similarly charged particle they mutually repel each other and so remain in suspension. When two particles of opposite charge come into contact, the charge is neutralised, and the two particles unite and are precipitated. The electrolyte added must, therefore, be one which will increase the negative charge of the material as a whole, so as to effect the precipitation of the impurities, (which are chiefly electro-positive) and retain in suspension the electro-negative particles of clay and, along with them, some silica which is also electro-negative. The coarser particles of silica, pyrite, felspar, mica, &c., do not become charged, but settle on account of their size and weight. If the liquid is too viscous, the impurities will not settle properly, and it must then be diluted until the density and viscosity are such that sufficient separation is effected without the loss of clay by sedimentation.

A similar process of purification is used as a preliminary stage of the Schwerin electro-osmosis process[21], but instead of the clay being allowed to settle it is caused to migrate to a rotating electrode immersed in the liquid and is scraped off in the form of a stiff paste. Schwerin found that all clays did not behave thus, and only migrated so when they carried an electric charge. This charge is supplied,

in some cases, by the addition of organic colloidal matter, such as humic acid prior to the use of alkali.

The apparatus for treating clays consists of a wooden trough with one electrode of wire gauze, and the other a metal drum rotating in the trough containing the alkaline clay slip. The electric current is then passed through the fluid, using the gauze and drum as poles. In this apparatus, a further slight purification is effected owing to the tendency of the impurities to travel to one electrode whilst the clay (with some silica) travels to the other. The clay and finest silica particles assume a negative charge, but that of the silica is so slight that the silica remains almost stationary in the fluid, whilst the clay travels to the anode. Pyrite, alumina and ferric oxides are positively charged and travel with the water towards the cathode. The dominant feature in the speed of migration is the valency of the material attracted to the diaphragm.

The chief use of the electro-osmosis process is for producing a clay paste sufficiently dry for commercial purposes, as the greater part of the purification is effected before the current is applied.

The electro-osmose process whilst theoretically interesting, is not, at present, regarded as of much practical importance, as the use of electricity to separate clay and water is more costly than other equally efficacious methods. Moreover, the finest particles of free silica migrate simultaneously with the " true clay " so that only a very limited purification by the electric current is possible, the greater part having been done by the electrolyte added, which is not an essential part of the osmosis process.

In East Germany, it has been used for some time on a commercial scale, and about two years ago an English syndicate was formed to exploit the Schwerin patents, and to supply certain requirements of the Optical Department of the Ministry of Munitions. For further information on the electro-osmose process for the treatment of clays see " The British Association Report on Colloids, II, 1918," pp. 42–4, 47–52.

A possible method of purifying clay and separating the colloidal silica is due to Billitzer (1905), who found that in $N/2$ to $N/10$ solutions of hydrochloric acid, the charge of colloidal silica is changed from electro-positive to electro-negative so that the careful addition of acid should enable a practical separation of colloidal silica from colloidal clay to be made. The importance of this suggestion appears to have been overlooked.

In separating clay and water, as when it is desired to dry a clay slip or suspension, use may be made of the electric and colloidal properties of the clay. This is an essential feature of the Schwerin electro-osmosis process[21] previously described. On passing a current of electricity through the clay slip, the clay migrates to, and is retained by the rotary electrode (anode) and the water tends to travel to the cathode, so that the clay removed from the drum is drier than from a filter press worked at a pressure of 150 lb. per sq. inch. Thus, some ball clays may be obtained with only 17 or 18 per cent. of water, and so dry that it cannot be pugged, whilst the same clay,

when removed from a filter press would not contain less than 30 per cent. of water. The clay acts like a porous diaphragm made of capillary tubes and this shows the well-known phenomena of endosmosis.

In making articles of clay and allied materials by the casting process, *i.e.*, by pouring a suspension of the materials into a mould and, after a suitable time, pouring off the surplus fluid, it is important to have as concentrated a suspension as is reasonably possible. When water is used, only moderate concentrations can be used, but by adding a small percentage of a suitable electrolyte such as sodium carbonate or water-glass, or both, the amount of clay in suspension can be doubled and the casting process greatly facilitated. This use of an electrolyte, is based solely on the assumed colloidal nature of the clay. Care must be taken in choosing the electrolyte, as some substances such as sodium carbonate if used alone will cause a very high surface tension, with the result that the slip " balls up " and may cause the inclusion of air bubbles, whilst sodium silicate used alone causes the clay to flow in " strings " like a thick syrup. A suitable mixture of the two, however, is excellent, and gives a smooth flowing stream without any tendency to the defects just mentioned. Slips containing a suitable electrolyte require far less time in the mould than those slips in which plain water is used. This is necessary in consequence of the smaller proportion of water present and is of great practical importance as it reduces the number of moulds required. With a good stoneware slip containing soda, a mould may be used five or six times in succession without drying and with slips of leaner clays, the moulds may be used still more frequently. On the other hand, the salts absorbed by the plaster tend to make the moulds less durable when soda is present in the slip.

A slip to which soda or other electrolyte has been added feels more soapy and plastic than one with plain water; on passing it through a sieve, it does not flow so readily and tends to form long syrupy strings, and, on long standing, little or no separation occurs. A soda-slip also flows more steadily and with less tendency to include bubbles of air than one made without an electrolyte and the painting of portions of a mould with slip, which is sometimes essential to ensure the production of a good surface on the ware is entirely unnecessary when a suitable electrolyte is used.

In increasing or reducing the plasticity of a clay or earth, so as to make it suitable for the manufacture of various articles, the methods most extensively used are based on the assumed colloidal properties of the clay. Plasticity is increased by, methods (p. 130) which increase the amount of colloidal gel in the material and it is reduced by the methods (p. 131) which will lessen the amount of irreversible colloid gel, or by converting it into a hydrosol. Thus, the processes of *ageing* and *souring* the clay are dependent on an increase of colloidal matter by the prolonged hydrolysing action of the water on the clay, followed by a fermentation or acid-producing action which coagulates any hydrosols previously formed.

Weathering.—The reduction of large and hard masses of clay and shale is often greatly facilitated by exposure to weather, *i.e.*, to the action of air, sunshine, frost and rain. When so exposed, many clay materials disintegrate rapidly and may afterwards be made into a plastic paste much more easily than by any mechanical process of grinding and mixing with water. Different clays and shales are affected differently by exposure; some disintegrate after a few hours' exposure on a warm day, whilst others appear to require a succession of frosts and rainy periods. In most cases, the most feasible explanation of the physical changes which occur is that the conditions of exposure result in the partial peptisation of the colloidal cementing material which binds the particles of clay together. It is well known that sand grains soaked in concentrated glue and then suitably dried, form a hard rocky material which, on exposure or soaking in water, falls to powder as the colloidal element absorbs water, swells, and is no longer able to hold the particles together. It is suggested—though no definite proof is available—that when natural clays are exposed to weather a similar absorption of water by the colloidal matter occurs, and is followed by a corresponding disintegration of the mass. In the case of some clays, a certain amount of chemical change such as the fermentation of organic matter, or the oxidation of the pyrite, &c., also occurs and may also facilitate the disintegration, but the chief cause of the reduction of the material to a more or less pulpy mass, bears a much closer resemblance to the softening of the colloidal cementing mass than to any ordinary process of oxidation or other obviously chemical reaction.

Commercially, the weathering of indurated clays is of great importance, as it not only reduces the cost of grinding and mixing, but the weathered product is much more homogeneous and the water present is far more uniformly distributed than when the treatment of the clay is purely mechanical.

According to W. Taylor, the colloids produced during the weathering are *not* amorphous alumino-silicates, but *mixtures* of single gels produced by the mutual precipitation of positive and negative sols.

The ancient practice of storing clay in cellars for a long time, and known as *maturing* or *ageing*, is now seldom practiced to anything like the extent to which it was formerly thought necessary. Where hollow goods of very fine quality are made there is an undoubted advantage in thus storing the clay before it is made up into goods, but the keeping of clay in air-tight boxes for several years, as practised by the Chinese and more recently by Wedgwood and other famous potters, is no longer considered essential, though its beneficial effect on the clay cannot be denied. In Germany, the use of sumps, in which the clay and water remains in contact with each other for a considerable time, is still regarded as necessary.

In freshly pugged paste, there is only a limited amount of colloidal matter in an active form. Its amount may be increased by subjecting the paste to conditions under which any dry and horny colloidal gel will absorb water, swell, and form a soft friable jelly, and the same

result is obtained when a clay paste is stored in a cool place in a moist atmosphere for a sufficiently long time; the requisite coagulation occurs when sufficient acid is added to the material. This acid may be produced internally by the putrefaction of the organic matter, or it may be added artificially.

The swollen gel produced on prolonged storage is very permeable to water, and its structure may be compared to a series of solid grains wholly surrounded by liquid films which are not sufficiently thick to allow the particles to separate from each other or to flow appreciably. Such a structure has a powerful capillary action and consequently, it affects the distribution of water through the mass in a most thorough and efficient manner. This uniform distribution of the water largely— in conjunction with the coagulating and swelling of the colloidal matter—accounts for the increased ease with which an old clay-paste can be manipulated. The water in freshly pugged clay cannot be so uniformly distributed as when such a paste has been allowed to stand for several weeks, during which time the water is distributed through the mass by capillary attraction.

A much shorter storage of the clay paste, frequently in open sheds the material being covered with wet sacking, is known as *souring*. Its effect is undoubtedly to increase the active, as distinct from the dormant, plasticity of the clay, though there is a great variation in the extent to which this takes place. There is a widespread impression that souring is the result of bacteria or ferment-organisms, and some potters added sugar or honey to the clay to assist the fermentation but, whilst this may account for some of the observed effects, the hydrolysing action of the water present in the mass on the clay, silica, and iron hydroxide particles must not be overlooked. Rohland[5] suggested that the fresh clay paste is slightly alkaline owing to the felspar, &c., present in the clay being hydrolysed and converted into the colloidal form. The acids produced by the decomposition of any organic matter also present neutralise the cations; and the excess of hydrogen-ions produced coagulates the colloid matter and correspondingly increases the plasticity of the clay. This explains why the old vinegar " tip " of bygone potters develops the plasticity. Previous to this, Seger[1] had found that clays which remain alkaline do not increase in plasticity on storage, but do so if they are acidulated with acetic acid.

As heat is a disadvantage, souring must usually take place in a cool, moist, shed or cellar, if it is to be really effective; though in opposition to this, it may be noted that slips which are dried by heat are often more plastic than those treated in a filter press.

Some firms apply souring or storage to clays which are highly plastic, not to develop more plasticity but to secure a better distribution of moisture through the mass, and, as they express it, " to bring it into a better and tougher condition."

The weathering of crystalline minerals generally yields gels or mixtures of gels; thus, talc is formed from the weathering of serpentine, and some forms of brown iron ore from yellow ochre, but this is not invariably the case. It is, therefore, possible that

some clays may be mixtures of gels whilst others are mixtures of colloidal and amorphous alumino-silicic acids.

The customary arrangement of the paste in the souring shed is to fill a large clean floor with the paste to a height of about 5 feet, cover it with wet sacking and keep the latter moist for the desired duration of the souring. As fresh clay is added to one part of the floor, the soured clay is removed from another. Where the output is sufficiently large, it is convenient to use an annular building with 12–20 entrances as the addition of fresh paste and the removal of the soured paste may then proceed continuously with a minimum of trouble and space. In some works, the clay is stored in the form of balls or blocks about 3 feet diameter. If the paste is allowed to sour in wagons, each holding about 1 ton, a considerable amount of labour is avoided, though the gain is largely counterbalanced by the cost of the wagons.

During the souring period, the paste must be kept moist and special care should be taken not to allow a dry crust to form on its surface.

Air as well as moisture is necessary for the effective souring of some highly plastic clays, as the desired decomposition of the organic matter cannot occur in the absence of air.

It is very important to cover the clay to be soured, with wet sacking, as if this is not done the plasticity will be decreased instead of increased. In time, the union of the particles and also the growth of larger particles at the expense of the smaller ones may occur. Both may lead to the enlargement of the interstices between the grains and to the solidification of the gel residue, by which the total volume of gel remains fairly constant under constant tension. Crystallisation and other processes which enlarge the interstices during ageing also increase the vapour tension. During ageing the total surface of the particles is reduced, but the interspaces grow larger. This indicates that ultramicrons grow at the expense of the amicrons.

The duration of the souring period varies greatly in different cases. In some cases, only a couple of days is allowed; six to eight weeks is much more valuable when practicable, and the ancient makers of some Chinese porcelains are understood to have kept their paste for a hundred years ! It is by no means unusual for the clay to be soured as much as six months and in exceptional cases, several souring periods are arranged. In making glass-house pots, for instance, the clay, is allowed to sour after a preliminary treading; it is then re-trodden, again allowed to sour and finally is re-pugged before it is ready for use.

· *Increasing the Souring Effect.*—As the results which occur when the paste is allowed to sour are so beneficial, it is obviously advantageous to develop them as much as possible. The chief methods of doing this are :—

> (i) To increase the amount of organic matter in the paste and to secure a more intense souring action. For this purpose, various putrefactive organic solutions such as wine, old vinegar, sewage, peat extract and also solid organic substances such as chopped vegetable matter, tannery waste, and even molasses

are sometimes used instead of plain water in pugging the clay. Such additions do not usually increase the plasticity of the clay to more than a small extent and they greatly increase the difficulty of burning the ware satisfactorily.

(ii) The addition of weak acid such as acetic acid, oxalic acid, or tannin (gallotannic acid) to neutralise the acid present or to discharge any hydroxyl-ions formed by the dissociation of salts present as impurities in the paste. One of the most interesting proposals in this connection is that of Acheson and Ries who have found that on the addition of a 2 per cent. solution of tannin to certain clays the plasticity may be greatly increased and the clay was apparently deflocculated and broken up into smaller particles, whilst the tensile strength was greatly increased. The use of a substance definitely known as an acid is not necessary; any substance which acts as a corresponding electrolyte of either organic or inorganic nature is advantageous because it neutralises the charge on some other electrolyte present in the paste and so effects a definite increase in the plasticity of the clay.

The great importance of souring is not appreciated as it should be, because the phenomena which occur are largely misunderstood and consequently, it is often omitted where it would be most useful. In the manufacture of bricks, and coarse goods, omission may not be serious, but it should never be omitted in the preparation of a paste for the manufacture of tiles, glazed ware, earthenware, fine pottery, and porcelain.

Ware made from a properly soured paste is less sensitive to sudden changes of temperature, can have thinner walls, does not break so easily, and is easier to produce as the souring increases the plasticity of the paste.

It is usually necessary to pug the paste after it has been soured so as to form it into a compact and homogeneous paste.

Certain clays have long been used as *absorbents* for grease and similar materials especially in fulling cloth and for medicinal purposes. Curiously enough, these clays are among the least plastic. Their usefulness depends on their absorptive and adsorbent properties.

Conversely, the use of clay in the preparation of *ultramarine* depends partly on its chemical composition, but chiefly on the colloidal nature of the product.

It should be remembered that clays can only be used for removing basic dyes and colours such as malachite green, as their removal from solution depends on the negative electric charge carried by the colloidal particles in the clays. Incidentally it may be noted that clays containing much colloidal silica or colloidal alumina give uncertain results when treated with malachite green.

Clays are also used in the preparation of several other colloidal substances including *Portland cement*.

The *clarifying power* of clays, when mixed with turbid fluids, also depends on the colloidal nature of the finely suspended particles

coalescing with, or being entangled with, the coarser particles of clay and being carried down by the latter. For this purpose, the clays should not be too finely divided, nor should they be highly plastic, as a somewhat coarser, porous material is more efficient and settles more readily. It must, however, possess sufficient power of adsorption to retain the finely suspended particles which cause the turbidity in the fluid which it is desired to clarify.

The use of clays as clarifying agents is particularly successful in the treatment of slightly oily effluents of spinning works, wool-scouring plants, distilleries, tanneries, dye-works, glue-factories, breweries, and other industries producing an effluent containing organic matter in a very finely divided or colloidal state. The best results are obtained when the colloidal matter in the effluent carries a positive electric charge as it is dissipated by the added colloidal matter bearing a negative charge. Domestic sewage, however, is not of this character, and, therefore, cannot be clarified in this manner. This fact confirms the essentially colloidal nature of the active ingredient in clay used as a clarifying agent.

The separation of water from clay in *filter presses* may, according to F. Ulzer[22] be facilitated by inserting suitable electrodes in the filter chambers, so as to coagulate the colloidal material and yet keep it from blocking the cloth.

The *extrusion of clay* through dies is made more rapid by the application of an electric charge, as described in "British Association Report on Colloids, II.," 1918.

The *binding power* of clays, like that of dextrin, gelatin, and other well-known colloids is used for uniting other particles into a compact mass as in the manufacture of refractory materials from non-plastic materials, "lead" in lead pencils, &c. Conversely, use is made of the colloidal property to reduce the excessive shrinkage of certain clays by the addition of suitable non-plastic materials.

In *agriculture*, the colloidal properties of clays play a most important part. It is now generally agreed that plants and other organisms growing in the soil, derive their sustenance chiefly from a film of colloidal matter surrounding the particles of inert material constituting the bulk of the soil and occupying some of the interstices between these particles. The soluble salts extracted from the mineral material in the soil or subsoil, or added in the form of a fertiliser, are adsorbed by this film of colloidal matter and transferred to the plant-roots, &c. There are several different substances existing in the colloidal state in soils, the more important being the colloidal "clay," silica, alumina, ferric hydroxide, and a mixture of partially decomposed organic substances collectively known as "humus." The last-named acts as a protective colloid for other substances, and absorbs many soluble salts. In association with other colloids, it determines the amount of water retained by the clay. Humus is negatively charged and is, therefore, coagulated into a gel by basic substances, such as lime.

The colouring matter in swamps is generally positively charged, and is, therefore, precipitated by colloidal clay. Alkaline humates are not colloids, but soluble substances. Being a negative colloid,

humus is precipitated by cations such as Ca, Fe, or Al which alter its permeability, absorption, and the way in which a soil " works." Fertilisers convert the silica gels into sols which rise by capillary action, and are again gelated nearer the surface of the ground or in the cells of the plants which absorb them. For further details, *see* " British Association Report on Colloids, II.," 1918, pp. 70–81.

In the manufacture of *detergents*, the use of clay has been developed to some extent on account of its colloidal properties, apart from its use as a filler.

It is well known that the amount of active colloidal material in clays can be increased by the addition of a certain proportion of alkali, which disperses the particles of clay, as shown on p. 119. F. E. Weston[23] made use of this fact in order to bring china clay into a highly colloidal state, and claimed that the colloidal clay thus formed could be used as a substitute for soap. His results are not, however, conclusive, and the clay-soap can only be used in special cases. It is of little value in toilet preparations, but as a substitute for the coarse soaps used in wool scouring it has been used with success, and several preparations are now on the market.

The chief advantages claimed for colloidal clay soap are that it absorbs dirt and grease, and removes them without any deleterious effect on the material, there being no injurious chemicals in the preparation, and that it is capable of removing unsaponifiable oily substances—a result which is not possible with the ordinary scouring media. It is stated to have a greater detergent power than ordinary soap and its antiseptic properties may be of considerable value.

It has been shown by Bancroft, the organic soaps depend partly on their solubility and decomposition (hydrolysis) for their detergent properties, and as such properties are not possessed by clay soap, they have a disadvantage in this respect.

As yet, the colloidal properties of clay have not been widely investigated in this direction, and a very large field may be opened out for the us of pure clays in soap manufacture, though the cost of purifying brick, tile and other clays—even though they are more plastic and contain more colloidal material—would, in most cases, be prohibitive.

REFERENCES.

[1] Seger, Gesamm., Schriften., ed. Dr. H. Hecht and E. Cramer (*Tonindustrie Zeitung*, Berlin, 1908).
[2] Asch, W. & D., *The Silicates in Chemistry and Commerce* (Constable & Co., London).
[3] Martin, G., *Chemical News.*
[4] Schloesing, *Compt. Rend.*, lxxix., 376–380, 473–477 (1874).
[5] Rohland, P., *Die Tone.*
[6] Searle, A. B., *An Introduction to British Clays* (Griffin, London, 1911).
[7] Ashley, *Trans. Amer. Cer, Soc.*, xii., 768 (1910).
[8] Schurecht, *Journ. Amer. Cer. Soc.*, 443–450 (1919).
[9] Kosmann, *Tonindustrie Zeitung*, 352 (1895).
[10] Zebisch, Sprechsaal, 1028 (1894).
[11] Mellor, Green & Baugh, *Trans. Eng. Cer. Soc.*, vi., 161–170, 1906.
[12] Ostwald, W., *Colloid Chemistry.*

[13] L'Hermite, *Ann. Chim. Phys.*, (3), 43, 352 (1855).

[14] Zschokke, *Chem. Technologie der Neuzeit*, Bd. 1, 775.

[15] MacMichael, R. F., *Brick and Clay Record*, 690 (1919).

[16] Bourry, *Treatise on the Ceramic Industries* (Scott, Greenwood & Son, London, 1911).

[17] Grout, W. *Va. Geol. Survey*, III. (1906).

[18] Cushman, *A. Trans. Amer. Cer. Soc.*, vi., 7 (1907).

[19] Bemmelen, A. Van, *z. anorg. Chem.*, v., 466; xiii., 233; xviii, 14; etc.

[20] Pukall, W., *Berichte d. Deutsch. d. Chem. Gesellsch.*, 43, 2107 (1910).

[21] *Brit. Assoc. Rept.*, II.

[22] Ulzer, F., *z. angow. Chem.*, 28, i., 308 (1915).

[23] Weston, F. E., *Chem. Age.*, Jan. 17, 1920, pp. 58–60; *see also Chem. Age.*, Jan. 31, p. 115; Feb. 7, p. 146; Feb. 14, p. 170; Mar. 6, p. 247; April 3, p. 351.

·Recent References.

Not Included in this Report.

Weston, F. E., *Colloidal Clay, China Clay Trade Review*, p. 337, 1920.

Hay, J. Gordon, *Colloidal Clay as a Catalyst in Oxidation and Hydrogenation* *Chem. Age.*, Feb. 21, 1920, p. 194.

Ormondy, W.R., *The Filtration of Colloids, Soc. Chem. Ind. Conference, July*, 1920 (*deals with the electro-osmosis of clays*).

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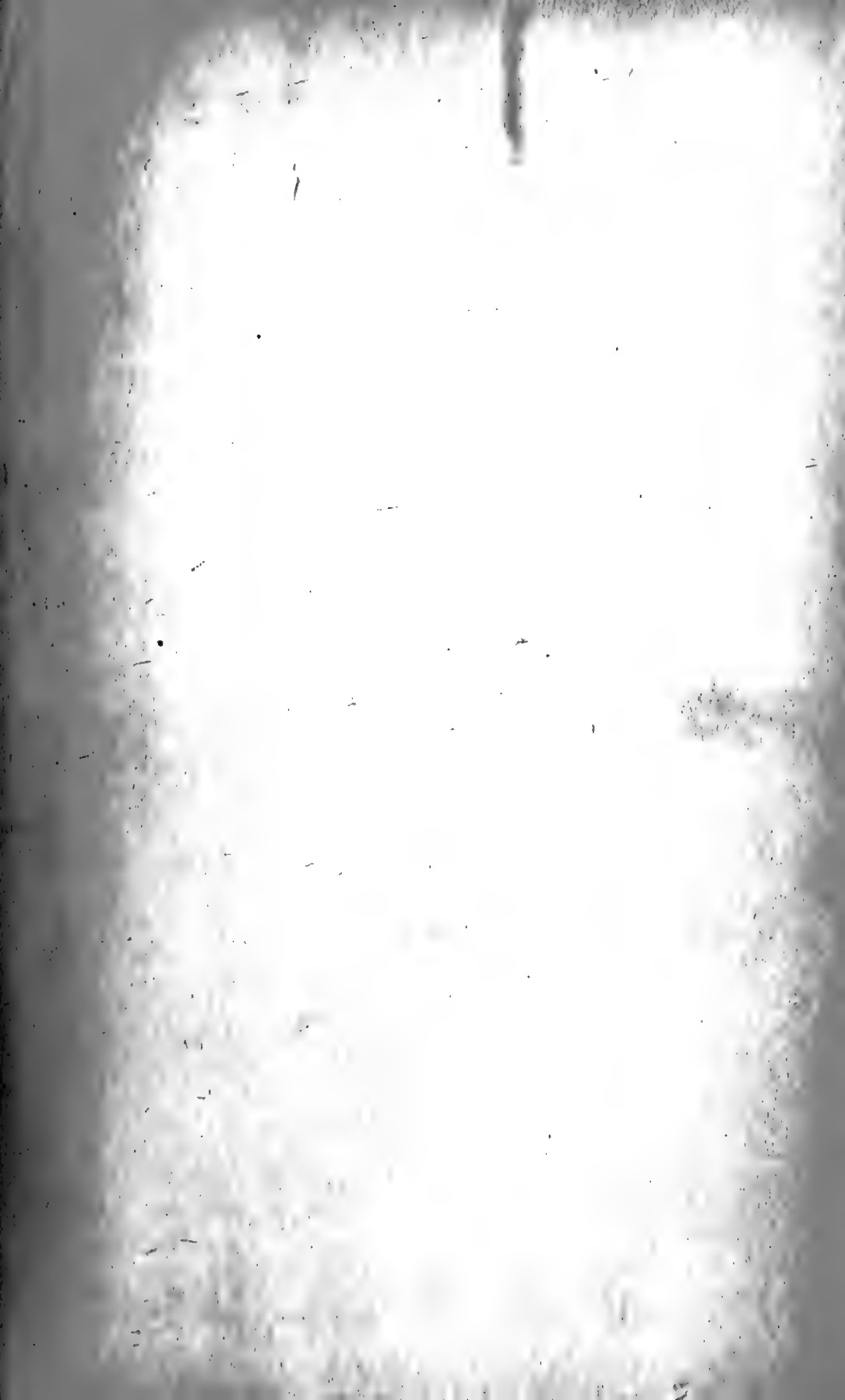Report on Training in Citizenship, 1920, 1*s.*

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