Sr. 1. A.

# BRITISH ASSOCIATION
## FOR THE ADVANCEMENT OF SCIENCE

# REPORT

OF THE

## NINETY-EIGHTH MEETING
(HUNDREDTH YEAR)

BRISTOL—1930
SEPTEMBER 3—10

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

1931

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# British Association for the Advancement of Science.

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* Subject, in a few instances, to acceptance of office.

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## CONFERENCE OF DELEGATES OF CORRESPONDING SOCIETIES.

*President.*—Prof. PATRICK ABERCROMBIE.
*Secretary.*—Dr. C. TIERNEY.

# TABLE OF

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

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

[*Continued on p.* xii.

# ANNUAL MEETINGS.

| Old Annual Members | New Annual Members | Associates | Ladies | Foreigners | Total | Amount received for Tickets | Sums paid on account of Grants for Scientific Purposes | Year |
|---|---|---|---|---|---|---|---|---|
| — | — | — | — | — | 353 | — | — | 1831 |
| — | — | — | — | — | — | — | — | 1832 |
| — | — | — | — | — | 900 | — | — | 1833 |
| — | — | — | — | — | 1298 | — | £20 0 0 | 1834 |
| — | — | — | — | — | — | — | 167 0 0 | 1835 |
| — | — | — | — | — | 1350 | — | 435 0 0 | 1836 |
| — | — | — | — | — | 1840 | — | 922 12 6 | 1837 |
| — | — | — | 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 | 682 | 45‡ | 2004 | 2042 0 0 | 1940 0 0 | 1868 |
| 229 | 107 | 678 | 600 | 17 | 1856 | 1931 0 0 | 1622 0 0 | 1869 |
| 303 | 195 | 1103 | 910 | 14 | 2878 | 3096 0 0 | 1572 0 0 | 1870 |
| 311 | 127 | 976 | 754 | 21 | 2463 | 2575 0 0 | 1472 2 6 | 1871 |
| 280 | 80 | 937 | 912 | 43 | 2533 | 2649 0 0 | 1285 0 0 | 1872 |
| 237 | 99 | 796 | 601 | 11 | 1983 | 2120 0 0 | 1685 0 0 | 1873 |
| 232 | 85 | 817 | 630 | 12 | 1951 | 1979 0 0 | 1151 16 0 | 1874 |
| 307 | 93 | 884 | 672 | 17 | 2248 | 2397 0 0 | 960 0 0 | 1875 |
| 331 | 185 | 1265 | 712 | 25 | 2774 | 3023 0 0 | 1092 4 2 | 1876 |
| 238 | 59 | 446 | 283 | 11 | 1229 | 1268 0 0 | 1128 9 7 | 1877 |
| 290 | 93 | 1285 | 674 | 17 | 2578 | 2615 0 0 | 725 16 6 | 1878 |
| 239 | 74 | 529 | 349 | 13 | 1404 | 1425 0 0 | 1080 11 11 | 1879 |
| 171 | 41 | 389 | 147 | 12 | 915 | 899 0 0 | 731 7 7 | 1880 |
| 313 | 176 | 1230 | 514 | 24 | 2557 | 2689 0 0 | 476 8 1 | 1881 |
| 253 | 79 | 516 | 189 | 21 | 1253 | 1286 0 0 | 1126 1 11 | 1882 |
| 330 | 323 | 952 | 841 | 5 | 2714 | 3369 0 0 | 1083 3 3 | 1883 |
| 317 | 219 | 826 | 74 | 26 & 60 H.§ | 1777 | 1855 0 0 | 1173 4 0 | 1884 |
| 332 | 122 | 1053 | 447 | 6 | 2203 | 2256 0 0 | 1385 0 0 | 1885 |
| 428 | 179 | 1067 | 429 | 11 | 2453 | 2532 0 0 | 995 0 6 | 1886 |
| 510 | 244 | 1985 | 493 | 92 | 3838 | 4336 0 0 | 1186 18 0 | 1887 |
| 399 | 100 | 639 | 509 | 12 | 1984 | 2107 0 0 | 1511 0 5 | 1888 |
| 412 | 113 | 1024 | 579 | 21 | 2437 | 2441 0 0 | 1417 0 11 | 1889 |
| 368 | 92 | 680 | 334 | 12 | 1775 | 1776 0 0 | 789 16 8 | 1890 |
| 341 | 152 | 672 | 107 | 35 | 1497 | 1664 0 0 | 1029 10 0 | 1891 |
| 413 | 141 | 733 | 439 | 50 | 2070 | 2007 0 0 | 864 10 0 | 1892 |
| 328 | 57 | 773 | 268 | 17 | 1661 | 1653 0 0 | 907 15 6 | 1893 |
| 435 | 69 | 941 | 451 | 77 | 2321 | 2175 0 0 | 583 15 6 | 1894 |
| 290 | 31 | 493 | 261 | 22 | 1324 | 1236 0 0 | 977 15 5 | 1895 |
| 383 | 139 | 1384 | 873 | 41 | 3181 | 3228 0 0 | 1104 6 1 | 1896 |
| 286 | 125 | 682 | 100 | 41 | 1362 | 1398 0 0 | 1059 10 8 | 1897 |
| 327 | 96 | 1051 | 639 | 33 | 2446 | 2399 0 0 | 1212 0 0 | 1898 |
| 324 | 68 | 548 | 120 | 27 | 1403 | 1328 0 0 | 1430 14 2 | 1899 |

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

[*Continued on p.* xiii.

*Table of*

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

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

*Annual Meetings*—(continued).

| Old Annual Members | New Annual Members | Associates | Ladies | Foreigners | Total | Amount received for Tickets | Sums paid on account of Grants for Scientific Purposes | Year |
|---|---|---|---|---|---|---|---|---|
| 297 | 45 | 801 | 482 | 9 | 1915 | £1801 0 | £1072 10 0 | 1900 |
| 374 | 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[1] | 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[2] | 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[3] | 539[3] | — | 21 | 5044[3] | 4873 0 | 1861 16 4[6] | 1914 |
| 287 | 116 | 628[4] | 141 | 8 | 1441 | 1406 0 | 1569 2 8 | 1915 |
| 250 | 76 | 251[4] | 73 | — | 826 | 821 0 | 985 18 10 | 1916 |
| — | — | — | — | — | — | — | 677 17 2 | 1917 |
| — | — | — | — | — | — | — | 326 13 3 | 1918 |
| 254 | 102 | 688[4] | 153 | 3 | 1482 | 1736 0 | 410 0 0 | 1919 |

| Old Annual Regular Members | Annual Members: Meeting and Report | Annual Members: Meeting only | Transferable Tickets | Students' Tickets | Foreigners | Total | Amount received for Tickets | Sums paid on account of Grants for Scientific Purposes | Year |
|---|---|---|---|---|---|---|---|---|---|
| 136 | 192 | 571 | 42 | 120 | 20 | 1380 | 1272 10 | 1251 13 0[8] | 1920 |
| 133 | 410 | 1394 | 121 | 343 | 22 | 2768 | 2599 15 | 518 1 10 | 1921 |
| 90 | 294 | 757 | 89 | 235[5] | 24 | 1730 | 1699 5 | 772 0 7 | 1922 |
| | | | | | Complimentary. | | | | |
| 123 | 380 | 1434 | 163 | 550 | 308[7] | 3296 | 2735 15 | 777 18 6[9] | 1923 |
| 37 | 520 | 1866 | 41 | 89 | 139 | 2818 | 3165 19[10] | 1197 5 9 | 1924 |
| 97 | 264 | 878 | 62 | 119 | 74 | 1782 | 1630 5 | 1231 0 0 | 1925 |
| 101 | 453 | 2338 | 169 | 225 | 69 | 3722 | 3542 0 | 917 1 6 | 1926 |
| 84 | 334 | 1487 | 82 | 264 | 161 | 2670 | 2414 5 | 761 10 0 | 1927 |
| 76 | 554 | 1835 | 64 | 201 | 74 | 3074 | 3072 10 | 1259 10 0 | 1928 |
| 24 | 177 | 1227[11] | — | 161 | 83 | 1754 | 1477 15 | 1838 2 1 | 1929 |
| 68 | 310 | 1617 | 97 | 267 | 54 | 2639 | 2481 15 | 683 5 7 | 1930 |

[6] Including grants from the Caird Fund in this and subsequent years.
[7] Including Foreign Guests, Exhibitioners, and others.
[8] The Bournemouth Fund for Research, initiated by Sir C. Parsons, enabled grants on account of scientific purposes to be maintained.
[9] Including grants from the Caird Gift for research in radioactivity in this and subsequent years to 1926.
[10] Subscriptions paid in Canada were $5 for Meeting only and others pro rata ; there was some gain on exchange.
[11] Including 450 Members of the South African Association.

# REPORT OF THE COUNCIL, 1929-30.

*Obituary.*

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

- Prof. J. O. Arnold.
- Rev. Canon H. J. D. Astley.
- Rt. Hon. the Earl of Balfour, ex-president.
- Rev. J. O. Bevan.
- Sir Edward Brabrook, former member of Council.
- Lady Bragg.
- Prof. H. L. Callendar.
- Mr. G. G. Chisholm.
- Major P. G. Craigie, former member of Council.
- Dr. W. G. Duffield.
- Dr. S. Z. de Ferranti.
- Mr. W. Heape.
- Prof. Augustine Henry.
- Col. Sir Thomas Holdich, former member of Council.
- Sir E. Ray Lankester, ex-president.
- Major P. A. Macmahon, ex-general secretary.
- Mr. E. T. Newton.
- Prof. K. J. P. Orton.
- Prof. W. H. Perkin.
- Prof. W. Robinson, who had accepted the Recordership of Section K (Botany) for the Bristol Meeting.
- Dr. J. M. Duncan Scott.
- Prof. Sir Baldwin Spencer.
- Mr. A. A. Campbell Swinton, a benefactor of the Association.
- Prof. Sir George Thane.
- Prof. H. H. Turner, ex-general secretary.
- Mr. H. W. T. Wager.
- Prof. T. B. Wood.

The Association was represented at the funeral of Major Macmahon by the Astronomer Royal, and at the memorial service for the Earl of Balfour by Sir Josiah Stamp, General Treasurer.

*Representation.*

II.—Representatives of the Association have been appointed as follows:—

| | |
|---|---|
| Cape University College Centenary . . | Lady Sherrington, M. l'Abbé Breuil |
| National Conference for the Preservation of the Countryside . . . . | Dr. Vaughan Cornish |
| North-Western Naturalists' Union: meeting on proposed formation . . . | Prof. F. E. Weiss |
| American Society of Mechanical Engineers: 50th Anniversary . . . . | Major P. J. Cowan |
| Fresh-water Biological Association: meeting to consider establishment of research station . . . . . . . | Dr. W. T. Calman |
| Technical Committee on Nomenclature formed by British Engineering Standards Association . . . . | Sir James Henderson |
| Commemoration of the Centenary of Belgian Independence: Opening of new buildings, University of Brussels; Journées Médicales, Institut Pasteur; Congrès National des Sciences . . | Prof. G. H. F. Nuttall |
| International Zoological Congress, Padua. | Dr. F. A. Bather |
| Faraday Centenary Joint Committee . | Dr. F. E. Smith, Mr. O. J. R. Howarth |

The Association was represented by Dr. C. W. Kimmins and Prof. J. L. Myres at a conference called by the Association of Scientific Workers to consider the provision made for the Science Library, South Kensington. A committee including Prof. Myres was appointed to consult the Director and to approach the President of the Board of Education.

### *Resolutions from South African Meeting.*

III.—Resolutions referred by the General Committee at the South African Meeting to the Council for consideration, and, if desirable, for action, were dealt with as follows :—

(*a*) A resolution from Section A (Mathematical and Physical Sciences), dealing with the desirability of an observatory for terrestrial magnetism and atmospheric electricity in South Africa, together with a memorandum kindly furnished by Prof. A. M. Tyndall, Recorder of the Section, was circulated to the appropriate authorities in South Africa through the High Commissioner. The Council were informed that the Union Government is considering the matter.

(*b*) A resolution from Section C (Geology), supported by Section H (Anthropology), on the preservation of Nooitgedacht Farm in view of the geological and archæological interests of this area, was forwarded to the South African Association for the Advancement of Science.

(*c*) A resolution from Section D (Zoology), on the desirability of exchange of members of museum staffs, was considered, together with a memorandum kindly furnished by Mr. G. L. Purser, Recorder of the Section, by a committee of the Council. The Council adopted this committee's report, which was to the effect that actual exchange is not possible under present conditions, and that no good purpose would be served by calling attention to its desirability. On the other hand, the Committee was able to report that good results are obtained by enabling museum officers to work in other museums or in the field, and the promulgation of this view in appropriate quarters was recommended. This recommendation has been given effect.

(*d*) A resolution from Section D (Zoology), on the desirability of an international biological station in the Malay Archipelago, was forwarded to leading scientific societies in Holland, to appropriate societies and Government departments at home, to the secretary of the Pan-Pacific Science Congress, and to Prof. F. A. Went (Utrecht).

(*e*) A resolution from Section D (Zoology), on the desirability of extending marine biological investigation in South Africa, was referred to the appropriate authorities, and information has been received that the question will receive the careful consideration of the Council and Senate of the University of Cape Town.

(*f*) A resolution from Section E (Geography), on the desirability of expediting the topographical survey of South Africa, was forwarded to the appropriate authorities, together with a reference to the address by the President of the Section, Brigadier E. M. Jack. A memorandum was received from the Director of the Trigonometrical Survey, indicating that a beginning had been made with the topographical survey and the training of personnel. It was stated that much experimental work would be necessary before a more comprehensive programme could be considered.

(*g*) On the subject of a resolution from Section H (Anthropology), dealing with the protection of Australian aborigines, the Council are in communication with appropriate authorities in Australia.

(*h*) A resolution from Section H (Anthropology), dealing with the preservation of ancient monuments and remains in South Africa, was forwarded to the appropriate authorities there, through the High Commissioner, together with a memorandum kindly provided by the President of the Section, Mr. H. Balfour. Information was received that the Union Government intends to introduce legislation in this connection in the near future.

*Zimbabwe Investigation: Publication and Loan Exhibition.*

IV.—The publication of the results of Miss Caton-Thompson's investigation of archæological sites at and near Zimbabwe, in Rhodesia (*Report* 1929, pp. xvi, 368), which was the subject of a recommendation from Section H (Anthropology), has been offered to the Clarendon Press, Oxford. The book is to be in royal 8vo., fully illustrated, and published at 15*s*., the Council providing a subsidy of £100 from the balance of the special fund remaining.

V.—A Loan Exhibition of Antiquities from Southern Rhodesia was opened from April 7 to the earlier part of June by the kind permission of the Trustees of the British Museum, in the Assyrian Basement of the Museum. The Exhibition was under the patronage of H.E. the Governor-General of the Union of South Africa and H.E. the Governor of Southern Rhodesia; and included objects generously lent from the South African Museum at Cape Town, the Rhodesian Museum at Bulawayo, the Queen Victoria Memorial Museum at Salisbury, and several private collections; as well as the principal objects found during Miss Caton-Thompson's excavation, the air photographs taken by officers of the Air Force of the Union, and other photographs kindly lent. The Union Castle S.S. Co. generously provided free transport for the exhibits. The expenses of the exhibition were met out of the balance of the Association's special fund.

*British Association Medal for South Africa.*

VI.—The Council resolved that a capital sum of £200 be set aside from the balance of the Association's special fund for the South African Meeting, to provide from the income thereof a research medal and premium, to be awarded to a member of the South African Association for the Advancement of Science, the age of the recipient not to exceed thirty years and the paper giving the results of the research to be read at a meeting of the South African Association; the medal to be called the British Association Medal and to be awarded annually subject to the final adjudication of the Council.

The Council of the South African Association, in gratefully accepting the above proposal, decided to add to the capital the sum of £275, and referred the terms upon which the medal will be granted to a sub-committee for consideration.

### *Down House.*

VII.—The following report for the year 1929–30 has been received from the Down House Committee :—

The number of visitors to Down House during the year, since but excluding the date of the formal opening, June 7, 1929, has been approximately 11,000, a gratifying total, having regard especially to the fact that no public advertising has been attempted. The South-eastern Union of Scientific Societies, and other societies, have organised visits for their members, and further parties are expected during the present summer. Among others, the Committee understand that a party from the International Botanical Congress, meeting at Cambridge, may visit Down in August; the Committee have pleasure in recommending that opportunity should be taken to entertain these distinguished visitors.

Thanks have been tendered on behalf of the Committee to many generous donors of objects for preservation in the Memorial Rooms, which have been much enhanced thereby in appearance and interest since the opening last year. Mr. Buckston Browne himself has added many appropriate gifts, including a showcase in the New Study, to contain objects which could not otherwise be exhibited conveniently.

The Old Study has appropriately the closest resemblance to its appearance in Darwin's time; the furnishing is in great part original, and some of the maps on which Darwin worked in connection with the coral reef researches have been framed for exhibition here. These maps were received from Cambridge through the instrumentality of Prof. A. C. Seward. Mr. Buckston Browne has added much to the Donor's Room, while the Old Dining Room contains the portraits and cartoons which formerly were in the Council Room of the Association, and also the presidential banner of the Prince of Wales, which has been framed for safe keeping. Publications of the Association are shown in this room.

The Committee were especially glad to hear of the recovery of Darwin's letters to Müller in South America, through the good offices of Prof. H. F. Osborn. Prof. E. B. Poulton has kindly undertaken the study of these letters previously to their being deposited at Down House.

The Forestry Commission generously permitted an inspector to visit the estate and advise the Secretary on the preservation of the timber.

The gardens require extensive renovation, which is in progress. In particular, the adaptation and partial concealment of the foundations of the former school buildings, without undue expense, has exercised the ingenuity of the resident staff. Certain gifts of plants for the gardens have been received, but more will be welcomed. The staff at first included two gardeners; a third was engaged temporarily with the sanction of the Council, who desired the Committee to consider whether (having regard to financial considerations) he should be permanently employed. He is still at work, and at the moment the Committee can only report that there is more than ample work to occupy the three men, and that this must be so for a long period, if not permanently. It would seem that to make and keep the gardens beautiful is a duty which the Association owes both to the memory of Darwin and to the public. The general financial position is referred to below.

In connection with a town-planning scheme for the Bromley rural area, the authorities proposed to provide for cutting off, in the future, the greater part of the front garden, in order to obliterate a 'blind' curve in the road. However, they received a protest courteously, and proposed as an alternative that, if and when road-widening should take place, the Association should undertake to lower the garden wall in front of the house, the garden remaining intact, and any widening being effected on the other side of the road. This was agreed. There is no immediate prospect that the scheme will mature.

The Association, following upon its incorporation in 1928, received the certificate of the Registrar of Friendly Societies as a 'charitable' society. Possessing this certificate, it claimed exemption from rates upon Down House under the Act of 1843 dealing with the rating of societies' premises. The local assessment committee contested this, and summoned the Association for non-payment. It emerged in the hearing before the Bromley magistrates that the assessment committee would be willing to recognise exemption except in the case of the residential premises occupied by the Secretary. It was argued that his residence at Down House was a matter rather of convenience than of necessity; though he himself asserted in evidence that so long as his duties continue as at present, his residence in the house might be reasonably regarded as essential. The Bench, however, decided against the Association, and the Committee is not of opinion that the case should be carried further. The costs of the case have been an unforeseen charge.

The tenant of one of the cottages on the estate (Homefield), who was seriously in arrear with his rent, has now vacated the cottage, giving a promise to pay arrears by instalments. Sir Arthur Keith has applied to rent the cottage for his own use, and the rest of the Committee cannot but feel deeply sensible of the benefits which may accrue from his frequent presence at Down.

The 'initial' expenditure from the general funds of the Association upon the Down House property—that is to say, repairs outside the house itself, redemption of tithe, purchase of land, legal charges, equipment, &c.—has amounted to £2,600, and may be estimated in total at £3,000. The estimates of running costs show a deficiency on the income from the endowment and other sources—the annual income from dividends, rents, &c., may be set down at £1,120, and the expenditure on wages, heat, light, water, garden, rates, repairs, &c., at £1,200, making no provision for increase of wages, contingencies such as large repairs, or the possibility of adding appropriate objects by purchase to the Darwin collection, or any other equipment. The Committee, however, cannot but believe that additional financial support will be forthcoming, and that the possession of Down House alone goes far to justify the appeal for a Centenary Fund for the Association, as contemplated by the Council.

### *Centenary Meeting*, 1931.

VIII. His Majesty The King, Patron of the Association, has been graciously pleased to signify his approval of the arrangements made by the General Committee for the celebration of the Centenary of the Association in London, and of the nomination made by the Council in the following paragraph.

*President.*—General the Rt. Hon. J. C. Smuts, P.C., has been unanimously nominated by the Council to fill the office of President of the Association for the year 1931–32 (Centenary Meeting).

*Vice-Presidents.* — The following vice-presidents have been nominated by the Council. (These nominees, and those composing the London Committee following, have not been personally approached.)

(L.C. signifies members of the London Committee proposed below, § IX.)

H.R.H. the Prince of Wales, ex-president (L.C.).
The Archbishop of Canterbury.
The Archbishop of York.
The Prime Minister, the Rt. Hon. J. Ramsay Macdonald (L.C.).
The Lord President of the Council (L.C.).
The Secretary of State for the Dominions (L.C.).
The Secretary of State for the Colonies (L.C.).
The President of the Board of Education (L.C.).
The First Commissioner of Works (L.C.).
The High Commissioner for Australia (L.C.).
The High Commissioner for Canada (L.C.).
The High Commissioner for South Africa (L.C.).
The High Commissioner for New Zealand (L.C.).
The Lord Mayor of London (L.C.).
The Lord Mayor of York.
The Chairman of the London County Council (L.C.).
The Mayor of Kensington (L.C.).
The Mayor of Westminster (L.C.).
The Chancellor of the University of London (L.C.).
The Vice-Chancellor of the University of London (L.C.).
The Chairman of the British Broadcasting Corporation (L.C.).
The Chairman of the Port of London Authority (L.C.).
The President of the Royal Society.

The President of the British Academy.
Prof. H. E. Armstrong (L.C.).
The Rt. Hon. Lord Ashfield (L.C.).
The Rt. Hon. the Earl of Athlone, Governor-General of the Union of South Africa, 1929.
Rt. Hon. Stanley Baldwin (L.C.).
Sir Otto Beit, Chairman of Finance Committee, Imperial College of Science and Technology (L.C.). (*Died, Dec.* 1930.)
The Bishop of Birmingham.
Prof. F. O. Bower, President, 1930–31 (L.C.).
Sir William Bragg, Royal Institution, ex-president (L.C.).
G. Buckston Browne, Hon. Curator of Down House (L.C.).
Sir David Bruce, ex-president.
The Rt. Hon. Lord Buckmaster, Chairman of Governing Body, Imperial College of Science and Technology (L.C.).
The Rt. Hon. Viscount Byng, Governor-General of Canada, 1924 (L.C.).
Sir Frank Dyson, Astronomer Royal (L.C.).
Sir Arthur Evans, ex-president.
Sir Alfred Ewing.
Sir Ambrose Fleming.
The Rt. Hon. D. Lloyd George (L.C.).
Dr. E. H. Griffiths, ex-General Treasurer.
Sir Robert Hadfield (L.C.).
Sir Thomas Holland, ex-president.
Sir Arthur Keith, Royal College of Surgeons, ex-president (L.C.).
Prof. Horace Lamb, ex-president.
Sir Oliver Lodge, ex-president.
Rt. Hon. Lord Melchett (L.C.).
The Rt. Hon. Viscount Novar, Governor-General of Australia, 1914.
Hon. Sir Charles Parsons, ex-president (L.C.).
Prof. E. B. Poulton.
Sir David Prain (L.C.).
Sir Ernest Rutherford, ex-president.
Sir Arthur Schuster, ex-president.
Dr. D. H. Scott, ex-General Secretary.
Sir Edward Sharpey-Schafer, ex-president.
Sir Charles Sherrington, ex-president.
Dr. F. E. Smith, Dept. of Scientific and Industrial Research, ex-General Secretary; Sec. R.S. (L.C.).
Sir J. J. Thomson, ex-president.
The Rt. Hon. Lord Wakefield (L.C.).
Sir Alfred Yarrow.

IX. *London Committee.*—The vice-presidents marked (L.C.) in the above list, together with the following, are nominated as a London Committee, which, if appointed, the Council propose to summon early in the winter, to discuss the general outline and plans for the Centenary Meeting.

The President and General Officers, *ex-officio.*
The Bishop of London.
The Dean of St. Paul's.
The Dean of Westminster.
The Archbishop of Westminster.
The President of the Wesleyan Methodist Conference.

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The Members of Parliament for—
The City of London (2).
Kensington, South.
Westminster, St. George's.
University of London.

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The Sheriffs (2) of the City of London.
The Town Clerk of the City of London.
The Private Secretary to the Lord Mayor of London.
The Education Officer of the London County Council.
The Mayors of

| | | |
|---|---|---|
| Battersea. | Hackney. | St. Marylebone. |
| Bermondsey. | Hammersmith. | St. Pancras. |
| Bethnal Green. | Hampstead. | Shoreditch. |
| Camberwell. | Holborn. | Southwark. |
| Chelsea. | Islington. | Stepney. |
| Deptford. | Lambeth. | Stoke Newington. |
| Finsbury. | Lewisham. | Wandsworth. |
| Fulham. | Paddington. | Woolwich. |
| Greenwich. | Poplar. | |

The Town Clerk of Kensington.
The Town Clerk of Westminster.

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The Chairman of the Metropolitan Water Board.
The Director of the British Museum.
The Director of the British Museum (Natural History) (Dr. Tate Regan, member of Council).
The Director of the London School of Economics.
The Director of the Science Museum.
The Director of the Victoria and Albert Museum.
The Director of the Imperial Institute.
The Director of the Royal Botanic Gardens, Kew.
The Director of the Royal College of Art.
The Director of the Royal College of Music.
The Directors of Research Departments, Admiralty and Air Ministry.
The Headmaster, City of London School.
The Headmaster of Merchant Taylors' School.
The Headmaster of Westminster School.
The High Master of St. Paul's School.
The Masters (or Prime Wardens) of the 'great' Livery Companies:—

| | |
|---|---|
| Mercers. | Merchant Taylors. |
| Grocers. | Haberdashers. |
| Drapers. | Salters. |
| Fishmongers. | Ironmongers. |
| Goldsmiths. | Vintners. |
| Skinners. | Clothworkers. |

The President of the London Chamber of Commerce.
The Principal of Bedford College.
The Principal of Birkbeck College.
The Principal of East London College.
The Principal of King's College.
The Principal of the London Day Training College.
The Principal of Royal Holloway College.
The Principal of the University of London.
The Principal of Westfield College.
The Provost of University College.
The Rector of the Imperial College of Science and Technology.

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Members of the Advisory Council on Scientific and Industrial Research not otherwise indicated.

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Dr. F. A. Bather, member of Council.
Sir William Bull.
Dr. E. J. Butler.
Sir Dugald Clerk.
Prof. W. Dalby, member of Council.
Dr. H. H. Dale, Sec. R.S.
Prof. C. Lovatt Evans, member of Council.

Sir Walter Fletcher, Medical Research Council.
Sir John Flett, Geological Survey, member of Council.
Prof. George Forbes.
Prof. A. Fowler.
Sir Richard Glazebrook.
Sir Richard Gregory, member of Council.
Prof. Dame Helen Gwynne-Vaughan, member of Council.
Sir Daniel Hall, John Innes Horticultural Inst., member of Council.
Sir Sidney Harmer.
Dr. H. S. Hele-Shaw.
Sir James Henderson, member of Council.
A. R. Hinks, Sec. R.G.S., member of Council.
Sir Henry Lyons, member of Council.
Sir G. A. K. Marshall.
Sir Chalmers Mitchell, Sec. Zoological Soc.
Prof. G. T. Morgan, Chemical Research Lab., Teddington.
Prof. Karl Pearson.
Sir Joseph Petavel, National Physical Lab.
Prof. A. O. Rankine, member of Council.
Sir John Reith, Director-General, B.B.C.
Dr. A. B. Rendle.
Sir Robert Robertson, Government Chemist, Sec. Royal Institution.
Sir Ronald Ross.
Lord Rothschild.
Sir Napier Shaw.
Dr. F. C. Shrubsall, member of Council.
Dr. G. C. Simpson, Director, Meteorological Office, member of Council.
Prof. G. Elliot Smith.
Prof. A. Smithells.
Sir Thomas Stanton.
Prof. J. F. Thorpe, member of Council.

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X. The Council has appointed Committees to consider finance, meeting rooms, hospitality and reception, excursions and exhibits, and publications in connection with the Centenary Meeting.

The question of finance is dealt with in a later paragraph of this report.

After consideration of a number of possible halls for the inaugural meeting (and failing the Albert Hall, which will be otherwise occupied), the Wesleyan Central Hall and annexes have been booked for this occasion. In the event of a very large meeting the use of relaying will be unavoidable.

The inaugural meeting must necessarily be of an exceptional character, as numerous addresses and other messages are to be anticipated, and opportunity must be afforded for a number of speakers. After consideration of various alternatives it is proposed, if the General Committee and the President-elect concur, that the inaugural meeting, after the installation of the President-elect in the chair, should be devoted mainly to the above purposes, the President finally addressing the meeting; but that he should deliver the Presidential Address proper on a separate occasion, namely, the final evening of the meeting, Tuesday, September 29. It is suggested that this would form a fitting conclusion. The Wesleyan Central Hall has been engaged for this date also.

The requirements of the Association in respect of the Reception Room, offices, sectional meeting rooms, &c., will be best met in the institutions in and near Exhibition Road, South Kensington, and arrangements are in hand accordingly. Applications on behalf of the Council to the University of London, the Imperial College of Science and Technology, the Imperial Institute, the Science Museum, the Victoria and Albert Museum, the Royal College of Music, and the Royal Geographical Society have met with a generous response.

A preliminary scheme of excursions has been worked out, and the Sections will be informed of it.

In place of the usual local handbook for the meeting a new edition of 'The British Association: A Retrospect' is in preparation for distribution to members.

Sir Alfred Ewing has been appointed 'to prepare a paper . . . dealing with the whole question of the prime movers of 1931, and especially with the then relation between steam engines and internal combustion engines,' under the terms accompanying Sir Frederick Bramwell's gift to the Association (1903).

*Finance.*

XI. *General Treasurer's Account.*—The Council has received reports from the General Treasurer throughout the year. His accounts have been audited and are presented to the General Committee.

XII. *Centenary Fund.*—The Council has sanctioned an appeal for a fund, not only to provide for those expenses of the Centenary Meeting which in the case of the ordinary annual meeting are met by a local fund, but also for the more adequate endowment of the Association, especially in consideration of the widening of its activities which has been marked during the closing years of its first century. The Council has had under review the considerations—

(*a*) That the acquisition of Down House has made a considerable capital demand upon the Association's funds, and that the existing endowment of the house, generous as it is, does not cover all needs (as indicated in the report of the Down House Committee);

(*b*) That the Association, with adequate funds, might and ought to relieve the localities where its meetings are held of some of the expenses which now fall upon local funds, and the honorary local officers of some of their labours;

(*c*) That the Association should not be limited in its choice of meeting-places, whether at home or in the Dominions, by questions of expense and the amount of support likely to be forthcoming by way of membership;

(*d*) That the Association should be in a position more effectively to stabilise the assistance it renders to research by way of grants, to junior scientific workers to attend its meetings, and so forth.

The total sum for which the Council has considered that appeal should be made is £40,000.

XIII. *Cunningham Bequest.*—The Association received under the will of the late Lt.-Col. Alan Cunningham a bequest of £2,909 for the preparation of new tables in the theory of numbers.

XIV. *Lamarck Memorial.*—The Council made a contribution of £5 5*s.* toward the memorial to Lamarck, for which funds were solicited by the Société Linnéenne du Nord de la France.

*General Officers, Council, and General Committee.*

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

*General Treasurer*, Sir Josiah Stamp.
*General Secretaries*, Prof. J. L. Myres and Prof. F. J. M. Stratton.

The Council, under power delegated to it by the General Committee, appointed Prof. Stratton as Acting General Secretary on the retirement of Dr. F. E. Smith in December last, and takes pleasure in nominating him for confirmation in office. The Council has expressed its warm appreciation of the services of Dr. Smith.

XVI. *Council.*—The retiring Ordinary Members of the Council are:—Prof. J. H. Ashworth, Prof. C. Burt, Prof. W. E. Dalby, Sir John Flett, Sir Percy Nunn.

The Council nominates the following new members: Prof. H. Clay, Prof. W. T. Gordon, Dr. C. W. Kimmins, 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:—

F. C. Bartlett.
Dr. F. A. Bather.
Prof. A. L. Bowley.
Prof. H. Clay.
Prof. C. Lovatt Evans.
Sir Henry Fowler.
Prof. W. T. Gordon.
Sir Richard Gregory.
Prof. Dame Helen Gwynne-Vaughan.
Dr. A. C. Haddon.
Sir Daniel Hall.
Sir James Henderson.
A. R. Hinks.
Dr. C. W. Kimmins.
Col. Sir H. G. Lyons.
C. G. T. Morison.
Prof. A. O. Rankine.
Dr. C. Tate Regan.
Prof. A. C. Seward.
Dr. F. C. Shrubsall.
Dr. N. V. Sidgwick.
Dr. G. C. Simpson.
Prof. J. F. Thorpe.

XVII. *General Committee.*—The following has been admitted as a member of the General Committee: Dr. A. Hopwood.

*Miscellanea.*

XVIII. *The Royal Empire Society* generously afforded members of the Association who had visited South Africa for the meeting in 1929 an opportunity of reunion at one of the evening meetings of the Society, when Lord Lloyd was in the chair, supported by Sir Thomas Holland, and an address was delivered by Sir Richard Gregory.

XIX. *Corresponding Societies Committee.*—The Corresponding Societies Committee has been nominated as follows: The President of the Association (*Chairman ex-officio*), Mr. T. Sheppard (*Vice-Chairman*), Dr. C. Tierney (*Secretary*), The General Treasurer, the General Secretaries, Mr. C. O. Bartrum, Dr. F. A. Bather, Sir Richard Gregory, Sir David Prain, Sir John Russell, Prof. W. M. Tattersall.

XX. *Civil Service.*—The Council made a representation to H.M. Government regretting the absence of representatives of science and technology from the Royal Commission on the Civil Service, and through a committee is watching the progress of events in this connection with a view to further action if desirable.

XXI. *The British Association Rotating Coil* for absolute measurement of resistance was presented to the national collections at the Science Museum.

XXII. *Assistant Secretary of the Association.*—The Council has appointed Mr. H. Wooldridge to this office for a probationary period covering the Bristol and Centenary meetings.

# GENERAL MEETINGS IN BRISTOL, ETC.

The Inaugural General Meeting was held on Wednesday, September 3, 1930, at 8.30 p.m., in the Colston Hall. After the Lord Mayor of Bristol and the Vice-Chancellor of the University had welcomed the Association, Prof. F. O. Bower, F.R.S., assumed the Presidency of the Association, in succession to Sir Thomas H. Holland, F.R.S., and delivered an address (for which see page 1) on 'Size and Form in Plants.'

On Thursday evening, September 4, a Reception was given by the Lord Mayor and Lady Mayoress of Bristol in the Museum and Art Gallery. On Monday, September 8, the Council and Senate of Bristol University held a Garden Party at Wills Hall at 4.0 p.m. At 8.30 p.m. on the same day, the President, Council, and Headmaster of Clifton College received members at Clifton College. The President and Committee of the Zoological Society held an At Home in the Zoological Gardens on Sunday, September 7, at 3.0 o'clock.

## Evening Discourses.

Prof. E. V. Appleton, F.R.S.: 'Wireless Echoes,' 8.0 p.m., September 5, Students' Union, Victoria Rooms (see p. 426).

Dr. R. E. Slade: 'The Nitrogen Industry and Our Food Supply.' 8.0 p.m., Students' Union, Victoria Rooms (see p. 434).

## Public Lectures.

Public lectures were arranged in Bristol and neighbouring towns as follows:—

Sir Daniel Hall, F.R.S.: 'Apples; the Bearing of Research on Improved Production.' 5.15 p.m., September 4, Merchant Venturers' Technical College, Bristol.

Mr. R. A. Pelham: 'Some Aspects of the Colonial Problems in British East Africa.' 7.30 p.m., September 4, The Library, Taunton.

Prof. Winifred Cullis: 'Breathing Under Difficulties.' 8 p.m., September 5, The Town Hall, Weston-super-Mare.

Mr. L. S. B. Leakey: 'East Africa.' 3.15 p.m., September 6, The Merchant Venturers' College, Bristol.

Prof. J. G. Smith: 'Speculation and Investment.' 8 p.m., September 8, The Art Gallery, Cheltenham.

Dr. C. W. Kimmins: 'The Sense of Humour in Children.' 8 p.m., September 8, The Guild Hall, Salisbury.

Wing-Com. T. R. Cave-Brown-Cave: 'Airship R101 with Special Reference to Machinery.' 7.45 p.m., September 9, The Mechanics' Institute, Swindon.

Sir Josiah Stamp, G.B.E.: 'The Price Level and Scientific Control.' 5.30 p.m., September 9, The Banqueting Room, Bath.

Mr. W. M. H. Greaves: 'The Sun.' 7.30 p.m., September 10, The Royal Fort, The University, Bristol.

Mr. V. E. Nash-Williams: 'Caerwent and the Romanization of South Wales.' 8 p.m., September 10, Bingham Library, Cirencester.

Dr. J. A. Bowie: 'The Rationalisation of Industry.' 8 p.m., September 10, Crypt Grammar School, Gloucester.

Sir Arthur Keith's lecture on Dr. John Beddoe in Section H (*q.v.*) was also open to the public.

# DEGREE CONGREGATION OF THE UNIVERSITY OF BRISTOL.

# LEAVE-TAKING.

A special Degree Congregation of the University of Bristol was held in the University on Wednesday, September 10, at 11 a.m., when the degree of LL.D. (Honoris Causa) was conferred on Prof. F. O. Bower, F.R.S., President of the Association.

After the ceremony, the President expressed the thanks of the Association to the University for the hospitality and facilities afforded for the Meeting. The President and Officers of the Association then waited upon the Lord Mayor, members of the Corporation of Bristol, and local officers for the Meeting, at the Council House, in order to express the thanks of the Association to the City of Bristol.

# RESOLUTIONS & RECOMMENDATIONS.

The following resolutions and recommendations were referred to the Council by the General Committee at Bristol for consideration and, if desirable, for action :—

*From Section A.*

That the attention of the Council be drawn to the desirability of printing the volume of tables which has been prepared by the Mathematical Tables Committee.

That in connection with the above, the attention of the Council be drawn to the section of the Report dealing with the Cunningham bequest.

*From Section A.*

That the Council be asked to endorse the resolution following addressed to the Board of Visitors of the University of Oxford Observatory, and to urge upon the Board of Visitors the desirability of ensuring the continuance of the Seismological work carried on hitherto at Oxford under the direction of Prof. Turner.

Resolution :—

The Committee of the Section of Mathematical and Physical Sciences of the British Association for the Advancement of Science desire to offer to the Board of Visitors of the University of Oxford Observatory their sincere condolence on the loss that the Observatory has sustained by the death of Prof. H. H. Turner. The Committee further desire to place on record their high appreciation of his long and devoted services to the Sciences of Astronomy and Seismology.

The Committee express the hope that the death of Prof. Turner will involve no discontinuity in the Astronomical and Seismological work, of international importance, which is so honourably associated with his name, and with that of the University.

*From Section H.*

In view of the increasingly rapid disappearance of material relating to the popular arts and crafts of the British people, the Committee of Section H requests Council to ask His Majesty's Government to put into effect the recommendation of the Royal Commission on National Museums and Galleries for the establishment of a National Open Air Folk Museum in London.

The Committee further suggests that the Government might consider the possibility of utilising the Royal Botanic Gardens in Regent's Park for this purpose, in view of their admirable situation and the proximity of a building (St. John's Lodge) suitable for exhibition purposes and offices, providing this can be done without interfering with the scientific work already in progress on the site.

### *From Section H.*

The Committee of Section H recognises the value of the measures now proposed to be taken by the Government of the Australian Commonwealth for the extension of territories reserved for the Australian aborigines, and for the unification of the protective administrations. The Australian natives are among the most interesting and the most valuable peoples for scientific study, and offer opportunities for research of unique importance for future investigations in the early history of mankind. The Committee, therefore, while appreciating the practical difficulties, desires the Council to represent to the Commonwealth Government the need of anthropological training for the officials charged with native administration, and to urge the adoption of every means to prevent the extinction of the aboriginal peoples and the further disintegration of native society.

### *From Section H.*

The Committee views with alarm the increasing activity of unauthorised persons on archæological sites in South Africa and the Rhodesias. It therefore asks the Council to make representations to the Governments concerned urging that permission to excavate should be given to trained archæologists only, after consultation with the Directors of the Geological and Archæological Surveys in the area.

### *From Section K.*

That the attention of the Government be called to the limitation of opportunities for the growth of experimental plants in London, facilities for this purpose having been provided for many years at the Royal Botanic Gardens, Regent's Park, and that, whilst welcoming the suggestion of the Committee of Section H that an Open Air Folk Museum be established in that locality, the Committee of Section K urge the importance of the continued provision for botanical research of part of the grounds.

### *From the Conference of Delegates of Corresponding Societies.*

That the Council of the British Association be asked to represent to H.M. Government the need for the establishment of Nature Reserves in suitable areas in connexion with National Parks.

That the Council be asked to appoint a Committee to take cognisance of proposals relating to National Parks by the Government and other authorities and bodies concerned, and to advise the Council as to action if desirable.

# GENERAL TREASURER'S ACCOUNT

JULY 1, 1929, TO JUNE 30, 1930.

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The large excess of income over expenditure (£2,437 8s.) shown in the Income and Expenditure Account for the year may be really more than offset by various considerations.

(1) In respect of Down House :—

(*a*) A suspense account of £938 7s. is shown as an asset : it represents compensation paid to the outgoing tenant in 1928, and redemption of tithe, payments made during the previous financial year. It will be recommended that these should be written off in future, whether against general funds or against any further donation to the Down House funds which may be forthcoming.

(*b*) The Down House Account shows an excess of expenditure over income amounting to £866 10s. 5d. Of this, £482 4s. may be taken as non-recurrent ; but it will be seen that the excess of running costs over income from Down House funds has been approximately £200, and under present financial conditions it appears that Down House must continue to involve some such charge upon the general funds of the Association.

As for non-recurrent expenditure, taking the above-mentioned items together and including purchase of land, &c., the non-recurrent expenditure from general funds upon Down House has amounted to approximately £2,500, and may reach £3,000, inasmuch as not all the work of reconditioning undertaken by the Association (apart from the much more costly work upon the house itself, which was carried out by Mr. Buckston Browne) has yet been completed or paid for.

(2) The balance of £1,523 5s. 1d. remaining of the fund raised last year by Dr. F. E. Smith in aid of extraordinary expenses connected with the South African Meeting appears as income in the Income and Expenditure Account. Upon this fund the Council has undertaken certain liabilities (publication of the results of the Zimbabwe investigations and Zimbabwe Loan Exhibition ; foundation of a second South Africa Medal : see *Report of Council*, IV—VI), while any remainder has been considered as helping to cover such increased costs connected with the meeting as that of printing, as against the inevitable loss of revenue from the reduced number of membership subscriptions.

Office expenses during the past year show a general reduction. Among receipts, the apparent reduction of income tax recoverable as compared with the figure for last year is accounted for by the fact that last year's figure represented two years' repayment.

A review of the financial position, and a consideration of the requirements of the forthcoming Centenary Meeting and the enhanced responsibilities and activities of the Association, would appear amply to justify the Council's sanction of an appeal for a Centenary Fund (*Report of Council*, XII).

# Balance Sheet,

| Corresponding Figures June 30, 1929. £ s. d. | LIABILITIES. | £ s. d. | £ s. d. |
|---|---|---|---|
| | To *General Fund*— | | |
| | As at July 1, 1929 | | |
| 10,942 19 1 | As per contra . . . . . . | | 10,942 19 1 |
| | (Subject to Depreciation in Value of Investments) | | |
| | ,, *Caird Fund*— | | |
| | As at July 1, 1929 | | |
| 9,582 16 3 | As per contra . . . . . . | | 9,582 16 3 |
| | (Subject to Depreciation in Value of Investments) | | |
| | ,, *Caird Fund Revenue Account*— | | |
| | Balance as at July 1, 1929 . . . . . | 201 3 10 | |
| | *Add* Excess of Income over Expenditure for the year . . . . . . | 163 8 8 | |
| 201 3 10 | As per contra | | 364 12 6 |
| | ,, *Sir F. Branwell's Gift*— | | |
| | For enquiry into Prime Movers, 1931—£50 Consols now accumulated to £158 13*s.* 3*d.* | | |
| 76 7 3 | As per contra . . . . . . . . | | 80 4 2 |
| | ,, *Sir Charles Parson's Gift*— | | |
| 10,000 0 0 | As per contra . . . . . . . . | | 10,000 0 0 |
| | ,, *Sir Alfred Yarrow's Gift*— | | |
| | As per last Account . . . . . . | 9,389 2 3 | |
| | *Less* Transferred to Income and Expenditure Account under terms of the Gift . | 334 2 3 | |
| 9,389 2 3 | As per contra | | 9,055 0 0 |
| | ,, *Life Compositions*— | | |
| | As per last Account . . . . . . | 1,638 12 2 | |
| | *Add* received during year . . . . | 88 10 0 | |
| 1,638 12 2 | As per contra | | 1,727 2 2 |
| | ,, *Toronto University Presentation Fund*— | | |
| | As per last Account . . . . . . | 182 18 10 | |
| | *Add* Dividends . . . . . . | 8 15 0 | |
| | | 191 13 10 | |
| | *Less* Awards given . . . . . . | 8 15 0 | |
| 182 18 10 | As per contra | | 182 18 10 |
| | ,, *South African Meeting*— | | |
| | Sundry Donations in aid of Expenses As per last Account . . . . . | 1,728 9 3 | |
| | *Add* Unexpended balances of Grants in aid of Travelling Expenses refunded . . | 94 15 10 | |
| | | 1,823 5 1 | |
| | *Less*— | | |
| | Transfer to South African Association Medal Fund. £200 0 0 | | |
| | Printing Subsidy . 100 0 0 | | |
| | Transfer to Income and Expenditure Account . . 1,523 5 1 | | |
| 1,728 9 3 | | 1,823 5 1 | |
| | ,, *Lt.-Col. A. J. C. Cunningham's Bequest*— | | |
| | For the preparation of New Tables in the Theory of Numbers . . . . . | 2,868 16 4 | |
| | *Add* Dividends . . . . . . | 74 18 5 | |
| | As per contra | | 2,943 14 9 |
| | ,, *South African Association Medal Fund*— | | |
| | As per contra . . . . . . . . | | 200 0 0 |
| | ,, *Down House Endowment Fund*— | | |
| 20,000 0 0 | As per contra . . . . . . . . | | 20,000 0 0 |
| | ,, *Revenue Account*— | | |
| | Sundry Creditors . . . . . . | 271 19 0 | |
| | Do. Do. (Down House) . . . . | 203 13 0 | |
| | | 475 12 0 | |
| 63,742 8 11 | **Carried forward** . | | £65,079 7 9 |

# June 30, 1930.

## ASSETS.

| Corresponding Figures, June 30, 1929. £ s. d. | | £ s. d. | £ s. d. |
|---|---|---|---|
| | By *General Fund*— | | |
| | £4,651 10*s.* 5*d.* Consolidated 2½ per cent. Stock at cost | 3,942 3 3 | |
| | £3,600 India 3 per cent. Stock at cost | 3,522 2 6 | |
| | £879 14*s.* 9*d.* Great Indian Peninsular Railway 'B' Annuity at cost | 827 15 0 | |
| | £52 12*s.* 7*d.* War Stock (Post Office Issue) at cost | 54 5 2 | |
| | £834 16*s.* 6*d.* 4½ per cent. Conversion Stock at cost | 835 12 4 | |
| | £1,400 War Stock 5 per cent. 1929/47 at cost | 1,393 16 11 | |
| | £94 7*s.* 0*d.* 4½ per cent. Conversion Stock 1940/44 at cost | 62 15 0 | |
| | £326 9*s.* 10*d.* 3½ per cent. Conversion Stock at cost | 250 0 0 | |
| | Cash at Bank | 54 8 11 | |
| *10,942 19 1* | | | 10,942 19 1 |
| | *£8,134 2s. 3d.* (Value of Stocks at date, £8,069 18*s.* 1*d.*) | | |
| | ,, *Caird Fund*— | | |
| | £2,627 0*s.* 10*d.* India 3½ per cent. Stock at cost | 2,400 13 3 | |
| | £2,100 London, Midland and Scottish Railway Consolidated 4 per cent. Preference Stock at cost | 2,190 4 3 | |
| | £2,500 Canada 3½ per cent. Registered Stock 1930/50 at cost | 2,397 1 6 | |
| | £2,000 Southern Railway Consolidated 5 per cent. Preference Stock at cost | 2,594 17 3 | |
| *9,582 16 3* | | | 9,582 16 3 |
| | *£6,878 4s. 3d.* (Value at date, £6,872 9*s.* 11*d.*) | | |
| | ,, *Caird Fund Revenue Account*— | | |
| *201 3 10* | Cash at Bank | | 364 12 6 |
| | ,, *Sir F. Bramwell's Gift*— | | |
| | £151 12 0 Self Accumulating Consolidated Stock as per last Balance Sheet | 76 7 3 | |
| | 7 1 3 *Add* Accumulations to June 30, 1930 | 3 16 11 | |
| *76 7 3* | £158 13 3 | | 80 4 2 |
| | *£82 12s. 5d.* (Value at date, £87 5*s.* 3*d.*) | | |
| *10,000 0 0* | ,, *Sir Charles Parsons' Gift*— | | |
| | £10,300 4½ per cent. Conversion Stock at cost | | 10,000 0 0 |
| | *£9,733 10s. 0d.* (Value at date, £10,171 5*s.* 0*d.*) | | |
| | ,, *Sir Alfred Yarrow's Gift*— | | |
| | £9,389 2*s.* 3*d.* 5 per cent. War Loan (£50 Bonds) as per last Account | 9,389 2 3 | |
| | *Less* Sale of £334 2*s.* 3*d.* Stock under terms of the Gift | 334 2 3 | |
| *9,389 2 3* | | | 9,055 0 0 |
| | *£9,459 10s. 6d.* (Value at date, £9,349 5*s.* 9*d.*) | | |
| | ,, *Life Compositions*— | | |
| | £2,550 18*s.* 11*d.* Local Loans at cost | 1,653 12 2 | |
| | *£1,464 1s. 1d.* (Value at date, £1,645 7*s.* 2*d.*) | | |
| | Cash at Bank | 73 10 0 | |
| *1,638 12 2* | | | 1,727 2 2 |
| | ,, *Toronto University Presentation Fund*— | | |
| | £175 5 per cent. War Stock at cost | 178 11 4 | |
| | *£176 6s. 3d.* (Value at date, £180 13*s.* 9*d.*) | | |
| *182 18 10* | Cash at Bank | 4 7 6 | 182 18 10 |
| *1,728 9 3* | ,, *South African Meeting Fund*— | | |
| | Cash at Bank | | |
| | ,, *Lt.-Col. A. J. C. Cunningham's Bequest*— | | |
| | £1,488 18*s.* 0*d.* 2½ per cent. Consolidated Stock | 818 17 11 | |
| | £300 Port of London 3½ per cent. Stock 1949/99 | 216 0 0 | |
| | £100 Commonwealth of Australia 4¾ per cent. Stock | 93 0 0 | |
| | £100 New Zealand 5 per cent. Stock | 103 0 0 | |
| | £800 India 6 per cent. Stock at cost | 801 12 0 | |
| | £1,274 4*s.* 10*d.* Local Loans 3 per cent. at cost | 836 6 5 | |
| | Cash at Bank | 74 18 5 | 2,943 14 9 |
| | (Value of Stocks at date, £2,843 6*s.* 6*d.*) | | |
| *43,742 8 11* | Carried forward | | £44,879 7 9 |

## Balance Sheet,

| Corresponding Figures, June 30, 1929. £ s. d. | LIABILITIES—*continued*. | | | £ s. d. |
|---|---|---|---|---|
| *63,742 8 11* | Brought forward . . . . . . . . | | | 65,079 7 9 |
| | To Income and Expenditure Account— | | | |
| | Balance at July 1, 1929 . | £6,318 8 4 | | |
| | *Add* Excess of Income over Expenditure for the year. | 2,437 8 0 | | |
| | | | 8,755 16 4 | |
| *8,393 4 6* | As per contra | | | 9,231 8 4 |
| *72,135 13 5* | | | | £74,310 16 1 |

I have examined the foregoing Account with the Books and Vouchers and certify the same and the Investments and have inspected the Deeds of Down House and the Mortgage on Approved.

ARTHUR L. BOWLEY,
*Hon. Auditor.*†

*July* 16, 1930.

† Prof. A. W. Kirkaldy, Hon. Auditor, was unavoidably prevented from attending the audit.

# June 30, 1930—*continued.*

| Corresponding Figures, June 30, 1929. £ s. d. | ASSETS—*continued.* | £ s. d. | £ s. d. |
|---|---|---|---|
| *43,742 8 11* | Brought forward | | 44,879 7 9 |
| | By *South African Association Medal Fund*— Cash at Bank | | 200 0 0 |
| | ,, *Mr. G. Buckston Browne's Gift* in memory of Darwin—Down House, Kent | | Not valued. |
| | ,, *Do. Endowment Fund*— | | |
| | £5,500 India 4½ per cent. Stock 1958/68 at cost | 5,001 17 4 | |
| | £2,500 Australia 5 per cent. Stock 1945/75 at cost | 2,468 19 0 | |
| | £3,000 Fishguard & Rosslare Railway 3½ per cent. Guaranteed Preference Stock at cost | 2,139 17 3 | |
| | £2,500 New South Wales 5 per cent. 1945/65 Stock at cost | 2,467 7 9 | |
| | £2,500 Western Australia 5 per cent. Stock 1945/75 at cost | 2,472 1 6 | |
| | £3,340 Great Western Railway 5 per cent. Guaranteed Stock at cost | 3,436 7 5 | |
| | £2,500 Birkenhead Railway 4 per cent. Consolidated Stock at cost | 2,013 9 9 | |
| *20,000 0 0* | | | 20,000 0 0 |
| | *£19,247 6s. 0d.* (Value at date, £18,486 12*s.* 0*d.*) | | |
| | ,, *Revenue Account*— Investments:— | | |
| | £2,098 1*s.* 9*d.* Consolidated 2½ per cent. Stock at cost | 1,200 0 0 | |
| | £4,338 6*s.* 2*d.* Conversion 3½ per cent. Stock at cost | 3,300 0 0 | |
| | £400 5 per cent. War Loan Inscribed Stock at cost | 404 16 0 | |
| | | 4,904 16 0 | |
| | *£4,854 8s. 4d.* (Value at date, £4,950 16*s.* 8*d.*) | | |
| | Second Mortgage on Isleworth House, Orpington | 700 0 0 | |
| | ,, Down House Suspense Account— | | |
| | As per last Account | 938 7 0 | |
| | Purchase of land adjoining Down House | 275 0 0 | |
| | ,, Down House—Income and Expenditure Account | | |
| | Balance at July 1, 1929 . . . £699 13 7 | | |
| | *Add* Excess of Expenditure over Income for the year as per separate Income and Expenditure Account . . . 866 10 5 | | |
| | | 1,566 4 0 | |
| | Sundry Debtors and Payments in Advance | 717 8 11 | |
| | Do. (Down House) | 6 19 0 | |
| | Cash at Bank General Account . . . £2,626 15 6 | | |
| | *Less* Down House—Charges on General Fund . . . 2,582 17 0 | | |
| | | 43 18 6 | |
| *8,393 4 6* | Cash in hand | 78 14 11 | |
| | | | 9,231 8 4 |
| *72,135 13 5* | | | £74,310 16 1 |

to be correct. I have also verified the Balances at the Bankers
Isleworth House.

W. B. KEEN,
*Chartered Accountant.*

# Income and

FOR THE YEAR ENDED

## EXPENDITURE.

| Corresponding Period, June 30, 1929. £ | s. | d. | | £ | s. | d. | £ | s. | d. |
|---|---|---|---|---|---|---|---|---|---|
| *26* | *13* | *7* | To Heat, Lighting and Power | 20 | 10 | 4 | | | |
| *94* | *0* | *10* | ,, Stationery | 84 | 3 | 6 | | | |
| *1* | *0* | *0* | ,, Rent | 1 | 0 | 0 | | | |
| *201* | *14* | *6* | ,, Postages | 167 | 12 | 10 | | | |
| *438* | *11* | *9* | ,, Travelling Expenses | 118 | 10 | 5 | | | |
| *70* | *19* | *11* | ,, *Exhibitioners* | — | | | | | |
| *286* | *4* | *6* | ,, General Expenses | 229 | 18 | 6 | | | |
| *1,119* | *5* | *1* | | 621 | 15 | 7 | | | |
| *1,439* | *9* | *3* | ,, Salaries and Wages | 1,499 | 1 | 0 | | | |
| *75* | *0* | *0* | ,, Pension Contribution | 75 | 0 | 0 | | | |
| *2,882* | *0* | *3* | ,, Printing, Binding, etc. | 1,060 | 5 | 11 | 3,256 | 2 | 6 |
| *5,515* | *14* | *7* | | | | | | | |
| *25* | *0* | *0* | ,, *Dr. Klercker's Research Committee, Donation per contra* | | | | — | | |
| — | | | ,, Zimbabwe Loan Exhibition—Catalogue, etc. | | | | 52 | 3 | 10 |
| *80* | *0* | *0* | ,, *Prof. T. T. Barnard, for excavations at Bambata* | | | | — | | |
| *250* | *0* | *0* | ,, *Miss Caton-Thompson, for Zimbabwe excavations, per contra* | | | | — | | |
| | | | ,, Grants to Research Committees— | | | | | | |
| | | | Geography in Schools and Training Colleges Committee | 3 | 5 | 0 | | | |
| | | | Derbyshire Caves Committee | 50 | 0 | 0 | | | |
| | | | Mycorrhiza in relation to Forestry Committee | 50 | 0 | 0 | | | |
| | | | Uplands Bog Waters Committee | 12 | 19 | 0 | | | |
| | | | Tropical Africa Committee | 26 | 3 | 2 | | | |
| | | | Mathematical Tables Committee | 30 | 0 | 0 | | | |
| | | | Macedonia Committee | 50 | 0 | 0 | | | |
| | | | Formal Training Committee | 1 | 18 | 7 | | | |
| | | | General Science in Schools Committee | 1 | 16 | 0 | | | |
| | | | Educational Films Committee | 15 | 0 | 0 | | | |
| | | | Plymouth Table Committee | 50 | 0 | 0 | | | |
| | | | Taxation Committee | 30 | 0 | 0 | | | |
| | | | Zoological Record Committee | 50 | 0 | 0 | | | |
| | | | Palæozoic Rocks Committee | 10 | 0 | 0 | | | |
| | | | Vocational Tests Committee | 50 | 0 | 0 | | | |
| *888* | *2* | *1* | | | | | 431 | 1 | 9 |
| *24* | *10* | *2* | ,, *Law Costs of Second Mortgage, Isleworth House* | | | | — | | |
| | | | ,, Balance being excess of Income over Expenditure for the year | | | | 2,437 | 8 | 0 |
| *6,783* | *6* | *10* | | | | | £6,176 | 16 | 1 |

# Caird

## EXPENDITURE.

| £ | s. | d. | | £ | s. | d. | £ | s. | d. |
|---|---|---|---|---|---|---|---|---|---|
| | | | To Grants paid— | | | | | | |
| | | | Seismology Committee | 100 | 0 | 0 | | | |
| *950* | *0* | *0* | Naples Tables Committee | 100 | 0 | 0 | 200 | 0 | 0 |
| — | | | ,, Balance, being excess of Income over Expenditure for the year | | | | 163 | 8 | 8 |
| *950* | *0* | *0* | | | | | £363 | 8 | 8 |

# Expenditure Account

JUNE 30, 1930.

## INCOME.

| Corresponding Period June 30, 1929. £ s. d. | | £ s. d. | £ s. d. |
|---|---|---|---|
| *185 0 0* | By Annual Regular Members (including £52, 1930/31) | | 149 0 0 |
| *1,944 0 0* | ,, Annual Temporary Members (including £419, 1930/31) | | 1,252 5 0 |
| *834 0 0* | ,, Annual Members with Report (including £171, 1930/31) | | 273 0 0 |
| *71 5 0* | ,, Transferable Tickets for 1930/31 | | 10 0 0 |
| *91 10 0* | ,, Students' Tickets (including £10, 1930/31) | | 87 10 0 |
| — | ,, Unexpended balance of Donations in aid of Expenses—South African Meeting | | 1,523 5 1 |
| *25 0 0* | ,, *Donation Dr. Klercker for Research, per contra* | | — |
| *80 9 3* | ,, *Do. (Fcs. 10,000), per L'Abbé Breuil, for Prof. T. T. Barnard's Excavations* | | — |
| *250 0 0* | ,, *Do. Rhodes Trustees for Zimbabwe Excavations, per contra* | | — |
| — | ,, Zimbabwe Loan Exhibition—Sale of Catalogues | | 15 19 0 |
| *5 0 0* | ,, Lift Rent | | 1 17 6 |
| *104 5 6* | ,, Interest on Deposit | | 70 9 0 |
| *568 4 1* | ,, Sale of Publications | | 551 16 8 |
| *241 2 9* | ,, Advertisement Revenue | | 227 1 9 |
| *602 0 6* | ,, Income Tax Recoverable | | 264 3 9 |
| *37 16 11* | ,, *Unexpended Balance of Grants Returned* | | — |
| *22 10 0* | ,, *Liverpool Exhibitioners* | | — |
| *29 11 11* | ,, *Royal Charter Expenses—Unexpended Balance transferred* | | — |
| | ,, Dividends— | | |
| | *£135 0 0* Consols | 135 0 0 | |
| | *86 8 0* India 3 per cent. | 86 8 0 | |
| | *26 15 2* Great Indian Peninsular Railway 'B' Annuity | 26 18 2 | |
| | *34 6 0* 4½ per cent. Conversion Loan | 34 6 0 | |
| | *370 16 0* Do. Sir C. Parson's Gift | 370 16 0 | |
| | *53 6 6* Local Loans | 56 13 4 | |
| | *78 12 6* War Stock | 77 15 0 | |
| | *428 14 6* Do. Series 'A,' Sir A. Yarrow's Gift | 461 2 0 | |
| | *130 12 4* 3½ per cent. Conversion Loan | 130 12 4 | |
| *1,344 11 0* | | | 1,379 10 10 |
| | By Sir Alfred Yarrow's Gift— | | |
| | Proceeds of Sale of £334 2*s.* 3*d.* War Loan in accordance with the terms of the Gift | 334 2 3 | |
| *315 0 0* | Profit on Sale | 8 15 3 | |
| | | | 342 17 6 |
| *16 8 10* | ,, Interest on Mortgage | | 28 0 0 |
| *15 11 1* | ,, *Balance being excess of Expenditure over Income for the year* | | — |
| *6,783 6 10* | | | £6,176 16 1 |

# Fund.

## INCOME.

| £ s. d. | | £ s. d. | £ s. d. |
|---|---|---|---|
| | By Dividends— | | |
| | India 3½ per cent. Stock | 73 11 0 | |
| | Canada 3½ per cent. Stock | 70 0 0 | |
| | London, Midland & Scottish Railway Consolidated 4 per cent Preference Stock | 67 4 0 | |
| | Southern Railway Consolidated 5 per cent. Preference Stock | 80 0 0 | |
| *290 15 0* | | | 290 15 0 |
| *145 7 4* | ,, Income Tax Recoverable | | 72 13 8 |
| *513 17 8* | ,, *Balance being excess of Expenditure over Income for the year* | | — |
| *950 0 0* | | | £363 8 8 |

## Down House,

### EXPENDITURE.

| Corresponding Figures, June 30, 1929. £ s. d. | | £ s. d. | £ s. d. |
|---|---|---|---|
| 126 0 6 | To Wages of Staff | | 754 17 11 |
| 12 9 3 | ,, Rates, Insurance, etc. | | 149 2 7 |
| 111 3 9 | ,, Heat, Light and Drainage | | 115 6 2 |
| 277 6 7 | ,, Repairs and Renewals | | 27 3 4 |
| — | ,, House and Garden Sundries | | 37 1 9 |
| 11 1 5 | ,, General Expenses | | 56 7 11 |
| 11 2 3 | ,, *Photographs* | | — |
| — | ,, Photo Postcards | | 15 12 8 |
| 92 7 6 | ,, *Printing Booklet* | | — |
| 192 0 6 | ,, *Opening Ceremony, Catering, etc.* | | — |
| 833 11 9 | | | £1,155 12 4 |
| 497 15 5 | To Balance brought down | | 201 2 5 |
| 183 15 5 | ,, House and Garden Equipment, Repairs, Renewals and alterations to Buildings, Fences, Paths, etc. | | 482 4 0 |
| | ,, Law Costs— | | |
| | Rates Appeal, etc. | 121 10 6 | |
| | Tithe Redemption | 39 13 8 | |
| 18 2 9 | Purchase of Land | 21 19 10 | 183 4 0 |
| 699 13 7 | | | £866 10 5 |

# June 30, 1930.

## INCOME.

| Corresponding Figures, June 30, 1929. £ s. d. | | £ s. d. | £ s. d. |
|---|---|---|---|
| 115 8 4 | By Rents Receivable | | 86 16 8 |
| | ,, Income Tax Recoverable | | 55 2 0 |
| | ,, Donations | | 14 17 9 |
| | ,, Sale of Postcards | | 6 14 7 |
| | ,, Dividends— | | |
| | 4½ per cent. India Stock | 194 18 2 | |
| | Fishguard & Rosslare Railway 3½ per cent. Stock | 84 0 0 | |
| | New South Wales 5 per cent. Stock | 98 8 9 | |
| | Great Western Railway 5 per cent. Stock | 133 12 0 | |
| | Australia 5 per cent. Stock, 1945/75 | 100 0 0 | |
| | Western Australia 5 per cent. Stock | 100 0 0 | |
| | Birkenhead Railway 4 per cent. Stock | 80 0 0 | |
| 220 8 0 | | | 790 18 11 |
| 497 15 5 | ,, Balance carried down | | 201 2 5 |
| 833 11 9 | | | £1,155 12 4 |
| 699 13 7 | By Balance being excess of Expenditure over Income for the year | | 866 10 5 |
| 699 13 7 | | | £866 10 5 |

# RESEARCH COMMITTEES, Etc.

## APPOINTED BY THE GENERAL COMMITTEE, MEETING IN BRISTOL, 1930.

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

### SECTION A.—MATHEMATICAL AND PHYSICAL SCIENCES.

Seismological Investigations.—Sir Henry Lyons (*Chairman*), Mr. J. J. Shaw (*Secretary*), Mr. C. Vernon Boys, Dr. J. E. Crombie, Dr. C. Davison, Sir F. W. Dyson, Sir R. T. Glazebrook, Mr. Wilfred Hall, Dr. H. Jeffreys, Prof. H. Lamb, Sir J. Larmor, Prof. A. E. H. Love, Prof. H. M. Macdonald, Prof. E. A. Milne, Dr. A. Crichton Mitchell, Mr. R. D. Oldham, Prof. H. C. Plummer, Prof. A. O. Rankine, Rev. J. P. Rowland, S.J., Prof. R. A. Sampson, Sir A. Schuster, Sir Napier Shaw, Capt. H. Shaw, Dr. F. E. Smith, Mr. R. Stoneley, Sir G. T. Walker, Dr. F. J. W. Whipple. **£200.** (Caird Fund grant.)

Calculation of Mathematical Tables.—Prof. J. W. Nicholson (*Chairman*), Dr. L. J. Comrie (*Secretary*), Prof. A. Lodge (*Vice-Chairman*), Dr. R. A. Fisher (*General Editor*), Dr. J. R. Airey, Dr. A. T. Doodson, Dr. J. Henderson, Mr. J. O. Irwin, Prof. A. E. H. Love, Prof. E. H. Neville, Dr. A. J. Thompson, Dr. J. F. Tocher, Mr. T. Whitwell, Dr. J. Wishart. **£65.** (Caird Fund grant.)

### SECTION B.—CHEMISTRY.

Absorption Spectra and Chemical Constitution of Organic Compounds.—Prof. I. M. Heilbron (*Chairman*), Prof. E. C. C. Baly (*Secretary*), Prof. A. W. Stewart.

To Collect and Tabulate all available data on the Parachors of Chemical Compounds with a view to their subsequent publication.—Dr. N. V. Sidgwick (*Chairman*), Dr. S. Sugden (*Secretary*), Dr. N. K. Adams. **£10.**

### SECTION C.—GEOLOGY.

To excavate Critical Sections in the Palæozoic Rocks of England and Wales.—Prof. W. W. Watts (*Chairman*), Prof. W. G. Fearnsides (*Secretary*), Mr. W. S. Bisat, Dr. H. Bolton, Prof. W. S. Boulton, Dr. E. S. Cobbold, Prof. A. H. Cox, Mr. E. E. L. Dixon, Dr. Gertrude Elles, Prof. E. J. Garwood, Prof. H. L. Hawkins, Prof. V. C. Illing, Prof. O. T. Jones, Prof. J. E. Marr, Dr. F. J. North, Mr. J. Pringle, Dr. T. F. Sibly, Dr. W. K. Spencer, Dr. A. E. Trueman, Dr. F. S. Wallis. **£25.**

The Collection, Preservation, and Systematic Registration of Photographs of Geological Interest.—Prof. E. J. Garwood (*Chairman*), Prof. S. H. Reynolds (*Secretary*), Mr. C. V. Crook, Mr. E. G. W. Elliott, Mr. J. F. Jackson, Mr. J. Ranson, Prof. W. W. Watts, Mr. R. J. Welch.

To investigate Critical Sections in the Tertiary and Cretaceous Rocks of the London Area. To tabulate and preserve records of new excavations in that area.—Prof. W. T. Gordon (*Chairman*), Dr. S. W. Wooldridge (*Secretary*), Mr. H. C. Berdinner, Prof. P. G. H. Boswell, Miss M. C. Crosfield, Mr. F. Gosling, Prof. H. L. Hawkins, Prof. G. Hickling. **£15.**

The Stratigraphy and structure of the Palæozoic Sedimentary Rocks of West Cornwall. — (*Chairman*), Mr. E. H. Davison (*Secretary*), Mr. H. G. Dines, Miss E. M. Lind Hendriks, Mr. S. Hall, Dr. S. W. Wooldridge.

### SECTIONS C, D, E, H.—GEOLOGY, ZOOLOGY, GEOGRAPHY, ANTHROPOLOGY.

Expedition to investigate the Biology, Geology, and Geography of Lakes Baringo and Rudolf, Northern Kenya and Lake Edward, Uganda.—Prof. J. S. Gardiner (*Chairman*), E. B. Worthington and J. T. Saunders (*Secretaries*), Dr. W. T. Calman, Prof. J. W. Gregory, Prof. R. N. Rudmose Brown, Dr. L. S. B. Leakey. **£200.**

## SECTIONS C, D, E, K.—GEOLOGY, ZOOLOGY, GEOGRAPHY, BOTANY.

To organise an expedition to investigate the Biology, Geology, and Geography of the Australian Great Barrier Reef.—Rt. Hon. Sir M. Nathan (*Chairman*), Prof. J. Stanley Gardiner and Mr. F. A. Potts (*Secretaries*), Sir Edgeworth David, Prof. W. T. Gordon, Prof. A. C. Seward, and Dr. Herbert H. Thomas (*from Section C*); Mr. E. Heron Allen, Dr. E. J. Allen, Prof. J. H. Ashworth, Dr. G. P. Bidder, Dr. W. T. Calman, Sir Sidney Harmer, Dr. C. M. Yonge (*from Section D*); Dr. R. N. Rudmose Brown, Sir G. Lenox Conyngham, Mr. F. Debenham, Admiral Douglas, Mr. A. R. Hinks (*from Section E*); Prof. F. E. Fritsch, Dr. Margery Knight, Prof. A. C. Seward (*from Section K*).

## SECTION D.—ZOOLOGY.

Zoological Bibliography and Publication.—Prof. E. B. Poulton (*Chairman*), Dr. F. A. Bather (*Secretary*), Mr. E. Heron-Allen, Dr. W. T. Calman, Sir P. Chalmers Mitchell, Mr. W. L. Sclater.

To nominate competent Naturalists to perform definite pieces of work at the Marine Laboratory, Plymouth.—Prof. J. H. Ashworth (*Chairman and Secretary*), Prof. H. Graham Cannon, Prof. J. Stanley Gardiner, Prof. S. J. Hickson. **£50.**

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

On the Influence of the Sex Physiology of the Parents on the Sex-Ratio of the Offspring.—Prof. J. H. Orton (*Chairman*), Mrs. Bisbee (*Secretary*), Prof. Carr-Saunders, Miss E. C. Herdman. **£5.**

To consider the position of Animal Biology in the School Curriculum and matters relating thereto.—Prof. R. D. Laurie (*Chairman and Secretary*), Mr. H. W. Ballance, Dr. Kathleen E. Carpenter, Mr. O. H. Latter, Prof. E. W. MacBride, Miss M. McNicol, Miss A. J. Prothero, Prof. W. M. Tattersall.

A Preliminary Survey of Certain Tropical Lakes in Kenya in 1929.—Prof. J. Stanley Gardiner (*Chairman*), Miss P. M. Jenkin (*Secretary*), Dr. W. T. Calman, Prof. J. Graham Kerr, Mr. J. T. Saunders.

To try to arrange for the observation and recording of changes in the Flora and Fauna of St. Kilda since its evacuation.—Prof. J. Ritchie (*Chairman*), Prof. F. A. E. Crew (*Secretary*), Dr. A. Bowman, Prof. J. Graham Kerr, Dr. G. H. O'Donoghue.

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

To aid competent investigators selected by the Committee to carry on definite pieces of work at the Zoological Station at Naples.—Prof. E. S. Goodrich (*Chairman*), Prof. J. H. Ashworth (*Secretary*), Prof. E. W. MacBride, Prof. H. Munro Fox, Dr. M. Knight, Prof. J. H. Priestley, Prof. C. Lovatt Evans, Prof. B. A. McSwiney. **£50.** (Caird Fund grant.)

## SECTIONS D, K.—ZOOLOGY, BOTANY.

The equipment of a Fresh-water Biological Station at Windermere subject to the support of other bodies being forthcoming.—Prof. M. Drummond, Prof. F. E. Fritsch, Dr. B. Millard Griffiths, Dr. C. H. O'Donoghue, Mr. J. Omer Cooper, Miss P. M. Jenkin. **£40.**

## SECTION E.—GEOGRAPHY.

To report further as to the method of construction and reproduction of a Population Map of Great Britain with a view to the census of 1931.—Brig. H. S. L. Winterbotham (*Chairman*), Mr. J. Cossar (*Secretary*), Mr. A. G. Ogilvie, Mr. J. Bartholomew, Mr. H. O. Beckit, Mr. F. Debenham, Prof. C. B. Fawcett, Prof. H. J. Fleure, Mr. H. King, Mr. R. H. Kinvig, Brig. E. M. Jack, Prof. O. H. T. Rishbeth, Prof. P. M. Roxby, Mr. A. Stevens. **£25.**

To inquire into the present state of Knowledge of the Human Geography of Tropical Africa, and to make recommendations for furtherance and development.—Prof. P. M. Roxby (*Chairman*), Mr. A. G. Ogilvie (*Secretary*), Mr. S. J. K. Baker, Prof. C. B. Fawcett, Prof. H. J. Fleure, Mr. E. B. Haddon, Mr. J. McFarlane, Mr. R. A. Pelham, Mr. R. U. Sayce, Col. H. S. L. Winterbotham. **£10.**

## SECTIONS E, L.—GEOGRAPHY, EDUCATION.

To report on the present position of Geographical Teaching in Schools, and of Geography in the training of teachers; to formulate suggestions for a syllabus for the teaching of geography both to Matriculation Standard and in Advanced Courses and to report, as occasion arises, to Council through the Organising Committee of Section E upon the practical working of Regulations issued by the Board of Education (including the Scottish Education Department) affecting the position of Geography in Schools and Training Colleges.—Prof. Sir T. P. Nunn (*Chairman*), Mr. L. Brooks (*Secretary*), Mr. A. B. Archer, Mr. C. C. Carter, Mr. J. Cossar, Prof. H. J. Fleure, Mr. O. J. R. Howarth, Mr. H. E. M. Icely, Mr. J. McFarlane, Rt. Hon. Sir Halford J. Mackinder, Prof. J. L. Myres, Dr. Marion Newbigin, Mr. A. G. Ogilvie, Mr. A. Stevens, Prof. C. B. Fawcett (*from Section E*); Mr. C. E. Browne, Sir R. Gregory, Mr. E. R. Thomas, Miss O. Wright, Prof. Godfrey Thomson (*from Section L*). **£4.**

## SECTION G.—ENGINEERING.

Earth Pressures.—Mr. F. E. Wentworth-Sheilds (*Chairman*), Dr. J. S. Owens (*Secretary*), Prof. A. Barr, Prof. G. Cook, Mr. T. E. N. Fargher, Prof. A. R. Fulton, Prof. F. C. Lea, Prof. R. V. Southwell, Dr. R. E. Stradling, Dr. W. N. Thomas, Mr. E. G. Walker, Mr. J. S. Wilson.

Electrical Terms and Definitions.—Prof. Sir J. B. Henderson (*Chairman*), Prof. F. G. Baily and Prof. G. W. O. Howe (*Secretaries*), Prof. W. Cramp, Prof. W. H. Eccles, Prof. C. L. Fortescue, Prof. A. E. Kennelly, Prof. E. W. Marchant, Dr. F. E. Smith, Prof. L. R. Wilberforce, with Dr. A. Russell.

Stresses in overstrained materials.—Sir Henry Fowler (*Chairman*), Mr. J. G. Docherty (*Secretary*), Prof. G. Cook, Prof. B. P. Haigh, Mr. J. S. Wilson.

## SECTION H.—ANTHROPOLOGY.

To report on the Distribution of Bronze Age Implements.—Prof. J. L. Myres (*Chairman*), Mr. H. J. E. Peake (*Secretary*), Mr. A. Leslie Armstrong, Mr. H. Balfour, Prof. T. H. Bryce, Mr. L. H. Dudley Buxton, Prof. V. Gordon Childe, Mr. O. G. S. Crawford, Prof. H. J. Fleure, Dr. Cyril Fox, Mr. G. A. Garfitt. **£50.** (Caird Fund grant.)

To excavate Early Sites in Macedonia.—Prof. J. L. Myres (*Chairman*), Mr. S. Casson (*Secretary*), Dr. W. L. H. Duckworth, Mr. M. Thompson. **£25.**

To report on the Classification and Distribution of Rude Stone Monuments.—Mr. G. A. Garfitt (*Chairman*), Miss M. A. Murray (*Secretary*), Mr. A. L. Armstrong, Mr. H. Balfour, Dr. Cyril Fox, Prof. O. T. Jones, Mr. H. J. E. Peake.

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# THE PRESIDENTIAL ADDRESS.

## SIZE AND FORM IN PLANTS.

BY

PROFESSOR F. O. BOWER, Sc.D., D.Sc., LL.D., F.R.S.,
PRESIDENT OF THE ASSOCIATION.

Two years have passed since the Association last met in Britain. Events have happened in that interval which mark the close of the Darwinian Epoch. Down House, in which Darwin lived and worked, has been bought, restored and endowed by Mr. Buckston Browne and presented by him to the Association, who hold it in custody for the Nation. The house is now open as a shrine to those who treasure Darwin's memory. They may enter the study where the 'Origin of Species' was penned, or wander out to the Sand Walk, and draw such inspiration as those spots may yet afford to those who are face to face with problems cognate to his own. These years have also severed personal links with Darwin himself. Sir William Thiselton-Dyer, who died in December 1928, had been his frequent correspondent. It was he who, more than any other, carried the evolutionary stimulus forward into the botanical schools of Britain. Sir Edwin Ray Lankester, whose portrait by Orpen was a poignant feature of last year's Academy, died in August 1929. Not only was he the leading Zoologist of his time, but he has left a deep impress on general Morphology; for he was the first to analyse from the evolutionary aspect the degrees of 'sameness' of parts, whether in animals or in plants. These two octogenarians were among the latest links between Darwin himself and living men of science. And so this last meeting of the Association before its centenary next year falls at a nodal point in the personal history of Evolution.

Morphology, or the study of Form, was closely interwoven with the life's work of Darwin, and—to use his own words—'it is one of the most interesting departments of natural history, and may almost be said to be its very soul.' Since the Association has seen fit to choose as this year's President a botanist whose work has dealt specially with form in plants, the occasion seems apt for considering certain morphological questions that present themselves in this eighth decade since the ‘ Origin of Species ’ was published.

The word ‘ Morphology ’ was applied by Goethe in 1817, in a general sense, to the study of form. Though a pre-Darwinian, he showed rare foresight in insisting that the living form is only momentarily stable, never permanent. But years elapsed before that instability of form of living things, which he clearly saw, became the very focus of evolutionary theory. Even Goethe's prophetic gaze was blurred by the hazy imaginings of Idealistic Philosophy. The clarifying mind of Schleiden resolved that mist by resort to naked fact. In 1845 he stoutly asserted that the history of development is the true foundation for all insight into living form. This opened the way for a host of workers, who patiently observed and compared the facts of individual development, particularly in plants of low organisation. By them the field was prepared for the magic touch of Darwin ; and, in the enthusiastic words of Sachs, ‘ the theory of descent had only to accept what genetic morphology had actually brought to view.’

The effect of that theory should have been to sweep aside all Idealistic Morphology based on the higher forms, and to rivet attention upon organisms low in the scale. It was the habit of starting comparison from the highest state of organisation that was the fundamental error of the idealistic nature-philosophers ; even now traces of it still persist. An illuminating alternative was presented by that noble passage with which the ‘ Origin of Species ’ ends. Speaking of his theory, Darwin wrote : ‘ There is a grandeur in this view of life, with its several powers, having been originally breathed by the Creator into a few forms, or into one ; and that—from so simple a beginning endless forms most beautiful and most wonderful have been, and are being evolved.’ He forecast from the application of his theory that ‘ our classifications will come to be, as far as they can be so made, genealogies ; and they will then truly give what may be called the plan of creation.’

Whether there was only one original form of life or many is still an open question. Nevertheless, among the welter of organisms rightly held as primitive, the Flagellata may with some degree of reason be named as combining in their motile and sedentary stages respectively the animal and vegetable characters. They suggest a sort of starting-point from which the two kingdoms might have diverged. The probability of their common origin is strong ; but the divergence must have been early, each taking its own independent course, with increasing size and complexity of the individual. In tracing this I would ask your special attention this evening to the Kingdom of Plants.

The first of the laws laid down by Lamarck [1] in his *Histoire Naturelle* as fundamental in the evolution of animals and plants ran thus: 'Life by its intrinsic forces tends to increase the volume of every living body, and to enlarge its parts up to a limit which it determines itself.' When in unicellular organisms, following this law, a certain size has been reached, fission follows, and the equal halves separate as new individuals. In pluricellular bodies, however, the products of cell-division do not separate, but continue a communal life; and the individual may increase, with further division of its cells, to large size and complexity. We may picture how, based upon the mobile stage of a Flagellate, the aggregate might form an animal body with motility as a leading feature; on the other hand, based upon the sedentary stage, an immobile plant-body would result. The animal, adopting a predatory habit and colourless, might progress along lines of dependent nutrition, finding and ingesting food already organised; the sedentary green plant might evolve along lines of physiological independence, constructing its own organic supplies. Whether or not this be a true picture, the whole organisation of the two kingdoms diverged on the basis of nutrition. Herbert Spencer contrasted them physiologically, showing how animals are expenders, while plants are accumulators; that the former are limited in their growth by the balance of expenditure against nutrition; in the latter growth is not so limited. Thus, the problems that follow on increasing size may be expected to work out differently in view of the animal kingdom comprising organisms of high expenditure and not self-nourishing, while plants are self-nourishing accumulators.

The result of this difference may be illustrated by contrasting some of the highest examples of either kingdom; for instance, the elephant, with the trees of the forest through which he roams. On the one hand, the relative fewness of the mobile elephants, their less stature and compact form, their columnar legs needed to support the barrel-like body, the receptacle for ingested food, the economy of external surface and the highly developed internal surfaces. On the other hand, the height, immobility and large number of the trees, with their massive stems and

[1] Lamarck died in 1829, and the Association has contributed to a fund being raised for a Memorial by the Linnæn Society of Northern France.

highly complex shoots and roots, so necessary for acquiring food directly from the air and soil. We may further contrast the genesis of the individual in either case. In the mammal the parts are formed once for all, its embryology being an incident closed early in the individual life; but in the tree embryology may be continued for centuries, and is theoretically unlimited, except by death; during life it has the power of producing leaves and branches from every distal bud. The fact is that, though certain underlying principles are the same for both kingdoms, the working out has been distinct from the first. Hence, the morphology of plants must stand on its own feet; indeed, it has been said with some degree of truth that whenever botanists have borrowed their morphological outlook from the sister science they have gone wrong.

The normal development of a multicellular plant starts from the fertilised egg, and elaboration, both external and internal, follows on increasing size. Polarity, that is the distinction of apex and base, is defined in most plants of high organisation by the first cell-cleavage. The apex adopts at once the continued development that is its characteristic. Branching of various types follows in all but the simplest, to constitute the complex shoot, while correlative basal branching gives the root-system that fixes the non-motile body in the soil. The scheme of growth and branching thus started is theoretically open to unlimited increase, and the initiation of new parts is in point of number on a geometrical scale. This is suitable enough for organisms able to accumulate material, as plants do; indeed, the elaboration of the vegetative system will enhance its powers of self-nutrition, so far as the parts become functional; but this is never fully realised beyond the earlier steps.

The focus of all such development is the growing point, respectively of root or shoot. Anyone who carefully dissects a suitable bud, peeling off the successively smaller leaves, may finally see with the naked eye or with a simple lens a pearly cone of semi-transparent tissue at the tip of the stem. This is the growing point itself, which possesses theoretically unlimited formative power. It is like a permanent sector of the original embryo that is fed continually from the mature tissues below, and as continually forms fresh tissues at the tip. But as the tip advances, lateral swellings of the surface appear in due order, which are new leaves and buds. Various attempts have been made to link the genesis of these outgrowths of the radial shoot with the outer world as regards their position and number. But we have it as the latest authoritative statement on this point that such a relation does not exist. 'This much is proved,' says Prof. von Goebel, that, 'so far as we can see, the question relates to conditions of growth and symmetry that arise in the growing point.' 'All theories as to leaf-position that allotted a passive rôle to the growing point were mistaken, however acute the reasoning that was brought to bear thereon' (*Organographie.* 3rd Edn., part I, pp. 299–300). This is Von Goebel's summing up for external parts. On the other hand, within the growing point, and often, though not always, related to the external parts, there is a progressive formation of internal conducting tracts, continuous from the adult region upwards to the tip. A like reference of the origin and disposition of these vascular tracts to the growing point itself appears to be equally justified. In fact, the tip possesses the initiative for both.

The complex shoot that results from such initiation is exposed as it matures to external conditions which modify its form. Their effect is very obvious in the young shoot of the higher plants. As the shoot elongates its young tissues are soft and plastic. While in this state its form may be influenced by gravity, the incidence of light, mechanical contact and other causes which produce reactions of form called 'tropisms.' All of these promote the well-being of the whole. The net result becomes fixed as the part matures, and its constituent tissues harden. Thus, the adult form is the consequence of the primary initiation at the growing point, modified by the conditions to which the plant may have been exposed during the plastic period. This is a commonplace of the text-books. But amid all the careful analysis and experiment that has been devoted to the influences which thus affect form, one factor, insistent and unavoidable, has been habitually left out, viz., the influence of size. Reference is occasionally made in textbooks to the effect of surface-tension in determining the simple form in minute organisms, such as unicellular Algæ and Bacteria, and to the deviations from that simple form as the size increases, and the influence of surface-tension ceases to be dominant. At the other end of the scale of size mathematicians have calculated the extreme stature mechanically possible for a tree-trunk constructed after the ordinary plan, and of materials of known strength. The result is about 300 feet, and this coincides approximately with the limit of height of the canopy of a tropical forest. But in point of size practically the whole of the vegetable kingdom lies between the microbe and the forest tree. Unfortunately, the study of these middle terms, from the point of view of change of form as the size increases, has not been pursued by botanists with the same perception as zoologists have shown in the study of animals.

At the back of all problems raised by increasing size stands the well-known principle of similarity, which applies to all structures, inorganic as well as organic. It involves among other consequences that where form remains unaltered bulk increases as the cube, but surface only as the square of the linear dimensions. But in living organisms it is through the limiting surfaces, or 'presentation-surfaces,' as they are called, that physiological interchange is effected. Provided a surface be continuous and its character uniform, it may be assumed that such interchange will be proportional to the area of surface involved. If, then, the form of the growing organism or tissue were retained as at first—for instance, a simple sphere, oval or cylinder—its surfaces of transit would increase at a lower ratio than the bulk which they enclose. There would be with increase in size a constantly decreasing proportion of surface to bulk, and as constantly an approach to a point of physiological inefficiency. But any change from a simpler to a more complex form would tend to uphold the proportion of presentation-surface. Thus, the success of a growing organism might be promoted by elaboration of form. Naturally, other factors than that of size co-operate in determining form. Nevertheless, the recognition of such elaborations of form, whether external or internal, as do tend in point of fact to maintain a due proportion of surface to bulk as growth proceeds, should help to make morphology a rational study. The diffuse form habitual for plants, even the origin of leaves themselves, becomes intelligible from this point of view.

In the construction of any ordinary vascular plant there are three of these 'presentation-surfaces,' or limiting surfaces of transit, that are of prime importance: (i) the outer contour by which it faces the surrounding medium; (ii) the sheath of endodermis which envelops the primary conducting tracts; and (iii) that collective surface by which the dead, woody elements face upon the living cells that embed them, through which water and solutes pass in or out. Each of these may vary independently of the others, and each would be a fitting subject for observation as bearing on this problem of size. But as a test case of the relation between size and form, it is the collective surface where dead wood faces on living cells that will meet our requirements best, for its study can be pursued among fossils almost as well as in living plants. The problem is one not merely of current physiology of the higher plants, it is one of adaptive progress. Accordingly, measurements must be made of the wood of fossils as well as of living plants, and of young sporelings as well as of the adult.

We have seen that plants are essentially accumulators of material. A natural consequence of this is that primitive types, endowed with apical growth but with no secondary cambium, will enlarge from the base upwards. Any sporeling fern shows this. The leaves themselves increase in number; each successive leaf is as a rule larger than the one that came before, and the stem that bears them also expands upwards. In fact, it takes the form of an inverted cone. To grasp the size-problem for primitive plants the mind must be rid of the idea of the forest tree, with its stem tapering upwards, for that is a state of highly advanced organisation. The primitive form of stem is that of an inverted cone, enlarging upwards, with a solid core of wood within. A cone standing upon its tip is obviously unpractical. Not only is it mechanically unstable, but if the original structure be maintained so that the larger region above is structurally a mere magnified image of the smaller below, a constantly diminishing proportion of presentation-area to bulk must needs follow, in respect of all the limiting surfaces. Such stems would all tend to become physiologically insufficient. Our immediate problem is with the woody column. How can that due proportion of presentation-surface of the dead wood to the living cells, which physiologists hold to be essential, be maintained in the expanding stem, so as to meet the increasing requirements of transit and distribution of the sap?

This is not the place for a recital of the details of elaboration of the wood which have been observed and measured. It must suffice to state in general terms how primitive woody plants have met the difficulty in the absence of cambial thickening. The starting-point is a minute cylindrical strand composed of dead tracheids only. Some primitive types show nothing more than a conical enlargement of this upwards, with the cells more numerous than before. The approach of a locomotive at speed along a straight track may visually suggest such increase in size without change of form; successive photographs of it might be compared with successive sections of those simple stems enlarging upwards without change of plan. The largest examples of this are found in some of the early club mosses and ferns, in which there is an enlarging solid woody core. But for want of resource in this and other features they have paid the penalty of death. Most plants having this crude structure are known only as fossils, and no really large vascular plant lives to-day which shows it. Under present conditions it is only where the size is small that a simple mass of dead tracheids seems to be effective for water-transit. Thus we see that simple enlargement without change of form does not suffice.

In more resourceful plants a remedy is found in elaboration of the form and constitution of the primary wood. The changes which actually appear in it, as the size of the individual or of the race increases, are very various, but they all tend towards making the wood a living whole. The most efficient state would be that in which each dead woody cell or element faces upon one or more living cells, and this structure is approached in modern types of wood. In tracing the steps which have led towards it, whether in the fossil story or in the individual life of plants, we follow up an evolutionary history of high functional import. Actual measurements and calculations have shown in living plants the advantage that follows. It has been found that changes in the elaboration of form and structure of the primary woody column have saved, in specific instances, about 50 per cent. of the contingent loss in that proportion of presentation-surface to living tissue which would have followed if a simple cylindrical core had been retained. The structural changes do not, it is true, maintain the full original ratio of surface to bulk, but it may well be that saving even half of the contingent loss would bridge the acute risk and lead to survival.

The moulding and subdivision of the primary conducting tracts as a whole, or of the woody masses which they contain, present the most varied features. Their contours often appear arbitrary and even irrational, so long as no underlying principle is apprehended. They have presented a standing problem to anatomists. But when it is realised that as the size increases there is a physiological advantage in any elaboration of form whatsoever, a rational explanation is at hand. The variety of the forms assumed suggests the common principle underlying them all, which is that thereby a due proportion of presentation-surface tends to be maintained.

One of the simplest and most frequent examples of such elaboration of form is that of the fluted column, which in transverse section gives the familiar stellate figure characteristic of roots. It is also seen in many stems, and is described as 'radial.' Where the part is small the woody strand is roughly cylindrical, but where larger it often becomes fluted, with varying number and depth of the flanges. In many instances the ratio of their number to the diameter of the whole tract is approximately constant. The structure is in fact adjusted to the size. This is so in roots generally, in leafy stems and in leafless rhizomes—and a similar size-relation is even found in the fluted chloroplasts of certain Algæ. In all of these an obvious risk following an increase in size tends to be eliminated, viz., an undue loss of proportion of surface to bulk.

The somewhat technical facts thus briefly described may be taken as examples of a relation of form to size which is very general. They suggest the existence of a 'size-factor,' which is effective in determining form. The susceptibility to its influence resides in the part that shows the results. The internal contours are defined *ab initio*, instead of coming into existence during the course of development, as is the case with the convolutions of the mammalian brain. In the stem and roots of vascular plants the fully-matured conducting tracts may be traced upwards, with their outlines already defined, through successive stages of youth towards the growing point, which has been their source. Their form may be seen already outlined in its young tissue closely short of the extreme tip. This fact suggests that the susceptibility to the size-factor resides in the growing point itself, for immediately below it those tracts possess that form which will aid their function when they are fully developed.

Of all the factors that contribute to the determination of form in growing organisms there is none so constant and inevitable in its incidence as this size-relation. Its operation becomes manifest with the very first signs of differentiation of the embryonic tissues. The effects of other factors that influence form, such as gravity, light, temperature, contact and the rest appear later in point of time. Their influence is liable to diminish as the organism reacts to them by curvature or otherwise, and to vanish when the reaction is completed. Under experiment they may be controlled or even inhibited. But the operation of the size-factor is insistent; it cannot be avoided either under conditions of nature or by experiment, though the size itself may be varied under conditions of nutrition and the permeability of the presentation-surfaces may not be constant, with results as yet unknown. When we reflect that all acquisition of nourishment and transit of material in plants of primary construction is carried out through limiting surfaces, the essential importance of the size-factor is evident, for upon its influence the proportion of each presentation-surface itself depends.

The evidence that size itself is, among other factors, a determinant of form rests upon the constancy with which, in an enlarging organism, changes of primary form tend to maintain a due area of presentation-surface such as active transit demands. That evidence has been derived chiefly from the conducting tracts of primary individuals as they enlarge conically upwards, and from parts belonging to distinct categories, also from comparison of different individuals not necessarily of close alliance. Very cogent evidence lies in the variety of the changes of form by which the same end is attained. Finally, the converse facts bring conviction when, as often happens, a distal diminution of size in stem or leaf is accompanied by simplification along lines roughly the converse of those that follow increase. All this shows that a real relation exists between size and primary form. The term 'size-factor' has been used to connote that influence which affects form in relation to size, but without defining it except by its results. Nevertheless, we have seen that its action may be located in near proximity to the growing point, or in the embryo itself. It has not, however, been found possible to assign to that effect an immediate cause. The attitude thus adopted towards an undoubted factor seems justified by the broad logic of science, and by the practice of its highest votaries. When Newton put together his great physical synthesis he pointed out at the close of the *Principia* that the cause of gravitational force was unknown. 'Hitherto I have not been able to discover,' he said, 'the cause of these properties of gravity from phænomena, and I frame no hypotheses.' Likewise, in its own more restricted field of botanical phenomena, the size-factor may be recognised as effective in development, though the immediate cause of its effectiveness is still unknown.

The position thus adopted assumes the shoot to be a unit, not a congeries of 'phytons.' The elaboration of its form, whether external or internal, would be a function of the increase in size of that unit, and the result would tend to maintain the adequacy of the presentation-surfaces. This conception of the shoot and of its parts would accord with the views of General Smuts, as stated in his remarkable work on 'Holism,' published in 1926. Many present here to-day will have heard his Address in Cape Town last year, when opening the discussion on 'The Nature of Life.' All will value this masterly statement in brief of his theory. I suggest that the operation of the size-factor, whether in relation to external leaf-development or in the elaboration of internal conducting tracts, illustrates that 'measure of self-direction' ascribed by him to every living organism ('Holism,' p. 98).

The discussion of the problem of size and form in plants, which has occupied our attention thus far this evening, raises questions of profound significance in the sphere of pure botany. There is, however, another interest inherent in the study of plants beyond that of pure science. I mean botany as applied to the needs of man. To-day this touches human life more closely than ever before. Every meal we eat, many of the clothes we wear, timber, rubber, a whole volume in itself; the drugs, narcotics, dyes and scents, and most of that vast tale of accessories that ameliorate life, depend for their supply, quality and often for their existence upon the skilled work of the botanical expert. He is trained in our schools and universities. His experience there is perfected by work on farms and plantations, in forests and in factories, often by adventurous life abroad. It would be superfluous for me to enter into detail on such matters, for happily the director of Kew presides over the botanical section, and he can speak with the fullest knowledge on the application of botanical science to modern life.

Government Departments are now linked more closely than ever with universities and technical colleges by the golden chain of grants. The botanical institutes that have sprung from this joint source are mostly focussed at such centres as Kew and South Kensington, Cambridge and Oxford, Harpenden and Merton, Long Ashton and Corstorphine, Plymouth and Millport, with important outliers such as Dehra Dun in India, the Imperial College of Tropical Agriculture in Trinidad, and the Research Station at Amani, East Africa; while similar stations are to be found in Canada, at the Cape, in Australia and New Zealand. Their activities are as diverse as their position. Agriculture, forestry, plant-breeding and distribution, seed-testing, mycology and plant pathology—these are but a few of the headings under which Applied Botany is now pursued; and a duly qualified staff is required for each. Kew itself, thanks to the foresight of the Empire Marketing Board, is developing ever more and more as a co-ordinating centre for the whole Empire. Highly specialised study such as this has sprung into existence in the last half-century. As regards Britain, its origin may be traced to the biological laboratory of the old Normal School of Science at South Kensington, where biological research was revived under Huxley and Thiselton-Dyer.

The first botanist there trained in pure science who turned the newly acquired vision to practical account in the interests of the Empire was Marshall Ward. For two years he investigated the coffee disease that had half ruined Ceylon. It is a long step from this individual effort in the East to the firmly established and efficient Mycological Bureau, recently housed at Kew in a new building devoted to the world-wide study of the fungal diseases of plants. Such advance along a single line of Applied Botany may be taken as an index of the progress from simple beginnings in pure botany to that widespread attack now being made upon the economic problems that face Imperial Agriculture. The history of it thus briefly suggested may be read as a parable, showing how natural is the progression from the study of pure science to its practical application. For there is no real distinction between pure and applied science. As Huxley told us long ago, 'What people call applied science is nothing but the application of pure science to particular problems.'

At the moment there is an unprecedented demand for botanical specialists to fill investigational and advisory posts at home and abroad, and there is a shortage of applicants. The realisation of this will doubtless be transmitted through the universities and colleges to the schools of the country and lead to an increased supply. On the other hand, it lies with the Government to react as other markets do in taking steps to equalise supply and demand. A condition of the success of a specialist will always be a thorough foundation upon pure science, and this will be fully realised in the selection of candidates. Government, whether at home or in the wider Imperial field, can make no better investment than by the engagement of the best scientific experts available. In respect of botany this has been attested by many well-known instances.

Some reference will naturally be expected here to the remarkable Address given by Sir William Crookes in 1898, when the Association last met in Bristol. He then forecast that, in view of the increase in unit-consumption since 1871 and the low average of acre-yield, 'wheat cannot long retain its dominant position among the foodstuffs of the civilised world. Should all the wheat-growing countries add to their area to the utmost capacity, on the most careful calculation the yield would give us only just enough to supply the increase of population among bread-eaters till the year 1931. The details of the impending catastrophe,' he remarked, 'no one can predict, but its general direction is obvious enough.' The problem is one of applied botany, with a setting of world economics and a core of physical chemistry. After raising the spectre of wheat-shortage before the eyes of his audience of 1898, Crookes laid it again by the comforting words, 'The future can take care of itself. The artificial production of nitrate is clearly within view, and by its aid the land devoted to wheat can be brought up to the 30 bushels per acre standard.' We who are living within a few months of the fateful year of 1931 are unaware of any wheat-shortage. Sir William Crookes' forecast of 1898 as to the advance in the production of combined nitrogen has been fully realised. Artificial fertilisers are not in view only, but at hand and in mass. Moreover, the northern limit of successful wheat-culture has been greatly extended by the production of new strains with ever shortening period between sowing and reaping, while the establishment of new varieties is extending the productive area in South and West Australia into regions where the rainfall is of short duration, and restricted in amount. The future, since 1898, has indeed taken care of itself; so that, notwithstanding the warning of so great a man as Sir William Crookes, the wheat-eating public is still able to sleep well at night so far as the wheat-shortage is concerned. What better example than this could we desire, not only of the importance of applied botany, but as showing also how its advance follows on research independently pursued. For the production of synthetic nitrogen, which has now become a commercial proposition, and the improvement of the strains of wheat by selective breeding along Mendelian lines, are both involved in solving this crucial question of food-supply. And both owe their origin to advances in pure science.

In conclusion, we shall all be conscious of the fact that a most distinguished former president of the Association has lately passed away, one who more than any man has influenced the policy of government in relation to science. I mean Lord Balfour. We recall how in 1904 he, so thoroughly imbued with the spirit of his Alma Mater, presided over the meeting in Cambridge. He was distinguished as a philosopher, great as a statesman, and particularly so under the stress of war. He it was who, after peace returned, used his rare influence in transforming the war-time experiment of a committee of the Privy Council for Scientific and Industrial Research into a permanent and essential part of modern government. But this was not all. His critical, constructive and experienced mind was led to formulate a still wider plan. A Cabinet Committee for Civil Research was to be established on the lines of the Imperial Defence Committee. He designed it so as to bring the whole national administration within the range of scientific influence. The Department of Scientific and Industrial

Research, so wisely kept in being after 1919, now forms part of that larger scheme. This department is responsible for making recommendations as to the expenditure of funds voted by Parliament for research, especially in relation to industry. Thus science is welcomed into the inner circle of Imperial administration. This the State owes to Lord Balfour.

And so, in this hundredth year of its existence, the British Association sees research recognised and fostered in the service of the State in a way never dreamed of in 1831, when a small body of enthusiasts met at York for the advancement of science. But though the individual seeker after truth may thus be involved in official harness, as of old an inner voice will yet speak to him. He will himself be as near to Nature to-day as he was in the simpler days that are gone.

# THEORIES OF TERRESTRIAL MAGNETISM.

ADDRESS BY
DR. F. E. SMITH, C.B., C.B.E., SEC.R.S.,
PRESIDENT OF THE SECTION.

At last year's meeting of the British Association in South Africa, the following resolution was passed by Section A:

> 'To urge the importance of the establishment, on a suitable site in South Africa, of an observatory for the study of terrestrial magnetism and atmospheric electricity.
>
> 'The establishment of such an observatory would add very greatly to the accuracy and value of the magnetic survey of South Africa which is now in progress. The Committee desires to call attention, moreover, to the fact that at present there is only one magnetic observatory, viz. at Helwan, Egypt, and regular observations in South Africa are much needed for the study of the earth's magnetism.'

This resolution, which was subsequently approved by the Council of the Association, states the need for yet another observatory in the Southern Hemisphere in order to advance the solution of one of the most attractive problems ever presented to the physicist—the problem of terrestrial magnetism.

I would remind you, too, that last year in South Africa we had the great pleasure of being welcomed by Dr. Beattie, the Principal of Cape Town University, himself a great authority on terrestrial magnetism, and who, in this city, at the meeting of the British Association in 1898, announced that a magnetic survey of South Africa had been begun. At the Bristol meeting of 1898 terrestrial magnetism played a most important part. An international conference on the subject was held in affiliation with Section A, and Sir Arthur Rücker, then professor of physics at the Royal College of Science, who taught me most of the physics I know, was president of the conference.

When that meeting in Bristol was held I was a student of Rücker's, and my first real contact with the problem of terrestrial magnetism was made by experimenting with a model of Wilde's magnetarium. This was a globe 18 inches in diameter, inside which was a smaller globe with wire coils round it, the axis of the coils being in general inclined to the axis of the outer sphere, which represented the earth. Between the two spheres was a spherical shell of wire gauze supporting another coil system. With a current in the inner coil system it was easy to produce a symmetrical magnetic field about the outer sphere, but no true representation of the actual magnetic state of the earth could be obtained with currents in the two coil systems alone, and Dr. Wilde, after reasoning that the underwater portions of the earth were the more susceptible to magnetisation, covered with sheet iron those portions of the interior surface of the outer globe which corresponded to the oceans. Small sheets of iron were also placed on areas occupied by great mountain ranges. The results of experiments with this model were certainly interesting, for the general shape of the magnetic field of the earth was remarkably well reproduced. The main idea was that electric currents circulated in the inner regions of the earth which were supposed to be of higher conductivity than the outer crust, but in this latter currents of lower intensity also circulated, but their distribution was subject to considerable variations by the shielding effect of the ocean areas and mountain ranges. It is of interest to note that the modern view of the earth's conductivity is that of an outer shell 250 km. thick of comparatively low conductivity and an inner sphere to which a conductivity of about $3{\cdot}6 \times 10^{13}$ C.G.S. units is attributed, the magnetic permeability being unity.

Terrestrial magnetic science has a long history, but much of the data on which theories have been based are not very exact. The first conception of the earth as a great magnet appears to be due to Gilbert, but it was not until Gauss made his analysis in 1838 that the nature of the earth's magnetism and its distribution were made clear. Shortly after the work of Gauss international co-operation in magnetic work was initiated by Humboldt and Gauss and supported by Herschel, Kupffer and Sabine. Six observatories were established in Russia under the direction of Kupffer and three were established in the United States, another at Simla and one at Singapore. Originally it was proposed to carry on the work of these observatories for three years, but owing to delays the period was extended to six years, and so desirable did it appear to continue this international work that a Magnetic Congress was called and held at the Cambridge meeting of this Association in 1845.

The principal question which that conference had to decide was whether the combined system of British and foreign co-operation for the investigation of magnetic and meteorological phenomena, which had then been five years in progress, must be broken up.

It is scarcely necessary to say that the Congress was not divided on this question, and resolved

> 'that the cordial co-operation which has hitherto prevailed between the British and foreign magnetic and meteorological observatories, having produced most important results and being considered by us as absolutely essential to the success of the great system of combined observation which has been undertaken, it is earnestly recommended that the same spirit of co-operation should continue to prevail.'

The spirit of co-operation which existed then exists still, but I venture to ask 'Do we make our plans sufficiently well?'

When data obtained in a small laboratory cannot suffice for the elucidation of a problem, and when a chain of laboratories, either in one country or distributed over the surface of the earth, is known to be essential for its solution, not only is it necessary to maintain the most intimate contact between the workers, but it is also necessary to plan much of the work as if it were under single control. The International Union of Geodesy and Geophysics is the body to which we must look not only to co-ordinate the work of the various magnetic observatories but to plan lines of attack, and ensure, so far as is practicable, that the accumulated results are such as would be obtained under single control. While my object to-day is not to put forward any programme for the consideration of the International Committee of Geodesy and Geophysics, I do make a plea for the adoption by many of the first-class magnetic stations of a programme including observations at the same times and with similar instruments of great sensitivity. As an illustration of what might be done, take the question of the simultaneity of occurrence the world over of a large magnetic storm. We know such storms affect the instruments of most of the magnetic observatories of the world, and two views have been advanced with regard to the time of commencement of such disturbances; one that a disturbance occurs simultaneously at all stations, and the other that there are time differences of the order of a few minutes. A definite answer to the question, 'Are there such time differences and what is their magnitude,' could be given within a year or less if a few chosen observatories were equipped with precisely similar quick-running magnetographs in addition to their slow-running ones, and time was automatically recorded on the charts every minute. To eliminate, or at least reduce to negligible dimensions, any error due to differences in the time bases of reference, wireless time signals should also be automatically recorded on the charts. Some fifteen years ago I had the task of recording the magnetic disturbances due to an electric train system, and it was found possible to correlate the magnetic disturbance and the starting of an electric train more than a mile away, with an error not greater than one second. When one considers the praiseworthy work of Bauer and others on vertical earth-air electric currents, it is apparent that their labours would have been tremendously simplified and their conclusions of much greater value had all the instruments used been precisely similar and a definite programme laid down.

Lest I be misunderstood, I wish to emphasise the undesirability of magnetic observatories being all alike and all similarly equipped. Each observatory should have its own particular problems and its own special methods of attacking them and thus preserve its individuality but, in addition, part of the equipment should be of an international type and part of the programme should be of a truly international character.

The early investigators showed that the causes of the earth's magnetism and its variations were problems of great complexity, and the first line of attack was naturally to accumulate data. For the work done no one can fail to have admiration, and particularly I wish to pay tribute to the international work of the Department of Terrestrial Magnetism of the Carnegie Institution of Washington, and to express regret at the destruction by fire of the non-magnetic ship 'Carnegie' and the loss of life caused thereby. The untimely death of Captain Ault, the master and chief of the scientific personnel, is greatly regretted.

## General Character of Earth's Magnetic Field.

The fundamental problem of terrestrial magnetism is the cause of the earth's magnetism, and a secondary one the cause of the variations. Let us consider the facts. Measurements of the intensity, declination and inclination of the earth's magnetic field at thousands of stations distributed over practically the whole of the earth's surface, show that the magnetic field is roughly that of a uniformly magnetised sphere with its axis inclined at about 12° to that of the earth's axis of rotation. An analysis of results leads to the conclusion that the earth's uniform field is equivalent to that produced by a magnetic doublet at the centre of the earth, the magnetic moment of the doublet being $8.04 \times 10^{25}$ C.G.S. units. If I is the intensity of magnetisation of the sphere and $r$ the radius of the earth, the magnetic moment is given by $\frac{4}{3}\pi r^3 I$.

Examination of the observations shows also that the intensity of magnetisation of the earth is slowly changing, and that the earth's magnetic field is slowly moving from east to west, the magnetic poles performing circular or spiral motions about the geographical poles. In addition to this secular change, there are yearly and daily changes, which, although fairly constant in character, are changeable in magnitude. There are also at times very violent changes in the magnetic elements, such changes being known as magnetic storms.

In an attempt to find the cause of the earth's magnetism, it is natural to consider all the possible ways by which the earth can function as a magnet, and even if we have to assume knowledge of the constitution of the earth's interior, or assume the existence of electric currents, or assume some physical state in the earth or in the atmosphere above it, in order to account for the phenomena, we must not cast aside such assumptions, unless it can be shown that they are unnecessary, and until data are obtained which show the premises to be false.

The first and simplest theory is that the earth is a large permanent magnet, due to the magnetisation of the material of which it consists. Another hypothesis is that the electric charge on the earth's surface produces the magnetic field by its rotation. A third possibility is that the earth is an electromagnet, the magnetising currents being either outside the earth or within it; such a system was roughly illustrated by Wilde's magnetarium. Other theories postulate the field to be due almost entirely to electric currents circulating within the earth or to be due to the rotation of the earth.

Fortunately, very valuable criteria have been given by Gauss and by Schuster, the former showing that the main origin of the earth's magnetic field is within the earth, and the latter that the cause of the daily variations is external to the earth's surface. Any predominant magnetic effect due to external causes need not, therefore, be looked for.

In the first place, it is well to consider the main or permanent field, i.e. that part of the magnetic field due to causes within the earth.

Gauss, in his Memoir on Terrestrial Magnetism, expressed the magnetic potential of the surface field of the earth in a series of spherical harmonics. Some time afterwards, Everett pointed out that the first term of this series corresponds to a uniform magnetisation of the earth, and thus a physical interpretation was obtained. In this uniformly magnetised sphere the magnetic potential at any point outside the sphere is given by $H_e r^3 \cos \theta / d^2$ where

*d* is the distance of the point from the centre;
*r* is the radius of the earth;
$\theta$ the angular north polar distance,
and $H_e$ the surface magnetic intensity at the equator.

The north magnetic pole is situated approximately at 78° N. 69° W., and the whole magnetic field rotates with the earth.

## Secular Variation and Possible Cause.

What do we know of this so-called permanent field? As already stated, such variations as occur daily, monthly or suddenly, as in the case of magnetic storms, are phenomena to be associated with other terms of the Gaussian expansion, and are not to be attributed to changes in the main or permanent field. Indeed, our information with regard to changes in this field is very limited, and the difficulty that confronts us to explain the existence and maintenance of the field is correspondingly great. The observed facts are that the declination, inclination and intensity are not constant, and over long periods of time their values change appreciably. The changes over a comparatively short period are well illustrated by the results obtained at Kew Observatory during a period of 50 years.

| Year. | Declination. | Inclination. | Hor. Force. |
|---|---|---|---|
| 1860. . . . . . | 21·6° | 68·3° | 0·1752 |
| 1870. . . . . . | 20·3° | 68·0° | 0·1776 |
| 1880. . . . . . | 18·9° | 67·8° | 0·1799 |
| 1890. . . . . . | 17·9° | 67·6° | 0·1820 |
| 1900. . . . . . | 16·9° | 67·2° | 0·1840 |
| 1910. . . . . . | 16·0° | 67·0° | 0·1850 |

Very long period observations are not so reliable because of the dissimilarity of instruments and methods of observation, but it is certain that in London the declination has changed slowly from about 11° east in 1580 to 24° 30′ west in 1816, since when it has fallen to about 13° west in 1920. Similarly, the inclination has changed from about 72° 0′ in 1580 to 74° 42′ in 1720, since when it has gradually changed to about 66° 40′ in 1920. Examination of the available data leads to the conclusion that the magnetic field may be regarded as moving westwards along a parallel of latitude at the rate of a few seconds of angle per day, the rate of movement being such that, if continued for some hundreds of years, the field would make a complete revolution round the earth, the motion being in the opposite direction to that of the earth's rotation. The secular variation may therefore be regarded as caused by change in direction of the axis of magnetisation. If outer space is a conducting medium, there will be relative motion between the magnetic field of the earth and it, and the moving field will induce currents in the outer conducting medium, and these currents in turn will react and induce other currents and associated magnetic phenomena. There will also be mechanical reactions, and Schuster showed how these reactions can be calculated. It is certain that the induced currents must tend to destroy the motion of the inducing field, and that one effect must be to reduce the period of rotation. Such a reduction in the period of rotation would result even if the magnetic axis coincided with the axis of rotation, but when the two axes do not coincide there is another retarding couple acting on the magnetic field. A circular movement of the magnetic pole about the axis of rotation may be regarded as produced by two radial movements at right angles operating from that axis. Such motions of the magnetic field will induce currents in the conducting layer, and the reacting forces will tend to destroy the movements which produce them, that is, the tendency will be to make the two axes coincide. The total result is, therefore, to slow down and eventually destroy the rotation of the magnetic axis and to reduce the angle of separation of the two axes and eventually cause them to coincide. A bird's-eye view of the magnetic and geographical poles taken over a long period of time would reveal a spiral path for the magnetic pole, the latter drawing nearer and nearer to the geographical pole.

Schuster has calculated the value of the retarding couple for many variations of the conductivity of the outer medium. The retarding couple is zero when the conductivity is zero or infinitely great, and is a maximum when the conductivity is about $2.0 \times 10^{-14}$, which Schuster points out is one which normally would be regarded as being very small, as it is about $2.4 \times 10^{-9}$ that of mercury. The magnitude of the retarding couple for such a conductivity has been calculated to be such that it would take 125 centuries to lengthen the day by one second. Notwithstanding the smallness of this, Schuster points out that its existence would almost certainly have been detected by astronomers, and a value of the conductivity smaller than $5 \times 10^{-6}$ or greater than $1 \times 10^{-21}$ is more likely.

It is, of course, not necessary to assume a large volume of outer space to have uniform conductivity to produce such effects. An outer layer will suffice, and the conductivity may be uniform or patchy, but the reactions will be of the sign indicated. It is certain that the movements of the magnetic field are not simple as outlined above but are very complex, and that unexpected reversals occur, so that it is not possible to predict the conditions even 20 years ahead. The theory advanced is, however, still capable of explaining the variations, for any conducting layer may not only vary greatly over considerable areas, but there may be relative motion between the earth and portions of the layer which also varies.

Unfortunately, such an explanation of the cause of secular change does not help very much in explaining the cause of the field itself, but it is obvious that any complete theory must not only explain such a change but must also account for the present difference between the earth's magnetic axis and its axis of rotation and the variation in intensity.

### Theory of an Iron Core.

As is well known, the first idea was that the core of the earth is of iron and is magnetised. But, apart from our lack of knowledge of the constitution of the earth's interior, the difficulty has to be faced that at high temperatures iron loses its magnetic properties, and that increase of pressure depresses rather than raises the critical point. As the interior of the earth is at a high temperature the theory therefore loses support, unless it is assumed that at exceptionally high pressures and temperatures there is some restoration of the magnetic properties.

If the earth is taken to be a uniformly magnetised sphere its magnetic moment, as already stated, is given by $\frac{4}{3}\pi r^3 I$ where I is the intensity of magnetisation. The effect is that of a magnetic doublet and could be produced equally well by a spherical shell of thickness $(r-a)$ in which case if $a$, the thickness of the shell is 10 miles, the intensity of magnetisation would be about 50 to produce the magnetic forces observed. In a shell of such thickness the temperatures and pressures would not be so great as to destroy the magnetic properties of iron.

While the theory of an iron core or an outer shell permanently magnetised has little to support it there is little doubt that the irregularities in the earth's field are largely related to the structure of the earth's crust, and large masses of iron and iron ore must play an important part in the irregularities which are observed. Geophysical surveys by magnetic methods support this view.

### Electric Currents circulating round the Earth.

The next simplest theory is that the magnetic field is due to electric currents circulating round the earth, and this naturally gives rise to the question of the seat of origin of the electromotive forces necessary to maintain such currents. If the currents are uniform in density throughout the volume of the earth the magnitude of this density would be about $10^{-8}$ ampere to produce the necessary intensity of magnetisation. If we suppose that there was once a source of electromotive force but it has long ceased to operate, the currents produced would take a very long time to die down owing to self-induction.

In the case of a copper sphere of the size of the earth, Lamb has shown that three million years after such currents were generated and the cause removed, they would still have one-third of their original intensity. The electrical conductivity of the earth is, however, much less than that of copper, and if remnant currents are responsible for the earth's present magnetism, their value in the past must have been very great, and the cause of their origin would still be a mystery. But it is much more profitable to look for a possible electromotive force not only to produce but permanently to maintain a current system. Such a possible source was indicated by Larmor at a meeting of this Section of the British Association in 1919. Larmor pointed out that in the case of the sun, surface phenomena indicated the existence of a residual internal circulation mainly in meridian planes. If this circulating conducting material cuts a magnetic field which in direction is the same as that of the earth, circulating currents would be set up in such a direction as to augment the magnetic field and eventually a condition of equilibrium would be set up between the producing electromotive force and the attenuation effects. The system is, in fact, that of a self-exciting dynamo, and the energy of the system is obtained at the expense of the energy of the circulating conducting material.

While in the case of the earth any internal circulation of matter in meridian planes or near thereto is entirely conjectural, the theory does provide not only for the main field but also for the secular variation by changing the paths of the circulating currents.

Ross Gunn has recently put forward a theory attributing the magnetic field to electrical currents set up inside the earth in the high temperature regions where the thermal motions are considerable. Gunn suggests that the temperature of the inner earth is of the order of 10,000° and as a consequence the material will be highly ionised and the conductivity correspondingly great. In the case of the upper atmosphere, Gunn has analysed the motions of ions and electrons of long free path spiralling about the magnetic lines of force, and in such a case a diamagnetic effect and drift currents are produced. An extension of the calculation to the inner earth where the free paths are short is made, and it is considered that the primary current system of the earth results from the motions imposed upon ions having a mean free path of the order $10^{-7}$ cm., the motion being imposed by the internal gravitational electric field at right angles to the magnetic field. The currents produced augment the original field in a regenerative manner. Gunn is developing this theory of internal ion drifts.

## Magnetic Effects Associated with Earth's Rotation.

If the earth's magnetism were due to an iron core permanently magnetised, or to electric currents circulating round the earth, the rotation of the earth would play no part in the main or permanent field, although, as will be seen later, it may contribute to variations due to external causes.

Let us consider the possible ways a body may, by virtue of its rotation, act like a magnet. First, consider the earth as a body carrying a positive or negative electric charge. Maxwell first of all suggested the production of a current by the motion of an electric charge, and Rowland first demonstrated the effect. Ayrton and Perry, over 50 years ago, showed that an electric charge residing on the surface of the earth would produce a magnetic field by the rotary motion of the earth, but to produce a field of the requisite intensity the charge must be very great. If the surface density of the charge be $\rho$ the magnetic force at the equator parallel to the surface is

$$H_e = \frac{4}{3}\pi\rho r\omega$$

where $\omega$ is the angular velocity and $r$ the radius of the earth. If Q is the total charge on the surface the horizontal magnetic force may be written

$$H_e = Q\omega/3r = \frac{V\omega}{3}$$

where V is the potential In this case it is obvious that any small sphere charged at the same potential and rotating at the same angular velocity would produce the same surface field, since the radius of the sphere is not involved.

If, however, the charge be distributed uniformly throughout the earth —and this is necessary for uniform intensity of magnetisation—the value of the horizontal field at the equator is $Q\omega/5r$. It is, however, necessary to explain that a field of this intensity would not be detected by an observer moving with and on the earth's surface, for in such case there would be no relative motion between such an observer and part of the electrostatic charge. However, a field will be detected. The translatory motion of the charge would also produce a field, and to determine the total effect, i.e. the effect of the earth's rotation on a magnet situated on the surface of the earth, Schuster suggested a method somewhat as follows: Imagine the observer to be stationary in space, then the earth, by virtue of the rotation of the charge, will produce a field detectable by him. At the equator such a field will produce a horizontal force, that is, a force parallel to the earth's surface, of the value $Q\omega/5r$, where the charge Q is uniformly distributed throughout the volume of the sphere. To produce a stationary effect, that is to make that portion of the earth's surface near the observer have no relative motion with respect to him, let the earth be subjected to a translatory motion equal to the velocity of the part considered, but in the opposite direction. Such velocity is $\omega r$. The translatory motion of the charge Q is equivalent to a current $i = Q\omega r/2r$, since the charge Q takes the time $\frac{2r}{\omega r}$ to pass a given point. The magnetic force produced by the moving charge will be circular in shape and at right angles to the line of motion, and at the equator the force parallel to the surface will be

$$2\frac{Q\omega r}{2r^2} = \frac{Q\omega}{r}i;$$

hence, the horizontal magnetic force detectable by an observer on the earth's surface at the equator is

$$\frac{Q\omega}{5r} - \frac{Q\omega}{r} = -\frac{4Q\omega}{5r} = -\frac{16}{15}\pi\rho\omega r^2.$$

In this case the value of H is proportional to $\omega r^2$ for a given volume density, and hence a laboratory experiment would fail to detect such an effect.

If the charge on the earth be negative, the horizontal force at the equator due to rotation is in the direction north to south, and the field at the north pole is vertically upwards. By the translatory motion of the earth a horizontal field at the equator in the direction south to north is produced, and the field at the north pole has no component vertical to the surface of the earth. The resultant field, therefore, is such that there would be an upward vertical component at the north pole, and a south to north horizontal field at the equator. A field of this type does not exist in practice, the field of the earth being such that its direction is south to north at the equator and vertically downwards at the north pole. Moreover, it is not possible to produce by means of a single rotating charge, fields of the correct sign both at the pole and the equator, for if we change the sign of the charge the resultant fields at pole and equator are also changed in sign.

To overcome the difficulties of a surface charge Sutherland suggested an equal but opposite charge concentrated at the centre of the earth, thus neutralising the electrostatic field due to the surface charge but not the magnetic effect of the charges in motion. Later, he suggested that an inequality in the distribution of the earth's atomic charges might be a cause. If all the negative and positive electricity in the earth were spread over two concentric spheres slightly different in diameter, a combined magnetic field comparable with that of the earth could be obtained. In such a case, as the charges are enormously great, the difference in radii of the spheres would need to be but exceedingly small to produce the desired intensity of field. The difficulties due to the external electrostatic field and the magnetic field produced by the translatory motion of the earth vanish, since there will be no such fields, but, unfortunately, not only would such a field be symmetrical about the axis of rotation, but at internal points near the surface the electric field would be many millions of volts per centimetre.

There are a number of variants of this idea of separated charges. One is that the rotation of the earth brings about an electric polarisation in the atoms perpendicular to the axis of rotation, such polarisation producing a magnetic and also an electrostatic field. The direction of magnetisation of the field is not, however, that actually observed on the earth, the same difficulty presenting itself as that already considered with the charged sphere.

In 1891, and on several occasions since, Schuster has raised the question whether every large rotating mass is not a magnet, and as far back as 1891 he put forward the suggestion that the sun had a magnetic field associated with it.

Lord Kelvin in 1892 remarked: 'Considering probabilities and possibilities as to the history of the earth from its beginning to the present time, I find it unimaginable but that terrestrial magnetism is due to the greatness and rotation of the earth. If it is true that terrestrial magnetism is a necessary consequence of the magnitude and the rotation of the earth, other bodies comparable in these qualities with the earth and comparable also with the earth in respect of material and temperature, such as Venus and Mars, must be magnets comparable in strength with the terrestrial magnet, and they must have poles similar to the earth's north and south poles on the north and south sides of their equators because their directions of rotation as seen from the north side of the ecliptic are the same as that of the earth.'

This suggestion that every rapidly rotating body produces a magnetic field leads to the consideration of the most promising of all such bodies, namely the sun, the radius of which is much greater than that of the earth, and, moreover, its atmosphere contains vapours which are self-luminous and, therefore, give line spectra. The spectra produced have been examined at the Mount Wilson Solar Observatory, and the magnetic field of the sun has been revealed by the well-known Zeeman effect. Moreover, the intensity of the field has been measured at different atmospheric depths, and it has been found that the magnetisation decreases rapidly with height of the solar atmosphere. This decrease leads to the conclusion that the sun's magnetic field does not pass into outer space to an extent comparable with that of the earth, but apart from this restriction with regard to outer space, the sun's magnetic field is strikingly similar to that of the earth, for its direction of magnetisation is the same and the magnetic axis is inclined to the sun's axis of rotation at about 4°. Moreover, the magnetic axis is not fixed in position relative to the axis of rotation, but revolves slowly round it, so that, whereas the period of rotation of the solar axis is about 31 days, that of the magnetic axis is about 26 days. The intensity of the field at the poles is about 50 Gauss.

These observed similarities between the magnetic fields of the earth and the sun, especially as the physical conditions are so different, naturally lend support to the theory that the magnetisation is brought about by rotation, and the fact that the axes of rotation and magnetisation do not coincide, while disturbing, may possibly be explained by reasonable assumptions.

If rotation of matter is necessary to produce the magnetic fields of the earth and the sun, the angular velocity, the radius, and the density must be important factors. If the magnetic effect is proportional to $D\omega r^2$ where D is the density, the calculated intensity of the sun's field agrees with that observed, taking the earth's field as the standard. Unfortunately, owing to the square of the radius being involved in the expression for the field, an effect proportional to $D\omega r^2$ cannot be tested by experiments in the laboratory, as a value of $\omega$ necessary to produce a measurable effect could not be obtained. A magnetic effect proportional to $D\omega r$ can be and has been tested in the laboratory, but the effect is far too small to account for the earth's magnetism.

There are a number of ways in which small magnetic fields may arise in a spherical rotating conductor. For example, centrifugal force may result in the free electrons moving towards the surface until equilibrium is brought about by the resulting electrostatic forces. As an alternative to this, gravity may pull the electrons towards the centre of the sphere until again the resulting electrostatic forces restore equilibrium. In the first case Swann has shown that the horizontal intensity would change sign as an observer travels from the equator to the pole. In the second case the magnetic field at the poles would be of the reverse sign to those of the earth's field.

A theory which has been tested by laboratory experiments is one depending on gyroscopic action. If the magnetic condition of iron arises from the rotation of the electrons in the constituent atoms, the axes of rotation should tend to become parallel to the earth's axis of rotation. Only a slight change in orientation can be expected because of the forces due to adjacent molecules, but the net result must be to cause each molecule to contribute a minute magnetic moment parallel to the earth's axis of rotation. When a steady state has resulted there will be an angle $\theta$ between the two axes, and the axis of rotation will precess, i.e. it will trace out a cone. The net result so far as the magnetic effect is concerned is to cause each molecule to contribute a minute magnetic moment parallel to the earth's axis of rotation. The effect will be proportional to the angular velocity and not the radius, so that the effect can easily be tested in the laboratory. Barnett first succeeded by laboratory experiments in showing that magnetisation was produced by rotation, and that the intensity of the field observed was proportional to the angular velocity. The direction and general shape of the magnetic field of the earth could be accounted for by this gyromagnetic theory, but the intensity of magnetisation produced is far too small. The estimated value is about $10^{-11}$ times that of the earth. However, the laboratory conditions are so far removed from those of the interior of the earth that the restoring forces must be very dissimilar in magnitude, and that these forces are a direct factor was shown by Barnett's experiments. At present, however, the effect appears to be so very small that other causes must be sought.

## Possible Modification of Laws of Electrodynamics.

The difficulties confronting such theories as an electrically charged earth and the smallness of the gyromagnetic effect, have led to suggestions that the field may be due to some departure from the commonly accepted laws of electrodynamics.

In 1894 J. J. Thomson pointed out that if atoms exerted slightly different attractions on positive and negative electricity, then a large rotating body could produce a magnetic field, and in such case the intensity would be proportional to $\omega r^2$ so that no laboratory experiments could confirm or refute the theory.

H. A. Wilson has considered a case assuming that the electric and magnetic effects do not balance in electrically neutral matter. On such an assumption the gravitational unit of matter (about 4,000 grams) which attracts an equal mass 1 centimetre away with the force of 1 dyne might be expected to produce a magnetic field of the same order of magnitude as the electrostatic unity of electricity. Such an assumption leads to a correct ratio for the magnetic fields of the earth and sun, but an experiment made by Wilson showed that if moving matter produces a magnetic field like that due to a moving charge, then the mass equivalent of one electrostatic unit of charge is not less than 20,000 kilogrammes, and that matter having a velocity of pure translation has no appreciable field. It follows that if the earth's magnetic field is to be explained by some modification of the laws of electrodynamics, the modification must be such as to make rotation and not translation the effective motion.

Swann, who has put forward a theory based on a slight modification of the laws of electrodynamics, points out that the ratio of the magnetic fields for the earth and sun would be obtained also for an expression of the form $D\omega^4 r^4$ since the ratio of the values of $\omega^4 r^4$ differs inappreciably from that of $\omega r^2$. Swann so modifies the equations as to provide for the correct value of that part of the earth's field symmetrical about the axis of rotation, and the right value for the ratio of the magnetic field of the earth to that of the sun is also obtained; in addition, there is a slow death of positive electricity amounting to a disappearance of $\frac{1}{2}$ per cent. of the earth's charge in $10^{20}$ years, the corresponding surplus of negative electricity thus freed, after building up the necessary electric field, passing off continually as the atmospheric electric current by conduction through the atmosphere. According to this theory, spheres of such size that they may be used in laboratory experiments should give effects which are just measurable, and Swann and Longacre have made experiments with a copper sphere 10 centimetres in radius rotating at 200 revolutions per second, but the results obtained differ very appreciably from those calculated on the theory, i.e. an effect proportional to $\omega^4 r^4$.

As theory after theory breaks down when the calculated magnetic effects are compared with those actually observed on the earth, we are forced to conclude that our knowledge of the cause of the earth's magnetism is little more than conjecture, for of the theories put forward all that have been put to a practical test have been found wanting in some respect. Larmor's theory of internal rotation in a meridianal plane cannot be put to a laboratory test, and other theories based on slight departures from the accepted laws of electrodynamics are equally difficult to decide in the laboratory. It is possible that when we know the cause of terrestrial magnetism we shall know also the cause of gravitation. But it may be that we shall have to wait for many generations before the results of observations may confirm a theory. Swann, in putting forward his modifications to the accepted laws of electrodynamics, remarks that the positive electricity of the earth would, according to his theory, gradually disappear, but the disappearance of one half of 1 per cent. of it would take a hundred million million million years, which is rather a long time.

## Vertical Electric Currents.

There is, however, a possibility that a small portion of the earth's magnetic field may be due to vertical electric earth-air currents, which can easily be distinguished from currents circulating in the upper atmosphere or in regions beyond. If all electric currents are parallel to the earth's surface in the form of current sheets or the circuits are such that no portion of the earth's surface is included in them, then the line integral of the horizontal magnetic force around any closed area on the earth must be zero, since no element of current is enclosed by the area. If, however, an electric circuit cuts the earth's surface, the resulting magnetic intensity may be regarded as due in part to a current sheet parallel to the earth's surface and in part to a vertical current. In such case the line integral of the horizontal magnetic force taken round a closed area will not vanish but be equal to $4\pi$ times the enclosed current, and from its magnitude and sign the intensity and direction of the earth-air electric current within the closed area may be calculated. Gauss first applied such a test and found the line integral around a triangular area to be zero within the limits of the errors of observation.

Adolf Schmidt developed the work of Gauss, and on the basis of the Neumayer magnetic charts of 1885 found evidence for the existence of such earth-air electric currents. Schmidt's first estimate of the magnitude of such vertical currents, namely, 0.16 ampere per square kilometre, was no doubt excessive, and he concluded that the estimate was erroneous because of systematic errors in the charts.

Rücker chose areas in Great Britain, where the magnetic forces were well known, and failed to find any evidence of such vertical currents. Dyson and Furner made an examination of data available in 1922, and conclude that although there is some evidence, such currents are not indicated with any certainty. On the other hand, Bauer has made many calculations, and on all occasions has been forced to conclude that such vertical currents do exist. In 1896 he computed line integrals, taking parallels of latitude as circuits of the earth, the charts used being those of Neumayer's. A second investigation was made in 1904, and a third in 1908, the data used on the latter occasion being obtained from the magnetic charts for the United States, and the line integrals calculated for areas within the United States. In all cases vertical currents were indicated. In 1920 a further investigation was made, and from the results it appears that in certain areas there are upward electric currents, and in others downward currents, the current density varying from $+58\times10^{-4}$ ampere per square kilometre to $-53\times10^{-4}$ ampere per square kilometre. It appears very desirable that a definite answer should be obtained to this question of vertical currents, but calculations show that to obtain a decision it is necessary for measurements of the most precise kind to be made. I have studied the calculations of Bauer, and conclude that, while he is justified in his deductions, the data are not sufficiently reliable. The values of the horizontal force are not all observed values, and those from which they have been deduced have been obtained by many observers, by many instruments, and under very varying conditions, so that the probable errors are not small. My review leaves me exceedingly doubtful of the existence of such currents.

To illustrate the kind of precision required in the measurements, imagine a very large circular area of radius $r$ and assume that a vertical current $i$ is distributed uniformly over the area. The horizontal intensity produced by such a current at a distance $d$ from the centre of the circle is $2id/r^2$. If the current density is $30\times10^{-3}$ ampere per square kilometre, the value of $i$ is $270\pi$ C.G.S. units of current, and the horizontal force produced at 100 kilometres from the centre of the area is a little less than $2\gamma$, i.e. 0.00002 C.G.S. unit. The line integral round such an area would be considerable, but it is easy to see that if there were no vertical currents and measurements for one half of the circle were made with one instrument giving correct results while those of the other half were made with a second instrument giving values in error on an average of $+4\gamma$, the conclusion drawn would be that vertical currents of the values already assumed really existed.

Notwithstanding the difficulties, there is no doubt that sufficiently precise measurements could be made over a carefully chosen area, which would enable a definite decision to be reached with respect to such vertical currents. The experiments would necessitate not only the use of similar types of instruments but the establishment of small temporary stations with magnetographs so that simultaneous values of the horizontal force could be obtained.

## Variations of the Magnetic Field.

Usually, when the cause of a large scale phenomenon like the earth's magnetism is unknown, it is useless to look for the cause of the variations, but in the case of terrestrial magnetism this is not so. Schuster showed that the variations are not due to the same causes as those which produce the main field, but to external causes. Some of the variations are periodic in character and appropriate names such as daily, monthly and annual variations have been applied, while others of a violent and non-recurrent character are called magnetic storms.

## Diurnal Variations.

The data dealing with diurnal variations are very voluminous, for records at Kew Observatory have been taken for many generations. As the name indicates, a change in the magnetic elements takes place every 24 hours; in the case of Kew, the horizontal force reaches a minimum value about 10 or 11 a.m., and a maximum value about 7 p.m., the range being about 35$\gamma$ in the summer and 12$\gamma$ in the winter. On the other hand, the vertical intensity is a maximum about 10 or 11 a.m., and a minimum at 7 p.m., this variation being also less in winter than in summer; thus, an annual variation is superimposed on the daily one. It is observed that when the horizontal force increases the vertical force diminishes, from which it follows that no general rise or fall in the intensity of the earth's main field can explain the variations. The principal portion of the variation occurs in the daytime, and the variation appears to be beyond doubt a function of the sun's position above the horizon.

Schuster's analysis of the data shows that the daily variation is probably due to electric currents in the upper atmosphere, but in addition to the magnetic effects of these currents there is an effect due to currents induced in the earth by them. These induced currents are naturally in the opposite direction to the inducing ones, and hence the magnetic effects for the horizontal intensity are additive, while those for the vertical force are opposed.

Balfour Stewart first put forward this theory that the diurnal variations are due to electric currents in a conducting medium above the surface of the earth, and Schuster and Chapman have done much to develop it. In general, the theory attributes the variations to convective motions of conducting layers of air across the magnetic field of the earth and hence is known as the 'dynamo' theory. The general form and intensity of such a current system can be inferred from an analysis of the variation data. Chapman's analysis shows the system in the sunlit hemisphere to consist of two closed circuits which (at the equinoxes) may be taken as symmetrical with respect to the equator, their foci lying very nearly on the 11 a.m. meridian. As the electric currents are supposed to be induced by the movement of conducting layers of air in the magnetic field, such currents must also be produced near the ground, but the conductivity of the air near the ground is so low that their effect may be neglected. In the upper regions the movements, while larger, cannot be regarded as immeasurably greater than near the earth's surface, and the increase in current intensity can only be attributed to an increase in the conductivity, a view which Balfour Stewart was forced to adopt, although at the time there was little evidence to support it.

The magnitude of the dynamo effect is dependent on three factors—(1) the horizontal movement of the air, (2) the conductivity of the air, (3) the intensity of the vertical magnetic field. All these factors vary with latitude, and hence it is to be anticipated that the magnitude of the variations will also vary with latitude, which is the case. The intensity of the field can be calculated with considerable accuracy, but the conductivity and movements of the upper air are not known, although such movements are attributed to thermal effects and hence will be a maximum in the daytime. However, Chapman points out that if we assume the convective motion of the upper air to be of the same general character as at ground level, the current foci should be on the meridian at 1 p.m. or 2 p.m. instead of 10 a.m. or 11 a.m. as is observed. The conductivity of the medium is attributed to ionisation due to the sun, and Chapman is of opinion that the total electric conductivity must be of the order $10^{-5}$ e.m.u. This appears to be rather high, since P.O. Pedersen's analysis of the propagation of radio waves leads to a value of only $5.10^{-7}$. However, more recent observations on the reflection of wireless waves indicate a conductivity of the order $10^{-6}$ as the number of electrons per c.c. in the reflecting layer, and for a thick layer a conductivity of the right order is possible.

Appleton has shown that the height of the ionised layer gradually rises after sunset and reaches a maximum value about one hour before sunrise, after which a somewhat rapid lowering results. Normally, the height at night varies from 90 to 130 kilometres, but occasionally in winter heights as great as 250 to 350 kilometres have been measured. Appleton suggests that on some nights the ionisation in the lower layer has been so much reduced by recombination that penetration by wireless waves is possible. In such cases, however, deviation of wireless waves takes place at an upper layer which is richer in ionisation. The lower layer is formed again at sunrise and deviation by it again results. As the solar influence increases ionisation likewise increases, and the outer boundary of the lower layer falls, and evidence has been obtained of the formation of yet another lower region of ionisation.

As a first and crude approximation we may, therefore, imagine a spherical conducting layer to surround the earth, and in addition a conducting hemispherical cap over the hemisphere facing the sun, the height of this cap being a few hundred kilometres. Neither the complete spherical conducting shell nor the hemispherical cap are of uniform conductivity, and the matter constituting these layers moves with the earth, so that ionisation and recombination are always taking place.

While we have from wireless measurements fairly good evidence of the height of the lower conducting layers, our knowledge of the extent of the ionisation is not sufficiently good to enable us to do more than speculate on the merits of the theories advanced, for in addition to the dynamo theory there is one due to Ross Gunn known as the diamagnetic layer theory, and a third called the drift current theory. The differences between the theories are best brought out by considering the ionisation effects in the hemispherical conducting cap facing the sun. Pederson has calculated the number of electrons and ions per cubic centimetre at various heights, and he and Ross Gunn have considered the nature and magnitude of the conductivity of the upper ionised regions. They have shown that the conductivity varies with the direction of the magnetic field, the conductivity at right angles to the field being at times very small, and under certain conditions it approaches zero, while the conductivity in the direction of the field is unaffected by the field's intensity. It is pointed out that the transverse conductivity is reduced in the ratio $N^2/(N^2+\omega^2)$ where N is the number of collisions per second and $\omega$ is the angular velocity of the electrons in their spiral paths round the magnetic lines of force. Gunn estimates that at heights as low as 140 kilometres $\omega$ is very great compared with N, so that the conductivity approaches zero value. Hence, in layers where the conductivity transverse to the magnetic field is very small, such large circulating currents as are necessary for the dynamo effect cannot flow, and where there is an appreciable vertical magnetic field there can be but negligible horizontal electric currents. In the case considered by Gunn, where a charge in its spiral path can execute many revolutions between successive collisions, the spiral motion of the charge has the same effect as a small magnet opposed to the field, so that the whole hemispherical cap is equivalent to a diamagnetic layer, and to this diamagnetism Gunn attributes the diurnal variation.

There appears to be no doubt that such a diamagnetic effect does exist, and that it contributes to the diurnal variations, but it will be shown that its magnitude is much too small to explain the whole of the diurnal variation.

The intensity of magnetisation of the layer may be written—NKT/H where N is the number of ions per square centimetre column, K is Boltzmann's constant, H is the intensity of the magnetic field, and T the absolute temperature. Since N and T are greater in the daytime than at night, it follows that the diamagnetic effect will be greater during the day, and greater in summer than in winter. Gunn assumed a height for the diamagnetic layer between 150 and 180 kilometres, and assumed an absolute temperature of 1,000° K. He further assumed that the number of ions per cubic centimetre is proportional to the intensity of the incident solar radiation plus a number of residual ions left during the night period, but of course these latter take no part in the variation. With such data Gunn has calculated the maximum variation of the horizontal force due to the diamagnetic effect of the cap, and has got most excellent agreement with the observed changes. However, Gunn had to assume a value for N which is far greater than that given by Pederson, but as the latter includes no figures for the other ionised layers it is not safe to draw too definite conclusions.

When the effect of gravity is taken into account, Chapman has shown how, with the same value of N, the ionisation in the diamagnetic layer contributes far more effectively to the diurnal variation. It is shown that the less the contribution made by a charged particle to the transverse conductivity (relative to the magnetic field) the greater is the mean drift velocity which it experiences, and in the case of the earth's magnetic field such drift currents are eastward in direction. There is, in fact, a steady drift of electrons and ions in a direction perpendicular to the lines of magnetic force and the gravitational field. The drift velocity is greatest at the equator, and, if the ionisation is constant, the velocity decreases as the pole is approached in the ratio $\sin\theta/(1+3\cos^2\theta)$. Taking Pederson's value for the number of electrons and ions per cubic centimetre, the equatorial current intensity has been calculated by Chapman to be $4\times10^{-7}$ e.m.u., which, however, is only about one-fiftieth of the equatorial current intensity required to account for the variations observed. However, Pederson's values may be too low. But for the same ionisation values the effect is much greater than the diamagnetic effect, and this naturally puts the diamagnetic effect into the position of being only part of the cause of the diurnal variations. If for a time we assume such increase in the ionisation that the drift currents are sufficient to produce the variations desired, it must further be shown that the circulation of the currents is of the right form to produce the effects. It is obvious that a drift current of constant value always eastward would produce a permanent effect but not a variation, and any variation in current intensity will tend to produce an accumulation of charge. In the case of a diamagnetic cap, since the intensity is greatest towards midday we may imagine positive charge tending to accumulate in the p.m. hemisphere and negative in the a.m. hemisphere. But the cap is a good conductor in the direction of the magnetic field, and a current may pass from high levels to low levels and *vice versa* by travelling in the direction of the field. Below the diamagnetic layer or drift layer where the free paths are long there is a lower layer or layers where the free paths are short and the conductivity is not so anisotropic, and passage from the diamagnetic layer to such layer or layers and *vice versa* is assumed to take place along the magnetic lines of force in this way. In the northern part of the sunlit hemisphere the current system will, therefore, be from west to east in the diamagnetic layer, then south to north downwards along the magnetic lines of force, then east to west in the lower layer, and finally north to south downwards to the diamagnetic layer. This, of course, is a general rather than a detailed picture. In the southern portion of the sunlit hemisphere the current in the drift layer will again be west to east, then north to south downwards, east to west in the lower conductivity layers and south to north upwards to the higher layers. Such a type of current system may be imagined to result from drift currents or the 'dynamo' theory. With regard to the relative merits of the three theories, an effect of the diamagnetic layer appears certain, but with it is associated the drift current effect which is much larger. The diamagnetic layer effect must therefore be regarded as secondary in importance. The dynamo theory involves motions of the air as well as ionisation, and while on the whole the drift current theory appears to be superior, more information is needed on the number and distribution of ions and electrons in the upper atmosphere before coming to a final decision.

The fact that the foci are not on the noon meridian has not been satisfactorily explained. Chapman has suggested a combination of the drift and dynamo hypotheses as a possible explanation, the dynamo action being responsible for the advance of the foci by one hour, but no good reason for such advance is known. It is possible, however, that the drift current theory may alone be a complete solution. If the earth and its atmosphere did not revolve, the ionisation and the drift currents would still exist, and the foci of the currents would then be on the central meridian. With rotation of the earth and its atmosphere, ionisation effects begin at sunrise and soon afterwards a lower conducting layer is formed or strengthened. In the sunlit hemisphere we may assume, therefore, that the ionisation effects are greater on the p.m. side, and the conductivity and depth of the layers are also greater on that side. In the northern hemisphere it is possible, therefore, that the effective S. to N. current on the p.m. side is nearer the meridian than the corresponding N. to S. current on the a.m. side. Such asymmetry would move the focus of the system westward, i.e. towards the a.m. meridian. In practice the focus is near the 11 a.m. meridian.

Another diurnal variation, namely, the lunar variation, is also attributed to circulating electric currents, but in this case it appears fairly certain that a dynamo effect is the cause, the conducting medium being moved by direct tidal action. The results observed are in close accordance with the theory.

### Variation due to Solar Eclipse.

If, as we suppose, the sun's radiation is responsible for the conductivity of the upper layers, it follows that the solar diurnal variation is not the only magnetic change associated with the sun, for everything that affects the radiation will produce corresponding effects in the ionisation of the upper atmosphere, and hence changes in the magnetic field. An interesting instance is the magnetic effect of an eclipse of the sun. Some effect should obviously arise since there is a cancellation of a portion of the solar radiation and this will diminish the ionisation effects; the solar diurnal variation should, therefore, suffer a check. Recent eclipse results are in accordance with these views, though before 1900 doubt as to any measurable effect being obtained was almost universal. In 1900 special observations were made, and there have been others since at a number of stations, principally in the United States of America; the records show that small magnetic disturbances do result, the duration being roughly that of an eclipse. Analyses of the changes indicate that the cause is external to the earth's crust. The effect differs from that of an ordinary magnetic storm inasmuch as it begins, progresses and ends gradually, and a definite conclusion to be drawn is that the effect is due to changes in the upper atmosphere by the obliteration of the sun's rays due to the moon as an obstruction.

### Sunspots and Magnetic Storms.

Any unevenness in the radiation from the sun as it rotates must also affect the conductivity and hence produce variations. Examination of magnetic records shows that many variations are related to the sun's period and also to sunspot periods, and it appears not improbable that there is overlapping of several periods probably intimately connected. The results obtained show that with rise and fall of sunspot frequency there are corresponding changes in the diurnal variation. Moreover, the amplitude of the daily changes rises and falls with the intensity of the magnetic disturbance. It follows, therefore, that changes in amplitude of the diurnal variation in years of many sunspots is due to the same ultimate cause, namely, solar radiation, as that causing magnetic disturbance, but the existence of a 27-day period does not, as Chree pointed out, justify the conclusion that sunspots are the only disturbing sources. The same result would be obtained if the intensity were a function of the solar longitude and did not vary too rapidly with time.

Magnetic storms are marked disturbances of solar origin, and to explain these many theories have been advanced, but the facts are not easy of explanation. The belief in the connection of solar activity with magnetic storms is old, but one of the earliest and most striking declarations was made by the president (Lord Armstrong) of this Association in his Presidential Address in 1863. Lord Armstrong said: 'The sympathy also which appears to exist between forces operating in the sun and magnetic forces belonging to the earth merits a continuance of that close attention which it has already received from the British Association, and of labours such as General Sabine has, with so much ability and effect, devoted to the elucidation of the subject. I may here notice that most remarkable phenomenon which was seen by independent observers at two different places on September 1, 1859. A sudden outburst of light, far exceeding the brightness of the sun's surface, was seen to take place and sweep like a drifting cloud over a portion of the solar face. This was attended with magnetic disturbances of unusual intensity and of exhibitions of aurora of extraordinary brilliancy. The identical instant at which the effusion of light was observed was recorded by an abrupt and strongly marked deflection in the self-registering instruments at Kew. The phenomena as seen was probably only part of what actually took place, for the magnetic storm in the midst of which it occurred commenced before and continued after the event.'

Much progress has been made in our knowledge of the sun and its radiations since the pronouncement of Lord Armstrong. One of the first theories put forward attributed magnetic storms to the magnetic fields produced by streams of charged particles from the sun acting like an electric current and producing a direct magnetic effect. Schuster showed that such a stream moving between the sun and the earth would move in a magnetic field of constantly increasing intensity, and would be subject to a retarding force also continually increasing. Schuster considered a particular magnetic storm, and showed that on such an assumption the passage of the stream from the sun to the earth would take about a year, and hence the intimate connection between solar activity and magnetic storms would be very far from apparent.

Lindemann has overcome this difficulty by suggesting solar streams which are ionised but, on the whole, neutral. The groups of particles are assumed to be projected from the solar prominences, and the gases in these are of such high velocity, $10^8$ cm. per sec., that the journey from the sun to the earth should be possible in less than two days, without serious recombination taking place. Moreover, owing to its neutrality such a stream will not tend to spread outwards by the mutual repulsion of its constituent particles.

In a theoretical study of the motion of atoms from the sun, Milne has calculated the limiting velocity with which they can pass out of the gravitational field of the sun under the influence of radiation pressure, and the order of magnitude for such elements as hydrogen is $1.6 \times 10^8$ cm. per sec. If this value be taken as the average velocity, the particles will take about 30 hours to travel from the sun to the earth, and in this connection it is of interest to note that, while there is considerable difficulty in determining the maximum activity of solar eruptions and the corresponding maximum intensity of magnetic storms, yet an interval of 30 hours does roughly correspond with this difference.

Lindemann suggested that the negative ions might be lighter than the positive ones and stop at the uppermost layers of the atmosphere, while the heavy positive ions would penetrate deeper and be diverted to the polar regions by the magnetic field of the earth.

Maris and Hulbert attribute the increase in ionisation to the action of ultra-violet light. They conclude that at heights of 300 to 400 kilometres temperatures of 1,000° K are reasonable, and at heights exceeding 400 kilometres the free paths of the particles are very long, the motions due to formal impact considerable, and the ionisation entirely due to the action of ultra-violet light. When the activity of the sun increases it is assumed that there is a tremendous increase of ultra-violet light; thus, Maris and Hulbert estimate that if one ten-thousandth part of the solar surface (temperature 6,000°) were removed and there were exposed regions of black body temperature 30,000°, the total ultra-violet energy would be increased $10^5$ times, whereas the solar constant would be increased by only 1 per cent. The ejected ions give rise to a magnetic storm, for under the influence of gravity in a magnetic field the positive and negative ions will move in opposite directions and at right angles to the gravitational and magnetic forces, and so produce electric currents, the direction of which is roughly in circles corresponding to magnetic lines of latitude. Such currents will induce others in the earth, and owing to the rapidity of the changes the currents will be nearer the surface of the earth than in the case already considered, where the normal diurnal changes are comparatively slow. But the general effect of the induced currents will be the same, i.e. a greater variation in the horizontal force. Maris and Hulbert calculate that a blast of ultra-violet light may thus give rise to currents of the order $10^6$ amperes, and a change of magnetic field of the order of 0.001 C.G.S.

Recently, Chapman and Ferraro have suggested that magnetic storms are essentially connected with the approach of a neutral ionised stream towards the earth, the more important changes in the stream taking place in the direction of the sun at a distance equal to a few times the radius of the earth. As the stream is a highly conducting body and cuts the earth's magnetic field, electric currents are set up in the surface layers, and the first stage of the magnetic storm is attributed to the magnetic effect of these currents. Retardation of the stream results, and this retardation is naturally greatest at that part of the front of the stream in direct line with the centre of the earth. On either side the stream will advance and partly enclose the earth, and along the sides of the enclosure there will be charged layers due to the polarisation of the stream by the magnetic field. Across the space on the dark side of the earth it is assumed that a westerly current is set up due to charges passing over the space between the charged layers. It will be observed that this theory, which is being developed, has one distinct feature, inasmuch as the main electric current flows at a distance a few radii from the earth.

Whatever the ultimate action may be of a stream of charged particles from the sun, which Maunder, who first suggested such a theory, appropriately called a 'hose,' it appears appropriate to consider the sunlit hemisphere to be the one first affected by the stream, and to associate streams of great intensity with sunspots. It is known that sunspots are rare in a zone 5° either side of the equator, and are chiefly congregated in two zones, the mean latitude of which is about 20°. As we should expect the projection of solar matter to take place radially, we should similarly anticipate that when the earth is near the sun's equatorial plane the number of magnetic storms would be rare. Such is the case. Moreover, when the earth reaches a higher heliographic latitude north or south, the probability of the earth being hit by a stream is much greater. This is in accordance with observations. This also makes it clear why there may be large sunspots without magnetic storms, but we should not anticipate magnetic storms without solar disturbances of some kind, which, if intense, would probably be associated with spots.

### Simultaneity of Commencement of Magnetic Storms.

Bauer concluded from observations made in 1902 and 1903 that magnetic storms do not begin precisely at the same moment all over the world, the velocity of propagation of the disturbances being in general eastwards. On the other hand, Nippoldt had previously concluded that the times of commencement of disturbances over the entire region involved are not measurably different, and went so far as to suggest that they might be of use in determining longitude. Rodes concluded that magnetic storms do not begin at precisely the same moment all over the world; abruptly beginning ones which were investigated by him appeared to progress more often towards the west with a velocity such that it would require about four minutes to encircle the earth at the equator. Rodes put forward the hypothesis that the earth may enter a cloud of electrical particles projected from the sun, in which case the storm will in general be recorded by those situated in the foremost position of the earth's translatory motion. It follows that since the orbital velocity of the earth is such that it traverses a distance equal to its diameter in about $6\frac{1}{2}$ minutes, this also is the total time for the earth to become involved in a cloud. At the Madrid meeting of the section on Terrestrial Magnetism and Electricity of the International Geodetic and Geophysics Union, it was decided that a systematic investigation of sudden commencements of magnetic storms be made, and I believe that Prof. Tanakadate is much of the same opinion as myself with regard to the necessity for similar instruments and the most precise recording of time in order to obtain a decisive answer to this question. It should not be difficult to obtain.

### Need for more Precise Data.

This very hasty sketch of some theories relating to terrestrial magnetism reminds me of Dr. Chree's remarks that the deductions from such theories are just as hypothetical as the theories themselves, and I am very sensible that this rapid survey is not only incomplete, but that no theory considered is completely satisfactory. Moreover, while fully realising that they are vital links in any chain of evidence, I have avoided the companion subjects of auroræ, atmospheric electricity and earth currents, because to have considered them would have made my address far too long. I do, however, wish to emphasise that data of a precise kind are much needed to modify existing theories and to produce new ones, and I cannot do better than conclude with a remark of Rücker's in this city thirty-two years ago. Rücker said: 'If there be any who are inclined to ask whether the careful study of terrestrial magnetism has led, or is leading, to any definite results, or whether we are not merely adding to the lumber of the world by piling up observations from which no deductions are drawn, we may answer that, though the fundamental secret of terrestrial magnetism is still undiscovered, the science is progressing. . . . But there are special and cogent reasons why the science of terrestrial magnetism should be cosmopolitan. For those who would unravel the causes of the magnetic movements of the compass needle concerted action is essential. They cannot, indeed, dispense with individual initiation or with the leadership of genius, but I think that all would agree that there is urgent need for more perfect organisation, for an authority which can decide not only what to do but what to leave undone.'

SECTION B.—CHEMISTRY.

# A STATE EXPERIMENT IN CHEMICAL RESEARCH.

ADDRESS BY

PROF. G. T. MORGAN, O.B.E., D.Sc., F.R.S.,

PRESIDENT OF THE SECTION.

At the Bristol meeting of 1875 my predecessor, Prof. A. G. Vernon Harcourt, spoke to this section on the teaching of chemistry, and in the course of his very inspiring address he remarked that 'the science of chemistry would advance more rapidly if it were possible to organise chemists into working parties having each a definite region to explore,' and he went on to inquire: 'Is such an organisation in any degree possible?'

I propose this morning to describe the attempt recently made by a department of State, namely, the Department of Scientific and Industrial Research, to give effect to Prof. Vernon Harcourt's prophetic vision. The answer to his question is in the affirmative. Such an organisation is in some degree possible, and has actually become an accomplished fact. I must, however, leave for one of my successors in this Chair the further inquiry, Can such an organisation become permanent and still retain its primary and paramount function of chemical exploration?

## Origin of the Chemical Research Laboratory.

The work of the Department of Scientific and Industrial Research began in 1915, and during the ensuing ten years the Department had at various times become interested in investigations of a chemical nature, such, for example, as (1) large-scale researches on the chlorination of methane, (2) large-scale researches on the production of formaldehyde, (3) investigations on the production of glycerine, (4) investigations on the manufacture of chemical products from fish residues, (5) general researches on the corrosion of metals, (6) general researches on high-pressure reactions, including the reactions between carbon monoxide and hydrogen.

These investigations, which were undertaken mainly under the auspices of the Chemistry Co-ordinating Research Board, were carried out by isolated groups of workers, who were often located in widely separated laboratories. One group studied the corrosion of metals at the Royal School of Mines, another examined fish products in the Imperial College of Science and Technology, whereas a third experimented on the chlorination of methane and on the recovery of formaldehyde from waste liquors of wool-scouring at the Royal Naval Cordite Factory in Dorsetshire.

It soon became evident that some increase in economy and efficiency could be attained by bringing together under one roof these scattered groups of workers who would receive encouragement and stimulus by becoming part of a more centralised scientific organisation.

A suitable site was chosen on the Bushy Park Estate in close proximity to the National Physical Laboratory and the Admiralty Research Laboratory, and here in 1924 the building of a chemical laboratory was commenced on a plot of land allowing ample scope for future expansion.

The original plans drawn out by the architects of H.M. Office of Works made provision for three laboratory units each of rectangular shape and built round four sides of a central courtyard. The front and back of the hollow rectangle consist of two two-storey blocks ; the front block designed for general and special small scale laboratories with the necessary offices, the back block arranged to accommodate workshops, service rooms and heating plant. The two sides of the rectangle, which consist of two single-storey blocks with saw-toothed roofs, north lighted and with a clear head room of about sixteen feet, give adequate space for large-scale laboratories.

These buildings are constructed in steel and brick and so arranged that partitions can be readily removed for alterations or extensions. In the two-storey blocks the floors and roofs are formed of hollow concrete tubes, but in the engineering section of the building, where heavy superloading had to be considered, a more rigid type of construction in steel and concrete floors was adopted. In the interest of economy, plaster and other relatively expensive internal finishings were omitted wherever possible, any distemper or paint being applied to flush-pointed brickwork. The floors were covered with stout cork carpet, laid directly on the cement rendering.

The laboratories are equipped with specially designed fittings, the framing and fronts are of stained British Columbia pine, whereas the bench tops and other portions subjected to hard wear are in teak or Iroko wood. The internal drainage to laboratory sinks is effected by open stone-ware three-quarter circular channels finished with acid-resisting glaze. Wherever exposed internally, structural steel and joiners' work are coated with acid-resisting paint. The benches of small scale and special laboratories carry five services—gas, water, steam, vacuum and compressed air. Each room is amply supplied with electric current (D.C.)

In conformity with the neighbouring buildings of the National Physical Laboratory, a simple modern Georgian style was adopted in the design of the elevations of the new laboratory. The buildings are faced externally with multi-stone sand face bricks, reconstructed Portland stone being used sparingly in cornices, string courses and entrance doorways.

The construction of one of these units was started towards the end of 1924, and when scientific work was commenced in the autumn of 1925 about one-third of the first unit had been built, although actually only one room was ready for occupation. The fitting of the remaining laboratories and workshop was, however, rapidly effected, and by the end of 1926 the whole of the available space was fully occupied, the staff then consisting of the superintendent and ten chemists, with one engineering assistant and ten members of the artisan, clerical and general staff.

The frontage to the half unit was commenced in November 1927 and completed for occupation by Easter 1928, and the staff was then increased gradually to its present total strength of about sixty.

Beyond a small addition for stores and workrooms completed in 1929 there has been no further extension of the building, so that after five years rather more than half of the first unit has been erected and put into commission. There has been no attempt to force the growth of this State laboratory, which is still to be regarded as being at an experimental stage.

### Administration and Control.

The work of the laboratory is conducted under the guidance of a Chemistry Research Board, which has taken over certain functions of the older Chemistry Co-ordinating Research Board. This Board is charged with the duty of advising the Department on the programme of work to be undertaken at the laboratory and of exercising general supervision over its execution.

At the outset executive control was exercised by a part-time director of chemical research and a whole-time superintendent, but from 1927 to the present this responsibility has been vested in a whole-time director.

### Programme of Research.

At the present time the scientific and technical staffs are occupied on six specific items of research prescribed on the advice of the Chemistry Research Board, and 'working parties of exploration' are detailed to these mandated researches by the Director.

Now since these explorations were started at different times and in various circumstances, I propose to describe them simply in the order in which they have come under my notice. This arrangement is purely chronological, and has no bearing whatsoever on any order of merit or importance. Moreover, it is essential to success in any research laboratory that each researcher should regard his own investigations as the most interesting and important in the world.

When thus arranged, the six mandated researches are as follows: synthetic resins, low temperature tar, high-pressure chemistry, corrosion of metals, chemotherapy and research on water pollution. In addition to these prescribed investigations a certain amount of general research is carried out at the discretion of the Director.

### Synthetic Resins.

The growing importance of synthetic resins in chemical industry is gauged by the fact that the world's production of formaldehyde resins which was of the order of 9,000 tons in 1921 had increased to 13,000 tons in 1926, of which Great Britain was responsible for 16 per cent., as against 40, 24 and 8 per cent. derived respectively from the United States, Germany and France, other countries accounting for the remaining 12 per cent. Such resins are employed in the manufacture of moulding powders and electrical components. The production by industrially available means of resins of high dielectric capacity is a matter of national importance, and it was with this objective that an investigation of phenol-formaldehyde resins was begun even before the central laboratory was ready for occupation.

In May 1925 a chemist was appointed to work at this problem in the University of Birmingham, and attention was directed to formaldehyde condensations with homologues of phenol, namely, the cresols and xylenols. Experience soon showed that *m*-cresol and 1 : 3 : 5-xylenol were especially suitable for such condensations, which in the case of the former phenol were extended to a semi-works scale.

According to the nature of the catalyst employed, phenol-formaldehyde condensations yield, in general, one or other of two distinct types of resin. Alkaline catalysts lead to the production of resins of 'bakelite' type, which, although originally soluble and fusible, yet possess the property of moulding under the combined effect of heat and pressure into hard insoluble and infusible products constituting by far the more important group of phenol-formaldehyde resins.

Acid catalysts favour the production of resins of 'novolak' type, which, being permanently soluble and fusible, are utilised principally as shellac substitutes in lacquers and varnishes.

*Alkaline Condensations.*—After successful small-scale tests, alkaline condensations of formaldehyde were performed on 24 lb. of *m*-cresol, carried out under factory conditions in a plant of semi-works scale comprising a jacketed reaction vessel, reflux condensers, washing and storage tanks, drying and incorporating vessels and a hydraulic press with heated platens.

A systematic study of this alkaline condensation revealed the presence of several crystalline intermediates which precede the formation of resin. The latter was employed in the production of moulded articles and of laminated boards for electrical testing.

*Acidic Condensations.*—The chemical nature of formaldehyde-phenolic resins is still a matter of speculation, but the appearance of crystalline intermediates in the early stages of acidic condensations is of interest as denoting the course of these reactions. During these researches several crystalline intermediate products were isolated for the first time.

### Formaldehyde-ketone Resins.

In the foregoing formaldehyde-phenol condensations, acetone is sometimes used as a medium, but since in the presence of alkalis this solvent condenses with formaldehyde to yield resins, the chemistry of the process has been elucidated by a study of the interaction of formaldehyde and the ketones under alkaline conditions. As the homologous series is ascended the formation of resin decreases. Acetone yields mainly resin and small proportions of γ-ketobutanol, $CH_3 \cdot CO \cdot CH_2 \cdot CH_2 \cdot OH$ and of the tetrahydropyrone formed by dehydration of the tetrahydric alcohol $HO \cdot CH_2 \cdot CH_2 \cdot CO \cdot C(CH_2 \cdot OH)_3$. Methyl ethyl ketone gives considerable proportions of the following mono- and di-hydric alcohols, $CH_3 \cdot CO \cdot CH(CH_2 \cdot OH) \cdot CH_3$ and $CH_3 \cdot CO \cdot C(CH_2 \cdot OH)_2 \cdot CH_3$ with but little resin. Diethyl ketone furnishes no resin but leads to similar mono-, di- and tri-hydric alcohols.

## Low Temperature Tar.

There is at the present time in this country no process of chemical industry which is more in the public eye than low temperature carbonisation of coal. The matter is of supreme national importance, for the larger problems facing this mode of utilising coal are both economic and technical and turn on the exploitation to the best advantage of the resulting products: smokeless fuel, gas, aqueous liquor and tar. Now since any marked appreciation can be expected only in the case of the last of these products, it follows that processes tending to an increase in the value of the tar are of fundamental interest.

During the last five years a systematic study of the chemical constituents of low temperature tar has been in progress in the Teddington laboratory and, in our experiments on this material, quantities of the order of 40 gallons have been handled in the semi-scale plant. The starting materials, supplied by H.M. Fuel Research Station as part of the Government's scheme of scientific investigation into the utilisation of our national resources of coal, consist of pedigree tars derived from coals of definite origin carbonised under carefully controlled and reproducible conditions.

It was soon found that although low temperature tar had been produced at carbonising temperatures of about 600°, yet it could not again be heated even to comparatively low temperatures—round about 150°—without undergoing considerable alterations of a chemical nature. Accordingly, distillation processes were replaced by milder methods of extraction, and the tar was not heated above 120° until its more decomposable constituents had been removed.

A representative tar from a typical bituminous coal (Kinneil coal) was heated to 120° to remove light oils and adhering aqueous liquor, and the residue extracted by systematic use of solvents to separate it into its major constituents: neutral oils and waxes, aromatic hydrocarbons, bases, phenols and carboxylic acids. It was then noticed that each of these five main groups of products could be separated into two fractions, one portion consisting of crystallisable substances conveniently termed 'crystalloids,' the other portion composed of amorphous resinous materials to which the name 'resinoids' was applied.

## The Crystalloids of Tar.

*Waxes and Neutral Oils.*—From the least volatile fractions of neutral oils, waxes are obtained melting over a considerable range of temperature, and X-ray analysis of the less fusible of these waxes has revealed the presence of hydrocarbon chains containing 26, 27 and 29 carbon atoms.

The neutral oils contain both saturated and unsaturated hydrocarbons and also oxygenated substances reacting with ferrichloric acid, $HFeCl_4$.

*Aromatic Hydrocarbons.*—Naphthalene, a characteristic major constituent of high temperature tar, is present in low temperature tar, together with β-methylnaphthalene, but only in such small proportions that they have to be separated through their picrates.

The least volatile tar oils after removal of waxes and resins deposit on cooling a material analogous to the green grease of high temperature tar. This product consists principally of the methyl derivatives of anthracene, although a small proportion of this hydrocarbon itself may possibly be present. Oxidation of various fractions from this product leads to 2-methylanthraquinone, 2 : 6- and 2 : 7-dimethylanthraquinones and 2 : 3 : 6-trimethylanthraquinone. The proof of the orientation of methyl groups in these anthracene derivatives has involved the synthesis of the hydrocarbons and of their quinones (*Journ. Chem. Soc.*, 1929, 2203 and 2551).

*Bases.*—The volatile bases of low temperature tar are mainly tertiary amines although a small amount of aniline was detected. The following, bases were isolated and purified through their crystalline mercurichlorides : pyridine, *a*-picoline, 2 : 4- and 2 : 6-lutidines and symmetrical collidine ; quinoline and quinaldine were isolated as picrates.

*Phenols.*—Low temperature tars contain a high proportion of material extractable with aqueous caustic soda, but only a portion of this soluble extract consists of true phenols, the remainder is composed of non-phenolic substances which, however, dissolve in solutions of the alkaline phenolates. These non-phenolic materials are recovered from a caustic soda extract of the tar either by agitation with an organic solvent such as chloroform, or more simply by saturating the alkaline extract with salt. The true phenols remaining in the alkaline solution are separated into crystalline and resinous portions by solution of the former in light petroleum. Further fractionation of the petroleum soluble phenols has led to the isolation of the following compounds : phenol, the three cresols and five of the six possible xylenols. Bacteriological examination of the phenols of low temperature tar has shown that their germicidal value increases with rise of boiling point to an optimum fraction boiling at 140-170° under 5 mm. pressure. Moreover, it has been found that direct chlorination of these higher phenols of low temperature tar increases considerably their germicidal potency.

## The Resinoids of Tar.

With each class of crystalloid in the low temperature tar there is present a corresponding resinoid, which constitutes the least volatile portion of each major fraction. These products, which are termed respectively resinenes (neutral resins), resinamines (basic resins), resinols (phenolic resins) and resinoic acids (acidic resins), are obtained as amorphous powders after extraction of the corresponding crystalloids with petroleum or other suitable solvent. These resinous tar products are promising materials for further research from both scientific and industrial view points.

An extension of the solvent method of extraction to other varieties of tar from wood, peat, lignite and bituminous coal has revealed the presence in each tar of the four classes of resins, although in wood tars the amount of resinamines was very small. Coal tars produced by carbonisation at high and at intermediate temperatures show considerable variations in their resin contents.

## Aqueous Liquors of Coal Carbonisation.

The aqueous liquors which accompany low temperature tars have been extracted systematically with organic solvents in quantities of 30 gallons at a time, and in this way phenol, *o*-cresol, catechol and its two methyl derivatives resorcinol and quinol, have been isolated, together with a new type of resins to which the name resinolic acids has been given, as they are intermediate in chemical properties between resinols and resinoic acids. Resinolic acids in the presence of ammonia are largely responsible for the dark red colour of the aqueous effluents from gasworks. These aqueous liquors have also furnished on systematic extraction aniline, pyridine and α-picoline, and the series of fatty acids ranging from formic to *n*-valeric acids.

### High Pressure Chemistry.

During the past ten years increasing attention has been directed to the use of pressure as a means of facilitating the course of chemical reactions, and research on high pressure syntheses was started at the laboratory in 1926 on the recommendation of the Chemistry Co-ordinating Research Board, whose members were impressed by the possibilities revealed by the work of Patart in France and of the Badische Anilin und Soda Fabrik in Germany.

The plant required for this investigation was designed and built in the laboratory workshop, and the earliest experiments were carried out with hand compressors. Subsequently, motor-driven compressors and circulators were added to the equipment. This plant was first tried out with catalysts of the Patart type (normal or basic zinc chromate) in order to gain skill and confidence in the process. It was thus found that on passing the mixed gases (1 vol. CO, 2 vols. $H_2$) at the rate of 30,000 vols. per hour, measured at N.T.P. over unit volume of such a catalyst at 380° and under 200 atmospheres' pressure the hourly production of methyl alcohol was about twice the volume of catalyst space.

The addition of cobalt chromate or nitrate to the foregoing zinc chromate catalyst led to an interesting development, since with the more complex catalyst ethyl alcohol and other higher alcohols made their appearance, although methyl alcohol remained the predominant product. Small amounts of aldehydes and acids were also detected. By the use of mixed cobalt catalysts containing zinc, together with chromium or manganese, the following alcohols have been obtained in addition to methyl and ethyl alcohols: *n*-propyl, *n*-butyl, *iso*-butyl and *n*-amyl alcohols and racemoid 1-methyl-propylcarbinol $CH_3.CH_2.CH(CH_3).CH_2.OH$. So far only primary alcohols have been detected. Aldehydic products have been identified as follows: formaldehyde, acetaldehyde, propaldehyde, *n*-butaldehyde and also certain aldehydals arising from the condensation of the foregoing aldehydes and alcohols. Moreover, the synthetic products contain formic, acetic, propionic and *n*-butyric acids.

The addition of even small proportions of cobalt to copper-manganese oxide catalysts (Audibert type) has a marked effect on the production of ethyl alcohol and its homologues, and a similar result is noticed on replacing the cobalt in these catalysts by iron. Traces of alkali hydroxide promote the formation of higher alcohols, and in this respect potash is more efficacious than lithia.

### Helium from Monazite Sand.

In addition to their synthetic experiments, the staff engaged on high pressure chemistry have brought to completion a research on the extraction of helium from the monazite sand conveyed to this country from

Travancore for the manufacture of thoria and ceria required in the incandescent mantle industry. During this manufacture each gram of sand evolves 1 c.c. of helium at N.T.P., so that 100 tons of sand would discharge into the atmosphere approximately 100,000 litres of the gas. Our requirements of this raw material were entirely met through the kind assistance of the late Mr. Edmund White, formerly managing director of Messrs. Thorium, Limited.

The gas was liberated by heating the monazite at 1000° in heat-resisting steel pots in a stream of carbon dioxide, and the issuing gas was passed over cupric oxide at 500° to oxidise hydrogen and carbon monoxide. Carbon dioxide was then removed by aqueous caustic soda and the residual gas passed over metallic magnesium a t600° in order to remove nitrogen, and over metallic calcium at 580° to eliminate the remaining impurities. Several hundredweights of sand were thus treated and returned to Messrs. Thorium, Limited, who found that they could still employ the heated material in their process providing that it was mixed with a certain proportion of raw sand. The purified gas containing 99.5 per cent. of helium was compressed into storage cylinders.

## Corrosion Research.

The researches on corrosion were originally started by a committee of the Institute of Metals in 1916, and after eight years the more scientific developments of these problems were undertaken by the Corrosion Research Committee of the Department of Scientific and Industrial Research, this work being pursued in the Metallurgical Department of the Royal School of Mines until the workers concerned were transferred to Teddington at Easter 1928.

### Corrosion of Immersed Metals.

Research on the corrosion of immersed metals has been concentrated on an attempt to put the theory of this phenomenon on a secure quantitative foundation. For although earlier work in this country and in the United States had furnished a qualitative explanation of corrosive action in water or in salt solutions, yet this description of the process postulated the influence of more than a dozen factors on the corrosive rates of immersed metals. Accordingly, one aim of the present research is to acquire precise information as to the interaction of these factors, and another objective is to ascertain whether the lack of reproducibility in corrosion experiments is inherent in the corrosion process itself or whether it is due to imperfect regulation of all variables. Among these factors are purity of materials : metal, water, salt and atmosphere, constancy of temperature and pressure, and freedom from mechanical agitation. Zinc of a purity of 99.99 per cent., distilled water with an electrical conductivity of 0.058 gemmhos at 20° and purified oxygen were employed, and all experiments were carried out at 25° within a temperature range of ±0.02° over long periods of time, sometimes for upwards of six months.

Measurements of oxygen absorbed, corrected for any hydrogen evolved, made at frequent intervals during the course of such experiments, have enabled one to plot continuous corrosion time curves which are often sufficiently smooth and regular to be investigated mathematically.

The apparatus employed for this purpose is shown among the exhibits from the laboratory. Originally designed for zinc, it is now being used extensively for work on iron and steel.

Oxygen passes through water or salt solutions to the immersed metal either by diffusion or convection, but the latter mode of transference is by far the more effective at more than very shallow depths. Convection currents may arise in a salt solution owing to four different causes: (1) temperature changes, (2) density changes produced by evaporation at the surface layer, (3) density changes produced by differences of oxygen concentration, (4) mechanical agitation. The apparatus employed for these quantitative experiments is immersed in a thermostat and corrosion occurs in a closed space within it, so that the effects of temperature changes (1) and evaporation(2) are practically negligible, and special precautions are taken to prevent agitation (4). Accordingly, by removing oxygen from the neighbourhood of the metal, the corrosion process produces convection currents of the third category due to changes in concentration of oxygen. The velocity of these convection currents depends on the difference in density between the solution saturated with oxygen at the liquid surface and the solution next to the metal. Assuming that the latter solution contains very little oxygen, the velocity of convection will probably be proportional to the solubility of oxygen in the liquid, but the amount of oxygen carried by the current is also proportional to its solubility. Hence, the rate of corrosion ($y$) should be proportional to the square of the oxygen solubility ($x$), a relation which is expressible by the equation $y=kx^2$. This assumption has been verified for on plotting the observed rates of corrosion against oxygen solubility one obtains curves of parabolic form.

Hydrogen evolution due to the interaction of water or salt solution with metals such as zinc or steel is of greater importance than is generally supposed. Determinations of the hydrogen liberated during zinc corrosion have shown that a very small amount of impurity has a considerable influence on the amount of gas evolved. In N/10,000 potassium chloride measurable quantities of hydrogen are obtained from 99.99 per cent. zinc, whereas no hydrogen was detected from zinc of spectroscopic purity. The proportion of zinc corrosion due to evolution of hydrogen increases with concentration of potassium chloride, and with 2N-solutions it amounts to 17.4 per cent. of the total corrosion.

When all the foregoing factors are taken into account, successive corrosion experiments exhibit a high degree of reproducibility, and the curves indicate that duplicates differ from their mean value by 1 per cent. or even less. This constancy indicates that the corrosion of zinc and allied metals is not inherently erratic, but is quite a suitable subject for physico-chemical investigations.

### Atmospheric Corrosion.

Investigations of various types of indoor and open-air corrosion and of protective oxide films, previously conducted under the auspices of the Atmospheric Corrosion Research Committee of the British Non-ferrous Metals Research Association, were taken over by the Department of Scientific and Industrial Research in July 1927. This work was continued at the Royal School of Mines until April 1928, when the corrosion section was transferred to the Chemical Research Laboratory. The more outstanding results since obtained are as follows :—

*Composition of Green Patina on Copper Structures.*—Samples of the familiar green patina on exposed copper surfaces, obtained from typical localities, town, country, marine and urban-marine, were analysed completely. Contrary to the general belief, basic copper carbonate was found to be not the principal but only a minor constituent of the green patina. With the exception of the product from a purely marine atmosphere in which basic copper chloride predominated, the major constituent throughout was basic copper sulphate, and excess of basic sulphate over basic carbonate was greater in the rural than in the urban samples. Where urban and marine conditions coincided, basic sulphate predominated greatly over both basic chloride and basic carbonate.

It has recently been found that these constituents of the green patina tend to assume the chemical composition of the corresponding green copper minerals. In the limits, the basic copper sulphate of corrosion coincides in composition with brochantite, of which the co-ordination formula is $[Cu\{(HO)_2Cu\}_3]SO_4$, and the basic copper chloride of corrosion with atacamite $[Cu\{(HO)_2\ Cu\}_3]\ Cl_2$. Basic copper carbonate, on the other hand, tends to assume the composition of malachite $[Cu\{(HO)_2\ Cu\}]\ CO_3$. Complete agreement with the composition required by the co-ordination theory has been realised in corrosion products after 70 years' exposure and upwards. After shorter periods of exposure the basicity of the product is in a lower ratio than that of the corresponding minerals.

The complete analysis of these corrosion products entailed special precautions. The carbonates were decomposed by phosphoric acid instead of hydrochloric or sulphuric acid, and any hydrogen chloride and hydrogen sulphide simultaneously set free were eliminated by *p*-nitrosodimethylaniline and copper powder respectively.

*Corrosion of Magnesium Alloys.*—The growing use of light magnesium alloys for motor-car and aircraft work has necessitated increased attention to the corrosive properties of these metals. In 1929 a research was begun with the object of discovering improved methods of protection and of learning more about the nature of the corrosion. More than 500 different protective coatings have been produced by chemical means and tested for resistance to sea-water sprays. Of these coatings a few are sufficiently promising to warrant further study.

## Chemotherapy.

In 1927 a joint Exploratory Committee of the Department of Scientific and Industrial Research and of the Medical Research Council decided that there was need for organised research in Chemotherapy, and accordingly the Medical Research Council set up a permanent Committee to advise them on investigations in this field. To this Committee the Department has nominated three chemical members, including the Director of Chemical Research, and facilities have been afforded by the Department for a staff of three chemists to work on problems based on an agreed programme. These chemists have already prepared a considerable number of organic compounds of possible utility in chemotherapy, and these are being tested systematically under arrangements made by the permanent Committee. This work of national importance is a joint effort of several groups of chemists working in different laboratories, so that a wide and thorough search for greatly needed drugs and therapeutic agents is in progress.

The Teddington contribution to these researches may be classified under the two following main headings :—

1. *Analogues of Bayer* 205 *or Fourneau* 309.—Last year, in his Presidential Address to the Physiology Section of the British Association in South Africa, Prof. W. E. Dixon referred to the serious ravages produced in that continent by sleeping sickness (trypanosomiasis), and his admirable survey of the position from the view point of chemotherapy renders unnecessary any further elaboration of that aspect of the problem in the present summary.

The activity of medicaments of the Bayer 205 or Fourneau 309 type may depend more on the aggregate effect of the whole molecule rather than on the presence in the molecule of any particular group or arrangement. In this, as in other cases, there are no definite laws connecting therapeutic activity and chemical structure.

Compounds have been prepared in which the terminal aminonaphthalene-disulphonic radicals have been replaced by analogous complexes derived from aminocarbazole di- and tri-sulphonic acids or from the disulphonic acids of aminofluorene and of aminofluorenone, but so far the effect of this substitution has not been encouraging. The possibility of a beneficial introduction of arsenic into the fluorene nucleus has, however, been considered, and experiment has shown that trypanocidal activity is manifested when an arsinic acid radical is present in a fluorene molecule in conjunction with an amino-group.

2. *Organic Derivatives of Arsenic and Antimony.*—During many years organic arsenicals have received much attention, whereas organic antimonials have not been subjected to the same careful scrutiny, partly owing to the fact that they are more difficult to prepare in a state of purity, and partly because the curative results have been less promising.

Nevertheless, since antimony in organic combination appears to possess specific trypanocidal activity and some curative action in kala-azar, experiments have been made in the Teddington laboratory on the preparation of antimony analogues of the more successful arsenicals. Tryparsamide (phenylglycine-amido-*p*-arsinic acid) is used extensively in treating trypanosomiasis, and its antimony analogue has been under examination. In the more stable meta series, phenylglycine-amido-*m*-stibinic acid and certain allied compounds show a slight trypanocidal effect. The antimony analogue of stovarsol (3-acetylamino-4-hydroxy-phenyl arsinic acid), or more probably its internal dehydration product, has also exhibited some therapeutic activity.

Concurrently with this study of organic antimonials further experiments have been made on organic arsenicals produced by condensing atoxyl successively with succinic anhydride, and with a base such as ammonia, methylamine, dimethylamine, piperidine, or aniline. Certain of these derivatives have also exhibited a definite action on trypanosomes.

In addition to the preparation of antimonials directly applicable to therapeutic tests, our knowledge of the organic chemistry of antimony has been extended among aliphatic derivatives by the production of antimony analogues of the cacodyl group and in the aromatic series by the synthesis of cyclic antimonials analogous to the alkyl- and aryl-carbazoles.

### Water Pollution Research.

This research originated from a joint request made to the Department of Scientific and Industrial Research by the Ministry of Health and the Ministry of Agriculture and Fisheries.

During the past two years, experiments have been in progress under the auspices of the Water Pollution Research Board on the base-exchange method of water softening. One of the objects of this work has been to determine the most satisfactory way of carrying out the process, such points having been examined as the effect of varying the rate of flow of water through the bed of base-exchange material and the quantity, concentration and time of contact of the salt solution used in regenerating this material. There are two types of base-exchange material in actual industrial use, treated minerals and synthetic products prepared by interaction of solutions of sodium aluminate and sodium silicate. It appears from the result of the Teddington experiments that with treated minerals the exchange of bases is confined to the outer surface of the particles whereas with the synthetic materials diffusion to the inner surfaces or into the mass of the gel is an important factor. This study of the base-exchange process has also been extended to the case of waters rich in magnesium.

Disintegration of the base-exchange materials and contamination of the softened waters by silica and alumina have been investigated. At the rate of flow employed normally in water softening, the silica content of the water is not increased seriously and is certainly not greater than that often encountered in untreated waters.

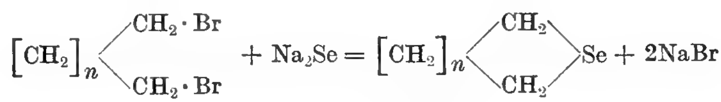
In addition to this practical work a report summarising existing knowledge of the base-exchange or zeolite process for water softening has been compiled and published.

### General Research.

*Investigation of complex aromatic hydrocarbons.*—In 1926 the Dyestuffs Industry Development Committee of the Board of Trade suggested that further fundamental research was desirable on the following coal tar products: acenaphthene, carbazole, fluorene, perylene and phenanthrene. Two of these suggestions were adopted and, with the assistance of two chemists, the Director, who is also a member of this Statutory Committee, undertook a study of acenaphthene and perylene, the work being continued until 1928. During this period considerable progress was made with the former hydrocarbon, the nitration of which was studied under anhydrous and hydrous conditions. For nitrations, in the absence of water, diacetylorthonitric acid and benzoyl nitrate were employed, the latter being a reagent discovered originally in 1906 by Prof. Francis of this university. Several new nitro derivatives were identified, and 2-amino-acenaphthene and 2-acenaphthenol (2-hydroxyacenaphthene) were prepared for the first time.

*Higher Fatty Acids.*—In order to identify the waxes isolated from low temperature tar a parallel research was made on the synthesis of individual waxes from the higher fatty acids. Those waxes containing an even number of carbon atoms were produced electrolytically by Kolbe's classical synthesis (1849), whereas their homologues containing odd numbers of carbon atoms were prepared by a more modern process due to Grün, Ulbrich and Krczil (1926). By these complementary processes individual waxes containing 27, 30 and 34 carbon atoms were prepared for comparative purposes. This inquiry necessitated the study of several higher fatty acids, including arachidic acid, and in such cases analytical data were confirmed by X-ray analyses carried out in the National Physical Laboratory.

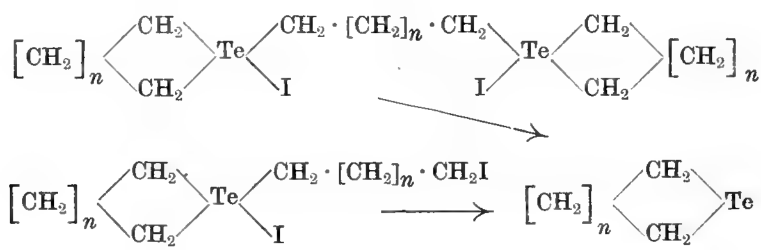
*Cyclic systems containing Selenium and Tellurium.*—Considerable progress has been made in the study of heterocyclic systems containing selenium or tellurium atoms. The selenium series has been prepared by a general method, the interaction of alkylene bromides and sodium selenide in an inert atmosphere.




$n = 1, 2, 3,$ or 4.

In this way the cyclic selenohydrocarbons with $n$=1, 2, 3 or 4 have been obtained for the first time. The five-membered ring, *cyclo*selenobutane (tetrahydroselenophen) and its next homologue, *cyclo*selenopentane, are formed by the foregoing reaction with considerable facility, but the four- and seven-membered rings show signs of instability, and in their production complex solid polymerides make their appearance.

In the tellurium series the corresponding cyclic derivatives are conveniently prepared by the action of aluminium telluride on alkylene halides. This process leads to the production of cyclic systems containing quadrivalent tellurium.




$n = 1, 2,$ or 3.

From the foregoing complex telluronium iodides the cyclic tellurohydrocarbon is obtained by thermal dissociation under reduced pressure. By such means *cyclo*tellurobutane and *cyclo*telluropentane have been isolated and some evidence was obtained of the existence of a four-membered ring.

*Aromatic Selenium and Tellurium Compounds.*—Phenol and the cresols have been condensed with selenium oxychloride when two types of seleniferous products have been distinguished, polar salt-like substances (Formula I) and non-polar selenides (Formula II).

I $[(HO . C_6H_4)_3Se] Cl$ $Se (C_7H_6 . OH)_2$ II

When the cresols are condensed with basic tellurium chloride the following types were distinguished, all containing quadrivalent tellurium: $HO . C_7H_6 . TeCl_3$, $(HOC_7H_6)_2TeCl_2$, $(HOC_7H_6)_3TeCl$. The more soluble of such selenium and tellurium compounds have been tested on trypanosomes, but so far no evidence of activity has been discerned.

*Studies in the Diphenyl Series.*—The *o*-xenylamine required in the synthesis of cyclic antimonials was formerly obtained in a somewhat tedious manner by the pyrolysis of diazoaminobenzene. This base has now been prepared by a method practicable on a large scale from commercially obtainable diphenyl.

*o*-Xenylamine and its homologues, for example, 4′: 5-dimethyl-*o*-xenylamine, are convenient starting-points for the synthesis of carbazole and phenanthridine derivatives.

### Residual Affinity and Co-ordination.

An experimental study of the effect of various co-ordinated addenda on the valencies of copper, silver and gold has been pursued during the past five years with the following results.

*Stabilisation of the cupric condition.*—In the absence of suitable addenda the cupric ion is unstable when in combination with less electronegative anions such as iodide, sulphite, thiosulphate, thiocyanate, selenocyanate and hypophosphite, but by co-ordinating this metallic ion with ethylenediamine ($en{=}NH_2.[CH_2]_2.NH_2$) stability is thereby conferred on the bivalent condition, and well-defined complex salts of the following types are obtained: $[Cu, 2en, 2H_2O]I_2$, $[Cu, 2en, R.OH] I_2 (R{=}CH_3$ or $C_2H_5)$, $[Cu, 2en]X$ where $X{=}SO_3$, $S_2O_3$, $S_2O_6$, $S_3O_6$ or $S_4O_6$ and $[Cu, 2en] Y_2$ where $Y{=}CNS$, $CNSe$ or $H_2PO_2$. Moreover, the following stable normal salts $[Cu, 2en] CO_3, 2H_2O$, and $[Cu, 2en] (NO_2)_2$ are obtainable with carbonate and nitrite radicals respectively.

*Stabilisation of the cuprous condition.*—Even more noteworthy than the preceding effect of ethylenediamine is the influence of addenda containing sulphur on the stability of the cuprous ion. Cuprous sulphate, an endothermic compound, decomposes in water with generation of heat and loss of half its copper, $Cu_2SO_4 + 5Aq = CuSO_4, 5Aq + Cu$, but by co-ordinating the cuprous ion with ethylenethiocarbamide, $etu = \begin{matrix} CH_2 . NH \\ | \\ CH_2 . NH \end{matrix} \!\! > \! CS$ the univalent condition becomes stabilised even in combination with nitrate, sulphate and acetate radicals. The following colourless water-soluble salts have been identified: $[Cu, 4 etu] NO_3$, $[Cu, 3 etu]_2 SO_4$ and $[Cu, 3 etu] CH_3CO_2$.

*Co-ordination compounds of silver.*—Since the silver ion is generally univalent, its co-ordination with ethylenethiocarbamide or other sulphur containing addenda does not involve any change of valency. It is, however, significant that [Ag, 3 etu] Cl is a water-soluble salt which remains colourless even after prolonged exposure to light.

A contribution to the chemistry of bivalent silver has been made by co-ordinating its ion with *a*-α′-dipyridyl (dipy), and the following coloured salts have been isolated [Ag, 2 dipy]$S_2O_8$ (chocolate brown), [Ag, 2 dipy] $(HSO_4)_2$ (dark brown plates) and [Ag, 3 dipy] $(ClO_3)_2$ crystallising in well defined, lustrous, black, acicular prisms.

*Stabilisation of the aurous condition.*—Co-ordination of gold salts with ethylenethiocarbamide has the same effect as with copper compounds. The fundamental univalency of the metallic ion becomes stabilised so that the following complex aurous salts have been identified: $[Au, 2\ etu]_2$ $SO_4$, $2H_2O$, [Au, 2 etu] $NO_3$, [Au, 2 etu] Cl, $H_2O$ and [Au, 2 etu] Br, $H_2O$. These compounds are colourless and dissolve in water to practically neutral solutions ($P_H$ value about 6·2). Conductivity experiments indicate that in dilute aqueous solutions these complex salts are highly ionised so that the complex radical $[Au, 2\ etu]^1$ plays the part of a compound alkali ion. The bromide of this series was mentioned last year by Prof. W. E. Dixon (*loc. cit.*) as being a compound which had the effect of delaying death when administered to animals infected with bovine tuberculosis.

### Chemical Engineering.

The mainstay of the foregoing investigations are the well-equipped workshops manned by five skilled artisans who are engaged on the production and maintenance of the appliances and plant required in the various research programmes. Appliances for high-pressure chemistry are a speciality of the laboratory workshops, and such plant includes bombs and pre-heaters for flow-through experiments with gaseous reagents, and autoclaves of various types for reactions with gases, liquids and solids. The researches on tar products call for automatic extractors, filter plant and stills operating under either ordinary or diminished pressures.

### The State Laboratory and the Scientific Public.

The twofold primary aim of any State research laboratory should be the collection and dispersal of scientific knowledge and information. For the former function of collection and discovery of new knowledge the exploring parties foreseen by Prof. Vernon Harcourt should supply an adequate means providing that each group proceeds under enlightened and inspired leadership. But for the complementary function of dispersal of information a chemical laboratory must depend largely on such well-established media of publication as the journals of the leading chemical societies. The greater part of the published research of the Teddington laboratory has appeared in the Journals of the Chemical Society and of the Society of Chemical Industry, although a certain proportion has been published in the Proceedings of the Royal Society, Journal of the Institute of Metals, and Proceedings of the Institution of Chemical Engineers. Grateful recognition should be recorded for the generous aid afforded by all these learned societies, and special thanks are due to the first two mentioned. It is my personal opinion that this mode of dispersing chemical knowledge should have priority over its publication in official governmental reports. First, because in this way the information radiates more rapidly to a wider public; thus each of the two chemical journals just mentioned has more than 5,000 registered readers. Secondly, because this form of publication is frequently preceded by a reading and discussion of the subject-matter at a scientific meeting, and lastly because the financial circumstances of the learned societies compel them to impose a limit on the length of communications which is conducive to brevity and conciseness.

### Relations with other Scientific Institutions.

Apart from substances of therapeutic interest prepared for the Committee on Chemotherapy, numerous other research materials have been distributed to colleagues in the universities and research institutions. Compressed helium and carbon monoxide have been rendered available for scientific workers requiring these gases. Organic derivatives of tellurium have been lent to the Cambridge University Chemical Laboratory for the purpose of physico-chemical measurements, and to the Birkbeck College for the demonstration of the parachor of this element. Compounds of special chemical interest have been supplied to the Davy Faraday Laboratory and to the National Physical Laboratory for the X-ray study of their crystal structure. It is a pleasant duty to refer to the aid received from the Government Laboratory in respect of micro-analyses and in connexion with the work on synthetic resins.

Reference has already been made to the close collaboration of the laboratory with H. M. Fuel Research Station in regard to the products of coal carbonisation. Certain preparations from low temperature tar have been submitted to the Cotton and Woollen Research Associations, for examination in connexion with the chemical treatment of textile fibres.

### Relations with Chemical Industry.

The associations of the laboratory with chemical industry have always been cordial and are daily becoming increasingly intimate. Prominent industrialists either individually or in their corporate capacity as members of the Association of British Chemical Manufacturers and allied organisations have visited the laboratory and sometimes repeatedly.

Arising out of these visits and informal conferences, more than a hundred samples of the research products of the laboratory have been distributed to interested enquirers.

Members of the scientific staff participate in the work of the Committee for the Standardisation of Tar Products Tests, the Bureau of Chemical Abstracts, the Corrosion Committee of the Iron and Steel Institute, and the Council and various Committees of the Society of Chemical Industry.

Although the laboratory is not a teaching institution in the academic sense of the term, yet facilities have been afforded for collaboration in research to chemists in training of approved qualifications. The two leading metropolitan gas companies have seconded to the laboratory for this purpose junior members of their scientific staffs who have worked at Teddington for periods ranging from six to eighteen months. The subjects so far selected for this collaboration have been high-pressure chemistry and low temperature tar.

In the foregoing description of the activities of the new laboratory I have endeavoured to speak as historian rather than as advocate, but if any justification is to be included I would take as the two leading points of my case: First, the scientific and industrial importance of the researches completed and in progress; secondly, the significant fact that of the sixteen members of the laboratory staff who have resigned during the five years, fourteen have gone into chemical industry to occupy positions of considerable importance and responsibility. The appreciation of chemical talent is a valuable function of this State laboratory.

### Anticipations and Current Tendencies.

Those who feel sufficiently interested in the realisation of Prof. Vernon Harcourt's vision should not fail to visit the exhibit of laboratory products now on view in an adjacent room, for these specimens, diagrams, models and photographs furnish a record of the researches of this youthful organisation which is far more realistic and appealing than any words of mine can be.

Certain of these investigations have an immediate practical objective; others represent the long view. It is, however, impossible to draw a definite distinction between these contrasted types. The aim of a State laboratory should rather be to encourage a judicious blend of the two.

The chemical preparations now selected for exhibition as representing the work of the first five years are only the more distinctive specimens of a much larger collection which is continually being accumulated and classified. In a similar orderly manner chemical knowledge is being collected and systematised in the files and card-indexes compiled by members of each exploring party. So soon as any particular research is sufficiently complete it is contributed to the appropriate learned society. Occasionally publication takes the form of patent specifications. By such concerted efforts the laboratory must come to be recognised as a storehouse of chemical information at least for those branches of the science which are included in the scope of its researches.

Is it desirable that this scope should be extended, and if so in what directions? This is not the occasion to discuss matters of departmental policy, but, in my present capacity, I may, like my predecessor of fifty-five years ago, indulge in anticipations of how future developments might be of advantage to chemical science in general and to British chemistry in particular.

### Inorganic and Mineral Chemistry.

An eminent authority has recently enquired what has become of inorganic chemistry, and this question is frequently repeated. The present answer is that, so far as this country is concerned, the subject is no longer investigated systematically. British chemists are now for the most part content to leave this work of exploration to their contemporaries in other lands. Yet the British Empire is endowed with mineral resources to an extent unsurpassed by any other nation or empire under the sun. It can scarcely be contended that in this respect we are rendering an adequate account of our stewardship. Although there are a few meritorious exceptions, one may say broadly that there is no sustained British attempt to study the rare earths, the less common alkalis, or the metals of the platinum group. Such chemical curiosities as beryllium, gallium, germanium, indium and thallium rarely excite the scientific interest of our investigators. Yet the chemical study of the less common elements, and especially of those grouped under the disparaging term of 'minor metals,' is a matter of considerable scientific importance and one which sooner rather than later is likely to yield results of industrial value. If proof of this statement is needed, reference may be made to the inert gases which were first noticed in 1894 and subsequently found by Ramsay and Travers to be five in number. To-day three of these gases are employed industrially.

I have already mentioned low temperature tar which is literally a burning question. The great German combination of chemical factories—the Interessen Gemeinschaft—have recently filed patents describing the catalytic effect of molybdic acid on the hydrogenation under pressure of this intractable material. They claim a clear volatile product obtainable in good yield and suitable for motor fuel. Further investigation shows that this beneficial catalytic influence is peculiar to molybdenum compounds and is not possessed by analogous compounds of the other metals of the sixth periodic family. It certainly pays to study chemically the idiosyncrasies of the rarer elements and their derivatives.

## The Organic Chemistry of Vital Products.

At the Bristol meeting of 1898, Prof. F. R. Japp's presidential address to this section dealt with the subjects of stereochemistry and vitalism. He called attention to Nature's method of preparing single optically active substances, and referred to the insufficiency of the mechanical explanation of vitalistic phenomena.

Considerable advances have since been made in our knowledge of the fundamental process of photosynthesis, notably as the result of suggestive discoveries by Prof. Baly and his collaborators, but nevertheless we still have much to learn from Nature in regard to the synthesis of carbon compounds. This study of the products of the vital activities of animal and vegetable organisms was the original province of organic chemistry, and to this circumstance the science owes its distinctive name. During the last eighty years, however, organic chemists have extended the scope of enquiry to many substances which are produced not as the result of vital forces, but through the agency of the laboratory arts.

For instance, the organometallic compounds, which have no counterparts in nature, have received intensive study because of their influence on the development of modern chemical theory, their practical application in many operations of organic synthesis and their utilisation as drugs, weapons of chemical warfare and antidetonants. No objection can be urged against the continued investigation of such important artificial products providing that naturally occurring organic materials are not overlooked.

Prof. Japp's address supplies the philosophic reason for a closer study of the products of vital activity, and at present other more mundane considerations may be adduced in support of such researches.

Political and economic forces are bringing into prominence the urgency for a mutually advantageous interchange of commodities between the constituent nations and colonies of the British Empire, and in this pooling of natural resources organic chemistry must play an essential part. Many of the natural products of the dominions and dependencies are in need of systematic chemical study.

Animal and vegetable fats have been mentioned by an investigator in that field as constituting a neglected chapter of organic chemistry, but the phrase is at least as applicable to many other groups of organic substances, for example: the essential oils, the natural gums and resins, and the numerous products ot fermentation processes.

By catalytic reductions, involving high temperatures and pressures, one obtains from the oxides of carbon many members of the homologous series of alcohols, aldehydes, fatty acids and esters. Plant life accomplishes similar results under ordinary atmospheric conditions. A comparative study of these two dissimilar sets of processes is clearly demanded.

The importance of imparting to organic chemistry an increasingly biological bias has been illustrated in a convincing manner by my immediate predecessor, Prof. Barger, so that anything more than a passing reference to this desirable tendency is hardly required of me. Perhaps, however, I should add that in stressing the need of more systematic research in inorganic and mineral chemistry and in the organic chemistry of vital products, I am convinced that the best results will only be attained if the problems are attacked with the newest weapons which the armoury of modern physics can provide.

The primary object of such investigations is the collection of accurate chemical information, but the workers in these two great fields should be stimulated in every possible way to keep a shrewd look-out for any practical applications of their scientific knowledge. When viewed from this standpoint it will be realised that a State experiment in chemical research such as I have described provides competent and enterprising investigators with favourable opportunities for developing their inventive talent in fundamental work of national value and importance.

SECTION C.—GEOLOGY.

# SOME EPISODES IN THE GEOLOGICAL HISTORY OF THE BRISTOL CHANNEL REGION.

ADDRESS BY

Prof. O. T. JONES, M.A., D.Sc., F.R.S.

PRESIDENT OF THE SECTION.

It is manifestly impossible for me, in the time at my disposal on this occasion, to deal adequately with all the events and processes which have played a part in the shaping of the Bristol Channel and its bordering lands. I intend, therefore, to select certain episodes or phases in the geological record which are in themselves of interest to geologists or have a bearing upon the geology and physical features of a wider region than the channel and its confines. Those of you who are not acquainted with it already may be reminded that a concise summary of the Evolution of the Bristol Channel, by Dr. F. J. North, was recently issued by the National Museum of Wales.

The geological history of the region raises many problems that are of interest both to geologists and geographers, but much work remains to be done before our knowledge is fully adequate to deal with them. The suggestions which I shall presently make are offered not as ready-made solutions of these problems, but rather with the idea of stimulating further investigations which will, I trust, lead to fuller knowledge.

As most of you know, the Bristol Channel is bordered in the main by various Palæozoic formations, but towards the east Mesozoic rocks form a large part of the coast and lands adjoining it. These strata are masked in places by Alluvial Deposits, the largest area of which is that known as the Bridgewater Flats.

The episodes which appear to be the most important to consider in relation to the history of the region are (i) the Triassic planation; (ii) the formation of the Mesozoic Cover; (iii) the Miocene movements; (iv) the late Cainozoic uplift; and (v) the Post-Glacial movements.

I am reluctantly compelled, however, on this occasion to confine myself to the earlier episodes concluding with the Miocene movements. To embark upon the more recent history of the region would entail prolonging this Address to an extent that would be unwarrantable.

I regret this the more as it was due to evidence which has come into my possession within the last few years regarding one of the most recent phases of the history of the region that I was led to choose the story of the Bristol Channel as the subject of my Address, though it is not inappropriate that I should deal with this subject at a meeting of the British Association in the City of Bristol, since the position and prosperity of the City is so intimately dependent upon this great waterway.

## The Triassic Planation.

Probably no single episode has contributed so materially to the shaping of the surface in this region as the intense erosion which succeeded the Post-Carboniferous or Armorican earth movements. Prior to these movements there had been a long period of sedimentation, which resulted in the accumulation of thousands of feet of Carboniferous and Devonian or Old Red Sandstone strata over the southern part of the British Isles. Mountain-building movements beginning in late-Carboniferous times folded these strata into great anticlines and synclines, the axes of which trend nearly east and west from South Wales and the west of England to the south-west of Ireland. Intense erosion of the uplifted areas followed in the New Red Sandstone period, and it is the generally accepted opinion of geologists that this erosion which furnished the materials and determined the characters of the Permian and Trias (or New Red Sandstone) occurred under arid continental conditions. The relation of the New Red Sandstone to the Palæozoic rocks proves that along the axes of greatest upheaval the Carboniferous and Old Red Sandstone formations had been completely removed before the close of that era. Indeed, in the neighbourhood of Cardiff the underlying Ludlow rocks had been eroded, and New Red Sandstone laid down in contact with the Wenlock formation.

The Palæozoic rocks are exposed in the core of an anticline which ranges nearly east and west from Penylan north of Cardiff through Llandaff towards the neighbourhood of Cowbridge. The thickness of the Old Red Sandstone in the north flank of this anticline has been estimated at 3,500 feet. The succeeding Carboniferous Limestone which occurs in both flanks of the fold varies in thickness from 880 feet to over 2,000 feet on the north side, and reaches on the south a thickness of 2,750 feet. It is, therefore, unlikely that less than 2,000 feet of limestones occurred along the axis of the anticline. With regard to the Upper Carboniferous, there is some uncertainty. Farther west, in the South Wales Basin, the combined thickness of the Millstone Grit and Coal Measures is between 7,000 and 8,000 feet, but towards the east, and especially at the south-east margin of the coalfield, these formations are greatly attenuated. We can reasonably assume, however, that about 2,500 feet of Upper Carboniferous strata overlay the Limestone. These figures show that at least 8,000 feet of Upper Palæozoic rocks had been removed from the region before the deposition of the Keuper division of the New Red Sandstone which rests upon the eroded edges of the older rocks. Strahan's estimate[1] that a thickness not far short of 7,000 feet had been removed from the crest of the anticline before the Keuper was laid down is, in my opinion, unduly conservative, and it may well have been exceeded by 2,000–3,000 feet. Even the lower figure is sufficient, however, to impress upon the mind the effectiveness of erosion under the arid climatic conditions that prevailed during the New Red Sandstone period.

[1] The country around Cardiff, p. 39, *Mem. Geol. Survey.*

In the immediate neighbourhood of the Bristol Channel the Keuper division only is represented; the Bunter and Permian rocks which occur farther south in Devonshire are wanting, and it is not improbable that much of the lower part of the Keuper is also missing. The erosion of the uplifted Palæozoic rocks must have commenced immediately after the Armorican folding, and continued until their worn-down remnants were covered by the Keuper sediments. It is interesting to speculate what became of the material which was removed from the area during the earlier stages of the New Red Sandstone. Although it is probable that Triassic sediments extend under later Mesozoic strata for about 50 miles eastward from the Channel, it is unlikely that either Permian or Bunter is present. Farther north the latter formation occurs, however, in great force under the Cheshire plain, but is overlapped southward under the Midland Trias plain. In Devonshire also, the Bunter is believed to be represented as well as certain older red rocks which are attributed to the Permian.

We must suppose that during these earlier periods the products of erosion were either swept northward towards the Cheshire plain, and there deposited, or eastward towards East Anglia and the south-east of England, whence they were removed by erosion during the later Mesozoic period and transported to areas outside the British Isles. On the whole, it appears probable that much of it found its way into the great New Red Sandstone basin which lay to the north of the east-west ranges of the Armorican mountains. The rocks of the Bristol Channel area may then have contributed in an important degree to the deposits of the Cheshire Plain, the Midlands and the north-east of England. The study of the mineral constituents of the New Red Sandstone of those areas may throw some light upon this question. It is on the whole improbable that there was a drift of materials southward from the Channel region. Those who have investigated the New Red deposits of Devon have found evidence that the prevailing direction of transport was northward. This is also borne out by the finding of pebbles containing marine Upper Devonian fossils in the Trias of the Midlands.

The Triassic rocks of the Vale of Glamorgan consist mainly of a Dolomitic conglomerate containing for the most part pebbles of Carboniferous Limestone and occasional pebbles of other rocks. It rests upon the denuded edges of strata ranging from Old Red Sandstone to Pennant Sandstone. The underlying surface is somewhat uneven, and in places the Trias is banked against scarps and hills of limestone, some of which were not submerged until the Lias period. Strahan remarks that 'then, as now, the Carboniferous Limestone formed scarps and the Old Red Sandstone stood up as rounded hills.'[2] At Llanharan, on the north side of the Vale, the base of the Keuper at an altitude of about 350 feet is in contact with the Pennant Sandstone. Here the Keuper, which consists of breccias, 'ends off against a steep Pennant scarp and can scarcely have extended many yards beyond.'[3] At this point the escarpment rises to nearly 900 feet on Mynydd y Garth, and there is little doubt that the angle at its base in which the Keuper rests is a feature due to Triassic

[2] Strahan. 'The Country around Bridgend,' p. 22. *Mem. Geol. Survey.*
[3] Strahan, *op. cit.*, p. 23.

erosion, but how far up the slope the Trias may have extended it is impossible to say. The thickness of that formation is variable, but even after making allowance for a slight rise of the base of the Keuper northward, it seems hardly likely that the thickness of the formation at that point would be sufficient to carry even the youngest beds of the formation to the summit of the Pennant scarp. The Triassic feature nearly coincides with the existing escarpment along the south side of the coalfield, but eastward of Llanharan the latter crosses the Palæozoic strata from the Pennant Sandstone until ultimately it coincides with the base of a group of sandstones and conglomerates forming the upper part of the Old Red Sandstone. From the foot of this feature the summit levels of the hills slope gently down towards the base of the Trias north of Cardiff, or towards the Alluvial flats between Cardiff and Newport, which are believed to be underlain by that formation. North-east of Cardiff the main escarpment swings, however, in a northerly direction towards Pontypool, and its course shows no relation to the trend of the base of the Trias. Between Newport and Chepstow there is, however, a sharp rise of ground which pursues a slightly undulating course across the Old Red Sandstone and Carboniferous Limestone, and thence across another Old Red Sandstone area between Chepstow and the southern end of the Forest of Dean. This feature stands in sharp contrast to the gently sloping plain to the south, and appears to be independent of the nature of the rocks. To the south of it the Triassic rocks are never far away, and it is not unlikely that here again, as at Llanharan, there is preserved a line of cliffs on the Trias plain of denudation, though in all probability somewhat modified by subsequent erosion. It may be that only a part of the feature is of Triassic origin, the remainder having been developed subsequently by the differential erosion of strata of varying resistances.

Strahan appears to have held the opinion that the main escarpment south and east of the coalfield was in existence in Triassic times, and that the Trias deposits had covered at least that part of it which is occupied by the Carboniferous Limestone. 'The main limestone throughout the greater part of its outcrop west of Pontypool shows the red staining characteristic of rocks which have been overspread by the Trias. In other districts also both in South Wales and elsewhere this alteration of limestone into iron-ore shows an obvious connection with the present or past distribution of the Trias. We know that the Trias steadily overlaps the Old Red and Lower Carboniferous rocks between Newport and Llantrisant, until it comes to rest on Coal Measures, and we may reasonably suppose that at no distant date (geologically speaking) it extended over all those parts of the limestone where the ore and the red staining occur.'[4]

The view implied in the above quotation is that the iron which is responsible for the reddening of the limestone and for the development of iron-ore bodies by the replacement of that rock was obtained from superincumbent iron-charged Triassic sediments.

A similar view is expressed by Dr. T. F. Sibly in explanation of the iron-ore deposits in the Forest of Dean and in South Wales, as the following quotation shows[5] :—

[4] The Geology of the Country around Newport. 2nd ed., p. 23.
Iron Ores of the Forest of Dean and S. Wales. 2nd ed., 1927, p. 88.

'The immediate source of iron appears to have been a mantle of Triassic (Keuper) deposits, highly charged with Ferric Oxide, which overlay the denuded edges of the Carboniferous strata. In South Wales the Conglomerates and red marls of the Keuper still cover the ore-field in the Carboniferous Limestone at Llanharry, and carry iron-ore in replacement patches in their basal beds. It cannot be doubted that these red rocks originally overspread the whole of the ore-bearing outcrops of the Carboniferous Limestone. In the Forest of Dean Triassic deposits do not touch the Carboniferous basin, but they occur to within a short distance of the southern end at Aylburton, and again in large thickness at no great distance to the east at Newnham on Severn. The former extension of the Trias over the Carboniferous basin may reasonably be invoked to account for the iron-ores by analogy with the condition in South Wales.'

If this is indeed the explanation of the hæmatitic iron-ores of the region, and of the reddening of the limestone, it involves a former extension of the Trias far beyond its present outcrop, and further it implies that the thickness of the deposit was sufficient to allow the formation to climb several hundred feet up the face of the escarpment. There are certain obvious difficulties in the way of accepting this explanation, and it seems to me that it requires further examination.

The iron-ore deposited around the rock particles of the Trias to which that formation owes its colour is in the dehydrated form of Ferric Oxide, which is extremely insoluble even in acidic waters. In most cases the ore which replaces the limestone is a hydrated Ferric Oxide approximating in composition to Göthite. There is no reason to suppose that the coating of the particles of the Triassic rocks has been dehydrated since it was deposited, so that if the iron-ore has been derived from the Trias this almost insoluble substance must have passed into solution and been reprecipitated.

Also there are extensive tracts of Carboniferous Limestone, particularly around Chepstow and in the Mendips, which must have been overspread by Trias, and in which no ore is developed, though it is true the rocks may be red-stained; near Chepstow especially the Trias still rests on the limestone.

Further, near Llanharry, in the Vale of Glamorgan, the basal beds of the Keuper Conglomerate have locally been converted into an iron-ore similar to that which occurs in the limestone on which the conglomerate rests. It is difficult to suppose that the enrichment was due to iron leached out of the upper part of the conglomerate. From the distribution of the ore bodies it would appear that they occur mainly near the boundary between the limestone and overlying rocks.

For these reasons it appears to me probable that the ore in the Trias and the ore in the limestone were both derived from the same primary source, that being probably the abundant pyrite which occurs in the Coal Measure rocks of the region in a finely divided form. The destruction of the Coal Measure shales, which in all likelihood stood at that time at a higher elevation than the surrounding limestone, led to the release of the pyrite, which on oxidation gave rise to acidic iron-bearing waters. It was probably due to the activity of such waters in passing over the limestone and accompanying or subsequent oxidation, that the ore in the limestone was formed. Similar waters entering the area in which the Trias was deposited would furnish the necessary iron content to those sediments.

If the only interest of the relation of the escarpment bordering the South Wales Coalfield to the former distribution of the Trias, and to Triassic erosion, was its bearing upon the mode of origin of the iron-ores of the region, I would not have referred to it on this occasion. But a much wider question is involved, affecting possibly the origin of the physical features of a large part of Wales and the south-west of England. If the escarpments bordering the south side of the South Wales Coalfield and the Forest of Dean are to be regarded as in great part of Triassic age, may not other comparable features in Wales be attributed to the same age and conditions of erosion?

The former extension of the Trias to the west of the Vale of Glamorgan is proved by a small area which occurs near Port Eynon on the south coast of Gower. This peninsula is in the main a plateau at an elevation of about 400 feet, and it is clear that in places at any rate the pre-existing rocks had been eroded almost to their present level before the close of the Trias. The escarpment which is so prominent on the borders of the Vale of Glamorgan is, however, not readily identifiable in Gower, though it may be represented there. Still more remarkable evidence comes from South Pembrokeshire, where Mr. E. E. L. Dixon has described deposits having the aspect of typical Keuper marls which occur in great masses of breccia in the Carboniferous Limestone cliffs.[6] These Gash breccias, as they have been called, appear to be masses of limestone which collapsed into subterranean caves during the Trias (probably Keuper) period. The region in which they occur forms at the present time part of an extensive plateau, but it is clear that Triassic deposits formerly occurred at some unknown distance above the present surface. From the fact that the Gash breccias are truncated at the top of the cliff in such a way as to indicate that they formerly continued to a higher level, Mr. Dixon is of the opinion that the old land surface during the formation of the breccia gashes lay at a considerable height above the present, and he attributes the deposition of the red marls to a slightly later period, possibly following some depression of the area. It is clear, however, that the greater part of the denudation which removed an enormous thickness of Carboniferous and other rocks from that area subsequently to the Armorican folding had already been accomplished before the close of the Triassic period. Mr. Dixon remarks that although 'the Triassic floor has undergone some later planation this has merely touched up the work of the earlier erosion' (*op. cit.*, p. 162).

In South Pembrokeshire the plateau rises on the whole northward; in that direction various formations ranging from the Old Red Sandstone to pre-Cambrian gneisses and volcanic rocks occupy its surface. The whole plateau appears to form one continuous feature, and it is not improbable that, as in the southern part of the county, most of the degradation which the greatly folded rocks have suffered occurred as the result of erosion during the New Red Sandstone period.

In North Pembrokeshire certain hill masses, such as the Prescelly Range, stand conspicuously above the general level of the plateau, and

[6] The Geology of the Country around Pembroke and Tenby. *Mem. Geol. Survey,* 1921, p. 158.

farther west, near St. Davids, isolated rocky hills such as Pen Bery, Carn Llidi and St. David's Head rise above the surface like islands out of the sea. The general correspondence of the present surface with the Triassic plain of erosion in various parts of South Wales suggests the possibility that the well-defined boundaries of the isolated hills that rise above the plateau level may, like the scarps overlooking the Vale of Glamorgan farther east, be also of Triassic origin.

The plateau of South Pembrokeshire is continued northward without a break into the remarkable even surface which truncates the Palæozoic rocks of Central and North Wales. This surface is relieved by occasional hill masses which stand conspicuously above its level. These include the summits of the mountains composed of Ordovician volcanic rocks which range from Cader Idris northward through the Arans and Arenig. The Plynlimon mass and a smaller mass near Drygarn belong to the same category. Several writers have called attention to the way in which this plateau abuts against the mountain masses which rise sharply to a height of several hundred feet above its surface. Except for their greater altitude above sea level, the relation of these hill masses to the surrounding plateau is closely similar in North Wales and in South Wales, so similar, in fact, as to invite the idea that they are of similar origin. W. M. Davis[7] has pointed out that, 'while the theory of marine planation was in vogue, it was customary to interpret all evenly truncated uplands—that is, uplands whose surface truncates their rock structure—as uplifted plains of marine abrasion, more or less dissected since they were uplifted. When the efficacy of sub-aerial erosion was recognised it became equally customary to interpret truncated uplands as once base-levelled and afterward uplifted peneplains. If Passarge's views be now accepted, it follows that no truncated uplands should, without further inquiry, be treated as having been eroded when their region had a lower stand with respect to base-level; the possibility of their having been formed during an earlier arid climate as desert plains, without regard to the general base-level of the ocean, must be considered and excluded before base-levelling and uplift can be taken as proved.'

Passarge, to whose views Davis refers, has given a description of great plains which have been eroded under desert conditions. He states[8] 'that these desert plains are not undulating with low hills, but are true plains of great extent, from which the isolated residual mountains rise like islands from the sea. The residuals may be low mounds only a few meters high, or lofty mountain masses rising several thousand meters above the plains. The plain surrounds the steep slope of the mountains with a table-like evenness; there is no transitional belt of piedmont hills, and no intermediate slope. . . . The bedding of the rocks is not flat, but disturbed; the plain therefore truncates the rock structures. . . . The products of weathering are usually spread as a thin veneer on the plain; the waste does not lie in place on the rocks from which it was weathered, but has been drifted about by wind and flood and has gathered in slight depressions. The waste veneer increases the smoothness of the plain, but the rock surface is also a plain. . . . Neighbouring areas contain

[7] 'The Geographical Cycle in an Arid Climate.' *Geographical Essays*, p. 310.
[8] Quoted from Davis, *op. cit. sup.*

extensive deposits of irregular strata whose composition and want of fossils indicate their desert origin. . . . Various additional details are given, with the conclusion as above quoted; these rock-floored plains are not uplifted peneplains, but are the product of desert erosion unrelated to normal base-level in which occasional water action has co-operated with more persistent wind action.'

The great plateau of Wales is not, however, horizontal at the present time, nor is the base of the masses, which might be regarded as residual, everywhere at the same level. The possible reasons for these differences will be dealt with later. In other respects, however, the features of the region conform to a remarkable degree with Passarge's description.

When we turn to the Mendip region we find features similar to those of the Vale of Glamorgan—cliffs eroded in Carboniferous Limestone against which the Trias is banked up; the cliffs were subsequently overwhelmed by the sea and covered by Liassic deposits. South of the Channel also many of the abrupt slopes around the Quantock Hills and south of Porlock and Watchet are in most cases fringed, at no great distance, by Triassic deposits, and it is not improbable that some of these steep slopes mark the edge of a former desert plain out of which residual hills rose abruptly. As in Wales, so in Devon an enormous amount of erosion of the Palæozoic rocks had occurred before the New Red Sandstone was deposited.

Whether or not it be the case that the great plateau of Wales is in the main the product of erosion under the arid conditions of the New Red Sandstone period, it is at least certain that on both sides of the Bristol Channel the pre-existing surface was eroded into a broad depression bounded by scarps trending generally in the direction of the present channel. Against these scarps some depth of Triassic deposits was banked up, but how far up the slopes the highest beds of the Trias may have reached is at present uncertain. From the fact, however, that the products of erosion during the earlier stages of the New Red Sandstone were removed from the area to regions more favourable for the accumulation of deposits, it may be inferred that the level of the surface was then relatively higher than the basins of deposition. There is, therefore, no evidence of a tectonic basin in the channel region at that period, but only of a depression caused by erosion between the uplands which bounded it on the north and on the south.

## The Formation of the Mesozoic Cover.

At the close of the Triassic period there was a general subsidence of the British region, and the sea invaded the area of Triassic sedimentation. The enormous extent of the Rhaetic formation, considering its small thickness, proves that before the marine invasion that area was almost a dead level plain, and it is probably safe to assume that marine sediments were deposited where Triassic rocks are now found, as well as in those areas from which the Trias has been removed by subsequent erosion.

The problem of the former extent of the Mesozoic rocks has exercised the minds of many geologists in the past, notably Ramsay, Hull, Strahan and Lamplugh. Strahan and Ramsay in particular regarded the problem from the point of view of the origin of the drainage systems of England

and Wales. Some indirect evidence of the probable former extent of those formations can be gathered from a study of their lithological characters, and more particularly from the variation in their thickness which takes place from the outcrop eastward. When Ramsay and Hull investigated the problem, the only evidence available was that obtained from the study of the rocks at their outcrop, but within recent years it has been supplemented by the information obtained from various deep boreholes which have penetrated a part or all of the Mesozoic cover. Much yet remains to be done before the evidence afforded by the Mesozoic rocks in regard to their former extension can be fully utilised. The work of Buckman, Trueman, Richardson and others on the zonal development of the early Jurassic rocks points the way to further studies in the same direction.

In the Vale of Glamorgan the Lias in places follows conformably on the Rhaetic ; in other places it overlaps on to the Carboniferous Limestone or older rocks, and then assumes a littoral type consisting either of conglomerates with limestone pebbles or of oolites. It is remarkable, however, that no pebbles of Coal Measure rocks occur in the conglomerates, although during the Trias the escarpment composed of these strata overlooked the area of sedimentation at a distance of only a few miles to the north. No evidence of littoral conditions attributable to that escarpment has been obtained in the Lias outcrops nearest to it, and Strahan[9] remarks that 'the absence of marginal types of limestone (in the northern part of the vale) suggests that the shore lay a good deal farther north, though at the same time it is hardly probable that the Lias overlapped the Pennant scarp.' This conclusion depends, however, on the extent to which the upland rose at that time above the base of the escarpment, and upon the thickness of the Trias in that region. If the escarpment was no higher than the existing feature, then taking into account the rise of the base of the Lias along the north side of the Vale, and the probability of some considerable thickness of Keuper having been removed from that area, it is possible that at least some of the Lias deposits would overtop the escarpment and come to lie upon the rocks within the coalfield. On the other side of the Bristol Channel, Woodward observed that although Palæozoic rocks rise at present to a great height above the level of the Lias, the latter shows no signs of littoral conditions, and he argues that the shore line must have been some distance away. This could only have been possible if the escarpment against which the Trias terminated was wholly or in great part buried beneath that formation. A former extension of the Lias westward into the Gower peninsula is indicated by the finding of 'several Liassic oysters, allied to *Ostræa irregularis*, from stalactites in the Carboniferous Limestone at Mumbles.'[10]

In the underlying Rhaetic beds, however, definite changes of lithology occur in a westerly direction. The normal shales and limestones of the Cardiff district pass, near Bridgend and Pyle at the western end of the Vale of Glamorgan, into coarse, impure sandstones, very similar to Millstone Grit. In places intercalations of red and mottled marls, only distinguishable from Keuper marls in containing fossils, occur a few feet above the

[9] *Mem. Geol. Survey*, 'The Country around Bridgend,' p. 59.

[10] A. E. Trueman, The Liassic Rocks of Glamorgan. *Proc. Geol. Assoc.*, vol. xxxiii., p. 278 (1922).

base of the formation. These lithological changes suggest that during the deposition of the Rhaetic the surface was inclined from west to east.

In the Vale of Glamorgan, however, only the lower zones of the Lower Lias are now preserved. In the absence, in the ascending succession, of definite evidence of conditions indicating the proximity of a shore line or of shallowing of the area, it can hardly be supposed that no higher Liassic strata were deposited. Taking into account the variation in thickness of the Lias, as proved at the outcrop and in various borings, I believe that anything between 600 feet and 1,000 feet of that formation may have been deposited in the region of the Bristol Channel.

It is true that the thickness of the formation in Somerset and South Gloucestershire is usually less than the lower of these figures, but such evidence as exists goes to show that apart from local irregularities the thickness increases westward of these places as well as southward and northward, and in North Gloucestershire the formation attains a thickness of nearly 1,400 feet. The amount of deposit accumulated at any place was probably controlled in the main by the amount of subsidence, and not so much by interruptions of sedimentation due to upheaval. The unequal subsidence during the deposition of the formation has, however, to be borne in mind in estimating the deformation that these rocks have suffered in subsequent geological periods.

Although the thickness of the Lias west of the Cotswolds exceeds 1,000 feet, it appears to fall off rapidly eastwards, and borings in the London area, at Ware in Hertfordshire and Culford in Suffolk, all proved the absence of the formation on the Palæozoic floor. Whether the formation reached its maximum thickness in the neighbourhood of its present outcrop, or continued to increase farther to the west, it is impossible to say, but we find that at Prees Heath nearly 70 miles west of the base of the formation, the Lower Lias, which is there succeeded by Middle Lias, has been proved by boring to exceed 400 feet.

In view of this considerable development of the Lower Lias at Prees Heath, and the presumption of some additional thickness of Middle and Upper Lias having been deposited, and in view also of the great development in North Gloucestershire within a few miles of the Palæozoic rocks, and the occurrence of the formation in the north-east of Ireland, it would be rash to assert that the Lias did not extend over the Palæozoic area of Wales and the Welsh Borders ; the formation may, indeed, have attained a thickness of several hundred feet over that area. If one could be sure that the Palæozoic region remained relatively stable during the deposition of the Lias, one would feel little doubt of the extension of the formation over it in force, but if Lamplugh was correct in assuming a zone of maximum thickness at or near the present outcrop the subsidence of the crust in that area might have been complementary to an upward warping to the west. In that event the Lias may not only have thinned away very rapidly towards the Palæozoic upland, but even deposits formed at one stage may have been removed from the area during a later stage of the Liassic period.

Of the Jurassic formations that intervene between the Lias and the Corallian, the predominant member is the Oxford Clay, which was probably laid down under much the same physical conditions as the Lias.

From the Dorset coast to the borders of Gloucestershire and Oxfordshire these formations attain a thickness of nearly 1,000 feet; they fall away to about half this thickness in Buckinghamshire and Rutlandshire, while they are absent in Suffolk. From Lincolnshire to North Yorkshire, however, they increase markedly in thickness. Their behaviour is, therefore, similar in general to that of the Lias, except that there does not appear to be any obvious reduction in these rocks on the line of prolongation of the Mendip region.

Both at the northern and the southern extremities of the Jurassic outcrop all the formations hitherto considered probably conform to the principle enunciated by Lamplugh, that the present outcrop coincides approximately with the areas where these formations attained their maximum thickness. It is doubtful, however, whether this principle holds good for the intervening region.

The curvature in the strike of the Jurassic outcrop between Gloucestershire and Yorkshire is repeated in the base of the Cretaceous, but its amount in that formation is even greater. There is no doubt that the form of the outcrop is due to post-Cretaceous folding in consequence of which the Mesozoic rocks have been arched up along a broad area, extending from the Midlands to East Anglia. The less pronounced effect of this folding on the Jurassic than on the Cretaceous outcrop is probably to be explained by the Jurassic rocks having already acquired a slight easterly dip before the deposition of the Cretaceous.

If we draw a line from the Dorset coast by way of the Cleveland Hills to the Yorkshire coast, that line will pass along the outcrop of the Jurassic rocks from the south coast to North Gloucestershire, and again near the north-east coast. Between these points, however, the base of the Jurassic swings away in a great arc to the eastward of the line, and in Lincolnshire is more than 40 miles distant. *Pari passu* with the divergence of the outcrop from the line drawn through its extremities, the combined thickness of the formations from the top of the Lias to the Oxford Clay diminishes progressively towards East Anglia, and so far as I can determine it appears that the lines joining points on the northern and on the southern part of this arc, where the formations are of equal thickness, are roughly parallel to the above-mentioned line. These facts render it probable that the line drawn between the Dorset Coast and the Yorkshire Coast is an isopachyte, or line of equal thickness in these formations, and that other isopachytes corresponding to diminishing thicknesses follow eastward until the line of zero thickness is reached under East Anglia. Further, it is likely that the trend of the isopachytes gives a clue to the general direction of the coastline. The subsequent arching of the Jurassic rocks has, however, brought to the surface different isopachytes of these formations, and along the crest of the arch the thicker zones have been eroded away. If the Jurassic rocks had been upheaved along a N.N.E.-S.S.W. line, it is probable that Lamplugh's generalization would have held true for the whole extent of their outcrop. Owing, however, to the later folding having occurred transversely to the isopachytes, the generalization breaks down. It probably holds only when, as frequently happens, lines of elevation tend to be parallel to pre-existing shore lines. The effects of persistent axes of elevation or non-subsidence, as for instance the Market-Weighton axis, must not of course be ignored; but these appear to have been responsible for only minor irregularities in the deposition of the strata.

Most of these formations, with the exception of the Oxford Clay, are variable in lithology and are probably of shallow-water origin. The Oxford Clay, however, is a formation so uniform over its whole outcrop that one would expect it to have occupied a wide area. As with the Lias, it seems difficult to believe that it died away before reaching the Palæozoic rocks in the west. Slow elevation of the western area may, however, have been in progress *pari passu* with the subsidence of the floor of the Jurassic sea, so that deposits laid down at one period may have been removed at a later period. It is not impossible, in fact, that the clays of one period may have furnished in part the materials for those of a later period.

Since there are so many doubtful elements in the problem, it would be unprofitable to discuss the possibility whether and to what extent the higher Jurassic formations encroached on the Palæozoic region to the west. Hull, Ramsay, Lamplugh and others have directed attention to the marked easterly attenuation of these formations, and particularly of the Kimmeridge Clay, all of which behave in a manner similar to the earlier Jurassic strata.

Briefly, although it is probable that at least the great clay formations of the Jurassic once extended well over the Palæozoic areas, it does not follow that they maintained their hold over that area until the close of the Jurassic era. It is a fact that the Jurassic rocks, particularly in the south-west of England and probably elsewhere, had acquired an easterly dip before the deposition of the Cretaceous, but whether this happened in one episode of movement between the Jurassic and the Cretaceous, or whether it is the result of a progressive increase in dip of each Jurassic formation due to a succession of uplifts in the west, cannot be determined from existing data. A closer study of each of the Jurassic formations on the lines of the investigation by Kitchin and Lamplugh on the Mesozoic rocks of the Weald, might throw light on this problem and resolve many of the existing uncertainties.

It appears to be the general opinion of geologists who have considered this matter that the Cretaceous sea spread widely over the western areas of Britain. According to Jukes-Browne's palæogeographic reconstructions, none of the Jurassic formations invaded more than the margin of the Palæozoic region, whereas the chalk sea is represented as having covered all but the higher summits; and if the chalk sea invaded the region it is not unlikely that a considerable thickness of deposits would be formed in that sea. Jukes-Browne was inclined to regard the residual elevations that rise above the Welsh plateau as defining the margin of the Chalk sea (Building of the British Isles, p. 335). Although he recognised (p. 334) that 'the pre-Cretaceous contours of the country were very different from those which it now exhibits, all heights have been greatly reduced by the work of sub-aerial agencies during Tertiary times,' he appears to have had no suspicion that the relative elevations above sea level of various parts of that tract may have been radically different then from what obtains at present.

Strahan was led to postulate the former existence of a blanket of sediments (probably Upper Cretaceous) over the Palæozoic rocks in order to account for the origin of the South Wales drainage system. His argument rests on the complete disregard of the structures of the Palæozoic rocks shown by the streams which traverse them, and he therefore assumed that the drainage must have originated on a cover of newer rocks and have been superimposed upon the older strata as the cover was stripped. Since these conditions apply more particularly to the elevated region of the coalfield which stands in general considerably higher than the surrounding plateau of Central Wales, he implied the extension of the sedimentary cover over areas which, according to Jukes-Browne, remained above the level of the Cretaceous sea. If, in fact, Strahan's explanation is correct, then either the plateau tract of Central Wales must have been covered by such a depth of Mesozoic sediments as would allow them to extend also over the coalfield region and the Brecon Beacons, or the whole plateau of Central Wales and some of its present residual elevations, particularly the escarpment of the Brecon Beacons, must have been developed by erosion subsequent to the Cretaceous period. There are, however, many difficulties in accepting the latter view.

Once it is granted that the sea may have spread over the Palæozoic area not once but possibly several times during the Mesozoic period, we have to recognise the possibility that the great plateau of Wales may have been eroded, as Ramsay believed, by marine erosion, and that the margins of the elevations which rise above its surface, including among these the escarpment of the Brecon Beacons, may be the sites of ancient cliff lines.

We have thus an alternative explanation to that previously suggested for this feature, viz., by marine planation as opposed to desert planation. Whichever view is adopted, however, it seems to be clear that these features do not now stand at the same relative levels as they once did. The evidence for this conclusion is dealt with in the next portion of my address.

### The Post-Cretaceous Movements.

Various authors have called attention to the important part played by the Miocene earth-movements in determining the physical features of the south-east of England. In that region these movements have affected not only the Mesozoic and early Cainozoic strata, but also the underlying Palæozoic rocks, which, having been folded in pre-Triassic times and eroded in subsequent periods, form a more or less even floor upon which rest the gently inclined Mesozoic strata. The Miocene folding was in the main responsible for the anticlines of the Weald and of the Isle of Wight, and the complementary synclines of the London and Hampshire Basins. Lamplugh has shown, however, that the anticlinal structure exhibited by the Cretaceous rocks of the Weald changes gradually in depth, so that the structure of the underlying Jurassic rocks is that of a syncline. This strange superposition of an anticline in the upper layers upon a syncline in the lower layers is explained by the warping of the area during the deposition of the Mesozoic sediments. Had it not been for deformation during sedimentation, the anticlinal arrangement evident in the surface rocks would have persisted downwards, and the Palæozoic floor would probably have been folded in much the same way as the superficial strata. I cannot conceive of the Mesozoic and Cainozoic cover being folded unless at the same time the surface upon which it reposes was affected, and there is little doubt that the relative movements of that floor during the Miocene folding were of the same type as in the superficial layers, but owing to warping during the Jurassic there is now no close correspondence between the form of the Palæozoic floor and the dominant surface features already enumerated.

Before proceeding to the inquiry which is the main object of this portion of my address, namely the extent to which these movements may have influenced the physical features of the Bristol Channel region, I wish to refer briefly to some of the characteristics of the Miocene folding as exhibited in the South of England and in Northern France.

Although we habitually speak of the anticline of the Weald and the syncline of the London Basin, each of these folds when considered in detail is not a simple structure.

The folding in the Weald was described in some detail by Topley,[11] who recognised that the so-called Wealden anticline is a compound structure which is made up of 'a number of minor folds traversing the district in lines more or less parallel to its general axis of elevation.' These axes are described in detail, and the more important ones are represented on the map accompanying the volume. No individual fold can be traced more than a few miles; it is well defined at some point on its axis, but both to the east and to the west the structure becomes less definite and ultimately imperceptible. Where a particular axis ceases to be traceable, its place is usually taken by another either to the north or to the south. Each anticlinal fold is thus an elongated pericline, the structure being most clearly defined in the central region of its traverse, near where the change of pitch from an easterly direction to a westerly direction takes place.

The axes of the numerous minor anticlines that go to make up the Wealden elevation are thus arranged *en échelon*. The place of any given anticline, say in the east, may be taken farther west by a pair of anticlines, one lying to the north and the other to the south, while the actual prolongation of the anticlinal axis may coincide with a syncline.

This is precisely the character of the folding worked out by Mr. W. B. R. King in the surface of the marls of the Middle Chalk in Northern France.[12] That author took advantage of the information afforded by a large number of bore holes that were put down in the war area in search of water, to construct contours representing the surface of the Middle Chalk marls, and thus obtained the form to which these originally horizontal strata were warped by Post-Cretaceous (probably Miocene) folding. The contoured map so obtained is most instructive. To the eastward of Amiens an anticlinal area near Rosières occupies the territory included between the Upper Somme and its tributary the River Avre. This pitches eastward on the east and westward on the west. If its axis is prolonged, however, for some miles to the west it almost coincides with a syncline, the axis of which runs parallel to the Lower Somme Valley. The anticline has therefore disappeared completely in a westerly direction.

[11] Geology of the Weald, pp. 216–42. *Mem. Geol. Survey.*
[12] *Quart. Journ. Geol. Soc.*, vol. lxxvii. (1921), p. 135, and Pl. III.

To the northward of it, the axis of Le Catelet with a westerly pitch becomes an insignificant structure farther west, and ultimately becomes a mere wrinkle on the south flank of another anticline, between Saulty and Bapaume, which in its turn disappears both eastward and westward. North of Amiens another anticline rises on the line Valheureux–Bernaville, which in its turn disappears in both directions ; its continuation to the east almost coincides with the syncline between the Le Catelet and Rosières anticlines.

Finally, in the northern part of the area considered by Mr. King, the main axis of elevation is that of the Vimy Ridge which is completely lost eastward in a region of indefinite structure between Douai and Cambrai.

I have no doubt that if any horizon in the Upper Cretaceous rocks of the Wealden area were similarly contoured, it would disclose a type of folding identical with that of the post-Cretaceous folding in Northern France. The study of the Weald and of Northern France reveals the major characteristics of the folding to be as follows :—(1) The anticlinal axes are impersistent and tend to be arranged *en échelon.* (2) The structures are in general most sharply defined near the points of maximum elevation ; in the lower-lying regions of the folded surface, particularly in those areas where the anticlines change their pitch, a simple fold is in general replaced by minor wrinkles.

Mr. King and others have called attention to the general correspondence between the folding and the drainage lines of the area, and in particular between the trend of the major streams and the main synclinal depressions, though Mr. King points out that the courses of the streams do not coincide with the synclinal axes but usually lie somewhat to the north of them.

Having studied the general characteristics of the movements which affected the later Mesozoic rocks of the south-east of England and Northern France, let us direct our attention to the folding that has affected the earlier Mesozoic rocks of the Bristol Channel region.

Unfortunately the only formation of that age which reaches the coast-line of the Bristol Channel is the Lias, and even of that formation only the lower portion is represented. In general the Lias succeeds conformably a thin development of Rhaetic, and this in turn overlies Keuper which rests with marked unconformity on the Palæozoic floor. Although there are indications here and there of minor interruptions at the boundary between the Keuper and the Rhaetic, the relation of these formations as a whole suggests conformity. Over large areas the Lias and Rhaetic have been removed by subsequent erosion, leaving the surface occupied by Keuper. In view of the relations between the formations it may be assumed, however, that wherever Keuper occurs at present it was formerly succeeded by Rhaetic and Lias. The converse, however, does not hold. There are many places where the Lias or Rhaetic rests directly upon the Palæozoic rocks without the intervention of the Keuper. These are areas where the uneven surface of the Palæozoic floor projected as islands out of the Rhaetic or Lias sea, and were only submerged during the deposition of the later Liassic strata. The regions which lend themselves most conveniently for study are the Vale of Glamorgan and the Mendip region.

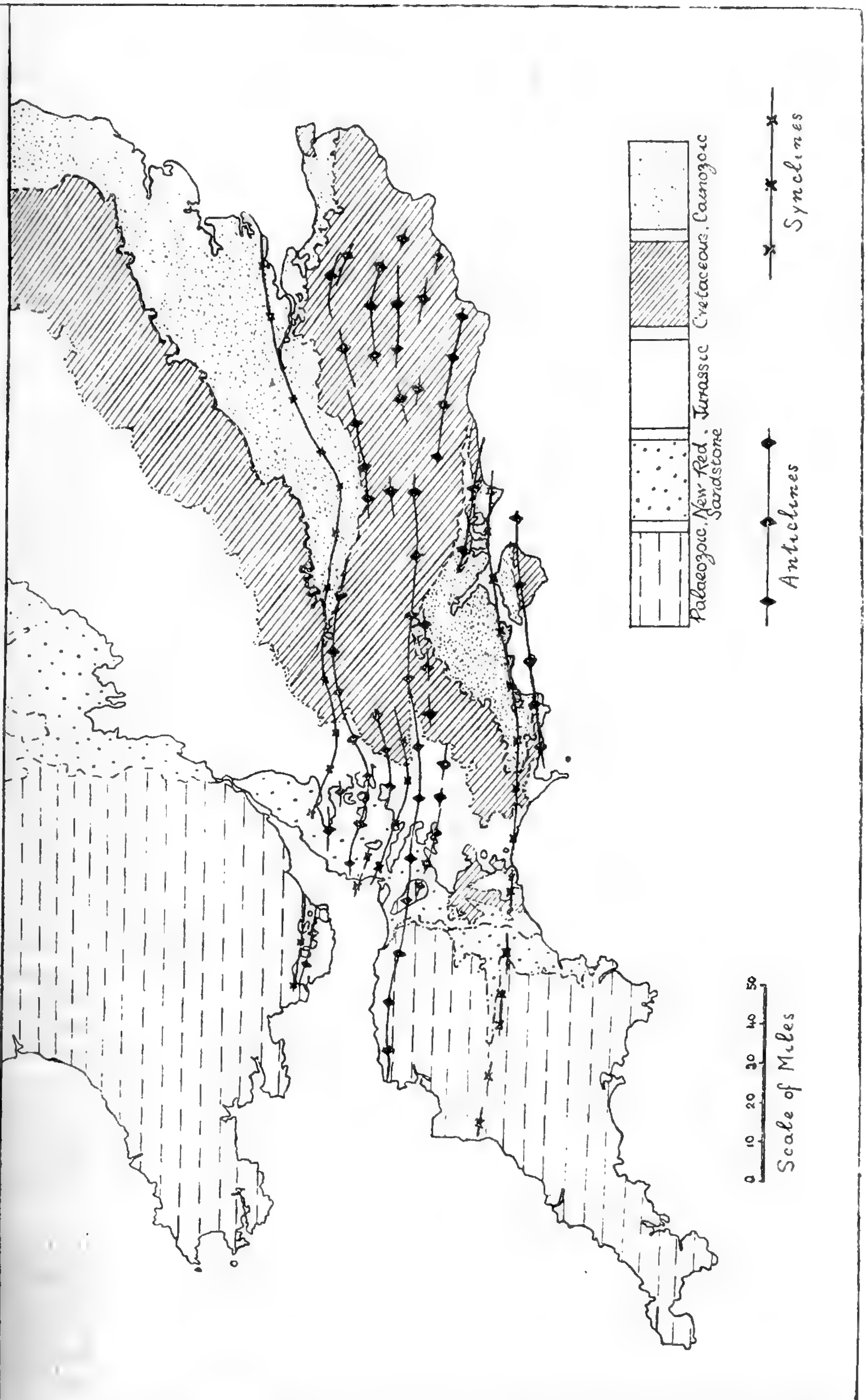
*Vale of Glamorgan.*—In the Vale of Glamorgan the normal succession of Lias-Rhaetic-Keuper prevails for some miles west and north of Cardiff.

Between Cowbridge and Bridgend, however, there are large tracts where Lias rests directly upon Old Red Sandstone or Carboniferous Limestone. I have attempted to estimate at numerous points the approximate present level of the Lias-Rhaetic junction, but where the Lias overlaps the Rhaetic and comes to rest upon the Palæozoic rocks it is no longer possible to do so, since in all probability there is overlap in the Lias itself, in which case the level corresponding to the base of the Lias must be lower than the altitude at which that formation rests upon the Palæozoic. Thus, supposing the Lias rests upon the Carboniferous Limestone at a height of 300 feet above O.D., and that there is an overlap of 100 feet in the Lias ; the level of the plane of the Lias-Rhaetic junction, if no overlap had occurred, would at that point be 100 feet lower.

On the other hand, it may, I think, be assumed that wherever Rhaetic beds were deposited they were followed by Lias. We can therefore make an estimate of the level of the base of the Lias in places where that formation has been removed, but the Rhaetic remains, and a less close approximation to the former level can be obtained in places where the Keuper alone survives. In the area west of Cowbridge there is too much uncertainty as to the amount of overlap in the Lias to make the attempt worth while. Around Cowbridge, the Lias-Rhaetic junction rises in places to over 400 feet, and then falls northward to about 100 feet, while southward this junction must be well below sea level. In a section drawn northward from the coast through the Cowbridge area, the base of the Lias thus rises to a crest line near the latter place, and then descends northward about 300 feet. Still farther north the Lias has been eroded away, but the Keuper surface rises in that direction, and it is certain that before the present northern edge of the Mesozoic is reached the base of the Lias must have attained an elevation of well over 400 feet. The anticlinal axis can be traced from Cowbridge eastward in the direction of the Leckwith Hills, north-west of Cardiff, where the maximum elevation is about 200 feet, but the anticlinal structure is not evident in those hills. Westward the continuation of the axis is indefinite, but it appears to range towards Bridgend, where the Rhaetic-Lias junction stands at over 300 feet. The anticline is probably not a simple structure but consists of two, if not of three, short axes lying *en échelon*, each axis going west being situated a little to the north of the one to the east. The depressed area to the north runs somewhat indefinitely from Llandaff to north of Bridgend, *i.e.* roughly parallel to the anticline and from two to four miles to the northward of it (see Plate I).

If, by applying the accurate zonal work of Trueman on the Liassic rocks of the region more precise determinations were made of the horizon of the Lias in contact with the Palæozoic rocks, it would be possible to trace the structures westward in greater detail than can be done with the available data.

*The Mendip Region.*—In the Mendips the Lias-Rhaetic junction reaches a height of at least 850 feet above O.D. south of East Hampshire, whence it descends both to the north and to the south. It is true that the local base of the Lias attains even greater elevations, but at such points the Rhaetic has been overstepped, and it is probable that owing to overlap the beds resting on the Palæozoic rocks are not those of the basal zone. A careful survey of the Lias should afford evidence of the extent




MIOCENE FOLDS IN THE SOUTH OF ENGLAND

NOTE.—Small areas of Palæozoic rocks appearing in the New Red Sandstone and Jurassic Outcrop are omitted.

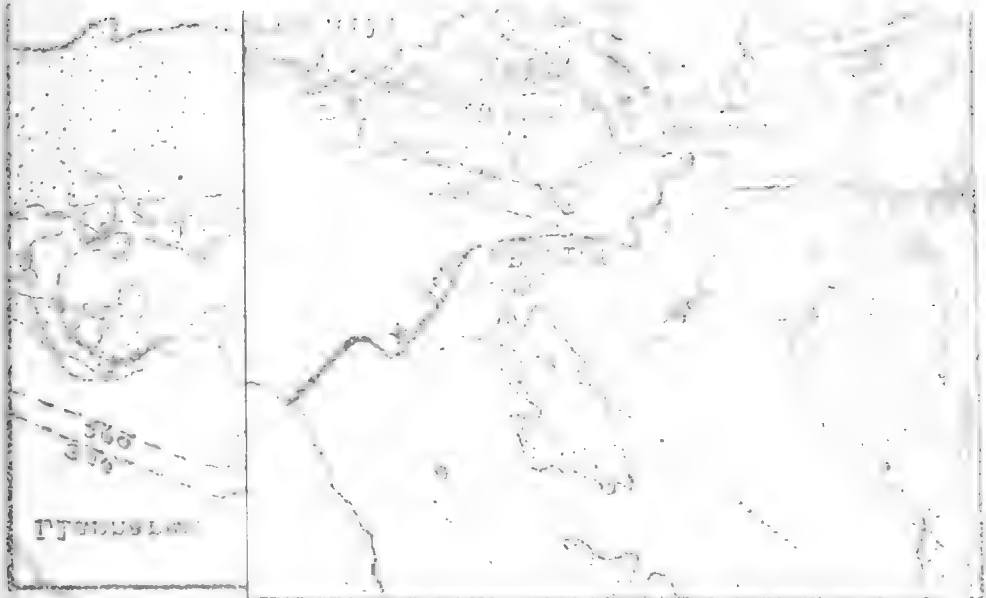
of the overlap, and thus lead to a closer estimate of the probable maximum height to which the base of the formation ascends. From the central Mendips the Lias, resting in general upon Rhaetic, declines somewhat rapidly southward to a level of about 500 feet around Wells and Shepton Mallet, and must reach the level of the sea three to four miles south of Wells.

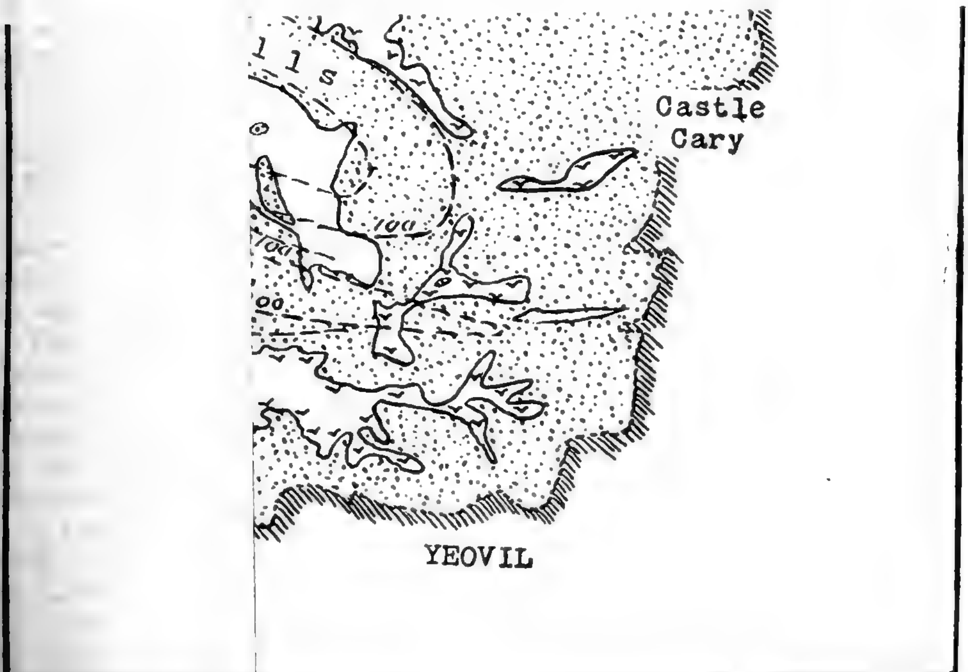
At Glastonbury Tor both Middle and Upper Lias overlie the Lower Lias, and the same succession is found to the east in the Pennard ridge. According to the section given by Woodward the base of the Lias at Glastonbury must be about 200 feet below Ordnance Datum, but southwards in the Polden Hills it rises again to between 200 and 300 feet above O.D.

Immediately north of the Mendips the basal plane also descends to considerably lower levels, and in the outliers of Lias and Rhaetic west of Chew Stoke it stands at approximately 400 feet above O.D. Still farther north, however, around the North Hill Carboniferous inlier, the base of the Lias rises again to about 600 feet above O.D., thence declining northward to less than 100 feet along the valley of the River Avon between Bristol and Bath. North of that valley there is a gradual rise of the base to 200 feet O.D. and in places to 350 feet. On a line of section drawn, therefore, from the northern suburbs of Bristol through North Hill, to Glastonbury and beyond, we have two well-marked regions of elevation, viz. one in the North Hill region and the other in the Central Mendips; and three synclinal depressions, viz. at Glastonbury, between North Hill and the Mendips, and in the Avon valley. The difference of elevation between the highest and the lowest points amounts to over 1,000 feet, and as the junction of the Rhaetic and Lias must have been as nearly as possible level at the time of the deposition of these rocks, the difference is a measure of the movements which the strata have suffered since they were deposited.

By using all the data available on the geological maps of the Bristol-Mendips region, I have drawn strike lines showing the approximate level of the basal plane of the Lias (see Plate II). On this map certain structures are immediately apparent; a well-defined anticlinal fold coincides approximately with the crest line of the Mendip range; north of it another anticline in North Hill which apparently continues eastward under Dundry Hill. Between these two areas of elevation there is a depressed region in which the structure is less clearly defined, though at all points the base of the Lias is at a lower level than to the north or south. Within it there appear to be two shallow synclines between which there is a subsidiary anticline near Farmborough. The small synclinal outcrop of Lias near Banwell at the western end of the Mendips apparently lies in the westward continuation of this depressed area. South of the Mendips the deep Glastonbury syncline with its east-west trend is readily detected.

So far as the evidence goes, the anticlinal areas appear to pitch east and west so that they are elongated domes. Unfortunately, the absence of the Lias along the west end of the Mendip range renders the course of the strike-lines uncertain, but the convergence westward of the Trias outcrop and the approach in that direction of the 100 foot strike lines, one on the south and the other on the north side of the range, makes it






es

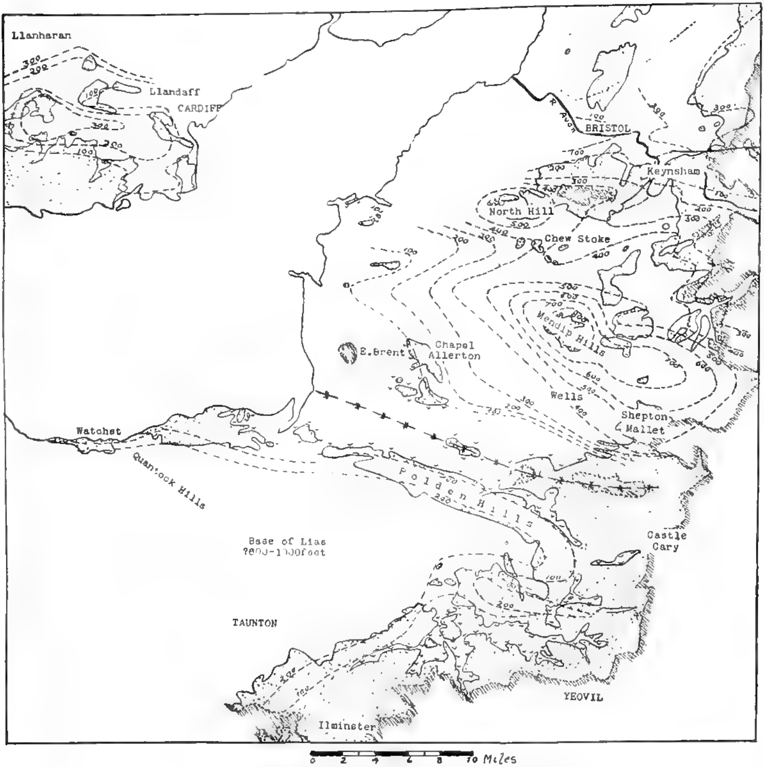
ng on Lower Lias.

tonbury syncline.

reen 150 feet and 250 feet above O.D.

*al History of the Bristol Channel Region, Section C.*

MAP SHOWING CONTOUR LINES IN THE BASE OF THE LOWER LIAS




Lower Lias outcrop stippled; inclined shading indicates later Mesozoic soils resting on Lower Lias.

* indicates Alluvium resting on Lower Lias. —*—*— indicates the Glastonbury syncline.

NOTE: Within the area enclosed by a dotted line north of Bristol the base of the Lias stands at between 150 feet and 250 feet above O.D.

*[Illustrating Address on Some Episodes in the Geological History of the Bristol Channel Region, Section C.*

[To face page 74.

probable that the crest of the anticline was lower in the west than near the centre.

Tracing these various folds eastward it is interesting to find that they do not continue far in that direction. The North Hill axis cannot be traced beyond the latitude of Keynsham, while the Farmborough dome appears to break up eastward into two shallow anticlines with an intervening syncline. The depression between the Farmborough anticline and the Mendips is replaced by shallow undulations.

The Mendip anticline itself is probably a compound fold consisting of two domes with their axes not quite in alignment, while there is some evidence of a shallow syncline on its northern flank.

The deep Glastonbury syncline appears at that place to be devoid of pitch, judging by the parallelism of the strike lines in its flanks ; eastward it is difficult to trace and probably dies away in minor undulations. The swing in the strike on its northern flank, near Blackford and Chapel Allerton, probably indicates the beginning of a subsidiary anticline and of another syncline, with its axis north of the Glastonbury syncline. It is probably in this subsidiary basin that the remarkable outlier of Brent Knoll occurs, which rises abruptly from the alluvial flats to a height of over 400 feet. The base of the Lias in this outlier is probably far below sea level.

The continuation of the Glastonbury syncline westward is indicated by a boring at Bason Bridge four miles south of Brent Knoll, where the base of the Lias lies at a depth exceeding 487 feet below Ordnance Datum.[13] Between Glastonbury and Bason Bridge there is, therefore, a decided pitch along the axis of the syncline. In the Shapwick boring situated between these places the base of the Lias was proved to be more than 235 feet below O.D.[14]

Regarding this map as a whole, it is obvious that there has been considerable post-Liassic folding. More interesting, however, is the type of folding which it reveals—that of elongated anticlines and synclines the axes of which are impersistent, and tend to replace one another *en échelon*. The character of the folding suggests that it is to be referred mainly to one episode of movement ; on a small scale it compares with the structures of the Jura Mountains as described by Heim.

There is a remarkable similarity both in type and in scale between the folding in this area and that proved in the marls of the Middle Chalk by King (*op. cit. sup.*). This similarity is sufficiently striking to prompt the question whether the characteristic features of the post-Lias folding in the Mendips may not be of the same age and origin as the folds of presumably Miocene age in the chalk of Northern France.

It has been shown by Trueman, Cox and others that there were differential movements during the deposition of the Lias, leading to increased thickness in subsiding areas and diminished thickness or stratigraphical and palæontological breaks in areas of uplift or non-subsidence. Among the latter the Mendip region is one of the most important, particularly in the Middle and Upper Lias. The intra-formational movements resulted, however, in much broader folds than those revealed by the present form

[13] Wells and Springs of Somerset, p. 57. *Mem. Geol. Survey*, 1928.

[14] *Ibid.*, p. 63.

of the base of the Lias; moreover the areas of greatest interruption in the succession do not in general coincide with the areas of maximum elevation.

If we examine a geological map of the south of England the effects of the Miocene folding on the Cretaceous and later strata leap to the eye, and the folds can be readily traced as far as the western limit of the outcrop of those rocks. There appears, however, at first sight to be a remarkable absence of folding in the Jurassic area west of the Cretaceous boundary, as if the folding so evident in the south-east died away in a westerly direction. I have never been able to conceive of any reason why the folding which has so strongly affected the Mesozoic and Cainozoic strata of the south of England should suddenly cease at the western limit of the Cretaceous tract. As a matter of fact, if the outcrops of the Jurassic formations are carefully examined and due regard paid to the surface relief, several shallow folds can be traced through the Upper and still farther west the Middle Jurassic outcrops. We have seen, moreover, that the Lias rocks have suffered considerable folding since they were deposited, but there remains a doubt whether this had occurred before or after the Cretaceous.

There are two possible explanations of the apparent absence of folding in the Jurassic outcrops. If a series of rocks which dip, say, to the east, is later subjected to folding, along say east and west axes, the effect on the outcrops will be less pronounced than if the beds were lying horizontally before the folding took place. The Mesozoic rocks from the Dorset coast to Wiltshire were obviously tilted eastward and subjected to erosion before the deposition of the Cretaceous, since the base of that formation oversteps westward and comes to lie in succession upon all the strata from the Purbeck to the Keuper. This explains in part why the Jurassic formations pursue a less undulating course than the base of the Cretaceous. If it were not for the fact that the Mesozoic rocks had acquired an easterly dip prior to the deposition of the Cretaceous, I believe that the effect of the folds on the outcrops would be just as prominent as it is in the younger rocks.

Another explanation is suggested by the general characters of folded regions. Just as there are anticlines and synclines in a direction transverse to the axes of folding which are culminating points of the movement, so along the axes there are culminating points where the pitch of the folds changes over. In the general direction of the fold axes, these regions tend to occur at fairly regular intervals, forming, as it were, a succession of cols and domes on the folded surface. A close examination of any folded area will show that in the neighbourhood of the dome culminations the movement is concentrated on one or two axes of uplift, whereas in the neighbourhood of the cols it is dispersed over many axes, no one of which attains a marked predominance over the others.

These characters are well illustrated by the folding of the Weald. The anticlinal structures which have such a marked effect in the centre of the Weald can hardly be recognised in the minor wrinkles that traverse the extensive chalk plateau between Winchester and Kingsclere, where the only fold of any magnitude is the sharp Kingsclere monocline. Farther west, however, several well-marked folds reappear in the Cretaceous, along which the Vale of Pewsey, Vale of Warminster, and the Vale of Wardour have been eroded. At the western end of the Weald the folds pitch in general to the westward, while in the above-mentioned vales the pitch is eastward. A change of pitch takes place somewhere in the middle of the chalk plateau between Salisbury Plain and the western end of the Weald. Just as the folding appears to gain in intensity and definition as one goes from the western end towards the centre of the Weald, so if the movement were continued westward beyond the Cretaceous outcrop its effects might be expected to become prominent again in the next region of dome culmination somewhere to the west.

Despite the fact that the Middle and Upper Jurassic outcrops appear to be almost devoid of folding transverse to their outcrops, there is a remarkable general correspondence between the main folds that traverse the base of the Cretaceous and those that affect the base of the Lias farther west.

The upfold of the Cretaceous rocks in the Vale of Pewsey, which may be regarded as the westerly prolongation in a modified form of the Guildford Hogback and the Kingsclere monocline, can in fact be traced in the Jurassic rocks; the axis of the upfold swings in a west-south-westerly direction towards Frome. Beyond that place it is prolonged in the anticlinal region of the Mendips, and the north-westerly trend of the Mendip fold west of Frome is, as it were, a mirror image of the north-easterly trend of the fold east of that place. The fold is partly replaced by a strike fault which partakes in the swing of the anticlinal axis.

The Vale of Warminster upfold is less obvious; it can be recognised, however, in the Jurassic rocks round Wanstrow, but it cannot be traced into the base of the Lias south of Shepton Mallet, unless it is replaced by a strike-fault which runs through the Jurassic outcrops nearly parallel with the Mendip fold. The marked anticlinal axis accompanied by a strike-fault, which traverses the Vale of Wardour, produces little apparent effect in the Middle Jurassic rocks, but in the continuation of the line to the west a strong upfold of the Lias is indicated by the strike lines of the base of the formation which swing in an east-north-easterly direction from south of Taunton to Somerton, and thence in a westerly direction to the south shore of the Bristol Channel at Watchet and Porlock. The same fold is obvious also in the trend of the Middle and Upper Lias from Ilminster, through Yeovil, Castle Cary and Glastonbury.

These correspondences can hardly be accidental, and it is my belief that the folding revealed by the differences of level in the base of the Lias indicates the continuation into the Bristol Channel region of the Miocene folds that traverse the south-east of England.

It is of interest also to consider the correspondences between the synclines of the Bristol Channel region and those farther east. One of these is immediately obvious. The extension of the Cretaceous rocks far to the westward of the main trend of the outcrop into the Blackdown Hills south of Wellington undoubtedly indicates a prolongation of the Hampshire basin. It may be observed that the Jurassic rocks exhibit a similar though less pronounced deviation to the westward.

The syncline that intervenes between the upfolds of the Vale of Wardour and the Vale of Warminster corresponds in position and direction with the well-marked Glastonbury syncline which is continued westward to the shores of the Bristol Channel itself,.and we may probably regard it as the master structure which has determined the existence of the Channel. The London Basin may be prolonged into the tectonic depression between North Hill and the Mendips, but I believe that its representative in the west is the syncline that extends in a west-north-westerly direction nearly along the valley of the Avon west of Bath, and it is not improbable that the course of that river was determined, like that of the Thames, by this downfold. In the western region the effects of the folding are more prominent in the south than in the north, so that the syncline of the London Basin becomes insignificant westwards and its place is taken by the Glastonbury syncline, which itself appears to die away eastwards. The same thing is found in the reverse direction in comparing the folds at the west end of the Weald with those near the centre. The maximum uplift on the west was along the Kingsclere fold near the northern margin, whereas farther east it was nearer the southern margin along the Battle axis of elevation.

The correspondences between the post-Liassic folding in the south-west of England and the Miocene folding of the south-east of England are so striking that it appears we may safely accept as a working hypothesis that the post-Liassic folding in the former district is in its main characteristics also of Miocene age. The precise effects of the folding would, however, depend upon the condition of that area at the time when the movement occurred.

I have already given reasons for supposing that the Palæozoic areas of the west were covered by the Cretaceous and possibly by earlier Mesozoic formations. It can hardly be supposed, however, that the thickness of the Mesozoic cover was as great as in the south-east of England. The Palæozoic floor would, therefore, be nearer the surface of the folded strata and its deformation would reflect much more closely that of the strata of the cover than does the Palæozoic floor of the south-east of England. Accepting as a provisional hypothesis that the Bristol Channel was determined like the London Basin by movements, in the main of Miocene age, it is of interest to examine the possible consequences of such movements within the Palæozoic regions that adjoin the Channel to the north and south.

The area to the north has been dealt with exhaustively by Strahan, to whose conclusions reference has already been made.[15] That author believed that the rivers of South and Central Wales originated upon a surface of Upper Cretaceous sediments which completely blanketed the Palæozoic rocks beneath, and that the direction of the main streams such as the Wye, Usk, Rhymney, Taff, etc., was determined by a tilt of the Cretaceous cover to the south-east, *i.e.* in the same direction as the dip of the Chalk outcrop from Wiltshire to the border of the Fens. He called attention, however, to another system of valleys such as the Neath, Tawe, Towy and others which had a south-westerly trend almost parallel to the strike of the chalk escarpment. Both systems were assigned to the Miocene period. The latter were attributed to movement or renewal of

[15] The Origin of the River System of South Wales. *Quart. Journ. Geol. Soc.*, vol. lviii., p. 207.

movement on certain 'lines of disturbance' along which the Carboniferous and older rocks have been profoundly affected. As a result the streams along these lines have exerted a trapping influence upon the south-easterly streams.

Not only in the region discussed in detail by Strahan which extends from the Vale of Glamorgan to the Vale of Towy, but in other parts of South Wales, especially north of the Towy, we find many examples of south-easterly streams which have been trapped by others flowing to the south-west.

Apart from these, however, if we look at any map of South Wales we shall find that the dominant trend of the main streams such as the Loughor, the Lower Towy and the Eastern and Western Cleddau is either south-westerly or southerly, but so far as is known it does not coincide with disturbances, as does that of the Neath, Tawe and others. This is also the trend of the Ogmore, which is exceptional among the set of south-easterly streams in the eastern part of the coalfield.

In the Towy Valley there are many gaps in the escarpment on the south side of the valley that are situated in the direct line of tributaries flowing into the Towy from the north-west, and apparently indicate diversion of the streams south-westward into the Towy. Still more remarkable instances occur along the Cothi, a tributary which enters the Towy at Abergwili. The lower part of the Cothi flows nearly due south from Brechfa for about eight miles. Above this place its valley trends north-eastward for over 20 miles and along its course numerous tributaries entering from the north-west have prominent wind-gaps corresponding to them, on the south side of the Cothi valley. There can hardly be any doubt that these indicate former diversions of the south-easterly flowing streams into the Cothi.

This has proceeded to such an extent that a tributary formerly flowing south-east into the Towy 25 miles above where the Cothi now enters has been diverted into the valley of the latter.[16] It is true that the courses of both the Towy and the Cothi coincide in large part with belts of disturbance in the Palæozoic rocks. It is remarkable, however, in the case of the Cothi, that the extension of the drainage along this belt to the south-west of Brechfa has been insignificant in comparison with that to the north-east. Only one such stream, Nant Pib, two miles west of Brechfa, appears to have been diverted.

While agreeing with many of Strahan's deductions regarding the origin of the South Wales drainage system, I am of the opinion that we may be dealing with the effects of two distinct movements, one, the earlier, which gave a prevailing south-easterly trend to the streams, and the other, a later one, to which was due the southerly or south-westerly tilt; the effect of the former predominating in the easterly region of South Wales and that of the earlier becoming more marked in the west. The earlier movement was in all probability that which occurred between the Cretaceous and the Eocene.

Strahan (*op. cit.*, p. 218) calls attention to the evidence that uplift in the west had already commenced when the Eocene was deposited; he

[16] O. T. Jones, The Upper Towy Drainage System. *Quart. Journ. Geol. Soc.*, vol. lxxx., pp. 568–609.

infers from it that the limit of the Eocene basin was not far distant [from Dorset], and that it seemed scarcely probable that Eocene sediments extended over South Wales. The rapid change eastward in the characters of the Eocene strata of the London basin and the Hampshire basin suggests that they were deposited off a land area lying to the west.

There appears to be independent evidence of a tilt in a southerly or south-westerly direction in Central Wales. In the northern part of Cardiganshire a well-defined 'Coastal Plateau' slopes gently from about 400 feet at the coast to about 800–900 feet at its inner edge, where the surface rises rapidly to a height of about 1,900 feet in the 'High Plateau.' The elevations of Plynlimon, Cader Idris and others to which I have previously referred stand sharply out of the higher plateau. Farther south in Central Wales the Coastal Plateau persists at about the same level, but the step between it and the High Plateau diminishes in height, since the surface of that plateau declines southwards. In South Cardiganshire, Carmarthenshire and North Pembrokeshire, where the two plateaus appear to have become merged into one, the Prescelly Range and other hills rise above its surface in the same manner as the hill masses farther north. Comparing the features of South-west Wales with those of North Central Wales, it would appear that the Prescelly range and other eminences in Pembrokeshire should be correlated with the summit masses of Cader Idris and Plynlimon, and the surrounding plateau with the High Plateau of Central Wales. Such a correlation implies that the surface of the High Plateau has been warped down in a southerly direction, so that it has descended to the level of the Coastal Plateau. The mutual relations of the two features suggests that this warping had occurred before the latter was eroded. The amount of warping is about 1,900 feet in a distance of 80 miles, or about 24 feet per mile on the average, but the slope appears to be relatively greater in the south than in the north.

A deformation of this amount would increase profoundly the erosive power of any stream that flowed in the direction of the tilt, and I believe it affords the most reasonable explanation of the great development of the south-westerly flowing streams in parts of South Wales. In the eastern part of the Principality, where the valleys traverse the upland area of the coalfield which stands well above the surface of the High Plateau, the effect of the south-westerly tilt is imperceptible, but it appears to become more effective as the level of the upland in which the valleys are carved diminishes in height to the south and west. It may be that in the eastern region the streams were too deeply incised in the surface to suffer diversion when the tilting movement occurred.

On the assumption that the High Plateau has been warped since its formation, its present form indicates that it was domed along a broad area extending eastwards from North Central Wales. If the direction of this dome is prolonged it meets the axis of elevation that has caused the great swing in the strike of the Cretaceous rocks in the Fen country. Moreover the transverse watershed in the Midlands that divides the southerly streams of the Warwickshire Avon drainage from the northerly streams of the Trent system lies approximately on the same line.

It is not impossible, however, that the present slope of the surface is due to movements of more than one age; thus, the south-westerly tilt may have been superposed upon a region which already had a general slope to the south-east. This would accord with the relations of the two main directions of the drainage system.

Rastall attributed the swing of the chalk outcrop to Miocene folding. In agreement with this view is the 'Alpine' trend of the fold and the fact that the Eocene beds of the London basin strike for some distance parallel to the Cretaceous.

There is a remarkable correspondence between the trend lines of the Palæozoic plateau in the west and the strike of the newer rocks in the east. The south-westerly tilt of the plateau stands in the same relation to the east-west trough of the Bristol Channel as the south-easterly tilt of the chalk in the Chiltern escarpment to the syncline of the London basin. The London basin has a general pitch to the east, whereas the trend lines of the plateau north of the Bristol Channel and the trumpet-shaped outline of the Bristol Channel which is a consequence of this trend suggest a syncline pitching to the west. In brief, the drainage of the western region is in its main lines a mirror image of that in the east.

Let us now turn to the area south of the Channel. The syncline of the Hampshire basin is prolonged to the west into the region between the Blackdown Hills and the Dorset coast. Still farther west lies the remarkable plateau of the central plain of Devon. Just as the streams in the Hampshire basin converge towards the centre of the basin before spilling over in the Solent, so the main streams of north and central Devon tend to collect from north and south in the centre of the plain before spilling over by way of the Exe, the Torridge and the Taw. One can hardly doubt that the surface of the central plain of Devon is an area where the Palæozoic surface has been warped into a syncline which is a continuation of the Hampshire basin.

The Exmoor range which intervenes between the Bristol Channel syncline and the central Devon syncline is the analogue in the west of the Wealden anticline in the east. If I am justified in my conclusion that the central plan of Devon and the Palæozoic upland of Wales have been warped into approximately their present form by Miocene folding, it is a logical deduction that the present elevation of Exmoor is due not to the greater resistance of the rocks in comparison with those to the south, but to the arching up of the surface during the episode of Miocene folding, thus causing streams to flow off its flanks into the Bristol Channel on the one side and the central plain of Devon on the other.

Some support is given to the correctness of this interpretation of the physical features of the western region by consideration of the trend of the watershed which crosses England from east to west and divides the streams that flow northward into the Bristol Channel or the Thames basin from those that tend to flow southward into the English Channel.

At the eastern end of the Weald the watershed coincides approximately with the crest line of the centre of the Weald, with a prolongation along the Battle axis where it separates the Rother from the Cuckmere. From the centre of the Weald it pursues a meandering course to the Hampshire Downs, in one place approaching but not actually reaching the line of the Hogback at Guildford. In the Hampshire Downs it swings in a north-westerly direction for several miles, then resumes its westerly course until it crosses to the north side of the Vale of Pewsey. It appears to have been determined locally by the position of the most important fold which traverses the chalk outcrop, and as this tends to occur farther north in the west than in the east, so the watershed migrates in that direction.

If, however, we follow from the Vale of Pewsey the water-parting between the streams that flow into the Bristol Channel and those that flow into the English Channel, we find that it swings in a south-westerly direction to the south of Yeovil and then westward along the north flank of the Blackdown Hills, and thence along the centre of the Exmoor Range. We have seen previously that starting at the Vale of Pewsey and going east, the principal axis of folding lies farther and farther south, until we reach the Battle axis north of Hastings; and that going west from that Vale the most important of the anticlinal axes lie farther and farther south. In the extreme west the anticlinal axis of the Exmoor range is the dominant structure. It will be observed that the water-parting behaves in a closely similar manner.

In accordance with the view herein expressed, I regard the Bristol Channel as having come into existence as a definite basin by folding during the Miocene period, and that the present form of the surface in Devon and South Wales owes its origin to warping, during the same period, of an ancient surface of erosion.

It appears to me that the number of correspondences and analogies between the physical features and particularly the drainage systems of the south-east of England and those of the lands on both sides of the Bristol Channel are so many and so close as to rule out mere coincidence, and I believe that the only hypothesis which satisfactorily accounts for them is the one which I have outlined. Whether the ancient surface was eroded under marine or under continental desert conditions is still a subject of uncertainty. There is no doubt, however, that the possibility of desert denudation having played a leading part in its development has a claim to serious consideration.

As I stated at the beginning of my address, I have had to omit all consideration of the later episodes which have given the Bristol Channel its present configuration and coastal features. It may appear that in the course of this address I have propounded more problems than I have solved. It is my earnest hope, however, that I have sufficiently indicated the interest and wide bearing of these problems, and that future workers will turn to them and apply to their further elucidation the ever-increasing knowledge and resources of our Science.

# THE TAXONOMIC OUTLOOK IN ZOOLOGY.

ADDRESS BY

W. T. CALMAN, D.Sc., F.R.S.,

PRESIDENT OF THE SECTION.

The selection of a systematic zoologist for the honour of addressing you from this chair implies a belief that systematic zoology may have something to say that will not be without interest to those whose studies lie in other fields. I am not sure how far this belief is generally shared. The anatomist, the physiologist, the field naturalist, the student of one or other of the innumerable specialisations of biological science, has always been inclined to regard with distaste, if not with contempt, the work of those whose business it is to denominate, classify and catalogue the infinite variety of living things. The systematist is generally supposed to be a narrow specialist, concerned with the trivial and superficial distinctions between the members of some narrow group of organisms which he studies in the spirit of a stamp collector; happy when he can describe a new species, triumphant if he can find an excuse for giving a fresh name to an old one.

It would be idle to deny the truth that there is in these criticisms, just as it would be easy, although unprofitable, to point out that the substance of them might be directed against the practice of most other branches of research. The specialist, of whatever kind, has a tendency to mistake the means for the end, to become fascinated by technique, and to suffer from a myopia that blurs his vision of other fields than his own.

I think, however, that there are some signs of an increasing appreciation of the usefulness and even of the scientific value of taxonomy among the younger generation of zoologists. More particularly, those who are concerned with the applications of Zoölogy to practical affairs are, for the most part, although not invariably, aware of the need for exact identification of the animals they deal with. They do not always realise the difficulties that may stand in the way of this identification. It is a common experience with us at the Natural History Museum to have some mangled fragments of an animal brought in by a practical man who expects to be supplied with the name of it while he waits. I am afraid that he often goes away with a low opinion of our competence.

It may not be without interest, therefore, if I attempt, in the first place, to give some idea of how matters stand with this part of the systematists' task, the identification and description of the species of living animals.

When Linnæus published in 1758 the first volume of the tenth edition of his 'Systema Naturæ' he named and described about 4370 species of animals. If we ask how many are known to-day the diversity of answers we get is some indication of the confusion that exists. Some years ago, at the request of the late Sir Arthur Shipley, I endeavoured to get from my colleagues at the Museum estimates of the numbers of species in the various groups with which they were specially conversant. Some of the answers obtained were very interesting. With regard to Mammals I was told 'anything from 3,000 to 20,000 according to the view you take as to what constitutes a species'. For the most part, however, the authorities consulted were unwilling to suggest even an approximate figure for a very different reason. They told me that great sections of the groups with which they were concerned were so imperfectly surveyed that it was quite impossible even to guess how many of the supposed species that had been described would survive reconsideration.

It may be worth while to consider for a little the second of the two obstacles thus indicated as standing in the way of obtaining a census of the known species of animals. In the days of Linnæus it is likely that a very experienced zoologist might have been able to recognise at sight any one of the four thousand species of animals that were then known, and when the expansion of knowledge had made such a feat no longer possible, the specialist who confined his studies to one section of the animal kingdom could still aspire to a like familiarity with the species of his chosen group. With this kind of knowledge it is literally true that, as has been said, a systematist recognises a new species by instinct and then proceeds to search for the characters that distinguish it. Some of the great zoologists who were still working in the British Museum when I entered it more than a quarter of a century ago, men like Albert Günther, Bowdler Sharpe, C. O. Waterhouse and Edgar Smith, had actually an amazing personal familiarity with vast sections of the animal kingdom. They had studied and digested all that had been written on their subject, and, if they did not carry the whole of this knowledge in their memory, they could, without searching, put their hand at once on the volume that would help them. They had no need of 'Keys' to help them to run down their species, indeed they rather distrusted such aids for they knew how easily they betray the heedless. Specialists of this type there must always be, and we may be thankful for it. Nothing can altogether replace that instinctive perception of affinity that comes from lifelong study. It has often happened that men such as those I have named were able, when confronted with new and aberrant types of animals, to allot them at once to a place in classification which subsequent research served only to confirm. As time goes on, however, the extent of ground that can be covered in this fashion by the most industrious worker is rapidly diminishing. The torrent of publications catalogued in the Zoological Record increases year by year, and the specialist, if he is not to be overwhelmed by it, must not allow his curiosity to stray beyond the limits of a narrow corner of the field.

By far the greater part of this literature is written by specialists for specialists, and much of it is unintelligible to anyone else. From the time of Linnæus, however, there have not been wanting publications that have a different aim. We have monographs, synopses, revisions, of all sorts and sizes, attempting to render possible the identification of species without demanding a lifetime of study for each special group. The ideal for such monographs would be, I assume, that they should be intelligible to, and render possible the determination of species by, any properly trained zoologist, even without previous experience in dealing with the particular groups of which they treat.

The Zoological Department of the British Museum may fairly claim to have done more towards this re-editing of the Systema Naturæ than any other institution in the world. The long series of monographs, of which the true character is somewhat concealed under the official title of 'catalogues,' is a monument to the learning and industry of the great zoologists who planned and executed them. Though they remain indispensable to all serious students of the different groups, however, they are now, for the most part, long out of date, and, vast as is their scope, they cover only a fraction of the animal kingdom.

In 1896 the German Zoological Society began the publication of 'Das Tierreich,' afterwards continued by the Prussian Academy, which was planned to give nothing less than a revision of all the species of living animals. Here again, however, after thirty-four years, only a small part of the ground has been covered and already the progress of research has rendered many of the earlier parts obsolete. Col. Stephenson tells me that Michaelsen's revision of the Oligochæta, published in this series in 1900, deals with exactly half the number of species enumerated by the same authority in 1928.

Apart from these attempts at comprehensive revision we have, of course, numerous surveys of local faunas on a larger or smaller scale, besides monographs of restricted groups, but hardly ever do these fit together without leaving gaps, geographical or systematic.

Take, as an example, the Brachyurous Crustacea or true Crabs. No revision of the Brachyura as a whole has been attempted since Henri Milne-Edwards' 'Histoire Naturelle des Crustacés,' published nearly a century ago. The student who wishes to identify a collection of crabs has to begin with local faunas, such as Alcock's invaluable 'Materials for a Carcinological Fauna of India,' and Miss Rathbun's monographs of the American species ; but for regions that have not been thus studied there is no way but to search out and compare the descriptions of species in innumerable obscure publications by writers who had often an imperfect knowledge of what had been done elsewhere. The genus *Pilumnus* is one that is abundantly represented in all the warmer seas of the globe. No revision of its numerous species has been attempted in recent times. I do not even know how the genus is to be defined from neighbouring genera ; and yet hardly any report on a collection of tropical crabs does not profess to describe at least one new species of the genus.

Another example from a very different group of animals is given by the aberrant Lamellibranch Mollusca forming the family Teredinidæ, commonly known as 'shipworms.' During the past ten years a great deal of attention has been given to these animals in the effort to discover means of combating or avoiding their attacks on the timber of harbour works and the like. Nevertheless, the taxonomy of the group remains in a state of the utmost confusion. There is no agreement as to the limits even of the genera, and the inconstancy of the characters that have been used for the definition of species is plain to anyone who studies a large collection.

Only in one species, the long-known and often-studied *Teredo navalis* of Linnæus, have we any detailed information as to variability and the changes that take place during growth. In these circumstances the publication of new specific names, except after prolonged study of ample material, cannot be regarded as a serious contribution to knowledge. Dr. Bartsch, of Washington, in his 'Monograph of the American Shipworms' (1922) simplified his task by the assumption that any species found on the coasts of the American continent must, of necessity, be different from any found elsewhere, and he was thus able to write 'n.sp.' after twenty-two out of twenty-nine specific names. It was soon shown, however, by other American zoologists, that this assumption was without foundation, and that the most destructive species on both the Atlantic and Pacific coasts of North America was the European *Teredo navalis*.

A thorough revision of the taxonomy of the shipworms would be a task of much difficulty, but it would be of great scientific interest and it might even be of great practical importance. Those who are carrying out experiments on the protection of timber, in this country at least, seldom trouble to enquire what species they are dealing with or even whether they are always dealing with the same one. Professor Barger, for instance, who speaks of *Teredo* as a 'species' does not seem to think that it matters. Perhaps it does not, but it is just possible that it does. We do know that different species differ greatly in susceptibility to changes in the salinity of the water, and it seems worth while to ask whether they all react in exactly the same way to the poisons that the chemists try to administer to them. The fact that our knowledge of their specific differences is still very incomplete is no reason why the chemists should not avail themselves of such knowledge as we have.

One cause that has encumbered systematic literature with uncounted pages of useless writing is the prevalent delusion that it is possible to give what is called a 'complete description' of a species. This phrase is apparently intended to denote an enumeration of the visible features of the organism so exhaustive as to include not only the characters differentiating it from the other species already known but also those that will serve to distinguish it from species yet to be discovered. Now a moment's reflection will show that a lifetime would not suffice for the 'complete description' of any animal whatsoever, and, on the other hand, a very little experience will convince one that it is impossible to predict the kind of characters that will distinguish the next new species. Some years ago I found that all the specimens of the genus *Squilla* in the Museum collection from West Africa differed in half a dozen constant, and, once they were pointed out, conspicuous characters from their nearest congeners. It happened that shortly before a German zoologist had given what was intended to be a 'complete description' of a *Squilla* from the same region. His account extended to two large quarto pages, and yet it succeeded in avoiding mention of every one of the features that proved to be distinctive of the species.

If every one who describes a new species were to restrict himself to a bare enumeration of the characters in which it differs from all the known species of its genus, systematic papers might be vastly diminished in bulk, although one suspects that the labour necessary to write them might be correspondingly increased. It may be a counsel of perfection to suggest that no one should introduce a new specific name without undertaking at least a partial revision of the genus including it, but there are very many instances where the multiplication of species might with advantage be postponed until we learn something about those that are supposed to be 'known.'

The number of described species of animals has been estimated at something in the neighbourhood of three-quarters of a million. It is not at all improbable that between a quarter and a third of that number would be suppressed as synonyms or put aside as 'species inquirendæ' by careful monographers and that in many groups the proportion would be far higher.

The prospect is not one that can be contemplated with any satisfaction. The successively expanding volumes of the 'Zoological Record' give us a picture of systematic zoology being smothered under the products of its own activity. The confusion will grow steadily worse unless systematists come to realise that the mere description of new species is a far less important thing than the putting in order of those that are supposed to be already known, and until, on the other hand, zoologists in general cease to regard taxonomy as a kind of menial drudgery to be done for them by museum curators.

I have alluded to another obstacle to obtaining an enumeration of the animal kingdom in the divergences of opinion as to what constitutes a species. I am not sure that these divergences are not sometimes over-estimated. I think that it will be found that in most Orders of animals there exists a considerable body of species regarding whose limits there is no serious difference of opinion among competent systematists; but alongside of these we find in almost every Order, in most families, and even in many genera, a 'difficult' residue in which the delimitation of specific groups sometimes seems to be little more than a matter of personal taste. My colleague Mr. Robson has recently brought together a great deal of information on this subject in his book 'The Species Problem,' to which I would refer anyone who needs to be convinced how complex the problem really is. For our present purpose it is enough to take the empirical fact that the majority of animals can, with more or less trouble, be sorted into assemblages or kinds that we call species. We have seen how imperfect and confused is the present state of knowledge even as regards the mere description and identification of these kinds.

The business of the systematist, however, does not end with identification. Even identification requires some kind of classification, if it is only the classification of the dictionary. Since the time of Linnæus, or rather since the time of John Ray, zoological systematists have believed in the existence of a Natural System of classification which it was their business to discover; since Darwin, it has seemed plain that this Natural System must be, in some way, based upon Phylogeny. It is now realised that the relation between the two is not always so simple and straightforward as it once appeared to be. Dr. Bather, in his presidential address to the Geological Society in 1927, discussed the historical and philosophical bases of biological classification. He concluded that 'The whole of our System, from the great Phyla to the very unit cells is riddled through and through with polyphyly and convergence,' and that 'Important though phylogeny is as a subject of study, it is not necessarily the most suitable basis of classification.' I am not sure that I quite understand what is implied by the second of these statements, but I do not suppose that even Dr. Bather would be prepared to suggest a system of classification entirely divorced from phylogenetic considerations.

Forty years ago the reconstruction of the evolutionary history of the major divisions of the animal kingdom was almost universally regarded as the chief end of zoological research. To-day, except among palæontologists, one might almost say that the phylogenetic period in the history of zoology has come to an end. When one recalls the extravagances of its later developments, the derivation of Vertebrates from Arachnids and of Echinoderms from Cirripedes, one cannot be surprised that zoologists of the modern school take little interest in it. If we accept this attitude, it follows that problems of affinity and relationship are not worth worrying about. We are told, in so many words, that our business as systematists is identification, not classification; that what we have to do is merely to devise some kind of key or card-index that will enable animals to be quickly and easily sorted into species. As far as the really scientific branches of zoology are concerned an artificial system of classification is as good as, and may even be better than, any other. An illustration of this attitude of mind is seen in a paper recently issued from Cambridge in which *Lithodes* is replaced, without explanation or discussion, among the Brachyura; which, on the card-index system, is doubtless its appropriate place.

It is quite true that the categories of the physiologist, the ecologist, the geneticist, and so on, often cut across the dividing lines of the most natural classification we can devise, but both the divergences and the coincidences are worthy of closer consideration than they sometimes receive. If there is any truth in the theory of Evolution it is obvious that functions and habits have an evolutionary history behind them, but it is no less obvious that this history has not been independent of the history of the organisms that display them. The details of this history we shall never fully know and even its broad outlines may perhaps always remain misty. A Natural system of classification expressing even these broad outlines may prove to be an unattainable ideal, but each step towards it holds out the promise of usefulness in other and possibly remote fields of research.

A great deal of current work and still more of current speculation in zoology seems to me to suffer from this neglect of the taxonomic outlook. In the zoology of the later nineteenth century the comparative method was still the chief tool of morphology. The relative importance of structural characters was measured by the extent of their persistence through larger or smaller divisions of the animal kingdom. This point of view tends to be lost sight of with the increasing emphasis on the experimental method. The systematic zoologist, in listening to the exponents of the modern lines of research is apt to be impressed by the little account that is taken of the vast variety of animal life. To say this is not to underrate in any way the advances that have been made in these lines within the present century or the revolutionary changes they have made in our views on many fundamental questions. Physiology, for example, is to-day a vastly different science from what it was thirty years ago, partly because the physiological laboratory has a more varied fauna than it had then. Nevertheless, the zoologist, conscious of the unending diversity of structure and of habits among animals, sees the physiologist's results against a background of which the physiologist himself seems to be sometimes forgetful.

One hesitates to suppose that the students of heredity are really so forgetful of this background as they sometimes seem to be. No doubt intense specialisation is needed for intense research; but the Poet of the Breakfast Table, laughing gently at the narrow specialism of the Scarabee, can hardly have foreseen the day when a university in his own country would have upon its teaching staff an officer named in the university calendar as a 'Drosophilist.'

It is possible, however, that the prevailing lack of interest in questions of phylogeny may have a deeper significance. Those departments of biology that are being most actively studied at the present day are preoccupied with the interplay of forces acting here and now. They ignore the impressions that time may have left on the material of their study. It is as though a crystallographer, studying a pseudomorph, should endeavour to explain its form in terms of its chemical composition and the forces governing the arrangement of its molecules, without taking account of its past history.

From ignoring anything, it is but a short step to denying its existence, and here, it seems, we have already arrived. Some of you may possibly have listened to a lecture delivered in London in the early part of last year by that very distinguished experimental biologist Dr. Hans Przibram, in which he suggested that we might have to consider the possibility that every species of metazoan had developed independently of all the others from a distinct species of protozoan. The same view was set forth by him in a lecture delivered in Paris on the Theory of Apogenesis (Rev. Gen. Sci. XI, No. 10, 31 May 1929, p. 293). As the English lecture has not been published I will translate as closely as I can from the French one. 'I do not think it likely' he says, 'that a single substance can have given rise to a general phylogenetic tree according to the classical diagram representing the affinities of species and their distribution in space and time. All the facts would be explained more easily by supposing that there existed, at the beginning, many organised substances developing side by side into species, each of the latter passing through stages more and more advanced without actual relationship of descent between the different species.'

Many authors have believed in a multiplicity of the primordial forms of life, but few have suggested an independent origin for grades lower than the main phyla. Przibram, with strict logic, has carried the same reasoning down to the individual species. Most biologists with whom I have discussed the matter refuse to take his suggestion seriously. This, I venture to think, is a mistake. Przibram has simply carried to their inevitable conclusion certain lines of thought that we meet with everywhere in current biological literature; that conclusion is either one of the most significant results of recent biology or it is the *reductio ad absurdum* of much contemporary work.

Geneticists have made us familiar with the doctrine of the inalterability of the gene, with its corollary of evolution by loss of factors, which, by the way, seems to differ little from Przibram's Apogenesis. The experimentalists have proved (if it wanted proving) the plasticity of the phenotype, as, for instance, when Przibram himself shows that the length of a rat's tail is a function of the temperature to which the individual and its immediate progenitors have been exposed. As for the inheritance of impressed modifications, the more unequivocal the experiments devised to demonstrate its reality the more clearly do they show it to be of so fugitive a kind as to have no significance in evolution. Palæontologists, as Dr. Bather has told us, have proved beyond the possibility of doubt the occurrence of parallel and even of convergent evolution, without telling us where we are to stop in applying the principle. Many supposed examples of adaptation fail to stand closer scrutiny, and therefore the whole idea of adaptation is declared to be a subjective illusion. All these results at any rate place no obstacles in the way of Prof. Przibram's suggestion.

It is to be noted that although the theory of Apogenesis is called a theory of evolution, it does not deal at all with evolution as that word was used by Darwin. It has nothing to say on the origin of species. On this question it is no more than a doctrine of special creation at one remove. It has no light to throw on classification. If we are to abandon belief in community of descent the whole architecture of the Systema Naturæ becomes meaningless.

Prof. Przibram claims that 'all the facts would be explained more easily' upon his hypothesis, but there is one point on which he speaks with a hesitant voice, and it seems to me a very significant exception. 'We cannot decide' he says, 'whether the differing though related species that inhabit islands or isolated territories are descended from a common source or result from the accidental separation of species which formerly occupied the region together.'

Let me recall to you the opening words of the 'Origin of Species.' 'When on board H.M.S. "Beagle" as naturalist, I was much struck with certain facts in the distribution of the organic beings inhabiting South America, and in the geological relations of the present to the past inhabitants of that continent.' So Przibram ends where Darwin began. The geographical and geological distribution of organisms, which for the one are merely the negligible residue of unexplained facts, were for the other the very heart and core of the problem he set himself to consider.

It is worth remembering that among Darwin's other qualifications as an interpreter of nature, he was an experienced taxonomist, and before he wrote 'The Origin of Species' he had produced one of the finest systematic works ever written in his 'Monograph of the Cirripedia.' Those of us who were present at the memorable Darwin-Wallace celebration of the Linnean Society in 1908 remember how the veteran Alfred Russel Wallace discussed 'the curious series of correspondences both in mind and in environment' which led Darwin and himself, alone among their contemporaries 'to reach identically the same theory,' and how he gave the first place to the fact that both he and Darwin began by collecting beetles and thus acquired 'that intense interest in the mere variety of living things' which led them to speculate upon the 'why' and the 'how' of 'this overwhelming and, at first sight, purposeless wealth of specific forms among the very humblest forms of life.' It might be worth while to inquire whether a training that proved useful to Darwin and to Wallace would not be of some value to students of zoology even at the present day.

My predecessor in this chair told you that 'The present position of Zoology is unsatisfactory,' and he found the chief hope for the future in the application of the experimental method. He may be right. I am not so sure. The experimental method has answered many questions and it will answer many more, but there are some questions, and these well worth the asking, to which experiment will never find an answer. No one will maintain that taxonomy by itself will answer them, but it will often suggest where the answer is to be sought for, and it will provide a standpoint from which both questions and answers will be seen in a true perspective.

Finally, I would recall a remark once made in my hearing by a wise old naturalist, the late Dr. David Sharp. Someone had been remarking on the decline of systematic zoology and predicting the extinction of systematic zoologists. Dr. Sharp replied, in effect, 'I have seen many passing fashions in zoology, many departments of research becoming popular and then falling into neglect; the one branch that will never fail to attract is the systematic one. The aesthetic satisfaction to be derived from contemplating the mere variety of animal forms, and from tracing the order that runs through all its diversity, appeals to a very deep instinct in human nature. There will always be systematic zoologists.'

SECTION E.—GEOGRAPHY.

# THE SCOPE AND AIMS OF HUMAN GEOGRAPHY.

ADDRESS BY

PROF. P. M. ROXBY,

PRESIDENT OF THE SECTION.

It is only within comparatively recent times that the term 'Human Geography' has been employed even by geographers themselves. To the general public it is hardly as yet familiar and, although it has now found its way into geographical literature and text-books, its real meaning and scope are liable to a more than ordinary degree of misconception. Yet to many of us it represents a very vital and important part of geography, and the present address is an attempt to review some of its fundamental concepts and to estimate its value, actual and potential, as a contribution to the study of human societies.

Its emergence and significance cannot be understood apart from the evolution of the modern conception of geography as a whole, but on this wider theme I wish to say no more than is essential for my special purpose.

It is not necessary to remind the present audience that although geography has in recent times acquired a new technique and prominence, the subject itself is of vast antiquity. It had its beginnings in the first efforts of thinkers to understand the world in which they lived, the significance and relationship of terrestrial phenomena and the place of man in the scheme of things. As such it had an honourable position in the Græco-Roman world and, as conceived by various philosophers of the Greek mainland, of Asia Minor and of Alexandria, was essentially a philosophical study concerned with a reasoned description and, so far as their limited horizon permitted, an interpretation of the Earth, including its relations to man. Plato and Aristotle, indeed, were capable of singularly facile generalisations about the effects of climate on human behaviour, and we are reminded of Demolins' sweeping statements about maritime societies by Plato's analysis of the influence of the sea, which he tells us 'breeds double-dealing and perfidy; it spreads a spirit which is faithless and friendless over the inner life of a state and over its relations with neighbouring states.' But in the main the Greek approach to geography was scientific and informed by the same desire for a synthetic view of the Earth as of a whole made up of related parts as animates us to-day. The same is true of at any rate some of the Arab geographers, from whom came the principal contributions to geographical knowledge and thought in the long period of the eclipse of the scientific spirit in Europe during the Dark and Early Middle Ages.

The era of the great discoveries, coinciding with the effects of the Renaissance in liberating human thought, revolutionised men's conceptions of the Earth and was a tremendous stimulus to geographical enquiry, but until the nineteenth century speculation about the significance of geographical facts in relation to man remained abstract and doctrinaire. It came, indeed, principally from philosophers such as Montesquieu rather than from geographers themselves, who, handicapped by mediæval traditions, presented their material in arbitrary divisions, so that, as Ritter puts it, 'the whole subject of relations was unstudied.'

It is to Ritter and Humboldt that we owe the real beginnings of human geography as an integral and, indeed, from Ritter's standpoint, the crowning part of the subject-matter. To appreciate the greatness of their work we must realise how critical for the whole future of geography was the period in which they lived. It was a period in which great masses of new geographical data were being accumulated, but so long as these remained unsystematised and unrelated, they tended only to increase the inchoate and amorphous character of a subject which was rather a torture to the memory than a stimulus to the mind. It was a period, too, in which many independent, specialised sciences dealing with particular aspects of Earth Lore such as geology and meteorology were rapidly developing, so that the domain left to geography itself, according to the prevailing conception of its character, was increasingly uncertain. It was Ritter and Humboldt who rescued what seemed indeed to be a moribund subject and gave it coherence, individuality and an immensely enhanced significance. This they did by claiming for it not a distinctive segment in the circle of knowledge—which is to destroy its very essence—but a distinctive method and objective in the handling of data common to other subjects. Ritter gave the keynote to the whole modern development of geography when he said (in his *Comparative Geography*) 'It is to use the whole circle of sciences to illustrate its own individuality, not to exhibit their peculiarities. It must make them all give a portion, not the whole, and yet must keep itself single and clear.' The same note is struck by one of the greatest of later builders in the same field, Vidal de la Blache, in the notable summary of his conception of geography given at the end of a long life mainly devoted to its advancement: 'Nous avons connu longtemps la géographie incertaine de son objet et de ses méthodes, oscillant entre la géologie et l'histoire. Ces temps sont passés. Ce que la géographie, en échange du secours qu'elle reçoit des autres sciences peut apporter au trésor commun, c'est l'aptitude à ne pas morceler ce que la nature rassemble, à comprendre la correspondance et la corrélation des faits, soit dans le milieu terrestre qui les enveloppe tous, soit dans les milieux régionaux où ils se localisent.' Ritter, however, went further than to assert the essential principles of co-ordination, relationship and interdependence in the building up of the geographical synthesis. Geography, he maintained, could only escape disintegration 'by holding fast to some central principle; and that principle is the relation of all the phenomena and forms of nature to the human race.' The same conception permeates his *Erdkunde*, as for example in the passage: 'Nature refuses to be studied except in the great mutual play of all her powers, in the connection of all her manifestations. Only when thus studied does she irradiate with life and light all the paths which human activity dares to tread . . . Ought it not to repay our trouble for the sake of the history of man and of nations to take our stand . . . in the place of their united activities, and to consider the earth in its real relations to man; . . . to trace the course of the simplest as well as the most diffused geographical laws in results, some of which are settled and permanent, some changing, some living and organic?' And he foresees a time when the world of nature as well as of morals and mind shall have been so far compassed as to make it possible for the far-seeing among men 'sending their glance backwards and forwards, to determine from the whole of a nation's surroundings what the course of its development is to be, and to indicate in advance of history what ways it must take to retain the welfare which providence has appointed for every nation whose direction is right and whose conformity to law is constant.' It may perhaps be said that the main difference of view among modern geographers centres in the question whether 'the twofold study of distribution and of the correlation of phenomena' of itself 'assures the place of geography as a separate branch of knowledge' without necessarily involving their relationship to human life.

However this may be, the framework which the great pioneers of the early nineteenth century defined for the building up of a geographical synthesis, which in Ritter's view culminated in man's relationship to the Earth, was sufficiently wide to permit of many converging contributions. Workers in many fields of geography were henceforth guided by the same fundamental principles and methods, and whether in geomorphology, in climatic or human geography, the central object became to exhibit the Earth as a whole made up of related and interacting parts.

Thus, there has been developed by men such as Suess, W. M. Davis and de Margerie the outlines of a systematic interpretation of land-forms, a reasoned and synthetic view of the earth's surface-features (geomorphology). Similarly, there has evolved through the sifting and interpretation of meteorological data a true climatic geography which, as I think all who have profited by such an admirable and lucid exposition as Kendrew's *Climates of the Continents* will agree with me, is something quite distinct from meteorology, however dependent upon it. Again, the study of plant geography, *i.e.* of plant associations or types of natural vegetation in relation to specific types of physical environment, has been worked out in considerable detail and in intimate relationship to the geography of both climates and soils. So, too, we have a systematic geography of animal life. It is no doubt true that some of the workers in these contributory fields have been initially trained in the special science which supplied the data, *i.e.* have been in the first instance geologists or zoologists, but it is equally remarkable that many of them, when once they have acquired the geographical outlook, have changed their objective and become primarily interested in placing or interweaving their contribution in the geographical synthesis as such. For it is from these main sources—geomorphology, climatic and biological (plant and animal) geography—that we derive the data for building up that systematic geography of natural environments which is at once the objective of 'physical' geography and the starting-point of human geography.

Here I venture to maintain that the formulation by my honoured master, the late Prof. A. J. Herbertson, of his scheme of the Major Natural Regions of the World, whatever criticisms in detail may be directed upon it, represents one of the most fruitful and constructive achievements in the development of modern geography, and it was the work of a man who had deliberately trained himself for his task by severe discipline in many branches of analytical science.

All these developments have been based upon the firm foundations which Ritter and Humboldt laid down, and are examples of the truth of Vidal de la Blache's dictum that 'what geography, in exchange for the help which it receives from other sciences can bring to the common treasury, is the art of not dividing what nature brings together.' The fundamental objectives of geography are the same to-day as those which the Greek philosophers of Asia Minor and Alexandria conceived. There is a 'Modern Geography' only in the sense that there has been a restatement of its scope and content in the light of all the new knowledge of the earth which more specialised branches of inquiry have revealed. It was the work of the great pioneers of the nineteenth century to disentangle it from these associated subjects and to ascertain the guiding principles through which and the means or technique by which contact and relationship with them could be most fruitful and helpful in the attainment of the ends for which all science stands. This clarification of its scope and methods was essential if geography was to be in a position to seize the opportunities for increased usefulness afforded by the conditions of the modern world. For the two circumstances which, granted vision and understanding on the part of its exponents, have inevitably enhanced the significance and value of our subject are surely these: that, on the one hand, our more complete knowledge of the earth and of the distribution of phenomena over its surface has made it possible to formulate far-reaching and valuable generalisations as to their co-ordination and relationship for which the material had hitherto been lacking, and, on the other, that the rapidly increasing interdependence and inter-sensitiveness of the different regions and peoples of the planet have made a synthetic view of the world as of a whole made up of inter-related parts—which is the prime object of geography—essential to human progress.

It is against this background of modern geography as a whole that the special aims and contributions of that part of it which we call Human Geography must be considered. The separate, departmental 'political geography' of the early nineteenth century is for ever discredited. Whatever value human geography may have is involved in its association with all the rest of the subject-matter. It is on the question of the precise nature of the relationship that difference of view arises. If time permitted it would be interesting to review the principal contributions to the philosophy of human geography which have been made since the time of Humboldt and Ritter, but I must be content with indicating what seem to me the main tendencies.

From the ranks of geographers themselves—as distinct from the views on the influence of natural conditions on human societies put forward from time to time by philosophers and economists such as Feuerbach, Engels and Marx, or historians such as Buckle and Meyer—the two chief contributions have come from the school of thought associated with the name of Ratzel and that associated with the name of Vidal de la Blache. Many English students of geography who may not have read the *Anthropogeographie* and the *Politische Geographie* of Ratzel are yet acquainted with the general tendencies and the standpoint of his school through the packed pages of Miss E. C. Semple's *Influences of Geographical Environment*, which is based upon his principles, and of the same author's *American History and its Geographic Conditions*, where we see their historical application. All, it is to be hoped, have studied Vidal de la Blache's conception of human geography as the master himself developed it in his incomparable *Tableau de la Géographie de la France* and in some of the great series of regional monographs written by his disciples of the French school.

'Determinism' and 'Possibilism' are the respective labels which have been attached to the two schools, and although labels, here as elsewhere, are liable to mislead, they sufficiently indicate a fundamentally different emphasis and attitude between the two in their treatment of the relationship of human societies to their natural environments. In the first or Ratzelian School the main emphasis is undoubtedly on the control of human activities by natural conditions, on the limitations which these impose, on the permanency of the stage, 'always,' as Ratzel insisted, 'the same and always situated at the same point in space,' and of the influences which it exerts, on the inevitability of particular developments, given a certain milieu. This attitude is even more pronounced in the works of some of the disciples of that other school of French human geographers or, as it is perhaps better to call them, geographical sociologists, who drew their inspiration from Le Play's *Les Ouvriers Européens*, although Le Play himself cannot be identified with all their views. Geographical 'determinism' reaches its culmination in the *Comment la Route crée le Type Social* of Demolins, who maintains that if history were to begin all over again it must in all essentials follow the same lines, given the same setting of the stage. Apart from the question of bias on the compelling power of physical circumstances, a criticism which has been levelled, as I think rightly, against the Ratzelian School, is that it is excessively dogmatic, and that, notwithstanding the vast amount of material which Ratzel himself and many of his disciples have sifted and classified with great skill, we are far as yet from having the data necessary for many of the big generalisations which they make.

The same criticism can certainly not be brought against Vidal de la Blache and his followers, whose discussions of these issues, while often extremely suggestive and illuminating, are rarely dogmatic or final in their conclusions or implications. The master himself did indeed deal in his larger works with what may justifiably be called 'principles' of human geography, but his teaching was always that the larger generalisations could only gradually emerge from a series of detailed and exact regional studies, and we shall all admit, I think, that his disciples have been very true to his precepts. The conception appears in the approach and particularly in the form even of the more ambitious work of Brunhes which bears the title *La Géographie Humaine*. It is hardly possible in a few sentences to characterise la Blache's concept of human geography, but I find its dominant note and one which brings it into salient contrast with the Ratzelian School in the following paragraph :—

'L'être géographique d'une contrée n'est point une chose donnée d'avance par la nature, une offrande du monde inanimé ; elle est un produit de l'activité de l'homme, conférant l'unité à des matériaux qui, par eux mêmes, ne l'ont point. . . . Si une contrée est une personne, c'est par l'effort de ceux qui l'habitèrent.' The emphasis here and throughout his work is not so much on the determinative influence of the stage *per se*, although this is always presented as a vital factor, as on the creative power of human groups to adapt themselves to and, within limits, to mould the natural environment, to leave their impress upon it and thus in the course of generations to transform it and give it a personality which is the outcome of the interaction. This personality is not constant. It may change with man's use or abuse of his habitat.

In all this doctrine a certain power of choice is implied, a power of choice which must increase with man's knowledge and control of the forces of nature. It is Febvre, not himself a member of the Vidal de la Blache school, but a friendly and by no means uncritical interpreter of it, who in that fascinating and penetrating if somewhat elusive work, *A Geographical Introduction to History*, flings down the gauntlet to geographical determinism in the bold challenge ; 'There are no necessities, but everywhere possibilities ; and man, as master of the possibilities, is the judge of their use.' However critical we may be of the validity or at any rate the adequacy of this as a general statement, particularly if applied to the historical evolution of various types of society, it does at least indicate an objective with which the work of human geographers is closely concerned. Von Engeln tells us in the Preface to his *Inheriting the Earth* that he wrote his book 'not so much to show that human organisation and development *have* been determined by geographic conditions as to insist that in the future they *should* be,' the implication, of course, being that man must study far more intimately the nature and possibilities of his geographical environment in order to achieve the harmony with it which lies within his power. This address is to be followed by a paper on the regional planning of a district which includes a great urban complex, separated by a belt still mainly rural and agricultural from a still vaster but more amorphous industrial area. It will deal with the various schemes of zoning, open spaces and land-utilisation by which it is proposed to guide, in the interests of the communities as a whole, the economic and in a sense also the social geography of a region which, in common with many others, has suffered much from thoughtless exploitation in the past. This is one of many examples of the present terribly belated movement in Great Britain towards orderly regional planning, which is essentially a conscious effort in constructive social geography, the attempt to utilise all elements in the physical environment for social well-being (as distinct from the ruthless exploitation of particular elements, e.g. coal, regardless of the wider social consequences which marked the earlier stages of the industrial revolution) and to harmonise the interests of neighbouring towns and countryside in a common scheme in which each has its place. To this movement geographers, I think it may be claimed, have already made some contribution, and at any rate it illustrates admirably the practical implication of the doctrine: 'There are no necessities, but everywhere possibilities; and man, as master of the possibilities, is the judge of their use.'

With this indication of some dominant tendencies in the setting and perspective of human geography, I pass to an attempt to define more closely its subject-matter and its different aspects. I believe that in essence human geography consists of the study of (*a*) the adjustment of human groups to their physical environment, including the analysis of their regional experience and of (*b*) inter-regional relations as conditioned by the several adjustments and geographical orientation of the groups living within the respective regions. The term 'adjustment' I take to cover not only the 'control' which the physical environment exerts on their activities, but the use which they make or can make of it. Human geography is the study of an interaction rather than of a control. The adjustment has distinct but usually closely-related aspects which form the main branches of human geography. The relationship between them is from the geographer's standpoint as intimate as that between the different branches of physical geography. The four principal aspects may be distinguished as the racial, economic, social and political.

The racial aspect implies an adjustment of a different character from the others, one over which man has had little control but which he can increasingly influence through his better understanding of the issues involved. I am well aware that in touching on racial geography I am treading on dangerous and controversial ground. Yet I am convinced that it is as necessary to find the right relationship between human geography and anthropology as it is between physical geography and geology, and that racial geography is as significant and essential a part of the geographical synthesis as is geomorphology. I think it is true to say that racial determinism, i.e. the explanation of characteristics in terms of race alone, apart from environmental conditions, is becoming as discredited as geographical determinism, the explanation of everything in terms of physical environment. Few serious anthropologists to-day uphold the conception of race put forward by Gobineau, the Demolins of racial determinism: 'Le groupe blanc, résidat-il au fond des glaces polaires ou sous les rayons de feu de l'Equateur, c'est de ce côté que le monde intellectuel inclinerait. C'est là que toutes les idées, toutes les tendances, tous les efforts ne manqueraient pas de converger, et il n'y aurait pas d'obstacles naturels qui pussent empêcher les denrées, les produits les plus lointains d'y arriver à traverser les mers, les fleuves et les montagnes.' The tendency in anthropology is certainly not in the direction of thus appraising racial types, so far as they can be definitely distinguished, according to an absolute scale of value or efficiency, but relatively to the geographical environments in which they are found. Their somatic traits are discussed in terms of regional adaptations and the fruitful hypothesis is put forward that so far from racial varieties being unchanging and fixed for all time they are continually undergoing slow modification and in process of becoming. Now the unit of the geographer's study is not race as such any more than it is climate as such, or any other physical element. His unit is the place or region. It is this concept—and I do not think it can be emphasised too strongly—which gives distinctiveness and individuality to his work. With the relationship of climate and other physical factors to race in a region, the geographer is closely concerned, and there are few more important aspects of his study than the composition, actual or potential, of the societies occupying the region. In the world of to-day there are many regions of 'closed' human associations, if I may borrow a useful term from plant geography, regions such as China or the Mediterranean lands as a whole, where the dominant racial type or types in possession are so numerous and well adjusted that the entry of any important new racial element is extremely unlikely. But there are other regions of 'open' human associations, at present thinly peopled but capable of holding a much larger population, whose racial future is uncertain. Such, for example, are Tropical Australia and parts of Malaysia, of Africa, even of Asia. Is it possible or desirable for the geographer in his study of these regions to confine himself to their resources and economic possibilities and not to consider at all, in the light of all that he can learn from anthropology, the relative aptitudes and adaptability, climatic and otherwise, of various racial groups for developing them, and the extent and manner in which co-operation between different groups may in certain cases be secured for this end ?

Take, for example, the highly important pronouncement made by General Smuts last autumn in one of his Rhodes' lectures at Oxford. In the course of his plea for the advance of native Africa through the introduction of a higher civilisation in the form of White settlement, he advocated 'a strong forward movement in the policy of settling the highlands of Eastern Africa which stretch in an unbroken belt, hundreds of miles broad, from Kenya to South Africa.' It is not for me to express an *a priori* opinion on the wisdom of this suggestion, but it raises vitally important issues of human geography which certainly ought to be faced before such a programme is really adopted. These issues are at once racial and economic in character. Do we yet know enough about the effects of a high plateau climate in equatorial latitudes on peoples of North European stock ? Even if it be granted that satisfactory acclimatisation of such peoples in the Kenya Highlands can be achieved, are the conditions of the plateau belt as a whole intervening between them and 'temperate' South Africa sufficiently similar to warrant the prospects of an equally good adjustment ? The tentative generalisation has been made that, from the standpoint of the success of 'White' plantations, there is a vital difference between the 4,500/6,000 feet altitude of the Kenya Highlands and other smaller mountainous 'islands' to the south, and the 3,500 feet level which seems to characterise most of Tanganyika. Or again, what are the prospects of making the 'fly belt' suitable for white settlement ? Or, granted favourable climatic and other physical conditions, have the economic relations likely to be established between the proposed white settlers and the native Bantu tribes been sufficiently considered from the point of view of the uses which the two groups, in the light of their race characters, antecedents and needs, are likely to make of the land ? It is not cartographical surveys alone—although these are vital and the basis of all others—which need to be made before such questions can be answered. Similar questions arise concerning the future of Southern Brazil, Malaya, parts of Central and Eastern Asia and many other regions where groups with different racial characteristics and aptitudes are in competition. The racial aspect is only one of several, but the study of racial distributions, based on anthropological material in the same sense that geomorphology is based on geological material, seems an essential element in the content of human geography. Personally, I feel it to be a distinct gain that in at least one university geography should be closely associated with anthropology, so long as it is not identified with it, just as in others it is more closely associated with economics or history or with physical science. Provided that the subject is kept free and unfettered, it is an advantage to have contributions from special angles. My colleagues will have no more doubt than I have that the field of geographical study, however wide, is definite, but I think they will also agree with me that a complete school of geography is a remote ideal and a complete geographer an almost impossible conception, so that some difference of emphasis between the various schools of geography is not only permissible but desirable in the interests of the subject.

It is unnecessary for my present purpose to elaborate what is implied in that aspect of man's adjustment whose study forms the subject-matter of economic geography. It is of course a fundamental and basic aspect, including the geography of production (with agricultural and industrial geography as its principal subsections) and the geography of exchange (commercial geography in the more technical sense). Partly because it is so fundamental and of such obvious utility and partly, no doubt, because the material is more easily available and the technique involved in its use easier to elaborate, it is this branch of geography which on the whole has made most progress in this country during recent years, so that we now have a large and growing literature in it, including both comprehensive works on its whole field and also detailed regional studies. Here it is no less than a duty to pay a tribute to the valuable service rendered by the geographical departments of the University of London. This development is as it should be, but yet I am convinced that we run the risk of losing the unity and cultural value of geography if we overstress the purely economic aspects and make, for example, the distribution, actual and potential, of products and manufactures the supreme objective. Economic geography serves one of its highest functions if it is closely linked with other aspects of human adjustment to physical environment which have so far received less attention. Of these, one of the most interesting and profoundly important is that which, for want of a better term, we usually call social geography. This may be broadly defined as the analysis of the regional distribution and inter-relation of different forms of social organisation arising out of particular modes of life which themselves represent a direct response—although we may concede to M. Febvre not necessarily the only possible response—to distinctive types of physical environment. A classical example of the importance of this aspect is of course the age-long conflict between nomadic, patriarchal pastoralists and peasant cultivators, socially organised on a territorial basis, along the grassland borders of the hot deserts in Africa and Arabia and round the edges of the steppe-belt in Euro-Asia. In modern times the problems connected with the inter-regional relations of differently organised groups in Africa and elsewhere have been greatly complicated by the impact of industrial Europe on their lives. Franz Schrader in that very illuminating sketch *The Foundations of Geography in the Twentieth Century*, which formed the subject of the first Herbertson Memorial lecture, rightly emphasised the profound disturbance of equilibrium with environment which the rapid transformation of man's relations to nature, through the achievements of applied science, has inevitably produced. It has particularly affected the traditional societies of intertropical Africa, the Monsoon Lands and the South Sea Islands whose mode of life and social organisation, once established as an adjustment to their milieu, often remained in essentials unchanged until they were so suddenly and in some cases so tragically drawn into the maelstrom of modern commerce. In the last analysis this disturbance is one of the chief causes of world-wide unrest, since equilibrium with environment is the first essential of happiness for human groups.

One of the greatest needs of our time is to discover what, for each type of regional environment or milieu, are the real factors in readjustment through which alone the recovery of equilibrium can be attained. What is involved is readjustment to all the local conditions of the habitat in the light of its new contact with other regions, its new place in the total scheme of world relationships. Modern Denmark would seem to be an admirable example of a successful readjustment of this kind. Statesmanship in such an Empire as ours is increasingly concerned with the task of harmonising the interests of many groups cradled in different environments, diverse in race, mode of life and experience, but under the conditions of the world to-day increasingly interdependent. Particularly is this apparent in the problem of the readjustment of African societies, one of the most critical and complex of our time, and one for the solution of which Great Britain has incurred heavy responsibilities. Such problems are as much geographical in character as those concerned with the regional planning of English districts and equally demand detailed surveys by investigators capable of analysing the social life and experience of human groups in their whole geographical setting, and of appreciating the significance of the new elements in their environment. De Préville's *Les Sociétés Africaines* is a brilliant and well-known example of social geography, admirably illustrating its main concepts, and, if a critical examination of it often raises doubts as to the validity of some of its big conclusions, that only the more emphasises the need for detailed local work on these lines, now that material is becoming available. Attention may be drawn to the series of papers to be given in a later session of this section which will deal with the programme of the subcommittee on the Human Geography of Tropical Africa, as an example of the contribution which systematic work in social geography may make towards the better understanding of these problems.

The modern tendency in geography to think of the Earth in terms of natural as opposed to artificial divisions should not lead to the neglect of political geography in the proper sense of the term; for the function of political geography is to study and appraise the significance of political and administrative units in relation to all the major geographical groupings, whether physical, ethnographic, social or economic, which affect mankind. It is essentially an aspect of adjustment to geographical environment, and it is precisely because it is so closely related to other aspects of adjustment, which, in the influences that they exert, are often conflicting, that equilibrium is so difficult to attain. The study of the mode in which geographical conditions have helped to mould the evolution of states in the past is of absorbing interest, however complex and difficult. The existence of favourable areas of characterisation possessing a considerable amount of natural protection, such as the English Plain and the Central Lowlands of Scotland, within which the social contact of originally different racial and social groups was easy, certainly provided the medium through which in Western Europe strong nation-states tended to take shape. The group consciousness which we call nationality seems to have followed rather than preceded the actual formation of such states. Nationality arose in relation to environment and widened its scope and allegiance with the increase of economic and political contact. Thus, Kentish and East Anglian patriotism, without entirely disappearing, were gradually merged into the larger conception of English patriotism. So, too, later, when greater intimacy of contact and realisation of the economic advantages of co-operation had furnished the *raison d'être* of the Union of England and Scotland, and the political unity of the entire island had been achieved, English and Scottish patriotism were correspondingly but only very gradually enlarged. Since the forces promoting the contacts and economic interdependence of regions are operating on a much bigger scale in the world of to-day then ever before, we might expect to see this process of political integration even more strongly marked, and the rapid territorial growth of the United States and other large political entities can be quoted as examples of it. But in the reconstituted Europe of our time we see this process arrested and even reversed. It is only 30 years ago that W. Z. Ripley, in his great work on *The Races of Europe*, after discussing the reasons for the extension of the Roumanian people over what he terms 'the natural barrier' of the Carpathians into Transylvania, asserted that 'geographical law, more powerful than human will, ordains that this latter natural area of characterisation—the great plain-basin of Hungary—should be the seat of a single political unit. There is no resource but that the Roumanians should in Hungary (which then, of course, included Transylvania) accept the division from their fellows over the mountains as final for all political purposes.' The prophecy has been falsified; the 'law' has been broken, although at the price of much economic dislocation, and in the arrangement of the Succession States the unity of the great plain-basin has been ignored. Nationality, as tested by linguistic and cultural affinities, rather than the economic orientation indicated by the physical conditions, has been accepted as the main criterion of the new units, although there is frequent departure from this principle. The New Europe is admittedly a great experiment in political geography. Its success would seem to depend on the possibility of reconciling the different factors. The most stable political units are undoubtedly those which most correspond to geographical realities, but these realities are not wholly limited to considerations of physical and economic geography. The distribution of groups related in culture and language is also a geographical reality. The ideal state from the geographical standpoint is one which neither divides groups culturally related nor interferes with the flow of trade along natural arteries and between regions economically interdependent. It may be, although as yet the indications are not very hopeful, that the urgent need of Europe for greater economic integration can be reconciled with the desire of the small nationality groups for cultural and political autonomy. It may be that economic federation or agreement among small sovereign states within the framework of the League of Nations will prove the only alternative to the 'Super-State' solution of the problem of European political geography propounded by Naumann in his *Mittel Europa*. At any rate, nationality, considered apart from its geographical setting, may be a very dangerous conception.

The problems of political geography in other parts of the world are no less interesting and important. Many of the political units of Africa, carved out in the course of a hasty scramble for power, are essentially arbitrary, and are far from representing natural integrations. It is, however, a welcome sign of a new order that in the allocation of the Mandates for Togoland and the Cameroons the cultural affinities and groupings of the peoples, as well as the physical conditions, were specifically recognised. Lord Lugard some time ago called attention to the great importance of this aspect in the problem of regional self-government in India, and the Simon Commission emphasises its significance. Even in our own country we have analogous problems, such, for example, as whether the county units, developed in relation to conditions of physical and human geography which have largely passed away, should be replaced or in part superseded by larger administrative entities more in harmony with the modern economic regions of the country, a subject discussed in a suggestive way by Prof. Fawcett in his *Provinces of England*.

I have tried to indicate the essential character of the principal aspects of human geography, each of them from the standpoint of the adjustment of human groups to their geographical environment. It is permissible and desirable to pursue special studies of these various aspects of our subject, but they find their fullest fruition when they are brought together and inter-related in a full and comprehensive treatment of regions such as Cjivič gives in his book *La Péninsule Balkanique : Géographie Humaine*. We can never really appreciate the problems of such countries as India, China and Russia until we have a comprehensive interpretation of their human ecology, to use the expressive term employed by the American geographer Barrows. In the future it is probable that geographical specialism in the Universities will be less concerned with aspects (such as geomorphology, climate and economic geography)—although this will always have its place—and more concerned with regions (the Mediterranean, Tropical Africa, the Far East, and so on). The geographer's parish must indeed be the world, but it is too large a parish for all parts of it to be studied in detail by any one man. He must, if he is entrusted with a University department, delegate responsibility for as many regional chapels-of-ease as he can find associates and colleagues to work them.

Of historical geography there is no time, nor is this the occasion to speak, except to give it its place in relation to what has been already said. Historical geography is essentially human geography in its evolutionary aspects. It is concerned with the evolution of the relations of human groups to their physical environment and with the development of inter-regional relations as conditioned by geographical circumstances. It has the same aspects and is permeated by the same concepts as human geography. The primary object is not, as has been too often supposed, to explain historical events as determined by geographical conditions, but, on the other hand, historical geography is far more than history illustrated by a few maps. It is the critical study of an interaction and adjustment, whether exhibited in the history of settlements, land-utilisation, commercial and cultural relations or in the evolution and relationship of administrative units and states. As such, it is to human geography what history in the accepted sense is to politics or, as it is often called, contemporary history, an explanation, so far as it can be given, of how the existing position has been reached, the demonstration of the present as a phase in the whole process of becoming. It demands, and this is at once one of its most difficult and one of its most attractive aspects, the reconstruction of the physical setting of the stage in the different phases of development. It is, indeed, particularly concerned with tracing that 'changing expression which the appearance of the earth assumes' as modified by human action in all its manifestations. No study can be more truly illuminating, and without some knowledge of it as a background the significance of many modern problems of human geography is indeed hard to grasp. Between the 'time-line' of history and the 'space-circle' of geography, to use Brunhes' expression, there can be no arbitrary separation without grave loss to both, and there are welcome signs that historians and geographers are beginning to understand the basis of their co-operation.

We may claim for human geography that, rightly studied, it is a vital element in training for national and international citizenship. It can enable us 'accurately to imagine the conditions of the great world stage' and the place of the different regions within it. It is a valuable mental discipline, calling for an exact sense of proportion in appraising the value of many factors and more specifically developing the great quality of sympathetic understanding. The point of view and type of outlook which it fosters were never more needed than in the present critical stage of human development. Yet, not only through its value as an educational instrument, but also through the programme of constructive work which it advocates, can it contribute to the realisation of the ideal of 'unity in diversity,' and that seems the only possible ideal for the life of humanity on a planet, which, however small applied science may make it, will always retain its infinite variety.

## SECTION F.—ECONOMIC SCIENCE AND STATISTICS.

# RATIONALISATION AND TECHNOLOGICAL UNEMPLOYMENT.

ADDRESS BY

PROF. T. E. GREGORY, D.Sc.

PRESIDENT OF THE SECTION.

S. 1. In recent years British economic science has been enriched by the incorporation into its phraseology of two new terms of art: an incorporation which is significant on more than one ground. One German, the other American in origin, their adoption points to the international character of the social and economic problems of the age and to the directions whence we are accustoming ourselves to find inspiration; vague and uncertain as their content is, their use indicates a shift in the centre of gravity of economic discussion, for they relate to problems of production, and their use is thus a sign that that preoccupation with distributional problems, beginning with Ricardo, but especially characteristic of the last quarter century of British economic thought, has taken a new turn. Lastly, the circumstance that widespread currency has been given to those new and alien terms in connection with the public discussion of the questions to which they relate has created an unfortunate impression that the economic life of the world is being confronted by novel, vast and mysterious problems, of a kind hitherto unknown. The problems involved are indeed of the utmost importance, but when their character is analysed, it will be found that they derive their importance more from a change of scale than from the novelty of their nature.

S. 2. The phenomena to be discussed are capable, if not of exact, at least of fairly definite statement. Throughout the world a conscious process of reorganisation is taking place, involving both the structure of industry and the methods of production. To this process the name of rationalisation has been given. It is many-sided, but among the characteristic results of the rationalisation process are: a growing control over the market, a growing standardisation of process and output, and an increasing—in some cases, a very largely increasing—output per worker. These associated organisational and mechanical changes have, therefore, the result of economising the amount of labour *directly* required per unit of output, and—in so far as the distribution of goods over space and time is itself rationalised—of involving also a net reduction in the amount of labour required to place a unit of output *in the hands of the final consumer.* Given this trend the question arises: will not the opportunity for finding employment in industries subject to these processes of change also undergo a change? In other words, does rationalisation inevitably bring with it unemployment due to the technological alterations involved ? If an affirmative answer is given, then *part* of the existing volume of unemployment in Germany, the United States and Great Britain is not due to causes *local* to the area concerned, such as the popular explanations that unemployment is due respectively to the Gold Standard or the pressure of Reparations or the Wall Street slump, nor even to such *general* factors as the present fall in world prices, but must be directly ascribed to the technological or structural alterations which are taking place. Undoubtedly, if unemployment is resulting from technological changes, the social problem of dealing with it is greatly aggravated by the existence of other local and general causes of unemployment. But we are not entitled to assume that when the *local* causes making for unemployment have disappeared we shall then find ourselves with unemployment reduced to some pre-war 'normal,' for there is no reason to suppose that in the immediate future, the rationalisation process will come to an end. *If* rationalisation does cause unemployment, the post-war 'normal' may be higher, perhaps considerably higher, than the pre-war one. Thus, in the final analysis, we are face to face with the curious result that one of the most popular of all remedies for unemployment may in itself be one of the causes producing the evil for which a remedy is to be found.

S. 3. The first requisite in attempting to analyse the relationships between technical improvements and the volume of unemployment is an historical standpoint. The resistance to change is a permanent element in human society : no alteration in the structure of society or in its detailed economic arrangements can be made without some interference with vested interests. All abstract reason may teach that without mechanical invention and discovery and without improved organisation, the greater part of the world's present population would never have been born : all experience may prove that without economy of effort no increase in the standard of life is possible—nevertheless, change and improvement may be resisted, and upon grounds which deserve serious consideration. The problem, from this point of view, is one of the distribution of the gains and the sacrifices. No one will expect the farmer to rejoice at so bountiful a crop that it does not pay to cart it to market : to appreciate the significance of the law of diminishing returns is as important as to understand that the practice all round of the principle of restriction of output means lessened material welfare. Neither Robinson Crusoe nor a purely Communistic State would be distressed by the problem which we have to discuss here. Under Crusoe economics, mechanical invention and improved organisation would allow of increased consumption or increased leisure, or both, to the sole person interested : under a purely communistic régime improved organisation and technical progress (assuming them to be possible) would increase the national dividend or diminish the national expenditure of energy, or both, without necessarily making things worse for anyone concerned, for, ex hypothesi, goods would be still shared in common. In the Communistic State rationalisation might result in unemployment, but it would not mean, what it may mean under a régime of private property, a *very* unequal distribution of the gains and losses from the changes taking place—though even in a Communistic State some difference would in practice have to be made between the employed and the unemployed in order to diminish the attractiveness of leisure to the unemployed.

S. 4. The progress of technique has been the characteristic feature of the Western World since the eighteenth century, but it was in the early years of the Machine Age that the problems arising out of the contemporary developments were most fully discussed by economists. For this reason the discussion of the problem of Rationalisation by the Classical Economists has a direct significance for the present age: the problem of the 'Influence of Machinery upon the Condition of the Labouring Classes' which was debated by Ricardo and McCulloch, Chalmers and Babbage and Senior is in all essential respects the problem which vexes us to-day. Whatever may have been the attitude of the popularisers of economic thought, the original thinkers of the time were by no means so intoxicated with the progress of technique that they failed to see that it had its drawbacks. Ricardo, in his celebrated recantation in the Chapter on Machinery in the third edition of the *Principles,* finally arrived at the conclusion that the 'substitution of machinery for human labour is often very injurious to the interests of the class of labourers . . . . the same cause which may increase the net revenue of the country may at the same time render the population redundant, and deteriorate the condition of the labourer,'[1] and, in summing up his thought, argued roundly that 'the opinion entertained by the labouring class, that the employment of machinery is frequently detrimental to their interests, is not founded on prejudice and error, but is conformable to the correct principles of political economy.'[2] But this view, though it can be defended on adequate grounds, was based by Ricardo on reasoning which must be regarded as untenable. Charles Babbage, the most fervent contemporary apostle of the application of scientific method to economic life, discusses the whole issue very admirably in his work '*On the Economy of Manufactures.*' Whilst reduced prices, consequential upon the use of machinery, have a tendency to reabsorb the labour inevitably displaced, yet in order to prove 'that the total quantity of labour is not diminished by the introduction of machines, we must have recourse to some other principle of our nature.'[3] This principle turns out to be the influence of the increased *power* to enjoy upon the *desire* to enjoy: 'He who has habitually worked ten hours a day will employ the half hour saved by the new machine in gratifying some other want; and as each new machine adds to these gratifications, new luxuries will open to his view, which continued enjoyment will as surely render necessary to his happiness.' But this optimistic psychology of wants does not prevent Babbage from stressing, (*a*) the effects of new machinery in *redistributing* the demand for labour, so that 'considerable suffering among the working classes' results,

[1] *Principles.* McCulloch's edn., p. 236.

[2] *Op. cit.*, p. 239, *cf.* this with the utterances of a more modern pessimist: Capitalistic rationalisation, in the absence of constantly expanding foreign markets is 'driven back upon the home market: and there it defeats itself and creates around it a desolation of unemployment and human decay.' Labour 'deprived of its independent source of income (*cf.* Ricardo's "Gross Revenue") ceases to be effective in the market as a buyer, and thus defeats the aim of the reduction in costs which has been achieved.' G. D. H. Cole. *The Next Ten Years*, p. 109.

[3] This and the following citations are taken from the fourth edition of the *Economy of Manufactures,* 1835, paras. 404–407.

(*b*) the increased competition which rationalisation sometimes induces among the workers, for ' even though the increased demand for the article, produced by its diminished price, should speedily give occupation to all who were before employed, yet the very diminution of the skill required would open a wider field of competition amongst the working classes themselves,' (*c*) the difficulty in deciding whether, when improvements were made, the process of displacement should be gradual or immediate: ' the suffering which arises from a quick transition is undoubtedly more intense: but it is also much less permanent than that which results from the slower process: and if the competition is perceived to be perfectly hopeless, the workman will at once set himself to learn a new department of his art.' In the end Babbage was driven to adopt a very doubting tone: ' That machines do not, even at their first introduction, *invariably* throw human labour out of employment, must be admitted; and it has been maintained, by persons very competent to form an opinion on the subject, that they never produce that effect. The solution of this question depends on facts, which unfortunately have not yet been collected,' and he makes a powerful plea for further statistical information, which after the lapse of a century, one is still forced to echo.

Neither Chalmers, who believed in the doctrine of the Wage Fund, nor Senior, who did not, denied that the effect of machinery might be to increase unemployment. ' It is not the true vindication,' argues the former, ' that the making of the machines opens so great a source of employment, that the making and working of them together take up as many hands as did the making of the commodities without the machines; for, in this case, there would be no abridgment of labour, and no advantage to master-manufacturers in setting up the machinery. And it is not a sufficient vindication, that, when an article is cheapened by machinery the demand for it is so much enlarged, as still, in spite of the abridgment in labour, to require as many, if not more, labourers for its preparation as before: for this, though true of many, perhaps most trades, is not true of all.'[4] The true defence is that ' the fund, out of which wages come, is left unimpaired.' Senior's general position cannot be shortly described, but he does at least admit that when the demand is inelastic, employment declines, though this is to him the exception. Citing the case of a screw which ' in the manufacture of corkscrews, performed the work of fifty-nine men,' he argued that this example ' is as unfavourable to the effects of machinery as can be proposed: for the use of the commodity is supposed to be unable to keep up with the increased price of production, and the whole number of labourers employed on it is, consequently, diminished. This, however, is a very rare occurrence. The usual effect of an increase in the facility of providing a commodity is so to increase its consumption as to occasion the employment of more, not less, labour than before.'[5]

The classical school had thus, by the middle of the last century, resolved the problem into its constituent parts. Under what conditions will rationalisation involve unemployment in (*a*) a single industry (*b*) in all industries taken together? Or, is there some inherent ' principle of

[4] ***Pol. Economy, Appendix Note B on Machinery*, p. 56.**
[5] ***Political Economy in Ency. Metropolitana*, 1850, p. 166.**

human nature ' upon which reliance can be placed to solve the problem, after transitional effects have been overcome? Those were, and remain, the fundamental issues which have to be faced.

S. 5. Available figures do reveal impressive improvements in production in recent years, and gain added significance when placed in juxtaposition with figures relating to employment. An increase in *per capita* and aggregate output in a single industry accompanied by a decline in the number of workers engaged does not, of course, necessarily imply the existence of any unemployment at all, since an industry normally loses a certain percentage of its workers every year, and if the rate at which new entries take place is adjusted to the new technical conditions, the consequences of technical improvements can only be judged of indirectly. An increase of aggregate and *per capita* production over industry generally accompanied by growing or stable unemployment does, however, suggest that the rate of improvement is for the time being so great that over the range of industry covered, the chances of employment are diminishing: though unless the unemployment returns cover the whole, or a very significant part of the employable population, it may still be the case that, *indirectly*, the effects of rationalisation are being offset, in whole or in part, by an increase in the volume of employment in the occupations not recorded in the returns. And since production figures are biased by the choice of base years, the incidence of the trade cycle, changes in the demand for particular commodities and the like factors, even the co-existence of increasing aggregate and *per capita* output with increasing or stable unemployment is not by any means a completely valid test of the relationship between the elements in the problem.

The best advertised figures are undoubtedly those relating to the United States.[6] Put into their simplest form, the Census of Production figures show that between 1919 and 1927 (the last a year of comparative depression of trade), the number of workers in the four main divisions of American industry: viz., agriculture, mining, transport and manufacturing, declined by 7 per cent., quantitative output increased by 20 per cent., and output per worker by 30 per cent. approximately. The figures adduced by Mr. Woodlief Thomas carry the same implications with them: they relate to a comparison of the years 1918–20 as base with 1924–6.[7]

*Index at beginning of period*=100.

| | Workers | Output | Output per Worker |
|---|---|---|---|
| Agriculture 1924–6 .. .. | 95 | 112½ | 118 |
| Mining do. .. .. | 100 | 127 | 127 |
| Manufactures do. .. .. | 91½ | 128½ | 140½ |
| Railways do. .. .. | 91½ | 100 | 109 |
| Average do. .. .. | 93 | 120 | 129 |

[6] Some of the material cited below has already been made use of by me in an article: *Is America Prosperous?* Economica, No. 28, pp. 7–8.

[7] Woodlief Thomas—*The Economic Significance of the Increased Efficiency of American Industry* in *American Economic Review, Supplement*, 1928.

Accurate unemployment figures for the U.S.A. do not exist: estimates exist for 1928 which vary from 1.9 millions to 2.6 millions: one estimate for 1927 was 4 millions, whilst another authority gives an estimate which varies from $4\frac{1}{4}$ millions in 1921 to a minimum of 2 millions in 1927.[8]

A census of production does not exist in Germany. The revised index of production recently published by the Institut für Konjunkturforschung (Base 1928=100; comprising 31 weighted industrial groups) shows that production rose from a figure of 69.5 for 1924 to 101 in 1929. In the year of rationalisation, 1925, the index rose to 83.3, fell in the slump of 1926 to 79 and reached 100 in 1927. Whilst the maximum number of applications per 100 places available reached a peak at the beginning of 1926 (in the period 1924–9), and the employment situation is marked by great seasonal variations, nevertheless a competent German authority points out that in 1929 'the rise in unemployment as compared with the previous year, practically corresponded to the increase in the number of available workers caused by the age distribution of the population. In 1929 it was thus no longer possible for industry to absorb this increment.'[9] Some interesting figures are cited by the same authority, illustrative of the growth of efficiency in particular industries. In the Ruhr Coal industry, for instance, the total number of employees declined by 10 per cent. between 1913 and 1928, whilst the output per employee rose by 26 per cent. In 1929, production per employee rose another 9 per cent. up to June, whilst employment fell another two per cent., though the monthly figures are clearly affected by seasonal changes. In another industry directly competitive with British industry, the facts are even more striking: 'The index of labour efficiency in the German machine industry, using the first quarter of 1925 as a basis, averaged 142 per cent. for 1929, as compared with 133 per cent. for 1928, 142 per cent. for 1927 and 126 per cent. for 1926.'[10]

Even in the case of Great Britain, which is generally regarded as having lagged somewhat behind in the Rationalisation movement, more than one piece of evidence is available which suggests an increasing productivity as one of the immediate causes of unemployment. Quite apart from the recent speeches of industrial leaders at Company meetings representing such diverse products as cement, transport and rubber tyres, the production index of the London and Cambridge Economic Service when placed in juxtaposition with the employment figures, reveals a far more rapid growth in the former than in the latter. Thus between 1924 and 1929 the Combined Index of Production rose from 100 to 116.2: the employed population over the same period increased from some 9,500,000 to 10,020,000 persons, or some 7 per cent., whilst unemployment was greater by nearly 4 per cent. No doubt the position in Great Britain is extraordinarily difficult to weigh, since world factors of any sort unfavourable to trade and industry are likely to affect this country to a greater degree than more sheltered areas. Nevertheless, the figures do suggest a growing

[8] *Recent Economic Changes in the U.S.*, 2 vols. 1929, *v.* Vol. II, pp. 469–78.

[9] *Germany's Economic Development during the Second Half of the Year* 1929, published by the *Reichs-Kredit-Gesellschaft*, 1930.

[10] *Germany's Economic Development*, &c., p. 14.

divergence between the movement of production and the movement of employment.

S. 6. Whilst the foregoing analysis may be sufficient to establish a presumption that in recent years the process of rationalisation has been responsible for the creation of part of the existing volume of unemployment, in the end one is forced back upon general economic reasoning. Three general sets of circumstances have to be examined: the motive of rationalisation, the circumstances under which rationalisation takes place, and the methods of rationalisation actually adopted.

(1) The first point is simple. The motive of rationalisation is in all cases to reduce costs from the standpoint of the capitalistic producer: it is not the reduction of 'real' cost or 'social' cost. It may very well be the case that a process which reduces pecuniary costs from the capitalistic point of view also reduces 'real' cost: a new technique may involve less actual psychic strain to the worker employed. On the other hand, standardisation may involve elements of social loss: a lowering of the standard of skill or reduction of the creative and æsthetic element in work. It follows from this that whenever wage costs per unit of output form a substantial element in the price of the product per unit before rationalisation, it will pay the producer to reduce that cost, if necessary, by the displacement of labour by mechanical instruments. It does *not* follow that unemployment *must* ensue, since we have still to take account of demand for the product, and of the indirect effects of the economies introduced. But unemployment *may* follow. And from this point of view it is important not to overlook the circumstance that the attractiveness of reducing wage costs per unit of output is not an absolute magnitude: it is a function of the wage cost itself and of the economies to be realised by alternative processes. Now it is at least significant that at the present time the rigidity of wage rates is a striking element of the economic situation in this country: all other prices are falling but the price of labour is not. The same is true of Germany: at least as regards unskilled labour. In 1929 'weekly wages on standard time schedules' of unskilled labour were between 75 per cent. and 80 per cent. above 1913: the cost of living was only about 55 per cent. above the pre-war level. In the United States, the check to immigration has given labour something like a quasi-monopoly. Under these circumstances, to economise labour as much as possible represents merely ordinary business prudence.

(2) The effect of rationalisation upon the chances of employment obviously differs when the striving after economy is the result of a period of intense demand for goods and services of all kinds, or when the striving after economy represents an attempt to meet the exigencies of falling prices, or an unfavourable economic situation generally. The war period represents the first alternative, the present moment the second. During the War, rationalisation was forced on because there was an insatiable demand for goods at a time when a large proportion of the able-bodied workers of the country were absorbed by the Army. At the present moment, when industry is suffering from a contraction of the market and when, on other grounds, there is already a surplus of labour available, the position is obviously different. Again, we are not entitled to assume that unemployment must ensue, for we must again deal with the demand side

before we arrive at a final conclusion, but in general it is at least clear that unemployment is *more likely* to ensue from rationalisation than was the case during the War.

(3) Lastly, as regards the *methods* of rationalisation. Here, of course, the task of analysis is complicated by the fact that a large variety of rationalisation methods can be distinguished, the effects of which on the employment situation (even without taking demand conditions into account) may be very different.

(*a*) So far as so-called 'Financial Rationalisation' is concerned: that is, the writing down of book values and the consequential cleaning up of the balance-sheet position, there is obviously no direct connection with the problem of employment at all.

(*b*) But financial rationalisation, when it means—as it increasingly does—a greater degree of integration of enterprises, does affect the employment situation directly.

When integration involves concentration of particular types of output at different works, then, in so far as different degrees and kinds of skill are involved, a problem of mobility at once arises, for grades and types of labour formerly required at more than one point are now required, perhaps, at only one point. The greater the difficulty of getting labour to move, the greater the chances that the further consequences of concentration—improved processes, eliminating the kind of labour which is difficult to obtain by substitution of another kind, or the replacement of labour by machinery—may throw a particular kind of labourer out of work altogether. At the very best one is then left with the problem of reabsorption in another direction.

(*c*) Standardisation of types, whether occurring as a result of concentration of output at certain points within an integrated group forming part of a wider industry, or whether occurring as a result of deliberate agreement by all the producers within an industry, has also a direct bearing on the labour situation, in so far as repetition work in and of itself encourages the further use of machinery and the substitution of skilled by unskilled labour.

(*d*) Lastly, we are left with certain rationalisation methods which have as their object not the direct cheapening of the product, but control over the market, through common sales-organisations of one kind or another. Their effect on the employment situation obviously turns on the price and sales policy adopted: and they thus involve the question of demand, to which we must now turn.

In considering the relations between rationalisation, the market and unemployment, there is one obvious point which tends to be lost sight of in popular discussion. The degree of rationalisation which 'pays' is not an absolute magnitude, but depends on the 'shape of the demand curve.' Thus a complication is introduced through the circumstance that the point of optimum economy in production may involve a volume of output which, *if it is to be sold*, reduces the aggregate return below the maximum attainable if a smaller volume had been produced and marketed. In such a case—which cannot always be foreseen in advance—and given the absence of effective competition, the economies in labour cost may be eaten up by a rise in the overhead cost, and if there has been a reduction in the volume of labour directly employed, there are not necessarily any resources available by which that labour can be indirectly absorbed. The consumer pays the price which brings the maximum aggregate return. Unless this price is lower than the price previously ruling, he cannot increase his expenditure in other directions. The price *need* not be lower, because, though *prime* cost may be lower, *supplementary* cost, for the volume actually sold, may be so much higher as to lead to no general lowering of cost at all. In other words, rationalisation undertaken on technological grounds without taking into account demand conditions may not increase the aggregate national dividend and so may create an unemployment problem which it cannot solve. And we have no right to assume that the race of rationalisers never makes a mistake.

Returning now to the general problem, we necessarily employ concepts which are familiar to all students of economics.

(*a*) If the demand for an article has an elasticity greater than unity, a reduction in its price results in a more than proportionate increase in the quantity demanded. Thus, even in a rationalised industry, in which labour cost has been reduced, the greater the elasticity, the greater the derived demand for labour, and the greater, therefore, the opportunity for reabsorbing labour and of adding to the total quantity of labour employed. But *how much* labour will be needed, depends not only on the state of demand, but on the technical conditions in that industry.

(*b*) Even if the derived demand for labour *in this industry* has an elasticity of less than unity, yet provided that the demand for the product of the industry has an elasticity greater than unity, the *indirect* derived demand for labour may have an elasticity greater than unity. For the machinery and other equipment used by the industry has itself to be created by means of labour, and, if the output of the industry is expanding, it requires an expanding plant. Thus, the increased demand for labour in equipment industries which marks the first stages of a rationalisation movement calling for large quantities of new equipment, may continue *after* the first stages have been passed. But too much must not be based upon this. For if rationalisation is a continuous process, it will affect not only the industries supplying consumers' goods in the narrower sense, but also the industries subserving these industries.

Optimistic interpretations of the rationalisation process will generally be found to be based upon the assumption that what is true in some cases is necessarily true in all. The demand for certain popular luxuries is no doubt highly elastic, but it is equally clear that the demand for agricultural products, for example, is not. There is no reason whatever to suppose, therefore, that an all-round cheapening of products already available will necessarily absorb *all* the labour unemployed in consequence of technical changes, though no doubt that will be the case to some extent. But to what extent is unfortunately unknown.

S. 7. Nevertheless, is it not legitimate to argue with Babbage in the passage already cited that 'as each new machine adds to these gratifications, new luxuries will open to his view'? or with Professor Robbins,[11]

[11] *Economic Effects of Hours of Labour*, by Prof. L. Robbins (Economic Journal, March 1929, p. 25).

that the elasticity of demand for labour in general is greater than unity? Since rationalisation reduces the quantity of labour required for the production of the existing quantum of material welfare, in other words, will it not be possible to add to that volume of material welfare? Or must we argue with Mr. G. D. H. Cole that rationalisation 'might succeed in lowering substantially the cost of producing each unit of the national output: but it would only find itself unable to make use of the great new productive power of which it had become the master. For the problem of production cannot be solved unless the problem of distribution is solved with it; and the lowering of the unit cost of production, unaccompanied by a pouring of fresh purchasing power into the pockets of the consumers, will only mean a more determined policy of restricting output and a widening circle of unemployment'?[12]

But, *in the absence of falling prices due to monetary causes taking place coincidentally* (which, as we have already had occasion to point out, intensifies the employment problem) *rationalisation involves an increase of the monetary purchasing power in the hands of the consumer.* So long as money incomes in general remain the same, the margin between money incomes and expenditure goes up, in those cases in which the elasticity of demand for products of the rationalised industries is less than unity: or the cheapening of the articles results in a larger aggregate consumption of them, or, if the articles in question are subject to quasi-monopolistic conditions, the same amount is spent on them as before, but profits in the industries producing them increase, and larger profits mean additional purchasing power in the hands of entrepreneurs. The problem as stated by Mr. G. D. H. Cole is not the real problem at all: the real problem is: what use will 'consumers' make of the margin of purchasing power now available as a consequence of rationalisation?

If the answer to this question is that consumers will devote it to the satisfaction of new wants, then it will be true that in the long run rationalisation will not involve unemployment. But the run may be a very long one: not only because a transfer problem is involved, but because the newer industries themselves will not in all probability require as much labour as they might have, had not the whole atmosphere of industry been impregnated with the rationalisation spirit. From this point of view, an increase in the demand for those personal services which are least affected by the progress of mechanical improvement will help to solve the problem more easily than a demand for goods the production of which requires the direct application of labour to a smaller extent. The growth of the 'service industries' in the United States has been expressly adverted to by the very able group of American economists who last year published their Survey of the developments of the last decade in the United States.[13]

But consumers *need* not devote their available resources to the satisfaction of new wants. They may decide to 'hoard' their savings in the technical sense described by Mr. D. H. Robertson in his *Banking Policy and the Price Level*: or, in other words, they may desire to keep more of

[12] *The Next Ten Years, &c.*, p. 116.
[13] *Recent Economic Changes*, Vol. I, p. xvi.

their resources in a liquid form. If this hoarding takes place on a large scale a cumulative pressure is exerted on the price level, and the difficulty of absorbing labour is *ipso facto* increased. In the ceaseless combat waged in the human mind between the desire for greater gratification on the one hand and the desire for greater security in the shape of holding free resources on the other, it is not at all times true that it is the former passion which gains the upper hand. At the present moment it would appear as if the desire to abstain from additional consumption were more important than the critics of current standards of consumption would be prepared to admit.

However that may be, the problem of transfer that is in any case involved is one of sufficient difficulty. Contrary to general opinion, even in countries like the United States, with a high degree of labour mobility, transfer may involve not only considerable loss to the individual but also considerable delay in point of time: as appears from an interesting piece of evidence presented by the Brookings Institute of Economics to the U.S. Senate Committee on Education and Labour in the course of their investigation of Unemployment in the United States in 1928–9.[14]

S. 8. We have now arrived at the point at which it is necessary to apply the foregoing analysis in a more directly practical manner.

(1) Since the rationalisation movement is international in character, and, since it undoubtedly results in most cases in a reduction of cost per

[14] Summary of Testimony and Report of Institute of Economics of the Brookings Institution by Isidor Lubin, documented, p. 500–1 :—

'An investigation recently made by the Institute of Economics of the Brookings Institution reveals that most of the displaced workers have great difficulty in finding new lines of employment once they are discharged. A survey of some 800 workers in three industrial centres revealed that the newer industries are not absorbing the jobless as fast as is usually believed.

'Almost one-half of the workers who were known to have been discharged by certain firms because of curtailment in employment during the year preceding were still without jobs when interviewed by Institute of Economics investigators. Of those still unemployed over 8 per cent. had been out of work for a year, and about one-half had been idle for more than three months. Among those who had succeeded in finding work, some had had to search for jobs for over a year before finally being placed. More than one-half of those who had found jobs had been in enforced idleness for more than three months before finding employment. Only 10 per cent. had been successful in finding new jobs within a month after discharge.

'The new jobs, moreover, were usually secured at a sacrifice in earnings. Some workers, to be sure, were fortunate enough to find employment which paid higher wages, as was made evident by the fact that about one-fifth of them were making more money on their new jobs than before discharge. Forty-eight per cent, however, were receiving lower wages and about one-third were earning just about the same amount as they formerly did.

'And what kind of jobs did these men finally secure ? Trained clothing cutters with years of experience had become gasoline station attendants, watchmen in warehouses, timekeepers in steel plants, and clerks in meat markets. Rotary press operators were pressing clothes in tailor shops. Machinists were selling hosiery for mail-order houses. Welding machine operators were mixing salves for patent medicine manufacturers. A significant number of men admitted frankly that after some months of enforced loafing they had taken to bootlegging.

'It is evident that a large number of the workers now being displaced from industry are being forced into unskilled trades at a sacrifice in earnings and a consequent lowering of their standards of living. At the same time they are being made to bear the burden of unemployment, for which they are in no way responsible and over which they have no control.'

unit of output, no single country engaged in international trade under competitive conditions can hope to contract out of its consequences, good and bad, except at the expense of its international trade. This is in itself a sufficient reason for pushing ahead with rationalisation in this country.

(2) In the short run, rationalisation is not a remedy for unemployment, but, on the contrary, is itself a factor in making for unemployment, except to the extent that it stimulates demand in the constructional and equipment industries. But since a loss of markets due to progressive reductions in prices by rationalised industries in other countries also adds to the volume of unemployment in this country, the short run evil of unemployment in this country changes in character, rather than grows in volume. Industries are in part depressed because local costs of production are too high and unemployment ensues. Rationalisation reduces costs, but, until the lower costs have helped the industries in question to regain their market, *and expand it*, unemployment will remain. But unemployment resulting from rationalisation is a lesser evil than unemployment resulting from relative inefficiency.

(3) In the long run, since rationalisation effects a lowering of real costs, then, given a desire for a rising standard of life, there is no reason to suppose that the volume of unemployment will not again fall. But, in the absence of any definite knowledge of the elasticities of demand for different products, we cannot foretell in what directions an increased demand for labour will manifest itself. Both American and British experience would seem to show that the demand for labour in the existing body of industries is likely to shrink, absolutely in relation to the population, relatively in relation to the output: whilst every increase in the technical knowledge available to industry will make the demand for labour in relation to output smaller in the new industries, the rise of which we have every reason to suppose (to judge from past experience) will accompany reductions in real costs in the existing industries. Thus, the occupied population in the future is likely to be less 'industrialised' than in the immediate past: and the growth of trades and occupations outside the narrow concept of 'industry' will continue as rationalisation proceeds.

(4) The most optimistic view of the situation must take into account the fact that a grave transfer problem is involved, and that monetary and other circumstances having nothing directly to do with the rationalisation problem may accentuate the difficulties of transition. The first and most obvious step in the direction of ameliorative measures must therefore be an increase in the mobility of the working population.

(5) In estimating the probable duration of unemployment resulting from rationalisation, account has to be taken, not only of the state of technical knowledge, but of the movement of the population. Since rationalisation produces its most striking results when the aggregate demand for a product continually increases, a stationary population (and the most advanced nations are tending to stationariness of population), places a limit to the expansion of output in each of the several directions in which the economies of large scale production are most strikingly displayed. At the same time, the decline in the number of new entrants into industry, which is to be expected over the next few years, will diminish the immediate pressure. But it is quite possible that the normal level of

unemployment will be higher in the future than in the past: in which case, unemployment will cease to serve as an index of material well-being. The paradox of a rising standard of life with a higher level of unemployment may well be the result of the present tendency in industry.

(6) On the other hand, there are not wanting examples to show that demands for new products and services can be stimulated very quickly, provided they are sufficiently cheap: and there is therefore no reason to fear that we shall all 'starve in the midst of plenty.' What has been true of the motor cycle, the motor-car, the gramophone, the radio, artificial silk, the cinema, the popular press, books, travel facilities, greyhound racing and the rest will surely also be true of the future. No doubt we shall have to give up the belief that 'national power' is to be measured by a high percentage of occupied persons in a few 'staple industries': but just as we no longer think of measuring our 'national power' in terms of agricultural output, so we shall gradually see that those countries which have the highest standards of life *ought* to be those employing the largest proportion of their populations in the supply of 'luxuries.' All that stands in the way are economic and ethical standards no longer appropriate to the tendencies at work.

(7) There is an obvious relationship between the progress of rationalisation on the one hand, and the possibilities of a shorter working day and higher earnings from labour on the other. The rise in the standard of living and the shorter working hours which have characterised the progress of industry in the last hundred years were both conditioned by increased productivity: though it may be true that unless Labour's demands had been made, the spur to further invention and discovery might have been in part lacking. But at any given moment a balance must be struck between the demand for higher earnings and the demand for more leisure, for, if both demands are pushed to such an extreme that, if either were granted, the whole benefit of increased productive power would be exhausted, then the grant of the one excludes the grant of the other: and if *both* were, under these conditions, *simultaneously* asked for and granted, a new disequilibrium of costs and prices would be set up, which would inevitably cause a new wave of unemployment until further advances in technique and organisation had been achieved.

A shorter working day and higher wage rates are, of course, frequently defended, not on the legitimate ground that society can afford them with increasing productive powers, but on the ground that they are *direct* means for reducing unemployment, because they 'spread work' and stabilise 'working class purchasing power.' Unless accompanied by increasing productivity, however, they are incapable of achieving these results: for a shorter working day without a larger output would either involve lower wages or rising costs per unit: and rising money wages without increasing productivity would also result in disequilibrium. But given increasing productive powers, it is possible to lower prices to the consumer and pay the same wages as before for a shorter day or, with the same lower cost to the consumer, pay a higher wage for the same working day. Growing productivity, in fact, gives society a margin to 'play with,' and this margin is the source out of which unemployment can be relieved. But we have no right to assume that the process works without friction or that the fears of the workers are based entirely on 'prejudice and error.' In the end, one must rest one's hopes on the known elasticity and responsiveness of capitalistic society: an organisation which was capable of surviving the shocks of the war and post-war period is hardly likely to perish because it is learning to turn the arts of production to still better use in the future than it did during the last one hundred and fifty years.

SECTION G.—ENGINEERING.

# THE INTERDEPENDENCE OF SCIENCE AND ENGINEERING, WITH SOME EXAMPLES.

ADDRESS BY

SIR ERNEST MOIR, BART., M.INST.C.E., M.AM.SOC.C.E.,

PRESIDENT OF THE SECTION.

## PRELIMINARY STATEMENT.

As President of Section G I feel greatly honoured in addressing you after the many able men who have done so in the past. Many of my predecessors I have known personally, and it is with hesitation I address you now, in view of my knowledge of the giants they were in their various walks of life. To name a few, Sir John Fowler, with whom, when little more than a boy of twenty, I frequently walked under the shadow of that great structure, the Forth Bridge, created in the mind of himself and Sir Benjamin Baker, whom I met more frequently and in closer friendship while I was in charge of the erection of his masterpiece. When I was twenty-two, I was placed by William Arrol in charge of the erection of the first portion of the Forth Bridge on its present site. Though Sir William Arrol never figured as your President, he was a man of fearless action and great foresight. He never doubted that any difficulty arising would be overcome, and he was never fearful of results and of his powers to succeed. John Fowler gave to his partner, Benjamin Baker, full credit for his great achievement in the design of the Forth Bridge, that was and is still his outstanding work, though many triumphs awaited him in later years. The great and lovable Frederick Bramwell, though not technically so supreme a master, his mind being rather of the judicial character, as is shown in some of his arbitration awards, was another of those who have given you masterly addresses. Yet another is Sir John Wolfe Barry, with his broad mind and outlook, giving confidence to all who approached him, always showing a sense of fair play in his judgments between the companies whom he faithfully served, but never allowing them to influence his determinations between themselves and the contractors when they differed about their respective responsibilities for happenings entailing payments. This characteristic enabled him to get the work he designed carried out more cheaply, since risks were less onerous under him than under many others practising during his active career. Next, Sir Douglas and Sir Francis Fox were a pair of brothers who left their mark on big undertakings, and Douglas was among those who have addressed you metaphorically from this rostrum. Alexander Kennedy is now only a memory, but what a memory and example he was to us all, and what a delightful and varied character he had! I had the great good fortune to study under him and under Vernon Harcourt, another great personality, for whom I acted as demonstrator in his surveying class at University College, London. His work was mostly to collect and collate in his books the works of others on harbours and docks—very useful work to those who come after. Alexander Binnie has also given you a masterly address on the ancient scientific lore of the Roman Lucretius, who appears to have known so much more about atomic theories and the basis of matter than was known for many generations after his time. Among those still living there are many who have done honour to the judgment of this Association and whose memory and achievements will leave their mark, and whose names will resound down the centuries. Prominent among these are Sir Charles Parsons, of turbine fame, and Sir Dugald Clark. To follow after such men and do justice in your judgment to the theme of the application of science to engineering will be a great satisfaction to me, if I can fulfil the requirements, but it is with some misgiving, I repeat, that I attempt this task. It will be for you to judge whether I have succeeded when I have finished, and I hope you will bear with me in shortcomings if I do not live up to the high standard set by the men I have mentioned.

It has been my privilege and good fortune to have worked with and for many of those who have presided over Section G on some of the largest works during the last forty-eight years. You may gather from this that I began very young. Dr. Kirk was one of my early chiefs, the inventor of the block model for determining the resistance of ships. He also was the designer of the first triple expansion engine that propelled a ship to the Antipodes on a fuel consumption of 1.6 lb. of coal per h.p. hour, and as an apprentice engineer I worked upon her engines, both in the pattern and fitting shops. Here was an example of physical and mechanical science applied to engineering in a very special way, though the scientist may have lagged behind the engineer in this case. During my apprenticeship in Glasgow, I got in touch with Lord Kelvin, or Sir William Thomson, as he was then. I conceived an idea for doing away with the piston rod in steam engines, much as is done to-day in the internal combustion engine as we all know it in the motor car, while at the same time closing the lower end of the cylinder so that a two-stroke scheme could be employed without having to put pressure in the crank casing. I made a rough model and took it up one evening, after my ten-hour day was finished, to show my idea to the famous man. I was told by the butler that Sir William had a dinner party, so I hastily said I would call some other time. The butler insisted, however, that Sir William would be annoyed if he did not make me come in, so in spite of my protests I was ushered into the great scientist's study, and Sir William came in, having left his guests, and went through my idea critically for nearly an hour. I mention this to show what a great man Lord Kelvin was, quite apart from his inventive and scientific attainments, and kindly actions such as this no doubt encouraged every one who came in contact with him. What his dinner guests thought about it I do not know, but he did not seem to mind.

Some branches of engineering are more dependent on exact science than are others, but all must admit dependence on all or nearly all branches of science. I might say, on the other hand, that science in many of its branches has been helped, and urged forward to further investigations by the engineer stumbling on ground which science had not till then investigated, in the particular manner in which engineering had need. By collating results and making them applicable to the future needs of engineering, the scientific worker has on many occasions determined doubtful points and made the future use of discoveries and inventions by engineers and their allied workers possible. Thus, engineering practice and engineering inventive imagination and pure science have reacted on one another to the advancement of exactly applied knowledge. Simple facts and discoveries by scientific research workers have frequently not revealed their usefulness until those who apply them to the needs of men have tested them and shown their utility, and then the scientific investigator has made further advances, and indicated further possibilities in the use of the discoveries made either by the laboratory research worker or the engineer. My own experience, when dealing with practical civil engineering problems, has indicated some examples, and many doubtless exist unknown to me. The observation of natural phenomena has played a very important part in the advancement of science as applied to industry and to engineering problems of all kinds. The constructing engineer has not the time to investigate exactly the forces of nature with which he has to contend, and he has frequently to cover up his ignorance by the use of 'factors of safety' in making his designs, often using larger factors than are called for, and at other times not providing the necessary margins he thought he had allowed for. This is notably the case in connection with bridge designs and stresses arising out of the fatigue of metals and the hammer action of moving loads in bridge floor systems, and the effect of plunging rolling stock thereon. Investigations have been carried out recently by experiments on a large scale with actual locomotives, the results of which have been now collated by Prof. Inglis of Cambridge, into some useful formulæ, from which computations of possible stresses may be foretold. Some types of locomotives have been so bad in this respect that but for our old friend, the factor of safety, the structures of many bridge floor systems would long since have been destroyed by percussion and repeated hammer blows dealt upon them. It was long recognised by engineers that the fatigue of metals under stress arising out of (1) the range of stress, and (2) its reversal, demanded lower maximum stresses to be applied, and the scientist has now so investigated the facts that positive data are available and results can be forecast with accuracy and certainty. Newton did not evolve the value of 'g' out of his own mind, but by observation its value was ultimately determined to the benefit of all who use physical science. I do not know how we should get along without 'g' now, though apples fell long before it was discovered, and it would be difficult to think how the influence of the planets upon our tides, so important to many branches of civil engineering work, could have been foretold but for the practical discoveries of Newton and their ultimate scientific consolidation into mathematical formulæ. There is still a great deal to be discovered in connection with the action of tides and waves and their destructive effect on sea works, many of which fail undoubtedly from the lack of exact knowledge of such action and their reaction on the sea shores where harbours must be built. Just recently we have had an example of the carrying away of years of work at Antofagasta Harbour, Chile, by a series of tidal waves. Yet there is no existing formula extant which would have enabled the civil engineer to forecast this appalling disaster. The influence of sea action on structures built up of units of mass and at varying depths of water is not at the moment governed by precise knowledge, and the forms of breakwaters vary the world over. There ought surely to be some means of collating data for the guidance of civil engineers in the design of marine works, which would save them from having to rely alone on the failures or successes of those who have gone before.

## Voids.

The influence of air and water-filled voids and the effect they have on many matters that affect engineering practice is not, I think, fully realised.

Their bearing and their influence on design include within their range such widely different subjects as the solidity of breakwaters, their cost and their resistance to sea action; the combustion of fuels, both solid and liquid, and especially their rapidity of burning, ranging from explosions, in the case of propellants and blasting powders, to the corrosion of metals by oxidation. The minuteness of particles and the voids which surround them turn some innocent substances, such as coal-dust and flour-mill-dust, into dangerous explosives, and this is due to the intimate association of the air and its contained oxygen with the minute sub-division of their mass when they are powdered by grinding. The economy of fuel, both liquid and solid, when burning, is greatly enhanced by its fine sub-division, especially if provision for its intimate association with the atmosphere is arranged for. Explosion, as compared with combustion, is only a question of rapidity of combustion, the other extreme being the slow deterioration of metals due to oxidation as exemplified in the rusting of steel and iron structures. These examples show what a wide range there is in the phenomena that are in some measure attributable to the voids surrounding the particles of materials and their more or less intimate association with air.

I propose to give some examples later which have occurred in my own experience, and which emphasise the question of the importance played by voids in engineering work.

Another aspect of this subject is exemplified in capillary attraction, or the capacity of certain mixtures of minute particles either in a loose form or in the form of porous solids, which have the power of defying the action of gravity by raising water. In some cases I have known water raised several feet, which seems to me to indicate the existence of a power that so far does not seem to me to have been adequately defined and has some analogy, I imagine, to the diffusion of gases. This power, possessed by some materials, which do not dissolve in water, of acting in an unexpected way on contained fluids, when such fluids are free to move by gravity to lower levels, is due to something not to my knowledge fully explained. I imagine capillary attraction may be analogous to the flow of sap in growing vegetation. It might be due, though I have never heard it suggested, to changes of temperature that might produce this movement of vegetable fluids contrary to gravity. It is certainly not so in very fine sands formed of decomposed granites or in the porous structures of bricks and some rocks. I have known water rise up within a glass tube $1\frac{1}{2}$ inches in diameter filled with fine sand, composed of decomposed granite (the lower end of which was immersed in a dish containing $\frac{1}{2}$ inch of water) 6 or 7 feet in a few weeks. This, at first glance, might seem of little moment from a practical engineering point of view, but when subsoils having this characteristic have to be drained, so that trenches may be safely sunk therein, the effect of the included water on materials is a serious matter in the construction of engineering works. The lubricating effect set up by the water produces quicksands out of the very fine particles which, when dry, are relatively easy to handle by ordinary trenching methods. The included water makes a semi-fluid mass which becomes extraordinarily difficult to deal with in works involving excavation because the subsoil refuses to give up the water it contains, and will flow through slight crevices and under driven piling sunk into it many feet.

In connection with mixtures of concrete, voids play a very important part in the creation of what is practically artificial conglomerate. Further, the minimum size and the variety of sizes forming the inert parts of concrete mixtures make a great difference to the structure arising out of a mixture of sand and stone (commonly called 'the aggregate') with a varying amount of cement. It is well known that the size of the cement particles themselves make a difference to the necessary hydration, which reunites them after their divorcement by heat and subsequent grinding to a very fine powder. In the case of airtight or watertight concrete it is a matter of great importance that all interstices be filled with cement when it has become hydrated. It forms the adhesive matrix between the other materials, which economise by dilution and form the greater part of the mass.

It is well known that an infinite number of different sized perfect spheres would ultimately create a mass with no voids at all, if their variety of size be carried to an impossible extreme. In many structures it is an ideal condition of things to get as near as may be practicable to this state of affairs when, as for example, an impervious concrete for dams and tanks is desired.

Doubtless many of you know what is commonly called the sand lime brick largely made in Switzerland from glacial moraine in the rivers, where stones of many different categories and sizes are brought down by the melting ice-fed rivers into the valleys. I have seen excellent bricks made in the Aar Valley out of a combination of a large number of different sizes down to minute particles of crushed stone in which the remaining voids were so small that only $2\frac{1}{2}$ per cent. of very finely ground lime to form the matrix was all that was needed to make what was practically an artificial solid stone. I have never heard of any other sand lime brick with such a minute amount of lime matrix producing this effect. All this points to the necessity of a minute analysis of sizes and shapes of particles to get the most economic use of lime or Portland cement in the making of a mass of artificial stone. Cement concrete differs only from masonry on general broad lines. Masonry, in the form of concrete, can be put in with the shovel, whereas hewn block masonry may involve lifting appliances and, in any event, very careful handling and setting to produce the desired results. It is, for example, much more difficult to make water-tight masonry than water-tight concrete, because it is more difficult to thoroughly fill the voids in the masonry joints with a trowel or by bedding the stones in mortar than by mixing the ingredients as in concrete.

Further, in earthen dams and in earthen railway embankments the question of settlement is very largely bound up with the amount of voids left in the material when it is tipped.

During my work in the United States for the British Government during the war—1915-16—I had an interesting experience arising out of the testing of time fuses and the effects of voids thereon. The firm which had made many thousands of this particular fuse—it was called a No. 185 time fuse—had been very accurate in their manufacture up to a point; the timing had been very regular and the percentages of error on proving at the range very small. Suddenly, however, the proving guns told another story, and the timing of the bursting of the shrapnel was distinctly bad. I visited the factory where these fuses were made and went minutely into the possible causes of the change. As to the chemical analysis of the powder, which was compressed into the time-burning rings, there was no change indicated in this analysis and no change in manufacture or in the pressing of it into the time ring. For a time we were all at a loss to account for what was happening. I remembered, however, that British Navy cordite made in England had holes cast through the blocks which go to form propellants for big gun charges. These were carefully proportioned to admit of association of the air surrounding such cast blocks (by perforation) in order to regulate the speed of combustion. I thought there must be something, therefore, changed in the volume of the voids and of the contained air arising out of the handling of the powder when in transit to the factory. I asked the head of the firm whether he had recently changed the method of transport. He said, 'Yes, we have. We have been making an addition to our factory, and while this has been going on we have had to bring the powder in by motor lorries over temporary roads instead of by railway wagon.' 'Well,' I said, 'I think this is the secret of the errors in the timing of the fuses you have made, for the powder has evidently been brought in over the rough roads made of tree trunks (called "corduroy" roads in America), and has been more shaken than it had been when delivered by rail; the voids between the particles have been made less in consequence, and the included air is therefore less in amount, and contact with the particles of powder and the rate of burning is consequently different. This, then, is the possible cause of the burning in the time rings being irregular.'

To make a long story short, this was investigated and found to be the case, and when this rough motor transport was eliminated the same powder performed its proper function just as exactly as it had done before this temporary method of transport was introduced and without any change in composition or treatment in the manufacture of the fuses.

The breakwater I have just been constructing in Valparaiso Harbour, Chile, is founded in 187 feet of water upon a sandbank. This bank is deposited by suction dredgers from neighbouring foreshores within ten miles. It has spread out at its base to over a quarter of a mile in width, and the sand, in its passing through this great depth of water, has so consolidated itself as it falls upon the bed of the Pacific Ocean, and has so filtered out all its lighter particles on its way, that its consolidation has become very dense ; so much so that the fluke of an anchor let fall upon it does not penetrate into its surface. This sandbank has been brought up to a depth below water of 63 feet and graded quarried rock is spread upon its top. Firstly, what is practically quarry rubbish of variable sizes weighing about 2 tons per $m^3$, from which has been picked the larger rubble, is spread on the top of the sandbank ; then upon this quarry rubbish selected rock gradually increasing in dimensions as it gets nearer low water (which weighs about 1·7 tons per $m^3$) is placed. The quarry rubbish (or what is known in Chile as 'desmontes') is brought up to 66 feet below low water level, and thereon the larger categories of rock of 2 cwt. to $1\frac{1}{4}$ tons and from $1\frac{1}{4}$ tons to 10 tons in weight to 39 ft. 6 in. below low water are placed. This upper layer, containing the larger categories, is deposited in excess to allow for settlement. It is levelled by divers, and upon this surface are placed blocks weighing 60 tons upon a slope of about 70° to the horizontal and at right angles to the axis of the breakwater, interlocked on the inclined face throughout their depth, so that they can slide down and take up any settlements in the bank on which they rest. Before the 60-ton blocks are placed a period of one year is allowed to elapse, so that the whole bank can settle. Notwithstanding this period of one year the great weight of the superstructure does cause further settlement, extending over some months, resulting in a final settlement of about 3 feet.

None of the different categories of material appear to have been moved by the very heavy storms, locally known as 'northers' on the western coast of South America, neither have any tidal waves disturbed or earthquakes moved, as far as we can determine, this enormous bank to any serious extent. There is evidence of two earthquakes having occurred before the breakwater was completed which caused some relatively small disturbance of the sandbank. The rubble was deposited in its various categories by hopper barges, and in its passage through the water there is little doubt that anything of an earthy nature that might have adhered to the rock was washed off it. This has, no doubt, contributed to its solidity.

Valparaiso is the first large breakwater in which a sand foundation has been adopted to so large an extent, and there seems to be no doubt that in suitable situations, and in a sufficient depth of water, smaller material can be used for marine structures than has hitherto been employed. In shoal water, or near the low-water level, or where currents exist, sand should not be used. Small material may be satisfactory, however, at great depths, as evidenced in Valparaiso. Masses of sixty tons are needed on the Pacific Coast where, as on any coast, the vertical motion of the waves is converted by the shoaling bottom into a horizontal one. If local rock strata will not produce quarried rubble of sufficiently large categories, which is the case with a great many of the rocky formations on the Pacific Coast (due, no doubt, to earthquakes, which are numerous) artificial concrete blocks are essential to produce the necessary masses.

Experience at Dover Harbour, and also at Valparaiso, indicates that below a level of forty-five feet, even in structures composed of practically vertical sides with the heavy seas beating thereon, nearly at right angles—there seems little chance of disturbance of relatively small stones.

It should be understood, however, in this connection that thought must be given to possible currents arising from any cause whatever. It is well known that stones of considerable dimensions are carried along our London sewers if a velocity of a few feet per second arises.

Only one small movement of the 60-ton blocks in Valparaiso breakwater occurred in an exceptionally heavy storm, and that, I think, was attributable to an interesting and temporary modification of the top courses to assist construction. The movement was very small and, it adds to the interest to note, it was not on the sea side but on the sheltered side of the breakwater. It was attributable, I think, to the fact that a temporary longitudinal depression was left in the centre of the structure for construction purposes. The pier top has a total width of 45 ft. 6 in., and only the outer rows of blocks in the top course were placed. Thus, a longitudinal channel protected from the sea in which to transmit the blocks, for construction purposes, to the crane at the outer end of the work was provided. This depression formed a canal when a heavy swell existed and was filled by the sea. None of the joints in the breakwater blocks are cemented or filled with grout, therefore voids existed between them, into which columns of water flowed. Since water is incompressible, the falling masses of green sea on the surface of this filled canal acted like a hydraulic ram which transmitted vertical blows from the falling masses of fluid into horizontal pressure between the blocks. The blows moved the top courses of blocks towards the inside by a few inches but, fortunately, not sufficiently to require extensive demolition and replacement to put matters right. Had the storm continued for several days it is possible that these voids would have caused the partial destruction of the top courses of the breakwater. This canal was intended to be filled with mass concrete, and this has now been done, and no further trouble can arise from this cause.

Another and similar case occurred in the old Admiralty Pier at Dover, which is faced on both sides with granite blocks. A number of the blocks have been forced out at right angles to the axis of the breakwater by what was clearly equivalent action. In other words, the stone jointing was not full of cement mortar and voids were left. Water accumulated behind the inner ends of these poorly jointed blocks, and the force of the hammer blows reacting from the almost vertical wall created what amounted to a hydraulic ram, and so forced the granite blocks out of the face of the breakwater.

The exact action of the sea on structures is waiting the solution of the scientist to determine what forces exist and are exerted by moving masses of water in great storms. The knowledge of the effect of waves of any definite length or height is not determinable with sufficient exactitude by any scientific data that I know of.

I remember while the harbour works were under construction at Dover three concrete blocks piled one on top of the other, each averaging about thirty-five tons, were standing on the unfinished pier. They rested on some timber packings a few inches thick at a level of about 8 ft. above high-water mark. During a south-westerly storm in the Channel, they were carried across the pier many feet and into the sea on the lee side.

This could only have resulted from some reduction of the frictional resistances due to compressed air or hydraulic action on the under side of the blocks and between them and the pier top where a space or void existed. This upward pressure decreased the friction to such an extent that the force of the sea was enough to slide the blocks horizontally. It has been determined in the north-east coast of Scotland that blows due to sea action sometimes amount to from two to three tons per square foot on small areas and up to two tons per square foot on large areas. It would appear that some sort of 'pressure lubrication' was produced between the bearing surfaces which in mechanical design has not, I think, been developed as far as it might be to reduce friction between moving parts.

One has frequently observed, having poured hot water into a glass and turned it upside down on a wet glass shelf, with what small amount of force the glass begins to move along the shelf. This reduction of friction would appear to be due to the air finding its way under the edge of the inverted glass and the movement caused by some small inclination of the glass shelf on which it rests. This, I submit, is an indication of what happened by sea action to the pile of heavy blocks on the unfinished pier at Dover Harbour.

The only information the civil engineer can rely on at the present consists of historic facts which are referred to in different publications on the subject of constructed or destroyed breakwaters. All these point to the advisability of putting the top blocks of any breakwater structure in well-cemented joints, and the leaving of no voids, or joints, near the upper levels of these structures.

This is my excuse for giving a long description of experiences which have come under my own observation.

I have said that the effect of voids among rocks deposited for the purpose of the defence of harbours and ports against the sea, and the cost of the same, are important considerations in connection with sea defence works. The rocks are usually deposited free from all quarry rubbish and earth arising from the 'overbearing' which has been removed; such deposited rocks usually contain within their volume voids to the extent of 42 to 43 per cent. If the whole output of the quarrying operations were deposited without special selection, these voids would be reduced to 30 to 33 per cent.

As can be easily understood, these facts have an influence on the cost of construction. In this connection, the amount of selection which is involved (if a 'limiting minimum' size of rock masses is demanded), though not entering into the question of the volume of the voids, materially affects the cost of the deposited rock embankment as a whole.

The voids in any mass of irregular solid lumps are largely influenced (as I have already said) by the uniformity, or otherwise, of the sizes and shapes of the lumps of rock of which a rubble breakwater bank is composed. The cost of a rubble embankment is very much influenced by the necessity arising from specified selections of the limiting sizes of its parts. The rock strata from which the material is quarried also makes a great difference in the ultimate cost if it cannot be blasted into the categories desired. The effect of voids in the mass, especially near high-water level, or where the influence of the wind on the sea may result in the inclusion of air in the moving masses of water, is to make for the free escape of this air and, therefore, for the greater safety of the structure as a whole, though the density and weight of the whole mass containing the larger voids will be less.

This, of course, is in some measure a disadvantage so far as its resistance to the sea is concerned, and involves large unit masses of rock. In breakwater design, therefore, as in most things, it is a question of compromise, and it is found necessary—

(*a*) Either to prevent voids entirely where heavy seas are to be encountered near high-water level and to a considerable depth below it, by means of rectangular blocks built closely, or—

(*b*) To make big irregular masses of artificial rock, such as roughly shaped concrete blocks, or—

(*c*) To procure such large rough masses of rock from suitable rock stratifications at not too great a distance or expense.

As to greater depths of a breakwater, however, say at 45 to 50 feet below the low-water level, where there is no included air possible, the matter of voids and sizes of individual portions of the whole mass of the breakwater bank is not so important. I have already given some instances of the effect of included air between rectangular joints of built masonry and also of the movement by sea action of very massive blocks stacked a little above high-water level at Dover Harbour. I have also roughly described the structure of the great breakwater at Valparaiso in 187 feet of water where, at the lower levels, sand alone forms the basis of the biggest breakwater yet built. This Valparaiso work is an illustration of the varying needs of breakwater construction subject to very heavy seas, and is daring and original in its design, and great credit is due to the Chilean engineers, Señores Davila and Lira, in their advocacy of this novel construction.

There is another interesting matter to civil engineers in connection with voids, which arises when dredging materials by means of suction pumping. The ease of movement of the material to be pumped and the consequent economy in pumping are largely influenced by the water contained in and surrounding the particles it is desired to move and lift by suction. Mud or clayey material, for example, will allow the water sucked by the powerful pumps to slide over their surfaces, whereas sand and even lumps of rock the size of one's head, if surrounded by water-filled voids, will be moved with relative ease.

## Bacteriological and Entomological Sciences and their Influence on Civil Engineering.

Some very large and important works could not have been carried out without great loss of life but for the discoveries of Sir Ronald Ross, Sir Patrick Manson, Bruce and others in connection with the disease-carrying powers of certain mosquitoes, and especially the stegomyia and anopheles mosquitoes, which transmit yellow fever and malaria.

In the early days of De Lesseps' effort to build the Panama Canal the death-rate was very high indeed, and did more to make the first efforts a failure than anything else, unless perhaps finance or economics. I do not think Sir Ronald Ross has been recognised by our civil and other engineering institutions as much as he should be for his wonderful work in this connection. When the American Government undertook the construction of the Panama Canal the success of this great scheme probably owed more to the science of bacteriology than to civil engineering. The only other great difficulty which might have prevented its ultimate completion was the question of cost and the sliding in of its sides at the Culebra cut, which resulted from the local geological formation. It is a magnificent monument to the civil engineer nevertheless, and a great credit to those who designed and carried it out, but the science of medicine played as great a part, if not greater, than any other science in its accomplishment. In my own experience in the construction of the Port of Para in Northern Brazil, I have been much helped by this branch of science. While we did not entirely eliminate yellow fever from our staff, we did reduce it to a small number of cases and had very few deaths. We had also to fight yellow fever in Mexico and Colombia in connection with civil engineering works and not without loss among our staffs. My firm and the governments for which we worked owe their thanks to the help of medical science in the widest sense. There are still some fields to conquer in this connection, where we need the bacteriologist and medical scientist to aid us. I refer, *e.g.*, to Varugus disease which caused such destruction of life in the building of the Central Railway of Peru many years ago through the Varugus Valley which was named after the disease which existed, and still exists, to the injury of man and the advancement of civil engineering enterprise. I was in this valley in 1925, visiting the results of a terrible engineering disaster due to unprecedented rains. Rain had not fallen in that part of the Andes for thirty-five years, and when it did so it washed down the mountain side and buried the railway for miles, sweeping away bridges and diverting the river, and causing very great damage. The Varugus disease was still there and its causation unknown. Great care was taken to move away all the staff at night. This precaution was based on the experience of the engineers in charge of the work, and special measures were taken by the management under General Cooper which were productive of good results, but I was informed by those on the spot that they were still having trouble from the disease among their men.

The latest enemy for which the engineer wants the aid of the parasitologist and scientific medicine is the disease called 'Bilharziasis,' which is causing great trouble in Egypt and preventing the free movement of that splendid worker, the fellah, to the upper reaches of the White and Blue Niles to assist in the construction of the great dam and canal systems which are now engaging the irrigation engineers. The life-history of its mobile germ is being followed out, and we must all hope, from a humanitarian as well as an engineering point of view, for an early and successful attack on this, I think, the latest enemy of the civil engineer and the progress of his work. Black water fever on the west coast of South Africa, sleeping sickness and the tsetse fly are three more cases where the help of the sciences of medicine and bacteriology are invited to assist the pioneer engineer in parts of the world requiring transport facilities.

The civil engineer has been the means of helping himself and his fellows in one special case, that of working in high pressure air, which has involved serious injury in the past to the men employed in very important work. In this case, while death and injury occurred in a very high percentage only a few years ago, the dangers have now been much reduced, thanks to the civil engineer. Air under pressure is required in the sinking of bridge foundation cylinders and in the driving of subaqueous tunnels, and the resulting illness is commonly called caisson disease, diver's palsy or 'bends,' the latter name being due to the bodily distortions of the sufferers. This in its essence is a mechanical disease, and for this reason the medical profession did not advance far in ascertaining its cause. It was the civil engineer who through bitter experience and long-continued observation, found a cure. Chance gave me, forty-eight years ago, while at the Forth Bridge, the opportunity to study this disease during the sinking of the caissons which were being built and sunk under air pressure. At a later date the appalling death rate under the much higher pressures and much worse conditions that attended the construction of the Hudson Tunnel, New York, necessitated something being done to ameliorate the life of those who worked for me, and to make it possible to get the men to face the dangers of carrying on the construction of this, the first subaqueous tunnel built in the United States. The work had been commenced many years before, and much pain and many deaths had occurred, but without any cure having been discovered. Many months of continuous observation on these undertakings enabled me to ascertain certain facts and to devise a scheme of treatment—the use of recompression in a medical airlock—which is now always adopted on undertakings where compressed air is being employed, and with great success. These experiences also led me to discover contributory causes not realised up to that time, namely, among others, the necessity for much purer air than is required in workings at atmospheric pressure, and further the benefits arising from stage decompression. I had mules continuously under air pressure for many months, and they did not suffer at all from this long immersion but, like human beings, they suffered badly on passing through the air locks when coming out and getting back to normal atmospheric pressure. By simple re-immersion in high air pressure and by very slow decompression treatment, which is a very gradual withdrawal of the pressure while still keeping the air pure, I was able to reduce the death rate among the men at the Hudson Tunnel in 1890–2 from 25 per cent. per annum to 1½. In the Blackwall Tunnel in 1893 there were no deaths at all during the whole course of the works. Since those days Prof. Haldane, Prof. Sir Leonard Hill and Captain Damant, to mention the chief workers in this field, have carried the work of regulated decompression further and brought the rough discoveries I made and introduced to a more exact scientific basis; and much greater depths of immersion involving higher air pressures are now possible and safe by means of stage decompression. Here is a case where the civil engineer has helped the science of medicine—a return for some of the benefits received from the bacteriologist and medical man.

During my investigations I tried to ascertain the actual blood pressures in the human body but so far these have never been taken. Only comparative, not actual, pressures can be taken, but I feel sure there is a great deal to learn from a more complete knowledge of the effect of varying blood pressure, especially in its influence on the brain and spinal column. Also from the purely hydraulic and mechanical point of view including the possible distention and contraction of the veins and arteries due to the reaction on the muscles of their structures, where fluid pressures may vary within them and between wide limits. Engineers and medical men should work together on some of these things, with both bacteriologist and entomologist. Already the entomologist and chemist combine for the destruction of wood-destroying insects—the death-watch beetle, the pine 'bug,' the white ant and the toredo, fresh-water shrimp, dry rot—for the preservation of timber. The treatment of sewage is another matter where the bacteriologist has helped the civil engineer.

As influencing the health of workers, another question is the possible electrolitic action in the decay of teeth by the mixtures of metals in the stoppings. This appears to me wanting investigation. The currents set up are no doubt very small, but they may go on for years. Here the pure physicist might come in, if not the electrical engineer.

## Economics of Engineering Construction.

The economics of engineering construction naturally divide themselves into several categories. The first of these arises out of the purpose of the engineering works or enterprises. If we take Great Britain as our example, we have public health requirements, including water supply, sewerage, lighting, national road-making, and transport services. These are works in which the financial requirements are based and provided for on the credit of the community using them, issued and financed by the Government or Municipality through the investing public, arranged generally by some financial house and, from that house, allotted to the investor by means of a public issue, or they may be provided out of revenue by an Act of Parliament. These are among the easiest financial operations, especially when the borrowing is done by the Central Government, which commands the highest form of credit.

Another category involves enterprises that are to be financed on the basis of possible earnings and includes the public utilities not yet socialised, such as railways, docks, privately owned harbours, gas and electrical undertakings, and the bulk of the transport facilities, with the exception of roads and bridges. Since the abolition of the tolls, the latter have been undertaken, like works in the sanitary and health categories by the governmental, county, or other trusts or public bodies.

Finance for profit-earning enterprises is arranged through loans by issuing houses which as a rule are fully underwritten. Public issues are made and they have until quite recently been an easy matter. The experience of the common stock holder in this connection has not been always encouraging, and it often happens that his holdings are overborne by preference stocks and debenture issues, with the result that many investments, which in my youth were looked upon as beyond suspicion, have fallen heavily in capital value as well as in earning power. This state of things has naturally reacted on the extent of engineering enterprise and has in some measure reduced the training-ground for engineers and contractors at home, and therefore lessened the chance of Britain securing foreign or Dominion work in competition with the world. Naturally, a densely populated country, which for years has been providing itself with all sorts of facilities for transport, health and comfort, must sooner or later have been fully provided with the facilities envisaged in the categories above indicated. There seems little doubt that railway building and the great mass of engineering work that it involves has nearly reached saturation point in these islands although there is still much to be done abroad.

The next category that interests the constructive engineer, and makes for his employment, from the financial point of view is the needs of manufacturing and industrial undertakings. These comprise too many categories to attempt to classify them all. It takes, however, a greater faith and more enterprising spirit to raise money for this kind of work than it does for anything I have mentioned before, and, as a consequence, the financing of factories and ancillary works, immense chemical industries, shipbuilding yards, collieries, coal handling and loading devices, private harbours and gas works, with the greater financial risks that they involve and the greater probability of obsolescence as well as more rapid depreciation, requires higher rates of interest and provision of larger sinking funds to procure the necessary money.

One could enlarge on these lines, but I would prefer to deal with the questions of finance and economics connected with large civil engineering enterprises abroad and mostly undertaken for foreign governments. These are generally guaranteed financially by such governments and/or secured on monopolies under their control, and the effect upon those who act as financiers and contractors in connection with them needs careful consideration.

After the security has been provided and the finance arranged for, the contractor may possibly have to take his payments in scrip. Estimates for foreign work involve questions of exchange, values of local untrained labour and its efficiency in countries abroad, and other matters entailing considerable risk and requiring a knowledge of the science of economics.

The financing of the works of construction by the contractor may involve deposited guarantees, and will also require money for purchase and transport of plant and machinery, the engagement of staff with a knowledge of foreign languages, agreements as to supplies of material and provision of housing.

Many foreign governments demand large deposits in advance as guarantees of good faith, and undertakings by banking institutions as to the financial capacity and technical ability of those who offer for the work of construction.

Our British joint stock banking system does not lend itself to the provision of such requirements and guarantees, although there is some welcome evidence that this may be altered shortly, as intimated recently in the press.

Foreign contractors, on the other hand, do not have these difficulties to face to the same extent. The German, French and American banking houses especially have entered into this sort of business for years, and no doubt they share in the profits which arise, and also take some of the risks of losses with the contracting firm.

The responsibilities thrown upon a contractor who has made the arrangements indicated above vary considerably according to the accuracy or otherwise of his estimate based on the information given to him or otherwise obtained, and the amount of cover he provides for risks that are not taken by the employing government or company or scheduled by those who advised them. As examples, there are political risks arising from revolutions or wars threatened or actual, climatic risks including risks of sea action and possible destruction of the work, in some cases even the risks of design—risks arising out of exchange and depreciation of currencies in the country in which the contractor is working—risks of labour disturbances and variations of wage rates either in the country in which he works or arising out of, *e.g.*, a coal strike in England if he is dependent for supplies of fuel from this country; risks arising from the capital required and the interest on such capital; risks of diseases among the staff, either from the nature of the work or the climatic conditions, which may be so bad that the labour required is very difficult, or perhaps non-existent. The available means of access of transport are also important as well as the provision of food and other supplies. Apart from these, in some cases the contractor takes earthquake risks, those of inundation from rivers in irrigation works, the risks of the strata of the foundations on which heavy structures have to be placed, risks of the borings and data handed to him on which to make up his tender being accurate or not. Further, if it is a sea work upon which he is engaged and an immense rubble dam for the base of a breakwater is required, there is the risk of settlement into the sea bed which is nearly always thrown upon the contractor, and sometimes that has involved him in very great loss. Penalties for non-completion in time fall upon the contractor, but generally with fair provisions for extensions for unavoidable causes and *force majeure*. The amount of water to be pumped in deep excavations or in the strata through which tunnels are driven is generally his responsibility, and settlements, due to pumping in the surrounding areas, if pumping in fact causes settlement in the subsoil (which often happens) is generally another heavy responsibility.

Over and above all this, the efficiency of his supervisory staff and of all his local employees in a foreign country far from his base are matters he has got to take into account in dealing with his valuation of the works. Nor must the consideration of the solvency of the employer be left out of his reckoning. The estimation of the cost of getting suitable raw materials for the structure for which he makes a price depends, for example, among other matters, upon the suitability of rock arising from local stratifications giving, in the case of a breakwater, the large category rubble which goes to build up such banks.

Then if he takes, as he often does in South America, payments in bonds or Government securities, he has to run the risk of a fall in the values of the medium by which he is paid.

I have not even now covered the whole of the possibilities of loss which assail the constructor of large engineering undertakings, either at home or abroad, and for which, after he has assessed to the best of his ability their values and the contingencies arising from them, he must make allowance first in the unit prices of the work he is called upon to do, and finally in the gross amount arrived at through the detailed computations. None of the foregoing suggestions are academic. They have all occurred more or less in my forty-five years' experience of doing work for public bodies, governments and private companies in connection with my firm at home and abroad.

There are some risks it clearly would be better for the employing authority to assume rather than he should place them on the contractor. I refer, of course, to those which cannot be attributed to the incapacity or neglect of the contractor himself. For example, taking the risks arising from sea action, unfavourable foundations, uncertain geology, excessive amounts of water necessitating heavy pumping, risks arising out of, or due to war, and the adverse influence war or revolutions have upon commodity and labour prices: these are not things that with any amount of acumen on the part of the contractor can be provided for. If he adds to his estimate of cost sufficient to cover fully such possibilities, his estimates would probably be unduly high, and in that case the employing authority may have to pay for these risks not only what is in his estimate to cover them but on charges and profits upon these amounts in addition. Clearly it would be better for the employer, be it government, municipality or otherwise, to guarantee that the borings taken and other data given are correct, that the water pumped shall be paid for at so much per million gallons raised so many units in height, that should there be a cataclysm, either an earthquake or a sweeping away of the structure by unusual and heavy storms, quite uninsurable, then those things should be at the cost of the employing authority. In any event when the work is completed the employer gets the benefit of the overcoming of such risks and dangers and difficulties, therefore clearly he ought to pay for them and would be wise to pay only the nett cost. If he takes the responsibility for them on his own shoulders it will be cheaper for him rather than if the contractor includes sufficient to cover them in the prices for the work with profit added thereto. On the other hand, if the contractor has not allowed for and cannot face the loss, the employer will have to make a new bargain with some other contractor at much higher prices. The same may be said of the consequences arising from epidemics which again cannot be controlled by the contractor, although with proper sanitary organisation he may reduce them considerably.

The values of all raw materials required for construction might be fixed with advantage to everybody, to be increased or reduced according to what they actually cost and according to the rates of exchange. It is almost impossible, for instance, for a contractor doing work in South America to ensure any specific price for coal or for cement and timber if required in large quantities over a period of many years, except with considerable margins. Clearly cheaper offers would be given for constructive works if a number of these risks were excluded, and no profits were attached or sums added to the tender prices to cover possible and indeterminate risks.

It might be thought, after this recital of the large risks contractors have to take, that they would of necessity come to grief sooner or later. That more do not reflects in a large measure their capacity to assess what these things mean, and their knowledge of the science of Economics. It is also well known that some, at least, die wealthy.

Few contracts are now entered into on what was the old-fashioned "lump sum" basis, where the contractor undertook to produce something that the employer considered he required and demanded to the satisfaction of himself and his engineer, whether, in fact, the drawings indicated the whole of what was thought to be wanted. The "lump sum" had to provide any finished result that was implied in the documents or specification, or that might arise out of the requirements of the specification, in spite of any possible error of judgment in the design or in the description of the physical characteristics or geology of the country in which the work was carried out.

Apart from the "lump sum" system, when all these risks have been considered and, as already stated, an estimate has been made of the actual values of labour and material based on knowledge of what such material could be got for in the locality where the work is to be done, there has to be added something for the uncertainties and contingencies that I have described, as well as for profit. The capital requirements for construction purposes vary according to the place and the character of the work. For a large engineering contract where big deposits are not called for, the capital required may easily run up to 15 per cent., of the gross cost. In other words, the maximum peak that may be reached before the earnings from work done (less retentions held by the Government or company) may be easily 15 per cent. above the receipts at that time, and possibly on a five years' job, between three and four years may elapse, the time varying according to the margin for profit, if such accrues before the contract receipts cross the line of expenditures. In diagrammatic form one has to compute what the interest will be on the average capital for the time before such desired result is achieved. Thereafter capital, instead of having to be found, should begin to flow into the coffers of the contractor, and therefore be on the credit side. The initial expenditure being as I have indicated anything from, say, 12 to 15 per cent. of the gross cost of the work at a maximum peak, has got to be financed, either out of capital accumulated or by advance by some financial institution, and often a good deal of profit is sacrificed owing to the necessity of procuring these advances. If a contracting firm has three or four large contracts running at once, it may easily have to find either out of its own resources, or by borrowing, a sum approaching a million sterling. One contract may lose while another one gains, but with all the risks I have enumerated, and there are others which are not covered even by my long list, losses are often very heavy indeed. If these are not "shouted from the house-tops" but are borne with a grim determination and a mental "tightening of the belt," it is because contractors as a whole are a stoical people who prefer to keep their losses to themselves. I do not wish to discourage the young from venturing on the path that leads with hard work and luck to the summits that the successful contractor rises to, for the joys of fighting with nature and dealing successfully with the subtlety of man are the very breath of those who really give their life to this kind of work, and the pity of it is, in view of the confidence they have to show in themselves and others as well as in science and in nature, that the success they achieve in their adventuring, is sometimes far short of what they deserve.

My own great chief and partner, Lord Cowdray, was one of those who faced with great courage what might come, and never failed to complete anything he undertook, however severely he was called upon on some occasions to back his judgment and make good where nature or man had failed him. It may seem harsh to think or say, that he suffered more from man than he did from nature, but, whatever the obstacle, he ultimately triumphed to an unusual degree and he deserved to, for his courage was phenomenal. No risk ever seemed to daunt him, and if after taking every precaution that forethought could suggest, the allowances he made were not sufficient, he faced the music without complaining.

I am the first contracting civil engineer who has been honoured by the British Association and, therefore, I have thought that you would expect me to include some remarks on things I am specially conversant with, and this must be my excuse for having gone into so much detail on the economics of engineering construction.

It is obvious I have not attempted to deal with the influence which many sciences have upon engineering in its widest sense. It would appear unseemly, however, not to pay tribute to those sciences without which engineering cannot exist, the chief among them being physical and mechanical sciences and applied mathematics and all that those titles mean. They are subjects which have been enlarged upon before in addresses by your Past-Presidents and dealt with far more ably than I should feel it possible for me to do.

Another science that must not be forgotten in its connection with the raw materials that so largely enter into all engineering structural works is chemistry, the discoveries in connection with which have meant so much to the engineer.

Science, however, is being split up into so many different categories that there are a great number to which the acknowledgments of engineers should be accorded, and I am afraid there are many of this ever-growing group to which I have not paid sufficient, if any, tribute. My excuse must be lack of time to do them justice and not lack of appreciation of their helpfulness.

I have attempted to indicate the interdependence of the engineer on the science of the physiologist, the bacteriologist, the economist and the all-important science of finance, all of which enable the engineer to carry out his destiny by entering new paths and opening up, by the aid of railways and roads, vast areas to enable them to be made fruitful and suitable habitations for his fellows.

# EVOLUTION IN MATERIAL CULTURE.

ADDRESS BY

H. S. HARRISON, D.Sc.,

PRESIDENT OF THE SECTION.

THE systematic study, and the systematic teaching, of the material side of human culture receive less than their due share of attention in this country. Anthropology has established a footing in several of our Universities, but the need for students to enter on the study of material culture by the material means of laboratory courses is scarcely yet appreciated. The subject is passing through a phase from which the biological sciences long ago emerged, books, lectures, demonstrations, and Museum specimens in cases, being the chief sources of supply of information. Sooner or later there must be organised a system by the aid of which the student is not only encouraged to see, and sometimes touch, the human products that are under his consideration, but is induced to handle, measure, draw, and as far as possible dissect, such of the main types of simple artefacts as can be spared from their more spectacular Museum duties ; and also to carry out some of the methods and processes which he now learns by hearsay or by reading. The hands must come to the aid of the eyes and ears. Until he has done practical work of this kind, he is not even able to make the best use of the specimens he views through the windows of Museum cases. It is now well known that ordinary glass denies a passage to some of the more active of the sun's rays, but it is not so fully understood that the light of knowledge is also much enfeebled by its filtering action.

Another requisite for the furthering of the study of material culture, and of Anthropology in general, in this country, is the provision of a worthy setting for the unrivalled ethnographical collections which have been so long marooned in Bloomsbury. If the proper study of mankind is man, it is wrong to make a hole-and-corner business of it, especially in the centre of a Commonwealth which boasts of a never-setting sun. Ethnographically speaking, the sun has not yet risen, but it may be that the gleams of light in the recent Report of the Royal Commission on National Museums convey some promise of a dawn. However this may be, the founding of a National Museum of Ethnography would not only remove a grievous reproach to our scientific standing as a nation, and to our claim to be an instructed governing people, but would have important reactions upon the status and teaching of anthropology in this country, with increasing benefits to colonial and other administrative services, and to the alien races whose destinies they seek to guide. Whilst successive

Governments delay, collections and enthusiasts are lost, and yet another British opportunity becomes acclimatised elsewhere. It is not yet too late, but soon it will be.

That our specialists in the study of material culture are few in number because of a lack of chances and incentives, can scarcely be doubted. This leads in turn to inadequate descriptive and comparative work, and to a partial neglect of those problems of analysis and synthesis to which so much attention is given by American and Continental anthropologists. A training in the dissection of animals is perhaps conducive to a leaning towards analysis rather than synthesis, but at the same time it is the evolutionary problems that offer most attractions to one who is biologically-minded. In what I have to say to-day, I am trying to express conclusions that have developed in my own mind in the course of many years of contact with the material products of backward peoples, as well as with the theories and views of those whose artefacts and formulæ have reached a more bewildering complexity. If any psychologists are present my apologies are due to them, and I can only hope that they will be induced to demonstrate my errors by focussing their brighter light upon the human mental processes which I have tried to explore a little further by my own candle-power.

### Objective and Subjective Problems.

Some features of the evolutionary aspects of human material culture have received occasional attention since relatively early times, but no systematic treatment of the subject was attempted until Lane-Fox Pitt-Rivers did so much to establish the analogies that link the evolution of living organisms with that of human artefacts. My immediate, as well as remote, predecessor in this chair, Mr. Henry Balfour, in his Presidential Address in 1904, gave a summary of the methods and conclusions of Pitt-Rivers, together with an account of his own attitude towards some of the problems that emerge from the study of the development of artefacts. Since the date of that address, the question of the occurrence, or frequency of occurrence, of independent evolution, has become so much more insistent, that it seems worth while to re-examine in some detail the nature of the subjective processes that lead to discovery and invention, and of the objective outcome of these processes. In the course of his address, Mr. Balfour expressed the view that independent evolution should only be accepted as an explanation of similarity after an exhaustive enquiry into the possibilities of transmission; he did not, however, seek to put independent evolution out of court, since he said 'Polygenesis in his inventions may probably be regarded as testimony in favour of the monogenesis of man.' Prof. Elliot Smith, in his address to this section in 1912 (and elsewhere still more forcibly) lays great stress upon the essential conservatism of the human mind, and upon the fact that 'for the vast majority of mankind almost the sum total of their mental activities consists of imitation or acquiring and using the common stock of knowledge.' Others both here and abroad have taken up similar positions, but there still remain many whose attitude towards independent evolution is tolerant and even generous, and it is to these that the misleading term 'evolutionists' may be supposed to apply. Whether the term is the antithesis of 'diffusionist' is not clear, since both are ill-defined, and at times it would appear that the relationship is one of antipathy rather than antithesis. On the other hand, it is sometimes hopefully said that we are all diffusionists nowadays; but there are differences, and many wish to put at least the New World out of bounds.

Even though it seems unlikely that anything that is really new can be brought into this discussion, it is only by enquiring into detail that a better understanding is attainable. Whilst I am myself convinced, rightly or wrongly, that the part played by independent evolution in human material culture has been negligible, it is in no way my aim to prove that it has not occurred. Indeed, I see many reasons why in certain simple types of cases it may have occurred, though proof is difficult or even unattainable. My object is rather to enquire whether an analysis of discovery and invention, as manifested in human material affairs, will enable us to fortify our faith by more and better reasons. For, as Nordenskiöld has lately pointed out, faith plays a large part in the controversy over independent evolution, even though one side or the other claims to have more reason in its faith—itself a mixture of reason and credulity.

Avoiding controversial topics for the moment, we may begin by touching on some simple points in relation to man's material culture, and so pave the way to more debatable conclusions.

By the aid of *methods*, often dependent upon extraneous *means*, man employs *materials* for the achievement of *results*, many but by no means all of which persist as artefacts or other *products*. These objective categories call for further definition, and for some sub-division, since it is obvious on inspection that they are not homogeneous.

As regards materials, food stands alone, since it is the only material substance that man has always had to seek, and since the need of it persists as the ultimate incitement to all his drudgeries, as well as to some of his enjoyments. From the beginning, and earlier still, he has known how to forage for a living, and the discoveries he has made of new kinds of food stand upon a different footing from those concerning substances he does not eat. If one natural food was not available others must be found at all costs, and the lower the grade of culture the less fastidious the food-collector. It was a question of finding, rather than of finding out, and true discovery only came into play when methods were adopted of facilitating, stimulating, and controlling the natural processes of growth and reproduction of food-yielding organisms. In man's use of inorganic raw materials, on the other hand, he had from the beginning to discover not only the properties of the materials, but also the uses he could make of them, as well as the methods and means by which they could be made more serviceable. He tested and discovered foods by stress of need, and he knew what he could do with them, but this guiding pressure failed him when it came to sticks and stones, whose virtues lay at first outside his interests. He did not bring his mind to bear upon them, being under no compulsion, and it was only the obtrusive that attracted him. Some materials he could obtain ready for use, when he had discovered what their uses were, but others were of no value, or were in effect non-existent, until they had been separated, extracted or prepared. To get out of the blind alley of the Stone Age he had, for example, to make discoveries and evolve methods, relating to materials that only entered his environment on his own unpremeditated invitation. Metal made its first impression as a fascinating luxury, from which evolved a need.

Methods, like materials, are far older than man, and in so far as they make calls on bodily and mental powers alone, they may be called pure methods. But progress has depended upon the development of reinforced methods, involving the use of extraneous means. Methods are man's ways, and his means are tools and other artefacts, but it is not entirely true to say that he discovers his ways and invents his means, since he may discover his means as well as his ways.

Substance is the static warp, and method the dynamic woof, of man's material culture, whilst the products may be looked on as the fabrics, though these are not always tangible. Amongst the more obvious of those which are material in their nature are artefacts of all kinds, but it is clear that such products are themselves the means to further ends. These further ends are material in the case of implements, less immediately material in that of houses or canoes, and non-material in the case of shrines or musical instruments, which satisfy demands of social and individual mentality. And it is here that we find our objective point of view overlapping the subjective—our material products require for their explanation some understanding of such aims and ends as lie outside the field of primary material needs. As soon as we get beyond the study of the instinctive quest of food and self-protection, and pass to that of the aims of the human artificer, we realise that aims and ends as well as ways and means, are products of evolution. Man did very well before he was a man at all, and no one has given any reason why he ceased to be an ape. We may appeal to natural selection, to the inheritance of acquired characters, to orthogenesis, and though our belief in the ascent of man remains unshakeable, since the proofs are overwhelming, the reason why an ape-man became an artisan continues to elude us. To say that it was due to action and reaction between a developing brain, aided by versatile hands, and an environment which expanded as knowledge grew, is not an explanation but a statement. But if we cannot say why it happened, and why we have reached a point at which man aims at creating new needs, to the overcrowding of his artificial environment, we can at least attempt to understand something of the way in which natural man became unnatural, and eventually fell into the ways of civilisation.

### The Distorting Mirror of the Present.

Our attitude towards the problems that arise in the study of origin and development depends very largely upon the extent to which we ascribe to man a power of foresight enabling him to overrun the limits of environmental suggestion. If we assume that his progress has been based upon his opportunist reactions to such suggestion, we secure a standpoint from which to take a retrospective view of human progress. The visibility is not too good, and the details that are fundamental are often but obscurely seen, partly because the field of view is not only restricted by our ignorance, but is overshadowed by our knowledge. We can see too little of the past and too much of the present.

In all our attempts to gain an understanding of the progress of early man, we are, in fact, heavily handicapped by the training and experience which mould our thoughts from our earliest years. As children we acquire knowledge and 'general ideas' concerning materials, methods, and results, which make it impossible for us to picture the real character of the mental states of primitive man, and of the founders of the early civilisations. We leave our cradles to turn a tap or push a button that effects a miracle, and our minds are never quite the same again. We learn also that man has evolved, that his culture has evolved, and that there is good reason to suppose that progress will continue for an indefinite period. As Bury has shown, however, it was not until the latter half of the nineteenth century that the 'idea of human progress' received acceptance, though it had been in process of evolution for some centuries. It was not familiar to the Greeks and Romans, nor, we may assume, to the peoples of Mesopotamia and Egypt. It is difficult to assess the importance of the general idea of progress as an incentive in the detailed development of discoveries and inventions, though we may be sure that it is very far from being negligible. But if our own opportunism is inspired by a vision of an almost unlimited field of progress, the culture of ancient times was based on an opportunism sufficient for the day. Foresight is a cultivated aptitude, not a human instinct. The men who began the growing of grain did not look forward to feeding a multitude—population increased with the food supply, and cornfields were enlarged to feed the growing numbers. The idea of a multitude of men evolved with the multitude itself. The plough was not invented as a means of more efficient tillage, but was the result of the discovery that a pick or a hoe could be dragged through light soil so as to prepare a seed bed more rapidly than could be done by pecking up the soil; the implement got a new start in life by a change in the method of use, and it was improved as a result of discoveries arising out of its manufacture and employment, aided later by the adoption of modifications suggested by other contemporary devices or appliances. At no stage was there a premonitory vision of a method of agriculture, or a type of plough, having an origin in a mental conception cut off from its roots in the state of knowledge of the place and time. That kind of unconditioned foresight does not happen even nowadays, and we may assume it never will. It is only on the basis of his actual knowledge that man can reason and deduce, and though modern scientific learning may enable us to make predictions with some certainty, the result of experiment is either a confirmation of theory or it is a surprise—a true discovery; and the part played by chance in thrusting discoveries on man is well known to all of us. We differ from our forerunners in the fact that our receptivity and enterprise are fostered by the idea of progress, and are nourished through an organised system of traps and snares which provide discoveries and ideas for domestication and consumption. This is directional research, but real discoveries that are outside the range of existing theory are apt to bring about an opportunist change of direction by a revelation of new aims and possibilities. That is to say, we may be systematic and forethoughtful in our use of existing knowledge, and in our quest for new, but true discoveries reveal us as the opportunists men have always been. The modern discoverer or inventor appears, but only appears, to be able to look far ahead of the knowledge of his day, and the speed and scope of modern progress give a false impression of human powers in general.

It is not only our material equipment that is artificial, but our social and our moral codes, always excepting those minimum requirements that enable family, if not other groups, to hold together, since these are as instinctive in the species as is the quest for food itself. Our artefacts, material and immaterial alike, have emerged from the interaction between mind and matter, and between mind and mind; they were not devised beforehand for material or social ends, but arose out of the rough-and-tumble of an environment that grew as knowledge grew and artefacts accumulated. Aims and ends evolved with the discovery and invention of ways and means. The artificial environment has expanded with the progress of civilisation, but the human brain has not undergone a like inflation; nor, as far as can be seen, has the human mind undergone a change in its essential characters. It is still unable to form preconceptions of artefacts and processes that cannot be built up in the mind's eye, on paper or in practice, by combining facts, methods and principles that are known, with the aid also of discoveries arrived at by experience and experiment. Only as man became capable of transmitting his knowledge to his offspring, by precept as well as by practice, could he create a cultural continuity extending over many generations. Only when his speech became intelligible did his reminiscences acquire posthumous value, whilst hieroglyphs and alphabets served to make the future still more dependent on the past.

Viewed from any standpoint, it is clear that we have to make a big allowance for our own sophistication, when we are trying to explore the origins and growth of discoveries and inventions, and neglect of this precaution is not infrequent. In the case of pottery, for example, it is sometimes maintained that the plasticity of clay is so obtrusive, and its hardening by heat so easily made manifest, as to place the ceramic art amongst those human industries that may have been developed more than once, if not over and over again. The two essential properties of clay are obvious enough, given the conditions for its accidental hardening by fire, and both may have been discovered at various periods. Looking backward it seems evident to us that an early discoverer, looking forward, could have deduced from these two properties of clay the advantages of modelling this plastic stuff into the forms of vessels, and baking them to hardness. Some may think that the deduction was an easy one, but the ease is purely retrospective. The potter was not a product of predestination. The conventional theory of the origin of pottery through the plastering of clay on the walls of baskets may or may not be acceptable, but it is in any case a recognition of the need that is felt to bridge the gap between the discoveries of two properties of clay and the production of an earthenware pot. The discoveries were essential, but it is only in the light of our own knowledge that pottery appears to have been an inevitable result. In man's potential arts and crafts first steps must have been last steps far oftener than not, and many beginnings came to an end before they got a start. The first step or the next step may be within the range of vision, but the next but one is always out of sight—foresight is not farsight.

To take another instance of the hasty reasoning which credits ancient man with anticipatory conceptions that in ourselves are due to knowledge, it is sometimes suggested that there is no improbability in the idea of the multiple origin of the pyramid, since the observation that piles of loose materials readily assumed a conical form must have been frequently made. To this it may be answered that pyramids are not small, are not made of loose materials, and are not conical in form, but this is only a small part of the relevant reply. The affiliation is indeed inconceivable, since the evolution of a pyramid depended not only upon many material factors, but also upon a number of social and religious sequences. Pyramids were not preconceived as being more pleasing to the gods, and more elevating to the human soul, than any other geometrical monstrosity; nor were they built out of mere Euclidean bravado. For such structures, even in their various modifications of material, form, and function, to appear independently in Mesopotamia, Egypt, India, Cambodia, Java, and America, would have called for parallel networks of coincidences rather than for parallel chains. In any case we may disregard the pile of sand as a likely foundation for such structures, though it may be that we must put a mountain in its place.

A third example is that of the practice of mummification, and, as in the case of the last, it is one which has been dealt with faithfully by Prof. Elliot Smith. The assumption is often made that mummification originated in more than one part of the world, as a result of the observation that under conditions of aridity the bodies of the dead suffered desiccation instead of decay; and that this observation readily led to the evolution of processes of preservation by artificial means. That this is merely an assumption is obvious. Whatever may have been the course of events that led man to a belief in a future life, and however widely spread the notion of the attachment of the spirit, for a longer or a shorter period, to the corpse, it cannot be argued that a belief in the resurrection of the physical body formed part of the original structure of the immaterial artefact we call theology. However great the affection of the living for their dead, in one way or another the body had to be disposed of. An easily-discovered and very early method was that of putting it out of sight by covering it up, eventually by burying it, and this was perhaps the first definite funeral custom to be established. Instinct might have taken man as far as this. That under certain conditions the body, buried or unburied, did not entirely lose its human semblance, was in itself no inducement to the conservation of the dried remains. A shrunken body was of no more value than a skeleton, and new views of man's place in nature, and in supernature, had to be evolved before the preservation of the body became a means to an end. Whatever may have been the conditions under which this end or aim appeared, the main determining causes were not those of the natural environment—though this was no doubt an essential factor—but of the artificial, and they did not arise out of the 'psychic unity' of man. For the idea to originate in two or more regions independently, there must have been coincidences in social and religious sequences, as well as in the natural environment. For the independent evolution of artificial preservation out of natural desiccation, there would be needed further coincidences in the growth of the idea of preventing the decay of animal or human flesh, and of the means and methods of prevention; and finally, in so far as there is agreement in different parts of the world in the technique of preparing the body for the embalming process, there must be assumed still more coincidences, some of them significantly trivial. Whatever the ultimate decision, there is clearly no justification for the *a priori* assumption that man, directed by his common faculties, naturally and inevitably deduced the means and methods of mummification from the bare fact of desiccation. He made his way by a zig-zag opportunist route to an end which only came in view when he got near to it, and such routes do not run parallel one with another to the same destination.

## Man's Common Faculties.

The question as to the nature and importance of the common faculties of the human mind—the components of the psychic unity—is one which demands more attention than it has yet received from anthropologists, Bastian notwithstanding. This is especially the case in relation to the subject of independent evolution. For our purposes it would not only be needful to isolate the common faculties, but also to identify those which have a bearing on the progress of discovery and invention. Here we should meet with the primary and well-known difficulty of distinguishing between an inborn human faculty, and a traditional or inculcated mode of thought—an acquired type of reaction. Assuming we had progressed so far in the comparative psychology of *Homo sapiens*, we should still be left with the problem of determining which—if any—of the common faculties are directive in their nature. It is not a question of deciding which faculties are permissive, enabling man to react in a similar way to similar external stimuli, but of determining which of them give him the power, whoever and wherever he may be, to over-ride deflecting influences. Two environments may be similar, but only when they are the same are they identical, and our broad generalisations as to the cultural effect of surroundings such as deserts, mountains, forests, river-valleys, have a bearing upon the general mode of life they encourage or permit, and therefore upon a portion of the field which is open to the discoverer and inventor, but they ignore the differences in the details of any two environments of one general character; and it is discrepancies in detail that produce divergencies in end-results. The human mind is very prone to skid on trifles. Moreover, even on the assumption that two similar natural environments are so nearly identical as to lead to similar reactions, under the guidance of the common faculties of the human mind, there still remains the most important factor of them all—that of the artificial environment, in gross and in detail, which formed the starting point of two peoples whose artefacts and general culture are compared. Taking all these difficulties into account, we see that the common faculties of man, if they are to be powerful enough to keep his independent lines of progress parallel, must be of an initiating and controlling character. If they are of such a character, history should reveal a wealth of instances of their power to keep man steadily progressing on his course, in all grades and aspects of his culture. But history has no such tale to tell, since it is merely a story of one provisional expedient following on another—or, to use a word invented by the Poet Laureate, *Odtaa*. It seems evident that from our present point of view the common faculties of man are recessive rather than dominant.

That there are mental faculties common to all men is undoubted, and it was in part by the exercise of such faculties that man secured advancement. Of the evolution of the human brain we begin to know a little, but we are not able to draw a line of demarcation between the innate and the acquired powers of its cells and tracts. In both mind and body we inherit potentialities which only unfold under certain conditions. For the development of the body we may define what are normal conditions, and they must not depart too widely from natural conditions, but for that of the mind the conditions may be almost wholly artificial. Heredity provides the aptitudes, but the grist is delivered through the sense-organs, and whilst the brain is a natural growth, the mind is a cultural construction. Human thought is compilation—a rehash of the past in the present, with a short-sighted eye on the future—and no satisfactory record has ever been made of the mind of a man whose sole knowledge had been acquired without the tuition of his fellow-men, savage or civilised.

If it is difficult to identify the common faculties of which we are in search, by enquiring into the mind as we know it, we may ask whether we are entitled to assume that the only such faculties to be effective are those which had survival value in the final overshadowing by *Homo sapiens,* not only of his more distant ape-like kin, but of those nearer Hominoid relations whose remains have come to light in recent years ; whether, that is to say, the instincts and aptitudes of Neanthropic man which ensured his predominance, were identical with those which have led us into rivalry with nature.

The brain of Later Palæolithic man appears to have been like our own in all essentials, and a Cro-Magnon born to-day might become a skilled mechanic or an able bishop. But man had no more need to become a mechanic than he had to practise as a theologian, though he drifted into both professions. If the mental faculties that had survival value in the prevailing of our species were also those that were active in the initiation and pursuit of cultural advance beyond its needs, we are perhaps led to the conclusion that by far the greater part of human culture, material and immaterial alike, is an afterthought of evolution—an embroidering of the fabric. Man was given the means to earn a livelihood, and found himself commanding and inventing luxuries. In producing a new and cunning big-brained animal with hands, nature overshot her mark, and we are struggling with the consequences.

Time will not permit of more than a glance at this complex subject, and indeed it is one for the psychologist to unravel. Perhaps he has already done so, and I have overlooked it. The essence of my contention —and, of course, not mine alone—is that there are no common faculties of the human mind that are capable of overruling the vagaries of environmental and historical compulsions, and of directing man's progress in discovery and invention, in various times and places, along lines that are parallel. Beginning with the primary discoveries of early man, applied for material purposes, the prevailing outcome of his independent and opportunist reactions to the results of his own interference with natural materials and phenomena, has been divergence and not parallelism. If amongst some people progress has been notable, in others there has been degeneration or stagnation. The non-progressive and self-obstructive common tendency of man has been emphasised by various anthropologists, but by none more insistently than by Prof. Elliot Smith, who has done so much to make us think again. Whatever may be our emotional reactions to the theory of the origin and spread of civilisation maintained by him and Dr. Perry—and what seems too good to be true is not for that reason false—it has helped to bring about a big change in the general attitude towards the problems of the evolution of culture, and light, as well as smoke and heat, is emerging from the controversy.

## Discovery and Invention.

Having given brief consideration to some subjective problems, we may now return to the objective standpoint, and endeavour to distinguish and define the human achievements which we call discoveries and inventions.

It is obvious that discovery lies at the root of all man's material activities, since he must know something of the everyday behaviour of material substances before he can apply or adapt natural objects to his purposes. Discovery may result in the development of activities in which method remains the essential and controlling factor, as in agriculture and the domestication of animals, and we may then call the resulting system of techniques a discovery-complex; or it may initiate and further the development of artefacts, which we may provisionally call inventions. Perhaps few would be disposed to call an agricultural system an invention, and the same applies to techniques of metallurgy or weaving. If these arts are called discovery-complexes, what term may be applied to the products, such as bronze and woven cloth? Iron is an element, extracted from its ores, and man has not yet reached the stage of inventing elements. Bronze is an alloy of two elements, owing its first production to a series of discoveries, and we can scarcely call it an invention. We may perhaps best get out of the difficulty by using the term discovery-product for all artificially extracted, prepared, and compounded materials which have no significant form imposed upon them, but are merely the raw materials for the future production of shaped artefacts.

We may apply the term invention, for general purposes, to all shaped or constructed artefacts, in spite of the fact that many simple types, such as hand-axes and clay pots, are in reality products of discovery rather than invention.

If, as just suggested, woven cloth is classed with bronze and other discovery-products, it has to be recognised that textiles and other fabrics are in some respects intermediate between discovery-products and inventions. Although they possess characters of construction independent of those of the artefacts of which they are components, their origin and development present problems differing from those of the evolution of tools and appliances with which the idea of invention is commonly associated.

The application of the term 'an invention' to any and every shaped or constructed artefact can only be justified on the grounds of expediency, and it must be understood that the concession is not meant to embody a definition of invention as distinct from discovery, in the relation of these words to the subjective workings of the human mind, or even to the objective results. It would, indeed, be better to discard the word invention altogether, in any scientific treatment of the subject, since its edge has been blunted by common use and misuse. The word discovery also suffers from the same defect, since it is used in reference to identifications and localisations which are due to the application of deductive methods, as well as to trivial incidents of daily life.

After allowing for such inconsistencies of usage, a main cause of the difficulties that have always stood in the way of attempts that have been made to distinguish sharply between discovery and invention, is the failure to separate the subjective from the objective content of the words, and to this point we may give some attention.

The word discovery, in its bearing on material culture, relates only to the subjective appreciation of the properties or reactions of material substances or bodies, and it does not necessarily carry the implication of an objective exploitation of the knowledge gained. Only when the knowledge is applied to a useful purpose, more or less directly, for the initiation or development of a method or an artefact, does the discovery play a practical part. We may say, therefore, that a discovery is a subjective event, which may in many cases be utilised in an objective application, and that it is these applied discoveries alone that are factors in human progress. It is therefore necessary to qualify the word discovery and speak of an applied discovery, before we can obtain an objective as well as a subjective term. If we attempt to treat the word invention in the same way, and speak of applied invention as the objective aspect of invention, we realise at once that we are doing violence to our conception of the meaning of the word.[1] The word invention, in fact, unlike discovery, covers both subjective and objective meanings, and a failure to appreciate this inequality is the cause of misunderstanding. Moreover, whilst it is easy to distinguish between a discovery and an applied discovery as being subjective and objective respectively, I know of no attempt to make a corresponding distinction between the subjective and objective aspects of invention. At the moment we need not pursue this question further, since it will arise again at a later stage.

## Applied Discoveries.

It is clear that material progress began with discoveries relating to materials, objects, and phenomena, of natural or chance occurrence, and that the initial value of such discoveries lay in their immediate practical use. It may have been the behaviour of stones he handled that first aroused man's interest in them, but the utility of individual stones as implements was more important to him than the properties which made them useful. His generalisation was unconscious, or even instinctive, since animals discover, though man alone invents. The making of discoveries was not the result of a conscious search for means or methods to achieve

[1] The application of an invention such as the plough, to the purpose for which it was made, is clearly not comparable with the application of a discovery, such as that of the fusibility of copper, to the production of artefacts.

an end, though such of them as helped towards the immediate procuring of food found a ready application. Upon the discoveries which arose out of observation of simple natural phenomena, and of superficial properties of natural materials, were built up knowledge and experience which led man further and further away from his initial steps, until he was making discoveries about materials which owed their character or composition to his development of methods of treatment; they were, in a practical sense, his own creation, and until he had created them he could not learn their properties and uses. Man had no need of metals before he had discovered them, and until then they did not exist even in his imagination.

If we seek to trace the course of events that led to the use of bronze for tools and weapons, we see at once that not only were there many discoveries involved, but that these discoveries must have followed in a certain sequence. Our picturing of the course of evolution from hammered native copper to core-cast bronze must be in part speculative, but in the foreshortening perspective of time, and in the light of our own knowledge, we are more likely to see the chain of events as having fewer links than it actually had, rather than more. This is true even if we avoid the error of supposing that when a harder and more amenable 'copper' began to make its appearance, there was a rapid development of the idea of a still better bronze, together with definite conceptions of an ideal product. In so far as some samples of the new alloy showed themselves as more serviceable than others, there was a directional effort towards repetition—man's primary ambition—and when it was found that the presence of an 'impurity' in the copper ore was an ameliorating cause, there was a basis for experimental quantitative smelting, which was of the nature of directional research; but it was a dim foreshadowing of the corresponding process in our own day, not because the mind of man was differently endowed, but because it was differently equipped and trained.

The building-up of any discovery-complex, such as agriculture or metal-working, is clearly dependent upon single discoveries, following on each other, and some of them could only emerge at the end of a long sequence. Irrigation was not a starting point, nor was the smelting of copper ore. The *cire perdue* process of casting bronze may be taken as another instance. If we assert the origin of this process independently in two parts of the world, the rashness of the assertion will be proportional to the extent of initial metallurgical knowledge common to the two regions. If in both regions a complete ignorance of metal was the starting-point, the discovery of the *cire perdue* process independently is far less credible than if knowledge of bronze, and of methods of casting it, prevailed in the two regions. This is obvious, and is often overlooked.

That some discoveries are more 'difficult' than others is agreed, but it is perhaps not so clearly realised that the difficulties are of more than one kind. The discovery of the phenomenon of the development of heat by the compression of air led to the production of the fire-piston in South-East Asia, perhaps or probably in independence of the European appliance. The phenomenon was no less natural in its ultimate nature than that of the production of sparks from iron pyrites by percussion, but it was not so easily produced by chance, and it was not so easily reproduced by experiment. It was not only a difficult discovery, but it was also difficult in its appreciation and its application. The independent production of the fire-piston under experimental scientific conditions in Europe—and at present the available evidence seems to justify a provisional acceptance of this view—has no bearing on the general question of independent invention, since the significance of this is in relation to its occurrence under comparable cultural conditions.

Contrasting with difficult discoveries are those easy and easily applied discoveries, such as the flaking of stone and the cutting of wood, which occurred so early in the history of man as to be outside the range of controversy. Still more easy were the discoveries of new food-plants, though these are scarcely comparable with the type of discovery with which we are concerned. The baby who samples coal is pursuing knowledge and making discoveries, by the ancestral method of trial and error, and no instinct saves it from unpleasantness. It seems possible, however, that this readiness to explore the dietetic values of unfamiliar substances—which must often have been a condition of survival, if sometimes of survival of the prudent—lies at the root of the habit of substituting one material for another in human arts and crafts. At all events, it must have helped to form the mind of man before he was human. It may perhaps be accepted as one of our elusive common tendencies, even though it helps towards diversity rather than coincidence in discovery and invention.

In this discussion of discovery-complexes we have got no further than the recognition that they are systems of methods, which must have developed in a certain sequence, even though we cannot confidently reconstruct such sequences in detail. In many cases, moreover, not one but several sequences may be identified, a fresh start arising out of new discoveries. The casting of copper, for example, did not emerge directly from the method of percussion, but if the metal had never been hammered into the form of implements, we may safely assume that casting would only have been discovered and exploited, if at all, as a result of knowledge of a method of casting some other metal.

It is thus a further complication of the problem, that one process may owe its origin, in part at least, to a transfer of method from one substance to another, or to the influence of knowledge acquired in relation to another substance. It may be, for example, that the casting of copper implements was in part an outcome of the knowledge of the behaviour of waxes and fats, melted and allowed to cool. This process of transfer of ideas in technique is analogous with that factor of hybridisation in the evolution of artefacts for which I have proposed the term cross-mutation, and of which more will be said later.

The general conclusion to which we are forced by our consideration of the nature and results of discovery, is that there are no absolute criteria by means of which we can decide what part may have been played by independent discoveries in the production of similarities in human culture. We are safe in assuming that simple primary discoveries, such as that of the plasticity of clay, or the malleability of copper and gold, may or must have been made more than once, but we are equally safe in assuming that, with every step beyond the first, an independent repetition of the same sequence becomes more and more unlikely, and also that the more difficult a single discovery and the more difficult its application, the less likely is its fruitful repetition. It would be no great coincidence if in our own day a European and an American independently produced a similar mixture or alloy of iron with another metal, since they would in effect be carrying on a joint research with a common starting-point, and with the knowledge that others had succeeded in researches of a like nature. But a belief in the independent origin of bronze in the Old World and the New, in prehistoric times, would seem to depend upon a faith in coincidence. The discoveries of the nature and uses of copper, followed much later by the production of bronze, turned out to be a critical event in the history of man in the Old World, as we see on looking backward, but to those concerned it was at first merely a matter of a glittering material for ornaments, and later of slightly more efficient tools and weapons than those of stone. Eventually new types were evolved, of still greater efficiency, but these were not within the range of vision of the early metal-workers. There was no environmental compulsion, and economic inducement only came into play after the essential steps had been taken. It was as a malleable stone that copper acquired an importance which made the discovery of its fusibility a great event. The early metal-worker was not pushed along the path of progress; he did not know it was a path, and there were no sign-posts to prevent him from straying from his course, nor had he any vision of a metallurgical paradise, such as we inhabit. He was just as likely to take the wrong turn as the right one, and never strike the route again—he had no idea of where he was going, nor of any particular reason for getting there. His desire to repeat or imitate a successful achievement was conditioned by the appearance of new and better results arising out of the chances of empiricism, and there is no evidence of any other directive faculty or tendency. That bronze was ever produced at all is sufficiently surprising, and if the evolutionary sequence in the Old World was mirrored in the New, our wonderment is more than doubled.

I do not suggest that the sole factor in any case of assumed independent evolution of technique must be coincidence, or that coincidence on a small scale is unlikely, but I would argue that coincidence must have played a predominant part, and that the further we get away from simple cases and short sequences the bigger the draft on coincidence, until it becomes of incredible magnitude. But incredulity is not enough, unless it rests upon a knowledge of what it is that is incredible, and detailed analysis alone can supply this knowledge. It must be remembered, also, that discovery-complexes may depend very greatly for their development upon the evolution of artificial means or inventions. As examples may be cited the plough in agriculture, the kiln in pottery-making, and the loom in weaving. Although there are many things the hand can do unaided, there are others that it cannot do at all, and yet others that are better done by the help of tools or mechanisms. The analysis of discovery-complexes must therefore be made with reference to the inventions that have aided the process of evolution, or have rendered it possible, and it must be borne in mind that there are all grades of dependence upon artificial aids, simple or complex. Whilst distinguishing methods from means, that is to say, the means must not be disregarded, even though they are the objects of especial study as inventions, to which we may now turn.

## Inventions.

The general recognition of the gradual character of the evolution of human artefacts—so obvious even under modern conditions—makes it unnecessary to dwell upon it. There are, however, no accepted definitions of the kinds of developmental changes or modifications, viewed either objectively or subjectively. If the initial steps in the evolution of simple artefacts are due to discovery alone, as already suggested, we have to decide in what way such steps differ from those which can be called inventive, if difference there is; and also to enquire into the nature of any other factors that may play a part in evolution. Moreover, if we call all artefacts inventions, there is no term left for single inventive steps. If, for example, the outrigger-canoe or the Chinese repeating crossbow is an invention, what distinctive term can we apply to the steps by which it has evolved, assuming these can be identified as due to individual discoveries, or to true inventions, whatever these may be? There is also the possibility—or the certainty—that changes may occur which are due neither to discovery nor invention, but to some slower and more gradual process. If we are to have a clear understanding of the evolution of human artefacts these points need clearing up, if only on a basis of hypothesis. I have put forward elsewhere some tentative proposals, and although I must not recapitulate in detail, it is necessary to cover some of the ground again.

If we begin with implements which were amongst the first to achieve an individuality of their own, those made of stone are for many reasons the most convenient for our purpose. We can scarcely doubt that accident, perhaps often repeated, led to the intentional breaking of stones for the production of edged or pointed implements, which gradually evolved into standardised forms. To summarise a sequence of events that arose out of more than one discovery, we may say that the first artificially-shaped stone implement was due to the application of a discovery, and since the artificial shaping was a definite and decisive step, it may be called a *mutation*. Since also it was the first intentional conversion of a particular kind of natural object or material into a kind of artefact, it was a primary mutation. From such a mutation, perhaps occurring more than once, developed the many forms of stone implements with which we are familiar. A mutation of this or any other type is an abrupt and discontinuous step, contrasting with changes which are trivial in character, and which produce their effect by a process of summation. For these the name of *variations* is appropriate.

In the shaping of the early types everyone agrees that forms such as hand-axes and 'ovates' were not preconceived as models to be aimed at; they must have been the end results of a gradual process of change, in which the shapes emerged through an opportunist selection and imitation of those which were most convenient and effective. This was in effect a process of variation, casual at first but later becoming more selective and adaptive.

Simple stone implements are thus to be traced to a primary mutation, a sudden jump, followed by variation, a gradual process. They were in most cases made for hammering or crushing, for cutting, or for piercing, three functional duties which lie at the root of a large part of man's coercion of materials. They were evolved, not invented to serve specific purposes. Similarly, beginning with a primary mutation in each case, the fighting-stick became the club, with its immense variety of form; the digging-stick became the spade, and perhaps the spear, with its derivative the arrow; the pick became the hoe and finally the plough;

the hollow reed became the blow-tube. Even before the more evolved implements of these classes had got beyond their one-piece character, however, other factors than variation sometimes intervened. That is to say, whilst the field of variation is that of form, it is not in exclusive possession of this field. For the moment we may pursue the subject of form itself a little further, leaving factors on one side.

The stone hand-axe of Palæolithic type is found in many parts of the Old World, and although it has considerable range of form, it is sufficiently standardised to be regarded as a type. Along with it, but of later origin, are often found derived forms with an edge all round, which eventually appear as the fully-evolved sharp-rimmed ovate. It is a significant and perhaps curious fact that the general tendency of archæologists appears to be towards a tacit or expressed assumption that the presence of these two types, in regions as far apart as Western Europe, Egypt, South Africa and India, is due not to any common faculty of the human mind, nor to any directional pressure of human needs, but to a spread of people who had, as it happened, evolved the two implements in question. And yet they are simple types.

Similarly, when flakes having the characters of Mousterian points, or implements resembling Aurignacian gravers, are found in Eastern Africa, there appears to be no widespread desire to claim them as examples of the manner in which man independently arrives at similar types of implements through the working of his psychic unity. On the contrary, the immediate result is theory and speculation as to how and when Mousterian and Aurignacian man respectively reached the regions in which the implements are found. Not all archæologists take up this position without reserve, but there is no doubt that there is a general, if sometimes only provisional, adoption of the diffusionist and historical point of view. At the back of this must lie the belief that a similarity of form in flint implements affords, at least in some cases, and to many archæologists, sufficient grounds for theories of human spread. We must recognise, however, that in the case of stone implements the belief is usually based on something more than a single coincidence in type-form. There may be two types (such as the hand-axe and the ovate) occurring together, or more than two, and the improbability of independent evolution increases very greatly with each type added. This is true even when the artefacts are simple, but the weight of the argument from numbers is still greater when the artefacts are more varied and more complex; those who employ it in the case of stone implements in the Old World cannot logically deny its relevance to the occurrence of such appliances as spindles, looms, blow-tubes, pyramids, plank-boats and many other things, in the Old World and the New.

A point of considerable importance in the comparison of stone implements lies in the fact that it is not form alone that is invoked to prove identity. No other types of artefacts are subjected to such fervid concentration as are these, and a typical Mousterian 'point,' for example, must have an outline that does not fall outside a certain range of variation; it should be unchipped, or only slightly chipped, on the bulbar face, and it should show facets on the striking platform. There is involved in the definition an intermingling of characters of form, with others which are the outcome of method or technique, whereas a digging-stick, a paddle and many other one-piece implements, are defined by form alone. Stone implements are, in fact, often judged by the technique of their workmanship as well as by their general form, and the passion they inspire sometimes leads to an excess of typological zeal which makes classification an end rather than a means. But flint is wilful as well as hard, and the hand of the flint worker was often forced. He knew the form he aimed at and the flaking-angle he preferred, but he could not keep his material under absolute control, and some so-called types are flint-made as much as man-made.

Most of our difficulties in the search for criteria of form, in relation to independent evolution, arise out of the problems of separating the functional from the incidental and conventional. A digging-stick must have a point or a flattened end to penetrate the soil; a paddle must have a blade; a sword must have a hilt which can be grasped. But a digging-stick need not have a foot-rest; a paddle need not have a crutch-handle; a sword need not have a hand-guard. We can say that, speaking generally, a foot-rest on a digging-stick has a greater functional value than a crutch-handle on a paddle, but it is impossible to determine whether one of these is more likely than the other to give us instances of independent evolution. The questions as to how and why such features first appeared cannot be answered with any certainty. But if we consider features that are functionally meaningless, or unimportant, we are more often justified in the assumption that they are probably due to variation, and are less likely to be duplicated independently. As a matter of well-known fact, there are innumerable simple characters of form which are so typical of certain regions that they are diagnostic of the place of origin. Knobbed clubs, for example, are not uncommon, but the knob flattened distally and proximally is found only on African clubs, and in these 'knobkerries' the knob is also situated laterally to the axis of the weapon. The form of the knob itself is sufficient for determination, and this is true of the knobs, or heads, of many wooden clubs from the Pacific; whilst if we consider the whole form of a club from any part of the world, it is rarely indeed that there is any doubt as to provenance. There has been independent evolution, not always variational, but it has led to divergence and not to similarity. It is largely upon this result of independent evolution in producing characteristic features of form, that we are able to assign a place of origin to a wooden club, or other one-piece artefact, that lacks credentials.

On the other hand, there are certain types of clubs from Fiji, Tonga, and Samoa, that approach each other so closely as to present some difficulty except to the expert; but this is due to the recent intercourse that has taken place between the groups. Such facts as these are in direct opposition to the view that similarity of form is not to be trusted as evidence of diffusion. That similarities may occur through coincidence is not to be doubted, and perhaps this is the case, for example, with the broad Andamanese bow and an East African type which approaches it rather closely. It may be that the two forms had a common origin, but it is also possible that they were derived independently from broad bamboo bow-staves, the central narrowing for a grip, and the thinning out of the two ends, being features that they have in common with other bow-staves. In estimating the chances for and against independent origin in any instance, it is essential to make allowance on the one hand for functional necessities, and on the other for the compulsion of material. The more coercive have been the influences of these two kinds, the more necessary is it to seek for minor differences which have been produced by the free action of variation.

This brief discussion of the significance of form, as distinct from construction, leads us only to the conclusion that there are no criteria of an absolute character which can be applied to decide a case for or against independent evolution; only by close comparison and analysis of every case on its own merits can we justify our faith. It remains true, however, that there is overwhelming evidence to prove that the normal result of independent variation in form is diversity and not similarity.

A factor in material evolution which is usually recognised, but perhaps also usually underestimated, is substitution or translation. We may note in passing that substitution may be dictated by scarcity or absence of a material that has been commonly employed, or by a chance discovery that one material may be advantageously substituted for another, or that it may occur as a result of search for better materials. The first two reasons are clearly those which must have predominated in early days, whilst the last became operative only under conditions of experimental research—conditions which no doubt prevailed in some degree whenever well-fed communities developed wealth and leisure. Prosperity, not need, is the mother of invention, or at least the fairy god-mother. From our point of view the importance of substitution lay in its power as a factor in discovery and invention through the opportunist reaction of man to the ideas that came to him unsought. In employing a new material for old purposes, it is clear that the preliminary step would be a recognition of sufficient similarity to suggest the possibility of substitution. The endeavour to treat the new material in the same way as the old would inevitably in many cases lead to the discovery that it reacted in a different way, and a new line of evolution was opened up. This is sufficiently obvious in perhaps the most important of all substitutions—that of metal for stone. The result was not merely the production of better tools and weapons, but an expansion of man's knowledge which gave him a new insight into the possibilities of the raw materials of his environment. It is not too much to say that substitution has been a fundamental factor in discovery and invention from the earliest times, but it must be emphasised that coincidence in a particular substitution cannot be claimed as independent invention.

If a primary mutation was due to one or more discoveries made in relation to the behaviour of natural objects or materials, it is not unreasonable to suppose that similar discoveries concerning artefacts may have led to other mutations. As a hypothetical case let us consider the origin of the bamboo spear-thrower of New Guinea, which has a socket for the spear in place of the peg that is present on almost all other spear-throwers. We may suppose that this implement was derived from the ordinary type made of wood, such as is in common use in Australia, and that the first change was that of translation into bamboo. The carved or attached peg for the spear was at first retained, but during the manufacture or use of the appliance it would be easy for the discovery to be made that the bamboo rod readily supplied a natural socket, which would serve in place of the peg for effecting the discharge of the spear. Then followed the intentional construction of spear-throwers with socket instead of peg, and we may call the step—which may or may not be regarded as a progressive step—a free-mutation. If it happened as the result of a discovery, as suggested, it was free from any influences from other implements or mechanisms. It is impossible to be sure that no such outside influence was at work, but the step being decisive and discontinuous it was at any rate a mutational step, and not variational. We may assume with some degree of probability that free-mutation initiated the provision of the foot-rest on the digging-stick, a grip or handle on the stone knife, the detaching head of the spear to produce the harpoon, the sling-hafting of the flail, and that it was concerned in the origin of other types of hafting, as well as in the modifications of components to adapt or improve them for combination in a constructed artefact. Primary mutation, followed by variations which led to change in form, stimulated by discoveries in relation to method, and often influenced by substitution, led to other discoveries which could be applied for the improvement of the form or construction of artefacts, and these applied discoveries may be called free-mutations. In this way there were produced many implements of a simple character, some having form alone, others showing construction and often mechanism.

So far we have identified no inventive foresight of a kind that would lead directly to the subjective preconception of a new or improved type of implement, differing in any important respect from what had gone before. We know, however, that in our own times the inventor designs his products in advance. This is not to say that at some stage in the evolution of material culture there was a sudden change in the mentality of man. Discovery and imitation lay at the root of all his methods, initiated all his artefacts, and led to the appearance of free-mutations, but when he had established a variety of artefacts that had construction as well as form, he began that process of transfer and adaptation of structural and mechanical characters for which I have suggested the term cross-mutation. These, like the other mutations already defined, were abrupt and discontinuous changes which could not have arisen gradually by variation, but, unlike other mutations, they owed their origin to a combination of features, or an application of 'principles,' which had evolved in independence. The process corresponds to what Mr. Henry Balfour has laid great stress upon as hybridisation. It is a process involving foresight, in predicting the possibility of combination, and ingenuity in effecting it. A cross-mutation is a true invention, a product of the inventive faculty, unaffected by discovery in its first conception, though the inventor nowadays may need to make discoveries in relation to materials and methods before he can test the viability of his inventive forecast. Through it all runs the opportunist thread that may be plainly seen in the historical retrospect. Combinations for inventive purposes can only occur to the mind in an artificial environment in which the two (or more) elements of the combination are at hand, and these may have been evolved in entirely different artefacts or contexts.

Accepting these arguments as valid, an invention proper—as distinct from our loose application of the term to shaped and constructed artefacts in general—may be defined as a single mutational step which owes its origin not to discovery, but to a combining of structures or devices already in existence. The result is objectively a structural combination, which is preceded subjectively by the action of the mind in recognising the advantages and the possibilities of the hybridisation, and in thinking out the method of effecting it.

We may now inquire whether we are any nearer to the establishment of criteria of independent evolution in respect of artefacts—whether coincidence through mutation is more or less likely to occur than coincidence through variation in form. Except in so far as variation is limited by the form and nature of materials employed, and by the functional efficiency of the completed artefact, it has a very wide range, and it may give rise to features that are functionally meaningless, or even detrimental. Mutations, on the other hand, are much more closely determined by functional considerations; they result, as we have just noted, either from chance discovery that a modification of a particular kind is advantageous (free-mutations) or from a prediction that a modification by transfer and combination may be of value (cross-mutations). Useless mutations are much less likely to survive, even if hit upon, than are useless variations.

It is one of the provinces of variation to add to the effectiveness of characters which have appeared as a result of mutation, but it may also develop characters in such a direction as to lead towards, or away from, another mutation. The variational development of the push-quern into the saddle-quern was clearly away from any line of evolution that could lead to the mutational step, or steps, that produced the rotary quern. Variations and mutations react upon each other, but the process is not one which leads to parallelism or convergence.

As regards mutational criteria, it is evident that the occurrence of free-mutations depends upon the state of development of the artefact in which they appear. They may be primarily due to some feature of construction or material which gives rise to a chance suggestion for improvement, or this may arise through substitution, and their adoption may depend upon a variety of social as well as material conditions. In the case of cross-mutations there can only be a combination of features or devices if these are present in the same region at the same time; and where the case is one of simple adaptational transfer, there must be present in the appliance that profits by the mutation some feature of structure or working which suggests the possibility.

In a further treatment of the subject of inventions it would have been desirable to discuss such factors as change of function, change in method of use, and numerical mutation, but sufficient has been said to indicate the analytical method of approach.

If we take into account all the factors involved in the development of artefacts, even in simple cases, independent evolution involves coincidences, few or many. It is also clear that the further the artefact from the primary mutation which began it, that is to say, the longer the series of variational and mutational changes that has been undergone, the bigger the draft on coincidence. Nevertheless we are still unable to point to definite mutational criteria, and to say that it is impossible that some particular mutation—and especially a primary mutation—should have occurred more than once. But a primary mutation is only a first step. As in the case of discovery-complexes and discovery-products, it is necessary to consider each case on its merits, and endeavour to identify all the links in the evolutionary chain, or at least to determine whether there are few or many.

The general case against identity or similarity by independent evolution is, however, overwhelming, as is very widely admitted at the present day, but to a large extent it is based on cumulative circumstantial evidence, since there are grave difficulties in finding proofs that leave no loophole for the defence. This is especially true in relation to the question of New World origins. The American dread of Old World entanglements has given rise to an anthropological Monroe doctrine, and there are many who are prepared to postulate independent evolution in America on a scale which might give pause to the most patriotic evolutionist. No agreement on the general question can be reached whilst divergent views are held on this particular issue, and although the actual proofs of diffusion across the Pacific must come from other sources—or, as some think, have already come—I may be permitted to close this address with a brief contribution to the discussion.

### America.

The constituent elements of the material culture of the American Indians reveal, as is well known, many correspondences with Old World products of discovery and invention. Even in its highest grades, however, the general level was never as high as that of Egypt and Mesopotamia before 3000 B.C., and in many respects it was much lower. Taking a few culture-traits in detail, we may note that there was cereal culture with irrigation, but without the plough; corn-grinding with the push-quern but without the rotary quern; pottery-making with the use of manufactured moulds, but without the wheel; decoration with slips and paints but no mineral glazes; metal-working without iron. Considering tools and weapons only, we find that in stone, copper, or bronze, most of the important types of the Old World down to Chalcolithic times and later, were represented, and that all the chief methods of hafting were employed. But the hoe never became the plough; the pestle and mortar did not develop the mechanical devices found especially in Asia; the free bow and the sinew-backed bow were used, but not the highly-developed composite bow or the crossbow; the dagger never became the bronze sword; all the Asiatic methods of making the blow-tube were known, with the exception of that of boring out a solid rod of hard wood; the sledge and the travois never ran on wheels.

These are cases in which agreement with the Old World occurs up to a point and then breaks down. There are, of course, many other correspondences (and not in material culture alone) which are more or less precise, and some of them are especially reminiscent of Eastern Asia. The blow-tube has already been mentioned, but to this we may add cotton cultivation, tie-dyeing, gauze-weaving, mosaic-work, tripod supports for pottery, slat armour, and quipus. Some of these are not restricted in their distribution in the Old World, and to them we may add plank-boats, the loom, pan pipes, flageolets, steelyards and scales, and the *cire perdue* process of casting bronze.

Amongst those who believe in the independent development of

American culture, there is some difference of opinion as to the equipment of the original immigrants, coming across, it is supposed, from North-East Asia. The chief items mentioned by Kroeber (in 1923), for example, are the dog, bow, harpoon, fire-drill, woven and twined basketry, perhaps the spear-thrower, stone implements and the beginning of the grinding of stone; as perhaps introduced later from Asia, he mentions the skin-boat, sinew-backed bow, tailored skin-clothing, coiled basketry. It will be realised that for such a meagre initial outfit to expand into the complexity of the higher Amerind cultures, there must have prevailed a degree of opportunism and inventiveness comparable with that of the peoples of the Old World; and that for the results to be so closely similar, the 'common faculties' of man must have been in good working order.

In his recent Huxley Lecture, Nordenskiöld discussed this question of the inventiveness of the American Indian from a standpoint which did not exclude the possibility of outside influence. He was, in fact, in search of cases in which the evidence of indigenous origin seemed worthy of acceptance. It is not possible here to discuss the culture-traits he regards as American in origin and development, by far the most important of which are agriculture, pottery-making, and bronze-working, but it must be observed that in the main the case he unintentionally makes is that of the *non*-inventiveness of the American Indian, with regard to tools and other artefacts. Those which he accepts as indigenous in the New World are mainly either of simple types, very little removed from their beginnings, or are the results of substitution and other factors in which invention is not involved. We may exclude as irrelevant the discoveries of native food-plants, for reasons already given.

The idea of the progressiveness of the American Indian depends in reality upon the assumption that from the hunting and food-gathering culture of the first immigrants there were developed, without extraneous assistance, the high cultures of Mexico, Central America, and Peru. As we have just noted, however, his apparent progress, whilst advancing, on this assumption, more or less parallel with that of the Old World in numerous culture-traits, stopped abruptly at many points which were in the Old World over-run. He missed mutations that were essential to further parallelism, but by dint of discovery, substitution, and variation, he was led to the production of those superficial differences that characterise many American inventions and discovery-complexes. There was little that he did better than it was done elsewhere, and much that he never did at all. If on his own account he made the discoveries leading to such achievements as agriculture, bronze-working, pottery-making, and if he invented the hoe, the push-quern, the pestle and mortar, the loom, the corbelled arch, and much else, how was it that his genius failed him when it might have carried him still further along the lines determined by his 'common faculties'? Why was it that it was always he, and not his Old World rival, who fell behind in the race? Why is it, also, that there is so much in the higher, as well as the lower, American cultures that compel comparison with Eastern Asia, and not with, let us say, Neolithic or Bronze Age Western Europe? It would seem that we have to choose between explaining and explaining away, or, more precisely, between diffusion and the common faculties of man.

Without venturing further into this side of the question, there is one final point on which I may touch. It is customary for those who argue against the theory of post-Neolithic outside contacts with America, to direct their shafts against the view that the cultural influences came by the stepping-stones of the Pacific Islands. It is generally admitted that the Pacific has been colonised by more than one stream of immigrants from Asia, but that cultural influences of any importance reached America by this route is probably allowed by few. For my own part, although I would not exclude the possibility of some slight infiltration through Oceania, I would look to Eastern and South-Eastern Asia, and its islands, as the immediate source of sporadic incursions, extending over centuries, which added at intervals immigrants allied in physical and mental type, and possessed of cultures increasingly advanced, to the original stock of the New World. Knowledge of 'God's own country' lying to the eastward, began in North-East Asia, and may well have extended southward with the centuries. The further south the adventurers came from, the later the intrusive culture, and the higher its level. The more recent southern immigrants, perhaps possessed of ocean-going vessels, may have struck straight across to the New World they already knew of, or they may have coasted northwards along the shores and islands of Asia, and made a shorter crossing. They chose their landing places in the warmer latitudes such as they were accustomed to, leaving the northern coasts to the earlier and more hardy navigators who had preceded them. There is much technological and other evidence to support this view, but it cannot even be outlined in this address.

This is my last digression, and it is made to emphasise my strong belief that the culture of the American Indian is a derived culture, in all essentials, and that an explanation is to be sought in frequent contact with Asia, from the time of the first immigrants down to an uncertain period, but perhaps continuing into the early centuries of the Christian era, as Prof. Elliot Smith contends. We are only at the beginning of the study of possible or probable early relationships between Asia and America, and until we have a much more detailed knowledge of the archæology of both these continents, we must make the best of such material as there is, and of such lines of argument as may be valid or congenial.

## Conclusion.

To conclude, the views and arguments I have put forward may seem moderate to some, extreme to others, and I claim no more for them than that they may help to a better understanding of the mode of evolution of man's material culture, and to a fuller appreciation of the large assumptions upon which is based the belief in the frequency of independent evolution. Whatever may be our individual position in the diffusionist controversy, we must all admit, I think, that man has always and everywhere an environmental mind, and that he can only move forward on prolongations of the lines that can be seen by looking backward. 'One thing leads to another' is man's ancestral motto.

SECTION I.—PHYSIOLOGY.

# THE SYNTHETIC ACTIVITIES OF THE CELL.

ADDRESS BY

PROF. H. S. RAPER, C.B.E., F.R.S.,

PRESIDENT OF THE SECTION.

It has long been the custom to divide the chemical transformations which occur in living animals into anabolic and catabolic. The anabolic are associated with those processes of restitution which occur after functional activity and the catabolic with those accompanying the functional activity itself. This distinction must not be taken to imply that they are independent. In certain instances we know that the reverse is true. Hence the synthesis of one substance may require the concurrent degradation of another.

A review of present-day knowledge of chemical processes in the cell reveals the undoubted fact that physiologists have been more successful in investigating the phenomena of catabolism and chemical degradation in the cell than the processes of synthesis, although during life they may and do occur together. This might be expected. Catabolic processes may persist long after the cell has ceased to possess those fundamental properties which we associate with life. They may be demonstrated in cells which have been submitted to mechanical injury so as to destroy their structure or after various methods of treatment have been applied to extract the cell contents. Their investigation has thus proved relatively simple because restrictions as to experimental method have not been severe. But the anabolic processes of the cell are essentially those which occur only during life. To study them successfully demands that the cells in which they are taking place must be kept alive during the investigation. This in itself implies that the range within which the factors determining their progress or cessation can operate is probably as restricted as the conditions determining the life of the cell itself. It is not surprising then that with such severe limitations we have learned up to the present a great deal more about degradation than synthesis in the cell. Nevertheless, the synthetic activities of the cell have a fascination which in itself makes an attempt to discuss them attractive. And I must confess that it was partly for that reason that I chose the subject of this address. It was also my good fortune before proceeding to the study of physiology to pass through a school of chemistry and this may explain in part my predilection for the synthetic processes of living organisms. The chemist, if I am not mistaken, is more proud of his achievements in the synthetic field than in the analytic. There is more artistry about them. They are examples of the creative instinct by means of which man from the earliest days has striven to conquer Nature. But there is another reason for my choice of the subject. A little more than a hundred years ago the first synthesis of a product of animal life from inorganic matter—the production of urea from ammonium cyanate—was described by Wöhler. It seemed to me, therefore, an appropriate time to consider the bearing of the achievements of organic chemical synthesis on our understanding of synthetic processes in the living animal.

Wöhler's discovery did not bear immediate fruit. It required the genius of Kekulé with his hypothesis of the tetravalency of carbon and the mode of linking of carbon atoms to point the way to a rational method of illustrating the structure of carbon compounds, an organ of thought as valuable as language itself.

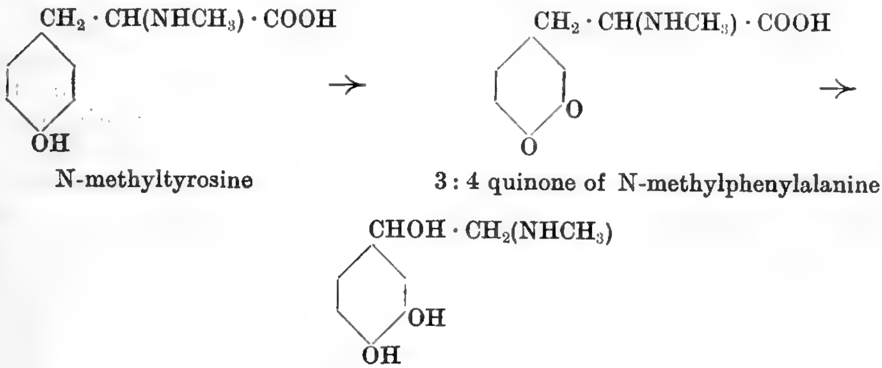
Once the structure of compounds could be pictured objectively it became possible to build them up from others of known structure. How aptly this exemplifies the dependence of progress on man's ability to illustrate his conceptions pictorially! Kekulé's hypothesis came thirty years after Wöhler's synthesis, but from that time on we have seen and acclaimed the discovery of the structure, in many instances confirmed by synthesis, of a large number of organic substances which are produced by living organisms. This discovery of the structure of products of cellular activity is of prime importance to the physiologist. Structure is the point from which we must always start when trying to find out how any particular substance is made in the cell. For this contribution to the solution of our problems of synthesis, if for no other, we owe organic chemistry a great debt. But the methods used by the chemist in synthetic processes have usually been such as could have no place in the living cell with its extreme sensitiveness to conditions such as temperature, reaction of the environment and osmotic pressure, to mention only the more obvious ones. The use of high temperatures to bring about activation of the reacting substances is inadmissible. Activation in the living cell must be achieved by other means. Acids and bases, except in such strength as will bring about only small changes in hydrion concentration, cannot be employed. All reactions must take place in an aqueous medium. With these and other limitations it can readily be appreciated that a few only of the usual processes of organic synthesis can be considered as operative in the cell.

It is not possible within the scope of this address to consider all the aspects of synthetic activity which are found in living organisms. Each one presents almost a distinct problem. I propose, therefore, to deal only with the animal cell, since its range of synthetic activity is narrower than that of vegetable cells and has been more closely studied, and only with the synthesis of a few substances which exemplify certain general problems.

Unlike the Chemist, the animal cell has a very limited choice of raw materials from which synthesis must start. These are the components of the common foodstuffs. When they have undergone the preliminary processes of digestion they provide in all about thirty substances which may be regarded as available for the building up of new compounds by the cell. They consist of about twenty amino-acids, two purine bases, three pyrimidine bases, three hexoses, glycerol and higher fatty acids. It is true that there may be other, hitherto undiscovered components of foodstuffs, which are important as raw material for synthesis in the animal cell, but they can be but few in number and present in small amount. Given this list of raw materials can we in every instance indicate which is the likely starting point for the synthesis of substances whose constitution is known or partly known? This question must still be answered in the negative for such well-known products as cholesterol and the unconjugated acids of bile. It is possible that the experiments of Channon who demonstrated that addition of spinacene to the diet of rats caused the cholesterol content of the liver to increase to double its usual amount may afford a clue to the origin of this substance, but it is difficult to conceive of spinacene being the raw material for this synthesis in the herbivorous animal. As for the bile acids, we are in complete ignorance of the substance or substances from which they are built up. Although closely allied in structure to cholesterol it is clear from the careful experiments of Enderlen, Thannhauser and Jenke, who failed to observe any increase in the production of bile acids in dogs after long periods in which cholesterol was added to the diet, that they are not produced from this substance.

With the purine bases, which are components of nuclear material and therefore present in all cells, this question of indicating the probable raw material for their synthesis must also be answered in the negative, but with less emphasis than in the case of cholesterol and the bile acids. The amino-acid histidine, which contains a five-membered ring similar to that found in the purine bases, is their most likely precursor on structural grounds. But we have as yet no experimental evidence that indicates clearly their origin from this amino-acid in the body. Until this has been proved it is perhaps useless to speculate as to the chemical processes involved in the transformation. The synthesis of cholesterol, that of the bile acids and of purine bases are therefore still problems in the purely chemical sense. When the chemist has indicated their likely solution it will be the task of physiology to find out the mechanism by which the cell brings about the necessary chemical processes.

An example of a synthetic product which bears a fairly close structural relationship to two of the amino acids which are found in proteins is adrenaline. These two amino acids, phenylalanine and tyrosine have practically the same carbon skeleton as adrenaline. Either of them might give rise to adrenaline by successive oxidation, methylation of the amino group and loss of carbon dioxide. That a part of the necessary oxidation process can be brought about by means of an oxidising enzyme has already been demonstrated. This enzyme, tyrosinase, will oxidise tyrosine to 3:4-dihydroxyphenylalanine. It is therefore conceivable that the first stage of the synthesis from tyrosine occurs in this way in the cell. The enzyme does not attack phenylalanine so that this seems less likely as a precursor than tyrosine although there is evidence that in other respects the metabolism of these two amino acids is closely related. In N-methyltyrosine we have an amino acid which is more nearly related to adrenaline than tyrosine, since in both the amino group is methylated. By the introduction of two atoms of oxygen in the appropriate positions and loss of carbon dioxide it should give rise to adrenaline. It is of considerable interest that on oxidation with tyrosinase a small amount of pressor substance is obtained. It is not possible to say as yet whether this is adrenaline. The amount produced represents only a small proportion of the reaction products, but it brings nearer the possibility that a methylated tyrosine by simple oxidation under the influence of an enzyme might give rise to adrenaline by the following scheme of reactions :—




N-methyltyrosine — 3 : 4 quinone of N-methylphenylalanine — Adrenaline

If, however, we consider how we may find out whether a process of this kind, operating in the cells of the adrenal gland under specific conditions, gives rise to adrenaline, many difficulties appear. One method of approach would be to find the conditions under which a production of adrenaline by the medullary cells of the gland occurs and then under these conditions determine whether introduction of tyrosine or possible intermediate products in the synthesis leads to an increased formation. But these conditions are not as yet defined. Attempts have been made by exposing the glands freshly removed from the body to solutions of tyrosine but no production of adrenaline has been demonstrated. Neither is there any evidence that the cells of the gland can either oxidise tyrosine in the same way that the enzyme tyrosinase does, or methylate it. The possibility has to be borne in mind also that even if tyrosine be the mother substance of adrenaline the processes requisite to produce adrenaline from it may not all take place in the adrenal gland. In that case the effect of hypothetical intermediate products on adrenaline formation would have to be tried. But methods of this kind are relatively crude. If they fail it does not necessarily mean that the hypothesis as to the gross chemical mechanism is wrong. When we remember that the oxidation of one substance may only take place if another is reduced ; that a reaction taking place in one compound may only be possible when some other reaction takes place along side it, in other words, that in the living cell there is a continuous and complex interplay of chemical reactions ; then it is not surprising that the discovery of the mechanisms by which adrenaline is formed, although a simple problem at first sight, is probably in reality very complicated.

The case of thyroxine is comparable with that of adrenaline. Thyroxine is a relatively simple chemical substance which could conceivably be produced by the oxidation of diiodotyrosine. The last named compound has recently been shown to be present in the thyroid gland and this makes the presumption that it is the mother substance of thyroxine all the stronger. Yet it has not been demonstrated either *in vitro* or *in vivo* that thyroxine may be produced from diiodotyrosine.

These two instances of adrenaline and thyroxine show that even when there is a reasonable presumption that we know the raw material from which the synthesis in the cell starts and the chemical processes concerned the proof that our hypotheses are correct is far from simple.

We may now pass on to consider a process in which the raw material is known with certainty but the chemical reactions by which the synthesis takes place are relatively obscure. I refer to the production of fat from carbohydrate. Lawes and Gilbert in their classical experiments at Rothamsted on the fattening of farm stock, showed indubitably that animals can produce fat from starch. It has been confirmed since by others. Since the starch is converted into glucose in the alimentary canal prior to absorption we may consider glucose as the starting point of the synthesis.

It involves the production from hexose units of straight carbon chains of 16, 18 and 20 or more carbon atoms such as are found in the naturally-occurring, higher fatty acids. The carbon atoms are present in even numbers and the carbon chains may be completely saturated or partly unsaturated. Any series of reactions which is put forward to explain this synthesis must therefore take into account these elementary facts if no others. The origin from carbohydrate at first sight suggests the coupling of hexose units end to end although this would only suffice to explain the production of acids containing multiples of six carbon atoms such as stearic and oleic acids. Such a scheme was put forward many years ago by Emil Fischer but has not found general acceptance. Apart from the difficulty of explaining the production of acids that do not have eighteen but some other even number of carbon atoms, it has never yet been shown that hexose molecules unite end to end using any of the usual methods which bring about the condensation of aldehydes. It is therefore improbable from the purely chemical point of view.

A more likely chemical explanation of the origin of the fatty acids is that they are built up, two carbon atoms at a time from some simple, reactive substance which is first produced by degradation of glucose. Acetaldehyde and pyruvic acid have both been suggested as probable participants in a reaction of this kind, the former condensing with itself, as in the well known aldol condensation, the latter either with acetaldehyde or with some higher aldehyde containing an even number of carbon atoms produced in the earlier stages of the reaction. By both of these methods it has been shown that *in vitro* aldehydes with an even number of carbon atoms in a straight chain can be built up step by step and these by oxidation can be readily converted into the corresponding fatty acids. Unsaturated linkages in the chain may be produced by either method so that this requirement in the hypothetical synthetic method is also satisfied. Further, the condensation takes place in weakly alkaline solution or under the catalytic influence of certain organic bases, so that drastic treatment is not necessary to bring about the reaction. So far this evidence for the mechanism of synthesis of the fatty acids is purely chemical and the grounds on which it can be put forward are largely chemical ones. Is there any physiological support for this scheme ? Studies of the synthesis of fat in animals have yielded very little so far. It was observed many years ago by Leathes that the fat in the liver of some animals might increase in amount if the organ were kept warm and under sterile conditions for a few days after its removal from the body. The addition of glycogen or glucose does not however have any effect on this increase in fat content. The process appears to be determined by something other than a mere excess of carbohydrate, but what this is is not known. It is however known that acetaldehyde and pyruvic acid, the intermediates postulated in the hypothesis just mentioned, can be produced in the body. Neuberg and Gottschalk detected acetaldehyde as a product of decarboxylation of pyruvic acid in the liver and recently Hahn, Fischbach and Haarmann have shown that under anærobic conditions pyruvic acid may be produced in muscle from lactic acid. But there the question rests for the present so far as higher animals are concerned. When we know what the conditions are which set the process of fat synthesis going, and when we are able to reproduce them at will in animals, it may be possible to determine what are the intermediate substances concerned.

More success has been achieved by a study of the formation of fatty acids in microorganisms, some of which possess this faculty in a remarkable degree. Bacteria which form butyric acid from glucose have been found to produce in addition both lactic acid and acetaldehyde. These same bacteria will also produce butyric acid from pyruvic aldol though not from aldehyde ammonia, aldol or pyruvic acid itself. Neuberg and Arinstein, who investigated this type of fermentation, conclude that pyruvic aldol is the precursor of the butyric acid when it is formed from carbohydrate. This does not negative its production from acetaldehyde, since the addition of sulphites to the fermenting liquid causes a fixation of acetaldehyde and inhibits the production of butyric acid, thus suggesting that acetaldehyde takes some part in the process. But it has also been shown that in this so-called butyric fermentation fatty acids containing an even number of carbon atoms higher in the series than butyric are formed, namely, hexoic and octoic acids. This makes it appear probable that the processes by which these lower members of the series of fatty acids are formed in bacterial fermentation may be the same as those by which higher members of the series are formed in animals. The production of fat is a process which is common to a great variety of living organisms both simple and complex, animal and vegetable. It is not unlikely therefore to be carried out by the same series of chemical reactions wherever it is found.

The intensive study during the last few years of the processes of alcoholic fermentation and the chemical events which occur in muscular contraction has revealed such close similarities that we are becoming forced to accept the view that certain fundamental activities of the living cell, whether animal or vegetable, may be carried out by an almost identical mechanism. It may therefore be that we shall eventually discover the reactions responsible for the synthesis of fat in animals by investigating the processes by which it occurs in vegetable forms, such as bacteria or moulds. Before leaving the subject of fat synthesis it is worth while to point out the economy of the suggested process of synthesis from lactic acid by way of acetaldehyde. It is a good illustration of the kind of serial reactions which one expects to find in syntheses in the cell and involves only the loss of water and carbon dioxide.

(1) $CH_3 \cdot CHOH \cdot COOH \rightarrow CH_3 \cdot CO \cdot COOH + H_2$
(2) $CH_3 \cdot CO \cdot COOH \rightarrow CH_3 \cdot CHO + CO_2$
(3) $2\,CH_3 \cdot CHO \rightarrow CH_3 \cdot CH : CH \cdot CHO + H_2O$
(4) $CH_3 \cdot CH : CH \cdot CHO + H_2 \rightarrow CH_3 \cdot CH_2 \cdot CH_2 \cdot CHO$
(5) $CH_3 \cdot CH_2 \cdot CH_2 \cdot CHO + O \rightarrow CH_3 \cdot CH_2 \cdot CH_2 \cdot COOH.$

Repetition of reactions (3) and (4) would produce aldehydes corresponding to the higher fatty acids, and only one atom of oxygen is required for the final oxidation of the aldehyde. All the hydrogen required for the reduction of the unsaturated aldehydes would be supplied by the dehydrogenation of lactic to pyruvic acid. But even if these reactions turn out to be the right ones the problems still remain as to how they are accurately controlled within the cell. Some of the substances concerned in them are diffusible and very reactive, and we should have to explain how diffusion away from the site of reaction is prevented. To overcome difficulties of this kind it is becoming common to invoke the intervention of surface forces. There is, however, not much experimental evidence as yet which helps us to explain by such intervention the mechanism of synthetic processes even of such a simple kind as the reversal of an enzyme action.

Freundlich in his Liversidge Lecture to the Chemical Society last year described experiments on the influence of charcoal in modifying the velocity of certain reactions. Bromoethylamine is converted by caustic soda into dimethyleneimine. The amine is more strongly adsorbed than the imine and therefore the conversion should be slower in the presence of charcoal than in homogeneous solution. Experiment verifies this. Further, dimethyleneimine is converted into bromoethylamine hydrobromide in presence of hydrobromic acid. Because the bromoethylamine hydrobromide is more strongly adsorbed than the dimethyleneimine the conversion should be more rapid in the presence of charcoal than in homogeneous solution. This also was shown to be true. In addition it was demonstrated that the change of the amine into the imine in neutral solution stops earlier when charcoal is present than when it is absent. The equilibrium obtained in homogeneous solution is thus disturbed in the sense that the formation of capillary active substances (*i.e.* those more highly adsorbed) is favoured. It follows from this that the stability of a substance on a surface may be greater than in homogeneous solution under similar conditions. It must be borne in mind, however, that in these experiments very large quantities of charcoal were used compared with the amounts of the substances whose equilibria were being studied. So much so that in the reaction in alkaline solution the bromoethylamine was practically completely adsorbed and the reaction was taking place entirely on the charcoal surface. Can one postulate such conditions during the continuous synthesis of substances in the cell, glycogen from glucose for example? It seems to me that to explain the rapid accumulation of synthetic products such as fat or glycogen which we observe in cells something more than a shift in the equilibrium of the reactions due to surface forces is necessary. Such a condition favouring synthesis could only operate for a time until the surface became saturated. We must therefore postulate some additional mechanism whereby the synthesised product is removed from the sphere of action, for if it diffused off the surface again it would be subject to the equilibrium conditions which are present in the solution. It may be that the arrangements in the cell are such that only small amounts of the substrate are dealt with at a time so that complete synthesis is achieved and the synthetic product removed. This implies that the synthetic activity takes place in cycles due possibly to cyclic changes in the activity of the active surface or to a mechanism whereby only as much substrate for the synthesis is admitted to the sphere of action as can be dealt with without saturating the active surface. It is difficult to translate experiments such as those described by Freundlich to explain synthesis in the living cell without additional mechanisms such as these.

We have also to consider how the synthetic product is dealt with in the cell so as to protect it from the disruptive agencies which exist there. Arrangements for this purpose must be present since we know that substances may accumulate in cells which contain enzymes that hydrolyse them. For example, fat and lipase co-exist in the liver cell and also glycogen and amylase. The phenomena of autolysis illustrate this same fact too. Whatever these arrangements are they appear in certain instances to be closely associated with the life of the cell, for after death they cease to operate and the synthetic product is again broken down. However difficult it is to form a conception of them it may be necessary to do so since they must form a part of any system which is put forward to explain synthesis as a result of the intervention of surface phenomena.

We may now consider two syntheses in which there is little or no doubt about the raw materials or some of the chemical reactions involved. These are the production of glycogen and of proteins.

Glycogen was first isolated in 1857 by Claude Bernard and a little later was analysed by Kekulé, who showed that it had the empirical formula $C_6H_{10}O_5$. It is only recently that its probable structure has been determined. Last year, Haworth, Hirst and Webb succeeded in preparing trimethyl glycogen and proved that on hydrolysis it gives rise to 2.3.6. trimethyl glucose. This observation supported by similar work by Karrer indicates that glycogen is constituted like starch on the basis of continuous maltose units, or what amounts to the same thing, a conjugated chain of $\alpha$-glucose units.

It has also been proved that when glycogen breaks down in the liver it gives rise to glucose. Lohmann and also Barbour have succeeded in obtaining glycerol extracts of liver and muscle which hydrolyse it, but the product appears to be a trisaccharide and not glucose. No enzyme which by itself hydrolyses glycogen to glucose has yet been obtained from animal tissues. It is of interest that pancreatic and salivary amylase produce isomaltose from glycogen. These results suggest that there may be some configurational difference between glycogen and starch which accounts for their difference in behaviour with diastatic enzymes. Be that as it may, it appears natural to assume that the synthesis of glycogen from glucose in the cell is brought about by the simple reversal of a hydrolysis which may be catalysed by enzymes under appropriate conditions. These conditions have, however, not yet been realised *in vitro*. The much simpler synthesis by enzyme action of disaccharides from hexoses—the first example being that of isomaltose from glucose by Croft Hill—has so far been seldom achieved. Barbour in the experiment just referred to was unable to demonstrate any synthesis of glycogen from the trisaccharide produced by the muscle enzyme even in highly concentrated solutions.

This failure to obtain evidence of the synthesis of glycogen from the products of its hydrolysis makes it legitimate to consider whether we are right in adopting the orthodox view that the synthesis of glycogen from glucose in the living cell is brought about by a reversal of action of the enzyme or enzymes which hydrolyse it.

Besides the facts about the constitution of glycogen and the nature of its hydrolytic products that have already been mentioned, there are others which merit consideration in any discussion of its mode of synthesis in the body. When an animal is fed liberally with glucose or fructose it converts a part of them into glycogen in the liver. The evidence for this is indubitable. It implies, therefore, either a conversion of fructose into glucose before the condensation to glycogen occurs or a conversion of both into some common form of hexose which then undergoes the condensation. Further, there is a considerable amount of accredited evidence that many substances not belonging to the sugar group can be converted into glucose in the animal body. Such substances therefore, must be regarded as potential glycogen formers. Several amino-acids such as alanine, glutamic acid, aspartic acid, glycine and serine come into this category. Then it is well known that glycogen can be formed from the trioses glyceric aldehyde and dihydroxyacetone as well as from methyl glyoxal and lactic acid, which are easily derived from them *in vitro*. In addition, glycerol and glycolaldehyde can give rise to glucose in the diabetic animal though the evidence in favour of the latter is not very convincing. It is easy to understand from the work of Emil Fischer on the synthesis of hexoses from glycerose how glycerol and the two trioses could give rise to a hexose in the body. It is also possible that methyl glyoxal by condensation and hydration could be converted into a hexose and that lactic acid, which has been shown by Dakin and Dudley to be capable of conversion into methyl glyoxal *in vitro*, could also undergo the same change. But the production of glucose from amino-acids is not explicable so easily except in the case of alanine and perhaps aspartic acid, and we must assume that in the process of catabolism which they undergo intermediate substances are produced which can condense to a hexose. Our knowledge of the metabolic changes which the amino-acids undergo suffices in some instances to offer a reasonable explanation of this on chemical grounds, but not in all. Whatever these processes may be which eventually result in the production of a hexose from such diverse substances, the most remarkable thing about them to my mind is that the hexose is always *d*-glucose. We have no satisfactory explanation for this striking stereochemical performance, but the facts suggest two possibilities. Either the condensation of the two— or three—carbon units to a hexose is brought about under such specific conditions of strain that only the *d*-glucose configuration can result, much as coins must be minted in a definite mould to become currency, or the hexose produced by these various condensations is a labile form which condenses to glycogen and is in turn hydrolysed to glucose. But to produce this hypothetical labile form of hexose or to bring about its condensation to glycogen would probably require such conditions of specificity that this second hypothesis seems unnecessary.

The view that any substance that produces glucose in the body may also give rise to glycogen is supported by good experimental evidence. Several of the glucose-formers that have been already mentioned are able to produce glycogen in the isolated, perfused liver, particularly in the liver of the tortoise. But the difficulties of this kind of experiment are considerable and the conditions for success are not completely understood. It is, however, essential that the liver cells be alive and adequately supplied with oxygen. It has also been demonstrated that insulin plays some part in enabling this synthesis to be brought about; but exactly how it enters into the process is not known.

Much more exact studies of the synthesis of glycogen in the animal cell than are possible with liver have been made with frog's muscle. Fletcher and Hopkins, in their well-known experiments on the production of lactic acid by muscle, showed that in undamaged muscle and in the presence of oxygen the lactic acid which is produced during the contraction process disappears. Meyerhof proved later that the disappearing lactic acid was largely reconverted into glycogen. Assuming that the oxygen which is necessary for this process is used in oxidising a part of the lactic acid, we get the relation that of four parts of lactic acid produced in the muscle, one is oxidised and three are utilised for the resynthesis of glycogen. Under more ideal experimental conditions and using thermal data, Hartree and Hill calculated that the ratio of lactic acid resynthesised to lactic acid oxidised is about four to one. The chemical mechanism of this synthesis is unknown. Under conditions in which muscle produces glycogen from lactic acid, Meyerhof was unable to observe such a synthesis from a series of substances including, among others, glyceric acid, glyceric aldehyde, dihydroxyacetone, alanine, glycollic acid, glycolaldehyde and acetaldehyde. The only substance discovered to behave like lactic acid was pyruvic acid, and Meyerhof has expressed the view that this is due to its preliminary reduction to lactic acid. We have thus no evidence of the intermediate substances between lactic acid and glycogen in this synthesis. On the other hand, we have the very positive evidence that it occurs only in the presence of oxygen. Although this suggests that a coupled reaction is concerned involving the utilisation for synthesis of energy provided by oxidation, no satisfactory picture of an oxidative reaction of lactic acid which would result in the production of glycogen, water and carbon dioxide has been put forward.

A consideration of all the data which have been accumulated regarding the synthesis of glycogen makes it probable that more than the mere reversal of enzyme action is concerned. It is certain that the cells in which it occurs must be supplied with oxygen. Fletcher and Hopkins also showed with muscle that its structure must be maintained and with liver the synthesis certainly only takes place in the intact organ. Do not these facts point to the conclusion that it is only the living cell that can bring about this synthesis? And if this be so, cannot we go further and suggest that the substances from which glycogen are produced or bodies derived from them must first become bound up with, or at some stage form an integral part of the living structure before they are converted into glycogen. The evidence at least suggests that some such conception of the process may not be far from the truth.

We may now pass to the consideration of the synthesis of proteins. In the early part of this century, due largely to the elegant methods introduced by Emil Fischer, rapid advances were made in our knowledge of the structure of proteins. These advances led to a picture of protein structure which has become generally accepted, namely, that the protein molecule is formed by the union of amino-acids through an amide linkage. It is, however, unlikely that this is the only bond of union between the amino-acid units. If it were, the widely different physical and chemical properties which are met with in the proteins would have to be explained largely on the basis of their amino-acid content. The differences in composition which they exhibit do not seem to be sufficiently great to warrant this assumption. The insolubility of the keratins, for instance, although accounted for in part by the high proportion of cystine they contain, this being a very insoluble amino-acid, is possibly also due to the presence of other modes of union of the constituent amino-acids in addition to the amide linkage. Similarly, the varying degree of liability, the phenomena of denaturation, and the physicochemical behaviour of different proteins are difficult to explain solely on a basis of differences in their components. There is much therefore still to be learnt about the actual structure of the protein molecule.

The investigation of the structure of proteins which are closely allied in origin, composition and general chemical behaviour by immunological and in part by chemical methods, has taught us how intricate the mechanism must be by which they are built up. The facts brought out by the classical work of Dakin and Dale on the albumen of the duck's and hen's egg serve to exemplify this. The only chemical difference that could be shown between these two proteins was concerned with the disposition in the molecule of some of the leucine, aspartic acid and histidine, which resulted in different degrees of racemisation of these amino-acids when the respective albumens were treated with caustic soda.

But when used as antigens in the anaphylactic reaction they were markedly specific. These results indicate that the stereochemical structure of the molecule is different in proteins which are very similar both in general chemical properties and in biological origin. They suggest that the protein molecule produced by a particular type of cell is always built up in a distinctive and, so far as we can determine, an unvarying pattern. We may deduce from this that although the general method of protein synthesis, that is to say, the mechanism by which amino-acids are joined up, may be the same in all cells, yet there must be arrangements in the cell which enable only one particular, final pattern, to result from the synthesis.

What are the methods by which the amino-acids are caused to combine? Those used by the organic chemist in the synthesis of polypeptides can have no place in the living cell although they have been of great assistance in helping to reveal the general structure of proteins. The use of proteolytic enzymes in an attempt to bring about synthesis under conditions which have been partially successful with other substances has often been tried and nearly as often has failed. Two examples of protein synthesis by enzyme action have been described. The most acceptable of these is

Taylor's production of a protamine by a glycerol extract of clam liver from the products of its complete hydrolysis.

But, so far as I am aware, this is an isolated observation which has not been extended or repeated with other protamines. The other is the preparation of so-called plasteins from the products of the partial hydrolysis of certain proteins by pepsin. A comprehensive review of the subject of plastein formation by Wasteneys and Borsook, who have themselves made notable contributions to this problem, has led them to the belief that in this phenomenon we have a true resynthesis of protein from some of the more complex of its hydrolytic products. Apparently only certain of these yield plastein on treatment in highly concentrated solution with pepsin under appropriate conditions, and the distinctive feature in them which makes plastein formation possible has not yet been discovered. It seems probable that it is both a property of certain proteoses and of pepsin that enables the synthesis to occur, and some evidence for this view is provided by the observation of Wasteneys and Borsook that the formation of bodies similar to plastein is brought about when benzene and some other substances are emulsified in a peptic digest of egg albumen. The plasteins obtained in this way have different physical properties from those obtained by the action of pepsin on similar digests. Wasteneys and Borsook suggest that this may indicate how differences in composition of the proteins synthesised by the organism are produced, certain proteose substrates being specifically adsorbed on particular surfaces in the cell and then undergoing reaction to form the synthetic product. Even if this be accepted as a possible explanation it still leaves many questions regarding protein synthesis unanswered, and not the least difficult of these is the problem of how the separate amino-acids are brought together to form the specific proteose substrates which one must postulate as combining—and in a definite order—to produce the particular protein which is characteristic of the cell which synthesises it. One of the great difficulties in accounting for the synthesis of protein by a reversal of enzyme action is that this synthesis only takes place, so far as we can observe, in a cell which is living.

The process by which the substance of the cell is increased, the building up of protoplasm, is one which must be closely allied to protein synthesis, since the material we call protoplasm is constituted for the greater part of amino-acids, united, so far as can be ascertained, by the same sort of linkage that we find in proteins. The protoplasm of the dead cell responds to all the tests by which we identify protein, it is subject to the action of hydrolytic agents in the same way and yields identical products when hydrolysed.

Ought we not therefore to look for some of the mechanisms of protein synthesis in the processes which operate when the living cell grows, and can we by any stretch of imagination account for this by a reversal of the action of one or more hydrolytic enzymes ?

It appears inconceivably difficult to do so. The extreme specificity of the reaction which necessitates that at a given phase of the synthesis one particular amino-acid and that one alone can be added as the next link in the molecule, requires such a multiplicity of enzymes and such a remarkable degree of control of their action as to be almost outside the range of probability. When we remember, however, that one of the prime attributes of life is that it is a dynamic condition, it does become possible to form a conception of protein synthesis in relation to that fact. The experiments of Willstätter and others have shown that to some extent the specificity of enzymes is accounted for by the 'carrier' with which they are associated. It is not inconceivable that a catalyst capable of bringing about the union of amino-acids in the living cell and ultimately fashioning its protoplasm may be attached to or associated with a 'carrier' which, instead of having a fixed configuration, as with the enzymes that we can extract from the dead cell, has one which is continually varying, this dynamic state being characteristic of the living material of the cell.

If, further, we could assume that the variations in the configuration of the carrier were cyclic, always going through a definite series of phases, it might be possible to account for the fact that at any particular phase of the cycle the configuration would be such as to favour the synthetic union of one particular amino-acid rather than any other because of its spatial arrangement. The assumption of cyclic changes in simple or complex living organisms is not new in Physiology, and it is not unlikely that they occur in parts even of the cell itself. The phenomena of mitosis are a somewhat gross illustration of such cyclic events.

It is perhaps difficult to justify speculations of this kind, but it appears to me that it is at least useful as an intellectual exercise to try and arrive at some conception, founded on what little experimental evidence we at present possess, of a possible mechanism which would account for the reproduction time after time of the intricate pattern of the protein of a particular type of cell.

Whatever be the mechanism of protein synthesis there is another fact concerning it which is of interest, and that is that the cell to maintain itself must apparently contain a certain concentration of amino-acids. If these are not supplied in the animal's food then the animal supplies them by maintaining its more important tissues at the expense of others. This would suggest that the conditions for nitrogeneous equilibrium which we find in the whole animal have their replica in each cell, but the concentration of amino-acids required to maintain the equilibrium is not the same for all. Hence an amino-acid deficiency such as occurs in protein starvation leads to autolysis of some tissues before others. The equilibrium conditions appear to be set at different levels. The fact that certain amino-acids are more necessary than others in providing for this balance may be partly due to their special importance in the protoplasm of those cells which are the first to break down when protein food is withdrawn or an inadequate protein is fed.

The possibility that protein synthesis is associated with some part of the cell which is undergoing cyclic changes, and is thus alive, raises the interesting question of the site of this and possibly other syntheses, such as those of fat and glycogen. Are all parts of the cell, that is to say, both nucleus and protoplasm, to be regarded as alive in the sense I have indicated, and therefore to be considered as regions in which syntheses depending on life can be brought about?

It is necessary not to confuse the terms irritability and life. It is true that what we term irritability is usually taken to imply that the tissue which shows it is living, but taking the nerve fibre as an example, it would appear that the maintenance, for a time at least, of the irritability of protoplasm and its restoration when it has disappeared—but not irreversibly—does not require the presence of the nucleus. It may require oxygen or the presence of certain ions, but this may merely mean that the labile state of the protoplasm has been uspet by the products of the cell's activity, and removal of these will restore it to its irritable condition.

We have no evidence that irritability as a manifestation of what we call life is more than the possession of extremely labile structures, sensitive to minute environmental changes. The nucleus, on the other hand, is essential to the continuous life of the cell and its growth. It appears also to determine very largely the magnitude of the respiratory processes which occur in it at rest, though not necessarily the excessive respiration observed in the recovery from functional activity. Can we therefore go so far as to say that the nucleus is the seat of those synthetic activities of the cell which appear to depend on its living character rather than on its irritability, or have we to regard the protoplasm as equally living so that it is able to reproduce itself and in addition bring about such syntheses as those of fat and glycogen ?

There is something to be said in favour of the idea that the protoplasm is not living in the sense in which the nucleus is, and therefore is less likely to be the seat of certain synthetic processes. It is, I think, quite a tenable view that protoplasm is made up, largely but not entirely of combinations of amino-acids such as we find in the proteins, and that it is synthesised by the necleus to serve special as well as certain general requirements. These special requirements must and do vary greatly with each type of cell.

Consider the mammalian erythroblast. The principal substance in its protoplasm is the protein hæmoglobin. It is doubtful whether this cell exists except to produce hæmoglobin. When it has matured the nucleus degenerates and disappears leaving the red blood corpuscle. Along with this the respiratory activity of the cell practically disappears too, and there is not much discoverable in it except hæmoglobin. It is certainly now not living in the sense in which its progenitor, the erythroblast, was. Is it not reasonable to suggest, therefore, that the production of hæmoglobin as the erythroblast grows and matures is a function of the nucleus and not of the protoplasm of this cell ? All semblance of further synthesis of hæmoglobin certainly disappears when the nucleus goes. The adoption of such a view does not imply that having produced the protoplasm of a cell the nucleus has nothing more to do with it. We know that in some indefinable way the nucleus in most cells controls the structure of the protoplasm and maintains its lability, but we have no knowledge as yet of the mechanism by which this is brought about.

In emphasizing the great similarity between the growth of protoplasm and the synthesis of proteins, other examples in addition to that of hæmoglobin come to mind. The cells of the Malpighian layer of the epidermis have to produce the protein which eventually becomes the keratin of the *stratum corneum*. This would appear to be one of their chief functions. When it is done the cell dies and the nucleus becomes functionless. The production of collagen by cells of the connective tissue and of milk proteins by the cells of the mammary gland are instances of the continuous synthesis of proteins which are not labile like the protoplasm of, say, the muscle or nerve cell. Their formation, however, may be due to the operation of the same general process. They are certainly non-irritable and non-living. It is difficult to imagine that while in the cell they possess the power of reproduction but lose it once they have been cast out.

If we accept the possibility that the nucleus of the cell may be the site of protein synthesis it is legitimate to consider whether the substances which appear in the protoplasm as discrete particles or masses such as glycogen or fat are produced in the nucleus or only in the protoplasm. We have no evidence as to this. The fact that they first appear as visible objects in the protoplasm is not evidence that they were produced there, and it has already been pointed out that their synthesis is apparently only possible in cells which are alive. They may be synthesised in the nucleus and stored in the protoplasm. Their disappearance from time to time may be conditioned by the liberation of enzymes from the nucleus to hydrolyse them. Such a view would do away with some of the difficulties which we experience in attempting to explain why substrates may be present in cells which, judged by their behaviour after death, appear to contain enzymes that hydrolyse them. The protoplasm, instead of producing such reserve materials, may serve as a convenient storehouse for them until the appropriate stimulus for their breakdown arrives.

One property of proteins, which may account in a general way for the presence of protein-like structure in protoplasm, is their buffering power. The nucleus is probably the most labile part of the cell. The chemical reactions proceeding in it may demand an environment that has to be finely controlled as regards changes in reaction or the concentration of certain ions. The protoplasm may thus serve as a protective layer between the nucleus and the external world, guarding it from changes which would otherwise terminate its existence. The known properties of the proteins, both chemical and physical, may be useful to this end, to which they are almost ideally suited.

Even if we can accept as possible the unorthodox view that the nucleus is the only living part of the cell, and is therefore the only part that can bring about syntheses which depend upon life, it does not solve our difficulties in explaining how they are achieved. It merely narrows down the possible sites in the cell in which they occur. The nucleus itself is a complex structure, and we have as yet few experimental methods for elucidating it. Most biologists would, I think, agree that the cell has arisen by a process of evolution from something simpler and eventually from non-living materials. It cannot have come as a 'bolt from the blue.' If we regard the nucleus as the only living part of the cell, then we may justly regard the protoplasm as something that has been acquired or developed in the process of evolution and is now necessary to its existence. We do not know definitely, however, of nuclear material which is living and devoid of its protoplasmic envelope unless such an arrangement is present in the bacteria. But the investigation of filterable viruses has given an indication that material possessing the prime attribute of life, namely, the power of reproducing itself, exists, possibly in simpler forms than we find in the smallest visible organisms.

If we agree that the cell has evolved from something simpler, then we might expect to find such elementary forms of life coexisting with it did we but know how to look for them. The filterable viruses may represent such forms, and their chemical characters may resemble those that we find in the nucleus of the cell. The ability to synthesise protein may be a property which living material only acquired at a late stage of its evolution, and that property may be one which in the process of time has come to be essential for the maintenance of the complex structure of the nucleus as we see it to-day.

I have dwelt for a time on these problems of synthesis in relation to the structure of the cell because, however speculative they appear to be in the present state of knowledge, it seems to me that it is necessary to get some picture of them. Progress in synthetic chemistry has been intimately associated with the ability to represent objectively the structure of substances and the changes which take place when they are undergoing reaction. We know much about the manifold chemical activities of the cell, but, until we can by deductive methods get some picture of its organisation, the problems which I have attempted to sketch briefly in this address will not be solved.

The view has sometimes been expressed that it is the task of physiology to study the reactions of the whole animal because the whole animal is the unit. That is perfectly true if our object be only to describe and account for animal behaviour. But there is just as truly a cellular physiology in which the cell is the unit. Until we can analyse and understand the behaviour of the cell we cannot expect fully to explain the reactions of the whole animal.

In stressing therefore the importance of studying the activities of the cell as such for the progress of our science, I feel that I am not at variance with some of my predecessors in this office. Like them, I have attempted to indicate the lines upon which progress may come in our endeavour to elucidate some of the problems to which I have referred.

Classical organic chemistry with all its achievements of synthesis cannot yet claim to rival the synthetic activities of the cell. That it will ever do so as regards the synthesis of our main foodstuffs, the proteins, carbohydrates and fats, cannot be foreseen. Both from the chemical and economic points of view the signs at present are not very hopeful. The synthetic breakfast table is a myth and likely to remain one. Man has, so far, not learnt from Nature the mechanisms of syntheses that she uses and on which his existence depends, so that the tilling of the soil and reaping of the harvest must for long remain his most vitally important occupation.

SECTION J.—PSYCHOLOGY.

# THE FOUNDATIONS OF CHILD PSYCHOLOGY AND THEIR BEARING ON SOME PROBLEMS OF GENERAL PSYCHOLOGY.

ADDRESS BY

PROF. C. W. VALENTINE, D.Phil.,

PRESIDENT OF THE SECTION.

The values of the study of the early years of childhood I take to be at least three.

First, there is the fascinating interest it has for those who love children or who marvel at the wonder of a developing mind; and this is motive enough for the genuine scientist. The importance of child psychology as a foundation for a science of education is also obvious. But for the 'pure' science of psychology, to use the current term, it also has wider values.

Most important, perhaps, is the fact that the most reliable evidence as to what is genuinely innate in human nature must be found in the study of human infancy. Even McDougall's brilliant contribution to the study of human instincts does not, I think, supply altogether satisfactory criteria of the innate impulses,[1] valuable as they are as first suggestions. For the fact that a similar impulse is displayed in (apparently) instinctive activities of higher animals does not *prove* that in man that impulse is instinctive or innate. McDougall himself only claims that it affords strong presumption. Nor does the possibility of morbid exaggeration of an impulse give us a satisfactory clue, for many actions exaggerated to an abnormal degree owe at least their specific nature to experience, as the study of the unconscious has shown.

The spontaneous occurrence of a new type of activity in the infant, of such a nature that it could not have been learned through experience, affords, I would maintain, the only certain proof of the genuine innateness of an impulse—unless some activity developing later can satisfy that criterion. Thus the foundations of child psychology are also some of the main foundations of human psychology as a whole.

A further value of the study of infancy lies in the fact that the elementary functions may be observed in greater isolation than they can be later. The instinctive impulses are seen in their crudest form, less cumbered than they are later by accretions from experience. Elementary cognitive processes, too, appear in their simpler forms and may be studied in their origins. The slow building up of ideas is impressed upon us. Thus twelve months elapsed (from nearly two years of age to nearly three

[1] *Social Psychology*, 9th edition, p. 49.

years) from the time one of my infants correctly used the number two to the time when he could apprehend three as a group, and in another case the interval was eighteen months—so far is the truth from the supposed idea of a sudden development of a 'faculty of number.'

A third value of the study of infant psychology is that it counteracts the tendency to interpret later childhood too much on the lines of adult experience, just as the study of childhood is a valuable check on over-rationalising our interpretation of adult behaviour.

Three recent developments have emphasised the importance of the study of the earliest years of life. The first is the assertion by the psycho-analytic school that the first four or five years of life are the most important in the fixing of character. Freud holds that 'the little human being is frequently a *finished product* in his fourth or fifth year.'[2] Adler goes so far as to say that 'one can determine how a child stands in relation to life a few months after his birth.'[3]

It is not my wish to underestimate the importance of the first few years of life, but rather to stress it. It seems, however, impossible to state, on the evidence we have before us, that the first four to five years of life are more important than, say, the years of adolescence. What exactly is meant by the assertion if it is made? It is rather like saying that the safety of a house-roof depends more upon the foundations than it does on the stability of the walls of the first or second stories.

The Freudians have certainly shown that in many cases the experiences of the earliest years may continue to exert a profound influence on the life and character of the child when he grows up, though he may have forgotten those experiences. It may also be admitted that if bad social relationships—say with parents—are set up in the first few years, that relationship may be fixated so that the parents' efforts later to change them may be futile.

What is not *proved*, as it seems to me, is that if a child suffers from an injurious social environment, or erratic and foolish discipline till, say, five or six, but enjoys a favourable environment thereafter, he is necessarily more handicapped than a child who has a satisfactory environment till that age, and then comes under wrong discipline or vicious influences continuing through the unstable and suggestible period of adolescence.

In any case it seems unnecessary to make extreme statements about the absolute fixation of character by the age of five or six. It is enough for our purposes if we admit that this early period is probably far more important for future development than was at one time thought.

The second recent development in psychological thought which has emphasised this view is that of the Behaviourist school. Take, for example, Dr. J. B. Watson's assertion that there are few genuine innate tendencies in man and the suggestion that any infant, if taken in hand early enough, can be 'conditioned' into almost any type of character. This assertion as to absence of instincts can undoubtedly, as I hope to show later, be combated by evidence from early childhood. Yet the readiness with which special treatment can rouse specific fears in an infant

[2] *Introductory Lectures in Psychoanalysis*, 1921, p. 298.
[3] *Understanding Human Nature* (translated by W. B. Wolfe), p. 42.

of one or two years is a further evidence of the sensitivity of this period to environmental influence.

Most of those psychologists who, unlike Watson, believe that man possesses many innate tendencies, are still uncertain as to what precisely they are. 'We do not know,' says Thorndike, 'what situations originally provoke smiling, laughing, crying, frowning, etc.' If the problem is to be answered, it can only be by daily observation of children from birth, and I shall suggest later some answers to this particular query of Thorndike, especially in relation to laughter and imitation.

A third influence that is proving a powerful stimulant to infant psychology is the attempt to press back the testing of intelligence earlier than three years, the lowest age for which Binet tried to devise tests. The valuable pioneer work done by Dr. Arnold Gesell has provided tests which are suitable for infants of nine, six and even four months, and has afforded some evidence of their correlation with normal mental development.[4] Dr. Gesell claims that it can be shown that one month or two of retardation shown at, say, six months of age, suggests a year or two of retardation at six years of age; and that mental deficiency may be detectable at a very low age by appropriate tests.

The definite relation of such tests to later estimated intelligence needs to be demonstrated further; and no doubt, as I shall show later, some of Gesell's individual tests for given ages will need to be modified or abandoned. For this, the daily study of infants in the home environment is again necessary. Such study suggests that some tests are too dependent on the mood of the moment to be reliable clinical tests.

In using Gesell's tests I have also found enormous individual variations in the suitability of different tests for a particular child at a given age. Thus, a child of, say, twelve months would be able to do some of Gesell's tests for two-year olds and yet fail in some of those for its own age. Nevertheless, on the whole a fairly steady I.Q. may work out even when only selections of the tests are used, as is shown in the graph below. Here is the performance on Gesell's tests of a child whose I.Q., tested by Binet tests later at ages 2 ; 9, 3 ; 4 and 3 ; 9 worked out on the average at 1.5. Her I.Q. by Gesell tests is as follows :—

*I.Q. by Gesell Tests.*

| | | | | |
|---|---|---|---|---|
| At 3 months | I.Q. .. | 1.4 | No. of tests tried .. .. | 26 |
| 4½ ,, | ,, .. | 1.4 | ,, ,, .. .. | 14 |
| [5] 8¼ ,, | ,, about | 1.25 | ,, ,, .. .. | 38 |
| [6] 1 yr. 4 mths. | ,, ,, | 1.2 | ,, ,, .. .. | 9 |
| 2 ,, 3 ,, | ,, ,, | 1.3 | ,, ,, .. .. | 14 |
| 2 ,, 9 ,, | ,, ,, | 1.45 | ,, ,, .. .. | 14 |
| | Average I.Q. .. | 1.36 | | |

[4] See *The Mental Growth of the pre-School Child.*

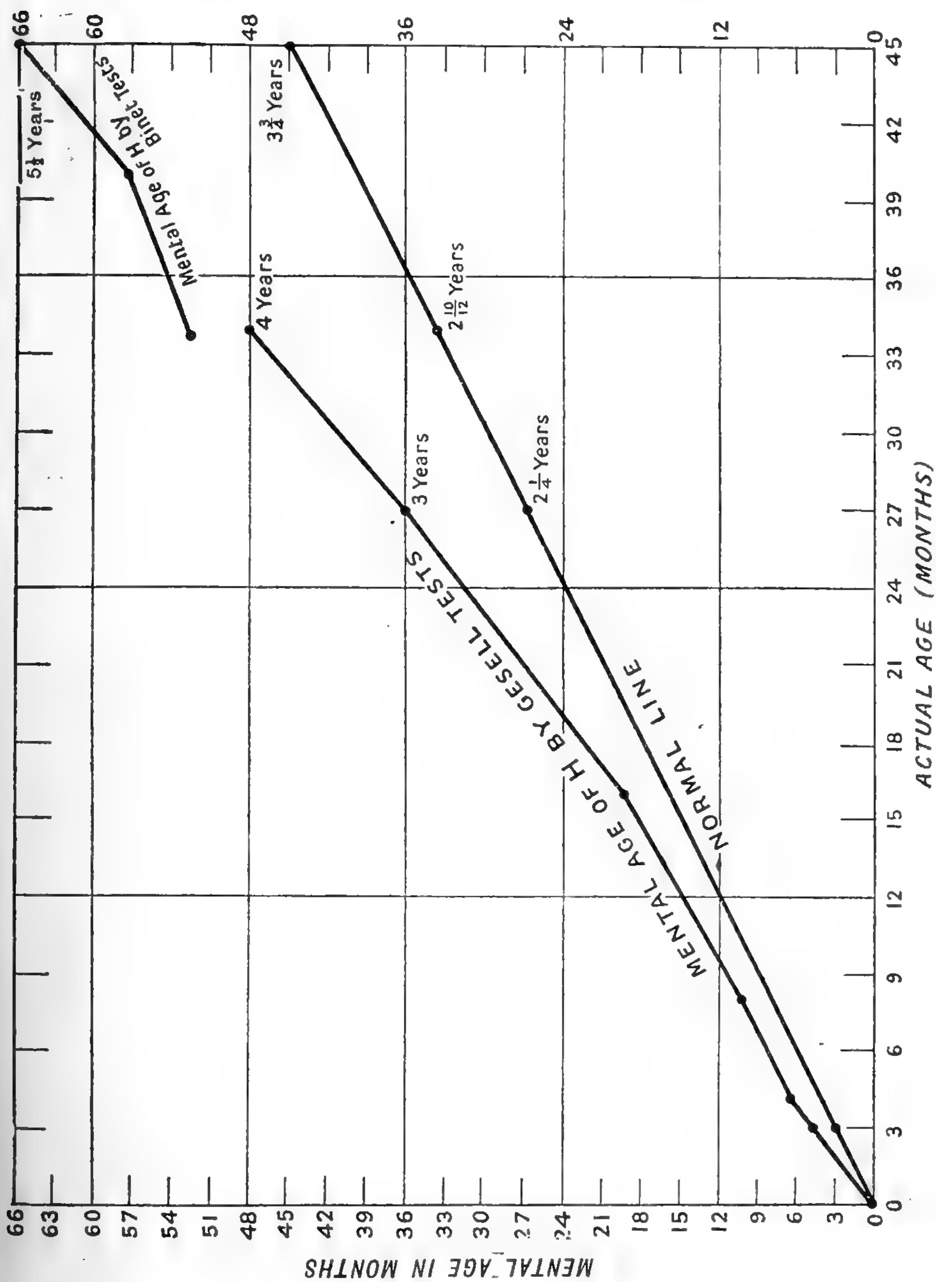
[5] The child was troubled with teething at this time, which probably lowered her score.

[6] The tests at 1; 4 were only nine in number, and confined to tests prescribed for 1; 6. Hence they are ignored in calculating the average I.Q.

GRAPH A.

*Showing Mental Age of a Child from Three Months*

By Gesell and Binet Tests.



The prognostic value of early observations is, however, more remarkable, I think, if we confine ourselves to some fairly specific capacity. Let us take the development of speech. In the case of several of my children, I made observations from the first days on the making of sounds and beginnings of speech. When spontaneous 'cooing' first revealed itself, I tested carefully for the first appearance of 'responsive cooing,' i.e. cooing in response to cooing.

The next graph shows various stages in the development of speech in three of my children and the ages at which they revealed the particular accomplishment. The lowest (i.e. youngest) line is that of Y who first showed spontaneous cooing at the age of four weeks. The second lowest line is that of B, who showed spontaneous cooing at the age of just under five weeks; the third line is that of A, who did not show it till seven weeks and is behind the others in all the other speech phenomena—practising new noises, first understanding of a simple word of command, onomatopœic noises, imitation of word-sounds, use of negative, two word sentences and so on.[7]

This may be a mere coincidence, or rather three series of coincidences. But it does not stand alone. Apart from the evidence afforded by Gesell, there are other functions in which I was able to trace similar parallel developments though not as detailed, and some early suggestions of clues to later type of temperament. Thus, I noted within the first three months of life that one of my boys was definitely more active in reflex responses than the other and generally less placid: and throughout boyhood he continued the more mobile, more excitable of the two.

Undoubtedly many similar examples are needed for confirmation; but if these phenomena are more than coincidences, we may well have, some day, not merely means of prophesying the future general intelligence of a child when it is six months of age, at least with a high degree of probability, but also of getting at this early age a fair idea of its future capacity for linguistic development, manipulative skill, and possibly even of its characteristic temperament, and so on.

I wish to emphasise the fact that we are still in the position of the mental testers of older children, say, 25 to 30 years ago. The main work is still the testing of tests. Excellent, for example, as Dr. Gesell's general lists of tests are, I am sure he claims no finality for them. Some of them are, I have suggested, too dependent on the passing mood of the child to be reliable for general clinical purposes at one or two sittings, though they may be used successfully if applied frequently by a psychologically trained parent in the home.

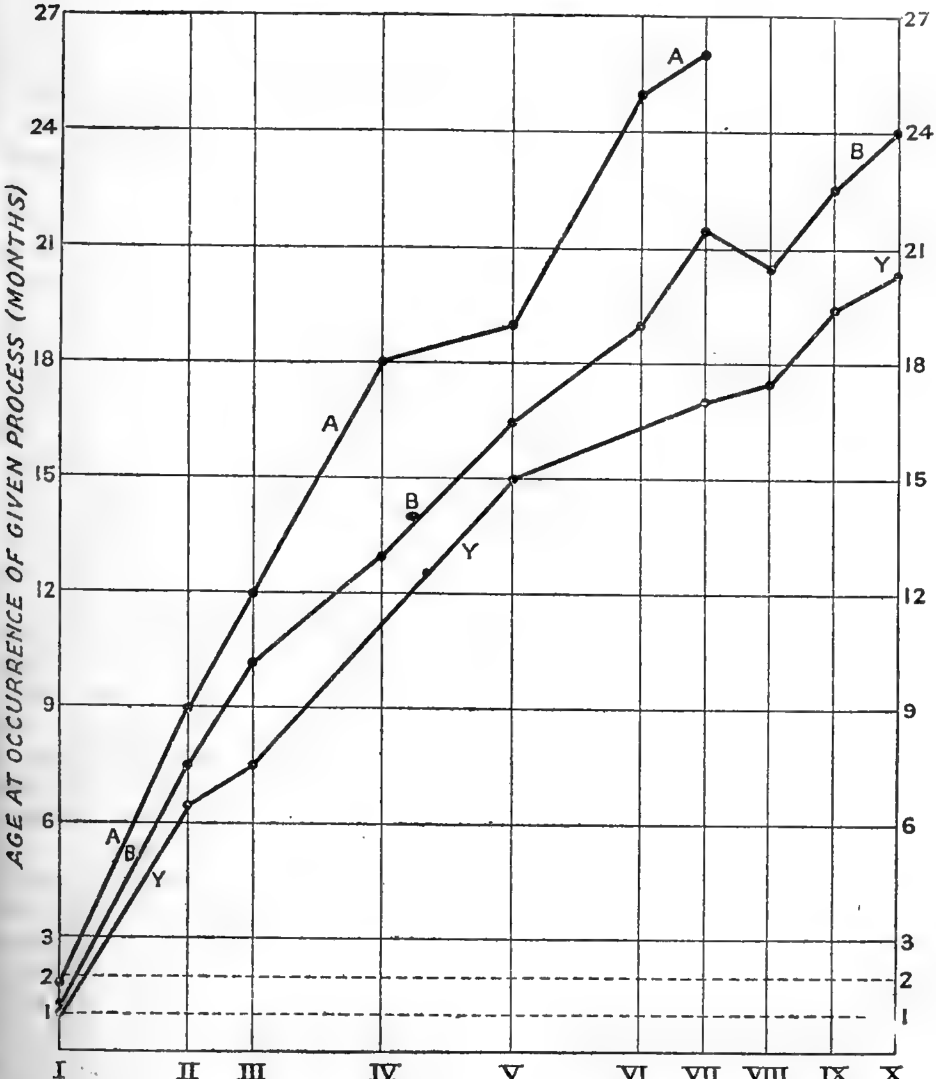
Even reflexes are not so reliable as tests as they might seem. This point may be illustrated by the facts gained from some experimental tests of the eye-blink reflex in response to a sudden loud noise, made on one of my little girls at six months. First, standardising the stimulus (a stone falling on a tin tray from a height of 3 feet) is essential, as is the leaving of time interval enough to avoid fatigue of the reflex. The

[7] I have not comparative records of all the various new speech developments. There may be material in my records for more, though the above were not, of course, specially selected. As language becomes more and more expressive of complex thought processes, the lines of B and Y intermingle.

GRAPH B.

*Illustrating prognostic suggestiveness of early tests for the development of speech.*

Speech Development of Three Children.




*Key to Graph.*

I. Responsive 'cooing.'
II. Practising new noises.
III. Doing one or two things at command.
IV. Onomatopœic sounds.
V. Imitating word sounds.
VI. Negative spoken as refusal.
VII. Two words used together (possessive).
VIII. Two word sentence.
IX. Three word sentence.
X. Asking name of everything.

remarkable fact emerged that, at this stage, with exactly the same stimulus, the eye-blink reflex may occur for a few times and then cease, and then recur again: it may occur nearly every time in about a dozen tests one day and in only one or two or even not at all on another day, and so on with no apparent cause of variation. The common definition of a reflex as the '*invariable* response,' etc., is misleading: and the testing two or three times of this reflex in a strange child at this age, quite unreliable.[8]

Further general daily observation and testing of this and other reflexes in my children suggested most decidedly that reflexes are affected by the general condition of the infant at the moment; that a reflex may appear for a time and then vanish (as the walking reflex at four weeks); and that the dividing line between the so-called reflexes (at least the sensation-reflexes) and instincts is hard to draw, not because all instincts are merely reflexes (as ordinarily understood), but because what have been called mere mechanical reflexes are more complex than supposed, less inevitable, more capable of improvement with practice, more the reaction of the whole mechanism, and otherwise more similar to instincts.

I have already contended that the occasional testing or study, at a given age, of a group of children, needs to be checked by day-by-day observations on the same child in the home environment. It is such study which convinces me of the importance of the influence of general 'tone,' condition or mood on performance. Dr. Gesell says that fatigue does not affect the result of tests much, and that even illness does not 'completely mask' the stage of development. This, I think, is only true if we refer to well-established reflexes or habits and responses; it does not hold of *nascent* functions that is functions during the period when they are just beginning to appear. I have noted frequently that new appearances may only take place, for the nascent period, under favourable circumstances. This applies to higher functions such as elementary thought processes, the use of number, and so on, as well as to such phenomena as imitation, laughter, and some reflexes; but one only fully realises this if one tests daily for some new function, somewhat earlier than it has ever been found by others.

Daily observations and a knowledge of the influence of varying circumstances on the child are still more important when we study the more complex problems of instincts. How far, for example, are fears dependent on innate disposition, and how far on suggestion or experience?

There is little doubt that the divergence of reports as to whether there are *innate fears* of animals, of furs, of the dark and so on, is due partly to differences of conditions under which the observations are made. If an infant in the mother's arms is less likely to show the eyeblink reflex at the bang of a door than if it be alone in a cot, the full fear reaction is still more likely to be affected by favourable or unfavourable conditions. The principle of summation of stimuli holds in the sphere of instinctive responses. In some experiments on the fear of animals intended to test Watson's theory that such fears are 'all due to experience and association,'

[8] A detailed account of this experiment is given in my article *Reflexes in Early Childhood: Their Development, Variability, Evanescence, Inhibition and Relation to Instincts. Brit. Journ. of Medical Psych.*, vol. vii., 1927.

I put before my little girl of twelve months a creeping woolly caterpillar, at which she gazed with apparently anxious fascination but without protest. I then blew behind her a whistle which had previously caused not the slightest fear, even when blown suddenly behind her. With the added slight disturbance, at once the lurking fear of the caterpillar seemed to spring into full activity, and she screamed and shrank from it.

Turning definitely to this question of Fear, I would point out that the innate basis of some fears of animals is by no means disproved by J. B. Watson's experiments, in which he produced in infants of eleven months fear reactions towards white rats, which had not been feared before, by striking a steel bar loudly behind the infants when they tried to touch the rats. For we must allow for the period of maturing, and the maturing of some of these fears of animals seems to take place only towards the end of the second year.[9]

The attributing of the fear of, say, dogs (which may suddenly appear, even when a child has constantly seen and heard and played with dogs) to some unpleasant experience is quite unsatisfactory in view of the way in which severe hurt will often fail in the same child to set up fear of, say, climbing. And the explanation that fears of animals or the dark are merely due to suggestion is faced by the fact that in these directions suggestion works with such amazing facility, whereas suggestion to children that they will ‘ catch their death of cold ’ if they play in the rain, or suffer violent pains if they eat a new, attractive looking fruit, often fails miserably. And neither experience or suggestion can account for the horror of the uncanny—of strange masks or horrid faces. Finally, I had ample evidence of a love of ‘ playing ’ with the fear stimulus, which I have seen lead children to ask over and over again for a game of ‘ lions ’ which has previously ended and again ends in tears and shrieks of fear. This love of playing with fear suggests a deep innate tendency, craving for stimulus denied it in our civilised life.

Let us consider another topic, important as a foundation of child psychology, namely *Imitation*. Here again the testing of children by a stranger, even if the child is in the mother's arms, is quite inadequate as a clue to the strength of the tendency to imitation, or as a test of the stage of development of the child. Prolonged experiments with one of my children showed very clearly the importance of the imitatee. Sometimes, for example, the child, at twelve months and two years, would imitate nearly everything done by the father (if only the father or other member of the family were present) but sometimes no one but the mother would be imitated.

There are extreme differences of opinion as to whether there is such a thing as a primary innate disposition to imitate, apart from any ulterior ends served by the action itself. Thorndike criticises Stout and Kirkpatrick for asserting its existence. Watson supports him. Drever finds his view incredible. Baldwin asserts that imitation ‘ is the controlling impulse.’ Koffka says that animals and children only imitate what they understand.

[9] I have dealt more fully with the development of fear in a paper on ‘ The Innate Bases of Fear ’ in *The Journal of Genetic Psychology*, September 1930.

What are the facts? Here no ordinary observations of what may be occasional coincidences is enough. Observations, indeed, of the first twelve months did quite convince me that primary purposeless imitations are constantly taking place; but to make sure I planned two series, of about fifty tests in all, on my little girl (Y) of twelve months. The actions done in front of her, usually by myself or her mother, were all of a type we thought she *could* perform; but we and others tried to think of actions she would not be likely to do, and they were included in the tests. Yet in the first series at twelve months, of thirty-seven tests there was imitation in thirty-one, and in the six failures there was, with one exception, a good reason for non-imitation, as that the child was playing with a toy, or that the action, like shaking the head, had come to mean something to the child, which she did not mean at the moment.

The actions included many which could not possibly be 'understood,' and which served no ulterior purpose, *e.g.* puffing as though smoking, opening the mouth wide, airing a garment by the fire, and various physical exercises.

A similar proportion of actions was imitated in a series of twenty-eight tests at two years of age. Y may have been exceptionally imitative. But a colleague who is thoroughly familiar with the methods and difficulties of experimental psychology gave his boy the same tests at twenty-two months, and found almost identical results.

It would take far too long to expound fully the psychology of imitation suggested by these observations and experiments, but I may mention one incident which gives, I believe, a clue to primary imitation in infancy. To see if a very simple action was imitated at twelve months I held my mouth open wide in front of my daughter Y. She showed signs of great annoyance, banging her hand to and fro at my mouth. Then her own mouth opened wide. Again when I opened my mouth she showed dislike, and even a suggestion of fear, and crawled away from me, but as she came round the back of a chair I noticed that she was crawling along with her mouth held wide open.

No one formula, I think, can cover all types of imitation at this stage: but there does reveal itself a general tendency to imitate actions which engross the attention for the moment, in the absence of competing sources of interest.

Another problem of general psychology, on which the study of infancy throws a light, is that of the causes of *Laughter*, as to which such varied theories have been held.

Those theories of laughter which are based on a study of wit and humour, fail to allow for the very basic origins of laughter. Again, theories have generally erred in being far too simple. The careful observation of the earliest occasions of laughter suggest that the causes are very varied. I give here the various causes in the order in which they appeared in the case of my boy B.

(i) The first clear laugh I noted in my children was that of B (whom I watched especially for laughter development) at the age of thirty-nine days—a laugh of delight at being put into position to take food. Several other observations showed that the getting of food or anticipation of it was the earliest cause of laughter, as Dearborn also noted in his daughter.

(Yerkes noted that his chimpanzee would frequently laugh in response to favourite foods.)

(ii) The next occurrence of laughter was in response to laughter of mother or father at ten weeks, followed by laughter even at a smile (eighteen weeks).

(iii) The third occurrence was caused by tickling, also at ten weeks.

(iv) The fourth type is laughter at the sight of a bright or pleasing object (twelve weeks).

(v) Next we come to laughter at a simple shock or surprise (eighteen weeks). Thus B laughed (three times) when I tore a newspaper—a sound which had previously caused an apparent start of fear. By this age (four months) laughter was very frequent. (Gesell found that 85 per cent. of the babies he tested could laugh at the age of four months.)

(vi) Mere repetition of a stimulus, or our imitating actions of B seemed to be enough to cause laughter at about six months. The comic effect of mere repetition remains long in operation; a comedian says 'Now we shan't be long,' and no one takes any notice. He says it again and people smile; again, and there is a roar of laughter. It is the six-months-old baby reaction.

(vii) The next cause of laughter in B (seven months) was the unfamiliar and unusual in the midst of the familiar—a 'shock' of a mere intellectual surprise, *e.g.* my falsetto voice, or the sight of his mother in a paper cocked hat.

(viii) At seven months there were also suggestions of laughter at mere recognition, but of this I was not certain.

(ix) Between eight months and twelve months another cause of laughter appeared—the successful accomplishment of some new activity, *e.g.* standing alone.

(x) The sight of the mild discomfiture of another is a well-known cause of laughter. McDougall, indeed, bases his theory of laughter on this phenomenon, suggesting that we laugh in order to avoid the contrary feeling of sympathy which would prove too exhausting if too frequently experienced.

It is notable that as many as eight or nine different causes of laughter were observed in B before anything of the nature of this last type was seen. Only in the seventeenth month did I note laughter of this type in B, who gave loud roars of laughter when his mother tumbled down a grass bank. It must be admitted, however, that occasions on which B would have an opportunity of seeing such slight discomfiture of others were rare. Possibly, therefore, the date of this type of laughter response should be considerably earlier. A sufficient number of examples of earlier kinds of laughter have, however, been given to indicate how very varied the original causes of laughter are and how the earliest appearances depend on the joyful satisfaction of elementary needs.

The fundamental nature of *thought processes* is another problem on which I believe the study of early child psychology will ultimately throw a flood of light. The intimate association of thought with language enables one to obtain material with relative ease. Far more difficult, however, is the interpretation of the words spoken by the child. We must throughout guard against two dangerous fallacies. First, the assumption that thought only develops when the corresponding word is used; the second that a word used by the child must necessarily have to some extent the same content of meaning as when used by the adult. In spite of these two great difficulties of interpretation, however, it is possible to make with some considerable degree of confidence inferences about the thought processes of the child when certain types of language are used in given circumstances, or when language spoken by the adult is obviously interpreted correctly by the child, as shown by his actions.

As an illustration of the contribution child psychology can make to the psychology of thought, let us take the much disputed question as to whether thought can take place without words.

On one point at least the observation of infants is conclusive—elements of a complex thought process can certainly function without the corresponding word being expressed. Take, for example, the many prepositions which at first are omitted but implied: 'Baby go (on) daddy knee.' (2; 1). 'Mummy hat (on) floor' (1; 10½). 'That too big (for) B., not too big (for) Daddy.' (2; 4). The functioning of the element without the word is still more clearly revealed during the period, which I definitely noted, in which the child sometimes used a given preposition and sometimes omitted it.

At a still earlier age, in a sentence such as 'Mummy, door' for 'Mummy open the door' an activity is evidently thought of when only two names of things are actually expressed in language. It is conceivable, of course, that the missing word appears as an auditory image. But the apparent absence of time interval is against this, though here is a point worthy of exact determination.

The great influence of feeling on the beginnings of concept formation is demonstrated by observations on the transference of the first names learned. Thus, two of my boys, both at the age of nine months, began to apply the first learned name 'Dadda' to toys and play generally, no doubt because of the fact that their father when with them was usually romping with them. Father was merely the play person—for a period, alas, all too brief. Similarly, Stern notes that his boy applied the name Beban (for Bow-wow) not only to dogs but to all things which interested him specially—animals, pictures, and his own shadow.

Light is thrown on the loquacity of some adults by the facts, frequently observed in children, first that they love to babble long before their talk can have any definite meaning, even for them; and later, that they love to repeat words and use words before they understand them. They will fit them into sentences when they are extremely out of place, or repeat them sometimes like so many nonsense syllables, though they may reproduce the sound of the words with great accuracy. I have to report that I noted this more decidedly in my two little girls than in the boys: but in one of them it has already helped to bring about a constant experimentation with the use of new and partly apprehended words which has resulted in a much richer active vocabulary than either of her brothers had at the same age, though at times it has led to a quaint half misusage, as when she said that she had a 'confidential' feeling that a certain thing was not going to happen. The 'verbalism' which Binet noted as characteristic of his daughter Armande, may thus be detected as early as the third year.

Perhaps the strongest impression left by the careful study of thought in little children is the astonishingly early appearance of the elementary thought processes. Time was when it was asserted that science could not be taught to boys below the age of fifteen because inductive reasoning was not possible before then. About a dozen years ago Dr. Cyril Burt caused some surprise by showing that all the varied elementary processes of reasoning can be traced in children's thought at the age of nine. I venture to state that all or very nearly all the elementary thought processes may occur by about the age of three, at least in children of intelligence quotients of about 1.3 to 1.5, and taking as our clue to elementary thought processes Spearman's classification of relations.

Certainly it is important that one should be carefully on the look out for these elementary thought processes. They occur sporadically. Like all mental developments in young children, they may appear one day and then apparently disappear and not function even when the situation demands it for some days or weeks. And they no doubt need favourable surroundings, such as a happy home and sympathetic parents or brothers and sisters, if they are to show themselves. It is the lack of observation under such specially favourable surroundings to which I attribute the extraordinarily late date at which Piaget, in spite of his most valuable investigations, places some appearances of thought processes, which I shall explicitly refer to later.

Suppose we take first, as fundamental elements of thought processes, Spearman's classification of relations, and enquire when these are first apprehended or 'educed.' Undoubtedly, as Spearman says, relations may be apprehended before they can be expressed in language; but we will confine ourselves to the earliest appearance of language in which these relations are implied.

The possessive noun (e.g. 'Daddy's hat,' 'Mummy's hat') occurs at 1 ; 4, and probably implies a relation of *attribution.*

The correct use of 'in' and 'on' at 2 ; 1 and 2 ; 2 reveals the education of the *spatial relation,* and sentences in which it is omitted but implied occur some months earlier.

'With' (I brush my hair with Daddy's brush) as indicating a medium or tool is used correctly at 2 ; 3 and reveals a relation of *agency*—for which I cannot find a true equivalent in Spearman's list.

Perhaps the most surprising example of the grasp of a relation at a very early age was a sentence by B at 2 ; 4. ''At too big (for) B, not too big (for) Daddy.' Here is surely the first glimmering of an idea of relativity.

The beginnings of a grasp of a *causal relation* occurs already at 3 ; 8. Thus 'What makes the water come again?' (asked when I forked the garden path to let the puddles of water escape.) Cf. 'What keeps the sky up?' asked by a boy of 3 ; 7, and again at 4 ; 1 'Why doesn't the ink run out when you hold up a fountain pen?' (See 'Scientific Interests of a Boy,' *Forum of Education,* 928, p. 21.)

That these examples of relations are not just fortuitous usings of words the child has heard spoken by adults is indicated by the fact that the appearance of one of these relations in thought is so often accompanied by a great delight in the new discovery, so that for some days or weeks a child will constantly be asking 'Why,' or practising the possessive relation by calling many things 'Daddy's hat, Mummy's coat, baby's pram,' etc. The earliest grasping of the causal relation no doubt has reference to living agency, and sometimes 'why' and ''cos' do not refer to causes at all, but to purposes. But the idea of causal relation in reference to physical things may possibly occur as early as 2 ; 11, when B asked why we saw our reflections in a train window.

The relation of *attribution* is implied in the correct application of adjectives to nouns ; this can be confidently inferred only when the conjunction is so unusual that one can be sure that the words were not previously connected in speech by anyone in the hearing of the child. This I observed before two years.

The relation of *likeness* must be grasped considerably before 2 ; 3, when it was apprehended between complex fundamentals : thus 'Y's car do like Daddy's car.' 'Daddy do like B does.'

I had evidence in various matters that new mental processes most readily arise when practical and spontaneous interests are aroused rather than in the course of formal tests. But the difficult *part-whole* relationship was ripe in Y at 3;8, not only when she used 'part of' correctly, but when it was tested by the question : 'What is a part of me ?' and she replied 'You could have only your eyes.'

The *relation of evidence* is perhaps the last to appear—at least to be clearly revealed in explicit language. But it was evident in Y's conversation at 3 ; 2 when taking care of a baby guest :

Y : 'I'm a big girl.' Father : 'No, you are a little girl.'

Y : 'I look after the little girl. *Well, then,* I'm a big girl.'

Further notes make it clear that evidence may be explicitly referred to as such at the age of 3+.

Thus we have all the possible relations apprehended by about three years of age. Now Piaget says that before the age of 7 or 8 'the child is perhaps incapable—whether in narrative, argument, or in any of his relations with other people—of differentiating between the various types of possible relations (cause, consequence or logical justification) and of handling them to good effect.'[10] Undoubtedly up to that age the child continues to make many errors in his use of these relations, as Piaget clearly shows. What the study of much younger children reveals is that a grasp of relations *begins* to appear at a much earlier age.

Other statements of Piaget appear positively fantastic when one examines the records of infant development. Thus he speaks of the 'universal tendency of the child to avoid relations'—though at two a child may be positively obsessed with the causal relation as its repeated 'Why's' show.

Again Piaget states that the child under seven is 'still ego-centric and feels no desire to communicate with others or to understand them.'[11] My notes reveal that the desire to communicate appears at least about the age

[10] *Judgment and Reasoning in the Child*, p. 19.
[11] *Language and Thought of the Child*, p. 126.

of two, and as often as one might expect a child of that age to imagine it had something to report which a god-like parent did not already know. I note, for example, at 2 ; 2 that Y repeated something 'over and over again in louder tones till I said "Yes," and so often in the last few months.' Nor is it merely the boasting report of what a child has done. For example, my wife and I were once met, on return home, by an excited little maid of 3 ; 0 who reported to us, with rapid and dramatic speech, a new fact just learned from a young brother, namely that when we died we should be made into birds and fly up into the sky. The impulse to communicate may even become explicitly conscious before four years. Thus I heard one of my youngsters of 3 ; 10 shouting out, 'Mummy, I've got something to tell you.'

Reciprocal relationships are undoubtedly hard for the child to grasp. Children of nine tested by Piaget showed that they were not clear as to the reciprocity of the brother-brother relationship. Yet it is wrong to suppose that the little child before even five or six cannot in any sense adopt the point of view of another. For example, I noted that when Y at 2 ; 8, sitting opposite to me, wanted me to see a picture in her book, she carefully turned it round so that I saw the picture right side up. And before four years, the reciprocity of the brother-brother relationship apparently begins to be grasped (though it has lapses), and the sentence 'I'm my own nurse to-day' (3 ; 9) involves a similar grasp of reciprocity.

Inference from general propositions, at least explicit inference, may not appear at this very early stage, though general statements involving a grouping of individual known facts are made before three, *e.g.* 'Everybody's here'—correctly stated, as to the family of seven, at the age of 2 ; 9 ; and a general proposition may be explicitly referred to as a reason : thus, 'Do you love Daddy ?' 'Yes.' 'Do you love H ?' 'Yes.' 'Why ?' ''Cos I love everybody.' Also the absence in a drawing of a general trait common to all members of a class (*e.g.* the trunk of elephants) may lead to the refusal to apply the name (*e.g.* 'Is that an elephant ?' 'No.' 'Why ?' ''Cos it hasn't got a trunk' 3 ; 5).

Thus we have at three years at least the beginnings of induction and deduction.

Lastly, can a little child assume, for a time at least, a hypothesis it knows to be false ? Piaget found children of eight and nine would not do so. In the Binet absurdity test about the man who said, 'If I ever kill myself, I won't do it on Friday, because Friday is unlucky,' Piaget said that the children, even of nine and ten, 'refused to admit the hypothesis.'[12] Now, undoubtedly, in such tests, children are attracted first by the, to them, most glaring absurdity. My boy of seven, for example, when asked what was absurd about it, said 'The man would not want to kill himself'—quite right from the child's point of view.

But certainly it is not true to suggest that children can never posit suppositions they know to be false, or that, until eight or ten, they cannot assume a 'detachment from the view of the moment.' Thus, at 3 ; 5 one of my little girls asked her mother to jump over the sofa. The

[12] *Judgment and Reasoning in the Child*, p. 65.

mother objected that she was too old, which brought the remark, '*If* you were a little girl you could.' And again, about the same age, she said '*If* mummy died, I'd be alone with my daddy.'

Here are further examples of a type of thought-process taking place when working in a familiar medium, at an age some six or seven years before the age at which it still fails sometimes to function effectively. The fact is that it is a mistake to attempt to make sweeping generalisations as to what takes place in childhood and what in adults. All the kinds of errors which are thought particularly characteristic of children occur in adult thought, frequently in some adults, less frequently in others. I have found graduate students who, like Piaget's nine-year-olds, will refuse to posit an incredible hypothesis for the sake of testing the formal accuracy of a syllogism. And even a Fellow of the Royal Society may base a generalisation about educational methods on his own experience when a boy and that of one or two children at the present day.

What does seem to be especially true of the child of, say, three years, is that these various types of thought process do occur only very sporadically at first: hence again the necessity of careful daily observation if the first appearances are to be noted.

I have tried to exemplify the way in which the study of the first three or four years of life is necessary for the foundation of child psychology and the fact that it may have important contributions to make to general psychology. It must be admitted that the field of infant psychology is still largely unexplored and that the method and technique are still greatly in need of improvement. When they are more perfected I venture to predict a rich harvest from this relatively neglected ground.

SECTION K.—BOTANY.

# PRESENT-DAY PROBLEMS IN TAXONOMIC AND ECONOMIC BOTANY.

ADDRESS BY

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PRESIDENT OF THE SECTION.

The honour of having been invited by the Council of the British Association to preside over the deliberations of Section K this year in this historic city was of no small personal pleasure to me, more especially since the first meeting of the Association I ever attended was the Bristol meeting of the year 1898, the second time the Association met in this city. At that meeting you will remember our distinguished botanical guide, philosopher and friend, Emeritus Professor F. O. Bower, our President at this third meeting at Bristol, was then president of Section K.

It is interesting to recall the fact that when the British Association first met at Bristol, in the year 1836, John Stephens Henslow, that fine botanical naturalist, presided over Section D. 'There are few men of this century,' as the late Sir William Thiselton-Dyer, the first president of this section, pointed out in his address at Ipswich in 1895, 'who have indirectly more influenced the current of human thought. For in great measure I think it will not be contested that we owe Darwin to him.'

In recalling Henslow's name in his historic connection with our meeting here this year, it is fitting to remember in passing what he did for the teaching of Natural Science in Cambridge, for up to the time he was appointed professor of botany, in the year 1825, Natural History was utterly disregarded at the university, and his predecessor in the Chair of Botany had not delivered any lectures for some thirty years!

Henslow's teaching methods are worthy of study even to-day, when we are rather prone to lose interest in the broader lines of our subject and to shut ourselves apart, owing to the many specialist problems with which we are confronted. Henslow realised the value of practical methods, and that Botany, from the stimulus it gave to 'the strengthening of the observant faculties and expanding the reasoning powers,' was a subject not only of intense interest but also a means to a liberal education.

As the present head of our great national institution for the study of Botany both pure and applied, Taxonomic and Economic, I look, somewhat with sorrow, it must be admitted, for young men who have been adequately trained to use their observant faculties with regard to the living plants, and to visualise the many problems which are opening up, and are already open, awaiting the men of vision and wide outlook ready to come forward in the keen spirit of adventure and to undertake their solution.

Our university students, I sometimes think, are apt to believe that salvation can only come through the brass tube of a microscope! Yet to a keen observer with the naked eye or a hand lens, in collaboration, no doubt, with the microscope, for some cognate genetical or structural question, there are problems as fascinating and of as much, if not of more, general importance than the detailed anatomical structure of some obscure though doubtless very interesting specimen.

The passing reference I have made to John Stephens Henslow somewhat naturally causes me to refer to two other great teachers, who, alas, are no longer with us, William Turner Thiselton-Dyer and Harold Wager. To Dyer, who died just before Christmas 1928, our section largely owes its foundation, but in thinking of him with Henslow and Wager, it is particularly as a teacher that I would recall his services to Botany. His association with Huxley in the memorable course of Elementary Biology at South Kensington, and his own courses in Botany in 1874 and the following year, assisted by Prof. S. H. Vines (who celebrated his eightieth birthday at the close of last year), again revolutionised botanical teaching in this country. Nor must we forget all he did towards securing the preservation of the Chelsea Physic Garden, and what the successful issue of his labours has meant for the furtherance of botanical teaching in London.

The death of Harold Wager on November 17 last year—president of our section at the South African meeting in 1905—is a very great loss to Biology. Not only was he a most skilful manipulator and, like Henslow, used always the simplest and most ingeniously contrived appliances, but he had the real temperament of the naturalist with keen powers of observation. Problems open to solution by scientific experiment were constantly perceived by him, which hitherto had been neglected, unseen by scientists lacking his inquiring mind and keen powers of observation.

Wager's influence among the amateur naturalists and professional teachers was outstanding. His unremitting scientific labours were a daily accompaniment to the conscientious fulfilment of his duties as an inspector of schools. The teacher of biology in the school, prone to follow the easier path of instruction through text-book and diagram, was constantly being reminded, through contact with Wager, of the wide gap that may exist between the formal description and the actual object to be examined. Just as the amateur naturalist, finding in Wager a kindred spirit, was led by his example to take more pains and extend the range of his scientific technique, so the professional teacher was encouraged to put aside mere repetition of second-hand facts, to observe for himself, to become, in fact, 'a Naturalist,' and thus to develop a new enthusiasm which rapidly communicated itself to his pupils.[1]

May the infectious enthusiasm which he communicated long remain amongst us.

Henslow and Wager, I think, must have had much in common; both realised the importance of strengthening the observant faculties, and I have referred especially to these two pre-eminent naturalists, since the

[1] See the obituary notice in *Nature*, December 21, 1929.

methods they pursued and realised to be so essential, are exactly those which are required to-day for dealing with the problems of Systematic Botany and its many associated studies.

Henslow, Dyer and Wager were, undoubtedly, outstanding types of men who possessed the gifts which we look for in our students, the love of experiment, keen powers of observation and reasoning, accompanied by a wide outlook, including the ability to see the full scope of a problem and its possibilities in both the pure and applied lines of research.

This brief consideration of the work and contributions of these three pioneers in our science brings me to the subject which I have selected for my address—Some of the Present-day Problems in Systematic and Economic Botany. Not that I feel very competent to address a learned botanical audience, since so much of my time is occupied with matters of administration of a large establishment and with correspondence on every sort of botanical subject with the Dominions and the Colonies, and with our botanical colleagues throughout the world; yet I have had the temerity to do so, because one's range over the subject is so wide that I am brought in touch, perhaps more than most professional botanists, with so many interesting and unexpected developments and side issues, which entail investigation and research in the domains of Systematic and Economic Botany, Physiology, Genetics, Ecology and Plant Pathology.

In the realm of Systematic Botany I suppose our most important problem is still that of the 'species.' Though botanists hold several views on this fundamental question they nevertheless continue in their work of species-describing, and the recently issued seventh supplement of the *Index Kewensis*, with its some 33,000 new specific names and new combinations, shows there is no diminution in this field of their very necessary activities. I, with many of my colleagues at Kew and elsewhere, must plead guilty to adding to the labours of our pundits in nomenclature.

It is, I think, hardly necessary to point out that Systematic or Taxonomic Botany is, like other branches of our science, in a healthy state of growth and development. In dealing with the study of the vegetation of the earth it is essential in the first place to be able to catalogue and describe our material, which may often be far from perfect. Herbarium specimens, as they have been received since early times, have been duly named and arranged, and very often the Taxonomist has had only a single specimen on which to base his studies.

In this way our Herbaria have become respositories of 'type specimens' of the utmost historical value, and from the study of these specimens all our earlier and well-known floras have been written; to give a few instances, from Kew only, I may refer to the 'Flora Capensis' and the 'Flora of Tropical Africa,' more especially the earlier volumes, the 'Flora of British India' and the 'Flora Australiensis.'

With the increase of our knowledge of the vegetation of the world, due to the larger numbers of people interested in botanical work and to more extended and more careful exploration, plants hitherto unknown are constantly being brought to light. As before, these are deposited in our Herbaria and described, and our knowledge of the geographical range of species and the composition of the genera has been greatly extended. But this, it now appears, is only the beginning of the enterprise, as may be gathered from a study of the developments that are taking place with regard to the floras of those countries or regions which are capable of being subjected to a more intensive type of survey.

In the case of any region or country, the first thing to be done is to assemble the material, to put it in order and assign names to the 'species,' that is, the definitely distinct assemblages of very closely related plants; but we are realising now, I think, that such work is rather of a preliminary and tentative nature, and cannot be regarded as in any way final. It is only when we are able to make an intensive study of the vegetation of a given area, and undertake a critical examination of a wide range of specimens, which appear to fall within the bounds of what may have been described as a 'species,' that we can feel justified either in accepting the original definition and description or, on the other hand, in forming an entirely new conception, which may result in the original 'species' being regarded as a 'habitat form,' a 'variety' or a 'genotype' or a member of a compound species.

Taxonomic workers, more especially in the past, have tended to fall into one of two categories, for to some a 'species' has covered a wide range of forms grouped around a mean type, while others have taken a more restricted view and their species have represented far smaller and more sharply defined classificatory units. Both methods have been of value; the broader view has had its advantage very often in relation to questions of geographical distribution, while the narrower one has caused us to inquire into questions relating to the origin of species themselves and the significance of so-called 'varieties.' They have also had their drawbacks, since in the one case many matters relating to the influence of habitat, general conditions, &c., have not been fully appreciated, while in the other the possibilities of hybridisation, segregation and adaptation have usually received little or no recognition.

The intensive study of the flora of a region, or of particular genera, such, for instance, as *Rubus*, *Taraxacum* or *Hieracium*, has led in some cases, I feel, to the adoption of a very narrow, or I might call it a 'parochial,' outlook, which has tended to detract from the importance of Taxonomic work in the eyes of the younger botanists entering on the paths of a botanical career from our universities. To them, and to many others, the sole aim and occupation of the 'local' systematist has seemed to be to browse along a hedgerow in order to find a somewhat dubious 'new species,' and then with no small pride to prepare and publish its description. In the past, in somewhat petty work of this character, no attempt was made to study effects of light and shade or other environmental conditions, or to make cultural experiments to test the validity or otherwise of the find. I believe it is now properly recognised that such short cuts to transitory glory are a hindrance rather than a help to the progress of Systematic Botany, and that the many problems which arise when a flora comes to be studied critically and intensively, can only be solved by means of experiment. Such experiments may involve controlled cultivation, genetical research and very careful tabulation of statistics before full light can be shed on the true nature of what may have been regarded as a large 'compound-species' or a host of small, closely-allied 'micro-species.'

Until I had the opportunity of visiting New Zealand I was not very greatly exercised about the problems underlying the species question, and was content, like others, to describe a new species without any particular qualms from a single specimen.

The extraordinary prevalence of hybridisation, however, in the New Zealand flora, seen under the able guidance of Dr. Leonard Cockayne and Dr. H. H. Allan, quickly made me realise how rash it would be to think of describing any New Zealand plant as belonging to a new species with only a single specimen before one.

Later, when on Rainbow Mountain, I found the erect shrubby *Gaultheria oppositifolia*, with its dry calyx and dry fruit, passing imperceptibly through an infinite number of intermediate forms into the prostrate alternate-leaved *G. depressa* (or *G. antipoda*), with red or white fleshy calyx-segments enclosing the fruit, it became clear that the question of 'species,' 'Linneons,' 'Jordanons,' and the rest, was a burning one, requiring the collaboration of the Systematic Botanist, the Ecologist and the Geneticist for its solution.

Here, then, is a large and vital problem which, to my mind, very greatly widens the interest and importance of our Herbarium studies, since problems relating to the possible hybrid origin of the plants we are dealing with demand careful study in the field, with visits to the countries where the plants are native.

I am reminded, from what I saw in New Zealand of the 'hybrid swarms' in *Gaultheria*, *Nothofagus*, *Myrtus*, *Veronica* (*Hebe*), and many other genera, of that remarkable Malvaceous genus *Nototriche*,[2] native of the Andes of South America. At the summit of the pass leading from Peru into Bolivia I collected, at an altitude of about 14,500 feet, some five quite distinct 'species,' growing close together under apparently identical conditions of soil. They were easily separable by their leaf and floral characters, but I wondered then, and I wonder now, why there were these five distinct forms in this small area, when apparently any one of them was good enough and perfectly adapted for the perpetuation of the genus!

Dr. Lotsy, I feel sure, would regard them as hybrids or of hybrid origin, and, unless we feel inclined to assume that Nature has evolved this multiplicity of forms for pure pleasure—a sort of botanical experiment in permutations and combinations—it is difficult to lay aside the view that hybridisation, in this genus and possibly in many other genera, has played a prominent part in the development of the multitude of described 'species' we see around us.

These tiny prostrate plants were many years old with deep tap roots, and they would be well-nigh impossible subjects for experiment, like so many of the remarkable South African examples to which Dr. Lotsy has so forcibly drawn our attention.

Then again the visitor to Australia cannot fail to be impressed by the multitude of 'species' in the genus *Eucalyptus*. The study of the literature is no less bewildering than is the study of the living trees, and we must,

[2] A. W. Hill in *Trans. Linn. Soc.* (Bot.), ser. II, vol. vii. 1909.

I think, believe with the late Prof. Anstruther Lawson, that hybridisation has played an important part in the production of many of the closely similar forms, which may or may not breed true, and which are so great a puzzle to the botanist.

In addition to questions relating to possible hybridisation, we are now also recognising the importance of soil factors in connection with our conceptions of species. To me it has always been fascinating, when wandering in the Alps or elsewhere, to note the changes in the vegetation when passing from one geological formation to another, more especially when, as is so often the case, it is a question of the presence or absence of lime. Near Mont Cenis, for instance, the line of demarcation between *Gentiana lutea* and *G. purpurea* is as sharp as if a dividing fence were present, and there are equally striking changes with other plants near Le Lautaret, where there are marked changes in the underlying rocks. Examples will readily occur to you, and in all such cases we are dealing with definite and well-marked species, but we may be permitted to speculate whether the allied species, now found restricted to certain types of soil, may not, in times long past, have diverged from a common ancestor.

It is when we come to more subtle cases, to some of which I shall refer later, where owing perhaps to a change of soil a physiological difference can be detected, without any obvious corresponding morphological change, that our interest becomes acutely aroused, and we realise the depth of our ignorance. Physiological varieties of this nature can sometimes be assumed to be due to the nature of the soil, and in the case of certain plants restricted to the Serpentine rocks, some well-marked morphological characters can also be recognised.

A somewhat parallel case, though of a different order, is afforded by the common Mistletoe, *Viscum album.*

Tubeuf, you will remember, in his monograph on the Mistletoe,[3] gives an account of the races or varieties of *Viscum album*, which are definitely associated with particular host plants. Much has been written about these 'forms,' and they have even been given definite specific names (e.g. *Viscum austriacum, Viscum laxum*), but for this there does not seem adequate reason. Three definite physiological races, however, are clearly marked, (1) the form which is found on deciduous trees, (2) that associated with the Silver Fir, *Abies pectinata* and other species of *Abies*, and (3) the form parasitic on *Pinus sylvestris*, *P. Laricio* and *P. montana*.

The races are so far distinct that seeds of the 'Pine form,' for instance, will not grow on the apple or fir, and *vice versa*. Physiologically, therefore, they are distinct, though morphologically they cannot be separated. A case like this suggests that we may be witnessing the advent of three species from one, and that eventually morphological differences may also become evident.

The vegetation of South Africa supplies some Taxonomic physiological problems of a like nature, which up to the present have not been satisfactorily solved by the Systematic Botanist. These relate to the difficulty of differentiating between two or more forms of the same species which, though distinct physiologically, cannot be separated on any structural

[3] 'Monographie der Mistel,' Karl Freiherr von Tubeuf (1923), pp. 661–672.

characters. Several such physiological strains are now known in South African species of the genera *Pentzia* and *Salsola.* There are two strains (physiological varieties) of *Salsola glabrescens* Burtt Davy, which grow side by side. One of these plants, with purplish-red young twigs, is closely grazed, while the other, in which the young twigs always appear to be pale-coloured, remains untouched by cattle or sheep until there is nothing else to eat. The selective feeding of the animals when grazing on a pasture bearing these two forms is very remarkable. Carefully collected dried specimens of these two plants, with flowers and fruit, have been critically examined at Kew, but apart from the colour of the young stems of the living plants, no single character can be found by which one form can satisfactorily be separated from the other. In the fresh condition the only distinction between typical specimens of the two forms mentioned, and also in two forms of another species of *Salsola,* is that of the stem-colour, and neither form possesses any odour that can be detected. It would be of great interest, therefore, could we discover how the animals are able to distinguish the palatable from the unpalatable form, since we might then become as acute as they appear to be in appreciating the significance of fine distinctions.

Certain South African species of the genus *Pentzia* afford another interesting case of the superior discriminating power of animals over botanists.

The Pentzias in question are strongly scented. It was noticed that sheep and caterpillars were feeding on a large stretch of country covered by these plants, and that some of them were being eaten while others were avoided. Representative specimens of all those that were being eaten and of those that were avoided were collected by our present Assistant for South Africa and brought to Kew for critical examination. No specific difference between the different ‘ types ’ could be detected during a preliminary examination in the field, though they could be recognised one from another by their external appearance.

Detailed field notes of each specimen were made on the spot for use in the herbarium investigation, since it was found that some of the plants were greedily devoured, some were most carefully avoided, while others were usually left untouched; when, however, the latter were grazed, as sometimes happened, unmistakable symptoms of nervous depression were produced in the animals.

On Taxonomic grounds we must regard all three forms as being members of one and the same species, since no morphological difference of any value can be detected between them.

Then again there are puzzling problems connected with the character of certain species on different types of soil, for it has been noticed in South Africa that, while a species may be a useful pasture plant on, say, a red loamy soil, yet when the same species, growing on tufaceous limestone, is eaten by stock a heavy mortality may result. Here, then, is another interesting problem in the domain of applied botany and soil chemistry, which, like the cases mentioned earlier, may also fall into the domain of Taxonomic Botany.

It is also very remarkable that the Indian Lac insect (*Coccus lacca*) has drawn our attention to the existence of two physiological forms of

*Schleichera trijuga* (Sapindaceæ), and to two forms of *Butea frondosa* (Leguminosæ), upon one of which it feeds while the other it does not touch; yet the Botanist is unable to separate them in either case!

These are all matters of great scientific interest in relation to questions concerning the possible origin of new species, but when, owing to some environmental change, a species which is valuable for grazing purposes on one type of soil is found on another type of soil to be definitely poisonous, the question assumes a wider interest and comes within the range both of the Taxonomic and the Economic Botanist. It may be in cases like these that we are witnessing the inception or the incidence of 'new' species, from the physiological standpoint, on a parallel with the morphologically distinct forms which have been shown to arise in the course of hybridisation.

The Taxonomist of the present day, I have attempted briefly to point out, is faced by many problems connected with the nature of his units and how they are bounded. He realises that 'the making of many "species" is a weariness to the flesh,' and that, especially when it is done with a narrow outlook, it is a hindrance rather than a help to progress. Further, Taxonomic work pursued in a narrow and unenlightened manner undoubtedly has tended to divert possible adherents from our ranks.

Taxonomy, I think it is fair to say has, until recently, failed to arouse the interest of the younger botanists mainly because it has not been put before them in an attractive manner, for the intimate inter-relationship of this branch of botany with ecology, genetics and cytology has not been properly emphasised. Traditional Taxonomy, as I have hinted, has until recently appeared to be a specialised and somewhat narrow occupation, and its disciples, with good reason, have often been regarded as born and not made. This tradition is by no means dead, with the result that botanical Taxonomy is apt to be thought of as a subject which is, dare I say, like golf has been sometimes considered, a pastime for those beyond middle age! Nor are its adherents always considered to be of the same intellectual calibre as their brother botanists who are engaged in what are regarded as the higher lines of research pursued in botanical laboratories. If this is so—and I believe there is some truth in the statement—it is, I feel sure, simply because the great importance of Taxonomy and its far-reaching interests have not been adequately presented or realised.

As it is so desirable that the importance and value of Taxonomic work in its widest sense should be better appreciated in our schools and universities, I think it is worth while to say something as to what is now implied by Taxonomy in the light of modern developments, in the hope that Taxonomy, combined with Ecology, may again occupy a prominent place in the studies of our developing botanists.

It is true, of course, that the Taxonomist must know his plants and must be able, with careful training, to use to the full his powers of observation and deduction, so that he can appreciate small differences, weigh evidence, and draw up descriptions in comparison with allied species, &c.; but he will not go very far if he stops there.

I have mentioned how unsatisfactory it is to work on single isolated specimens—though often, unfortunately, this may in some cases be all the material at our command—since isolated specimens, detached from their environment, do not allow the Taxonomist to judge to what extent a species may be plastic.

It may well be that the single specimen is not truly typical, for it may be of the nature of a habitat form, which Cockayne terms an 'Epharmone' —the 'phenotype' of Turesson—or, on the other hand, it may be a 'genotype,' that is, a 'Jordanon' according to Cockayne, and represent one of a group of such units which can be linked together in a 'compound species'; or, again, it may be one of a 'hybrid swarm' or a segregate resulting from hybridisation.

That we are appreciating now the problems surrounding every species which we are able to examine critically, through studying it in the field and if need be under cultivation, is a healthy sign; for it is, I think, clear that the Taxonomist, in undertaking experimental and field studies, will be able to throw much light on the 'origin of species,' and on the meaning and importance of the so-called 'variations' which such experimental study reveals.

This seeking after truth by means of experiment is not exactly a new development, though it may be claimed that the conception and planning, during the past few years, of new lines of inquiry has raised the status of these experiments to the definite plane of research.

It will be recalled that isolated experiments to test the persistence of individual forms, varieties or species have been made from time to time since Linnæus' day, but it is only in recent years that they have been carried out under careful control.

The classical experiments of Gaston Bonnier are well known. I may remind you that Bonnier cloned herbaceous perennial plants; half of an individual was grown in a lowland garden, while the other half was planted in a high mountain garden in the Western Alps or in the Pyrenees.[4]

His results were striking and full of interest. Fifty-eight of the species with which he experimented were able to maintain themselves at the higher altitudes, and underwent changes which caused them closely to resemble indigenous Alpine plants. Remarkable as his results undoubtedly were, it is unfortunate that they appear to have been conducted without sufficient control, and also that he did not work with a much larger number of individuals of the same species. We also lack the full details of his experiments, nor are there any herbarium specimens as evidence of the changes described, which would serve as records of these interesting experiments. It is therefore much to be hoped that his work may be repeated in France or Switzerland in the light of modern requirements, since in England it is hardly possible to carry out experiments of this character with regard to effects of altitude. Daniel,[5] working on *Asphodelus luteus*, transplanted portions of plants growing at Rennes to a seaside garden at Erquy. Such striking changes were brought about by this transplant experiment that he described the derived forms as a distinct species, *A. luteioides*.

F. Krasan[6] has also published papers recording the direct influence of the environment on plant characters.

[4] See *Rev. Gen. Bot.* (1920), XXXII. 305.
[5] *Rev. Gen. Bot.* (1921), XXXIII. 225, 316, 357, 420.
[6] *Flora*, XCVIII. 389 (1908).

It is when we come to the work of Turesson in Sweden, and the experiments conducted by Clements and by Hall in America, that the importance of transplant work to Taxonomists, Geneticists and Ecologists can be fully understood. Turesson's experiments[7] have been conducted with great care, and are of far greater value than any previous transplant work. He collected wild material from habitats studied by himself in Scandinavia and other parts of Europe, and worked with a large number of common, widely distributed and 'polymorphic' species. He has been able to prove that considerable hereditary potential variation exists within these species, and has shown that the naturally occurring variations can be grouped into different types confined to definite habitats. Turesson's experiments indicate clearly that there may be a close parallelism between mere phenotypic modifications (fluctuations of some) and heritable variation. It is only by experiment, however, that a decision can be reached as to whether a given variation has a gene basis or not; for it is only when its genetical nature is known that the admission of any variation as a distinct species or variety should be entertained from the Taxonomic point of view.

It is because Turesson has used so large a number of plants of each species in his experiments, that his contributions to the subject of 'race ecology,' or 'gene-ecology' as he terms it, are of so much value. As far as methods are concerned he has placed this line of work on a sound basis. As to his nomenclature, however, there may be some divisions of opinion into which I need not enter, as it is a matter which mainly concerns the experimental ecologist.

The value of Turesson's work, speaking generally, may be said to be that he has been able to come to conclusions as to the different types of variation shown by the plant he has observed, both growing wild and under cultivation, and has been able to demonstrate that in some cases they are of a heritable nature, while in others they are merely fluctuations.

The species problem, therefore, in the light of Turesson's experiments, which are borne out by what I shall have to say about our own transplant experiments, is definitely becoming an ecologico-genetical problem.

These new lines of research, which bring together Ecology, Genetics and Taxonomy, and are yielding results of value to botanists working on these three lines, are now being actively pursued at Potterne, in Wiltshire, and at Kew along somewhat different lines.

As I consider them likely to lead to results of considerable importance, I think it will be useful to give a short account of the experiments now in progress. During a visit to the United States, in connection with the International Congress of Plant Sciences at Ithaca in 1926, the transplant experiments that were being undertaken by Prof. H. M. Hall in California were studied. These are being carried out with cloned plants of several genera, and at different altitudes like those of Gaston Bonnier, and it seemed desirable to attempt experiments on somewhat similar lines in England. As, however, experimental cultivation at different altitudes in the British Isles would not be likely to afford results of any great value, it was decided by the Committee appointed by the Ecological Society, who were keenly interested in the proposal, to carry out experiments in growing certain

[7] See papers in 'Hereditas,' from vol. iii. 1922, onwards.

plants, of known genetical constitution, on four different types of soil under precisely similar climatic conditions in one spot.

Thanks to the kindness and keen interest of Mr. E. M. Marsden-Jones, now an honorary associate on the staff of the Royal Botanic Gardens, Kew, the experiments are being made by him in his garden at Potterne, near Devizes, in co-operation with Dr. W. B. Turrill. Four large raised beds, 37 feet by 10 feet, enclosed by old railway sleepers, have been made side by side, and each has been filled with a distinct type of soil—clay, chalky clay, calcareous sand and non-calcareous sand. Sets of the plants under investigation are also being grown on the natural upper greensand soil at Potterne and on the light sandy Kew soil in the Herbarium garden ground.

On each type of soil twenty-five individuals of each of six species are now being grown, all being of known genetic origin. Climatic conditions are being recorded throughout each year, and full records of all features connected with the growth and behaviour of all the plants on the different soils are being kept.

This is now the fourth year of the experiment, but it is the first year in which six different approved plants have been under cultivation.

A full report of the work up to October of last year has just been published in the 'Journal of Ecology,' and the following summary is abridged from the annual report of Kew activities during the past year.[8]

The species transplanted are *Centaurea nemoralis* Jord., *Silene vulgaris* Garcke, *S. maritima* L., *Anthyllis vulneraria* L., and *Plantago major* L., while during this year *Fragaria vesca* L. has been added.

It is interesting to find that the most obvious changes are taking place in *Silene vulgaris*, *S. maritima* and *Plantago major*.

*Centaurea nemoralis* has shown little change, but the general tone is better on the clays than on the sands, though flowering commenced first on the latter. The mean number of stems per plant was higher on the clays than on the sands.

In *Anthyllis vulneraria* morphological changes of a qualitative nature have not occurred, but some interesting facts regarding selection have been obtained ; unfortunately a high death rate has occurred on the sand and on Potterne soil. Edaphic factors are obviously important in causing the known natural limitation of this species, and these should be considered when it is proposed to cultivate *Anthyllis* as a forage plant. The transplant results suggest that on suitable land it would be a valuable and relatively persistent crop.

*Plantago major* has proved exceedingly plastic, even within five months last year, and even more so this year. The original plant was a dwarf form, and this habit has been very nearly retained on the sand and to a less degree on the calcareous sand, but it has become markedly luxuriant on the clay and somewhat less so on the chalky clay.

Some plants of *Silene vulgaris* on the calcareous sand have developed a markedly 'strict' habit, similar to that which has been many times observed in individuals among wild populations, and this may be found to have a genetical basis. On the calcareous sand the foliage has developed

[8] See *Journal of Ecology*, August 1930. *Kew Bulletin, Appendix I*, 1930, pp. 45–47.

a lighter-green colour, on the clay a more yellowish-green colour, and on the chalky clay a more blue-green colour than in the parent. Secondary growth has occurred especially on the sand. General tone was best on the clay and worst on the calcareous sand.

In *Silene maritima* there was a marked though irregular tendency for the plants on the sand to change to plants with smaller leaves, and with more anthocyanin and a flatter habit than in the parent. On the calcareous sand the leaves were narrower and smaller, the plants were flattened, and the calyces more red than in the parent. On both clays there was little change from the parent. General tone was best on the chalky clay and worst on the clay.

To sum up: *Centaurea nemoralis* does not at present appear to be plastic, but will survive under a wide range of edaphic conditions; *Silene vulgaris* is slowly plastic under certain edaphic conditions; *S. maritima* is decidedly more plastic than its congener; *Anthyllis vulneraria* is not plastic, and is not capable of survival under a wide range of edaphic conditions, and *Plantago major* is exceedingly plastic.[9]

It is obvious that the experiments, to be of real value, must be continued for a long period of years. The making of soils from raw materials can be slowly followed in the beds, and it is hoped that periodic analyses will yield useful pedological data.

In *Centaurea, Silene vulgaris, S. maritima* and *Anthyllis* genetical research is being continued which involves the use of lines from which the transplant materials originated, and this work is being correlated with field, laboratory and herbarium studies.

Apart from actual changes in the plants and from stages in soil-making, many interesting biological facts are noticeable. Since the plants are grown in the absence of competition, mass or individual differences must, on the whole, be due to edaphic factors. Plants, however, are individuals, and the history of a given individual is never exactly like that of any other. 'Accidents' also happen to individuals, and therefore records must as a rule be of a statistical nature with the limitations of this method.

Though the Kew-Potterne transplant experiments may be regarded as being only in their infancy, it is already evident that the experiments are yielding information of great value in the domains of Taxonomy, Ecology and Genetics. These results are of all the more value owing to the careful records which are being kept for each individual plant, whose history can be traced from the commencement, and may also be studied in the extensive series of herbarium specimens which are being preserved at Kew.

In addition to what the Taxonomist is seeking to discover from this intensive study of plants by means of 'transplant experiments,' he is also anxious to elucidate the problems associated with certain 'critical' British and European genera, such as *Silene, Centaurea, Rubus, Taraxacum* and *Hieracium*, in which the 'British' botanist or his Continental homologue have described a multiplicity of species. In the case of *Taraxacum* and *Hieracium*, the normal occurrence of parthenogenesis must surely entirely modify our conception of so-called 'species' in these genera, and make us

[9] These records were made before the September drought of 1929.

realise that they would both repay carefully arranged cultural and cytological research.

In the case of *Rubus* also it seems likely that carefully controlled experiments would possibly reveal the fact that habitat or hybridisation, rather than a '*fixed*' type, was the *raison d'être* of several 'species' now immortalised in the *Index Kewensis.* Whatever research may reveal in these genera, it has been shown in *Centaurea,*[10] as the result of careful genetical experiments, that at least three described 'species' are of hybrid nature or origin, for exact counterparts of *Centaurea jungens* Gugl., *C. pratensis* Thuill. and *C. Drucei* C. E. Britt., have been artificially produced by Marsden-Jones and Turrill at Potterne, and have been proved either to be hybrids or segregates from hybrids. It is evident also that some half-dozen other 'species' of *Centaurea* will have to be similarly reduced in the course of the next year or two, when the experimental investigations have been completed. From this work, and from similar experiments with *Silene maritima* and *S. vulgaris,*[11] it seems evident that hybridisation is common in the wild flora of Britain, and that 'hybrid swarms' occur, comparable to those to which Lotsy has called attention in South Africa, and Cockayne and Allan have demonstrated so clearly in the New Zealand flora.

In dealing with intra-specific variation within a polymorphic 'Linneon,' such as *Silene maritima,* Marsden-Jones and Turrill have wisely refrained from coining a number of new names, but have attempted to formulate a scheme comparable with chemical symbolism, which should prove of considerable assistance to botanists who are confronted with similar difficulties in other groups of plants showing similar polymorphism.

An important development, arising out of the more intensive study of wild species and possible hybrids and the associated genetical work and controlled cultivation, which is so pregnant of far-reaching results, is the need of greatly extended Herbarium records and field notes. For genetical work to be of permanent value it is essential that ample material of the parent plants and their offspring should be preserved for reference, and in the case of assumed wild hybrids, representative specimens of the parents and of all the linking forms are required.

I am glad to say that at Kew we have now established special 'herbaria' for genetical specimens and for hybrids, where specimens forming as complete a set as possible are kept together, apart from the General Herbarium collection.[12] At present it is fairly rich in certain groups of New Zealand hybrids, thanks to the kindness of Dr. Cockayne and his associates in the Dominion. We also have a very full set of specimens from the plants which are being used in our transplant experiments, which exhibit clearly all the changes which so far have been recorded. There is also a good series of mounted sheets showing the hybrid forms of *Centaurea, Silene, Saxifraga potternensis,* &c., which have been produced under controlled experimental conditions, together with a collection of similar 'forms' which have been discovered in the wild condition.

[10] See *Gardeners' Chronicle,* March 15, 1930, p. 210.
[11] *Kew Bulletin,* 1929, pp. 145–175.
[12] See *Kew Bulletin,* Appendix I. 1929, p. 42; 1930, p. 40.

In addition we are getting together a collection of specimens showing certain plants in all stages of their development, when growing wild under very different soil and climatic conditions, all of which should be of great value in careful ecological work.

I may also mention here, as a further development of our herbarium activities, which is proving of great practical value, that we have formed a collection of fruits and seeds, which it is hoped in course of time will be as comprehensive and complete as is the collection of the vegetative and floral specimens in the General Herbarium. The value of this collection to Mrs. Clement Reid and Miss Chandler in connection with their study of recent fossil fruits and seeds has quickly been recognised. It has also proved of great value for identifying samples of weed seeds, seeds accused of poisoning stock, seeds used for adulteration purposes, and those used as drugs. It is clear that the study of the seeds and their markings &c., of all the species of a large genus, may also enable botanists to arrive at some important deductions when making critical revisions.

These special herbarium collections, together with the 'Herbarium garden' recently established at Kew, are, I think, very important developments for our Taxonomic and Economic studies.

In the Herbarium garden,[13] for instance, many plants of botanical interest—'weeds,' perchance, to the ordinary gardener—can be grown, and their development studied in detail. While from these plants herbarium specimens can be prepared and preserved, showing not only all stages in development, but also the character of the root system, particulars which are usually so sadly lacking in specimens collected, often in haste, in the course of some excursion or expedition.

So much, then, for some of the modern developments and opportunities in the domain of Taxonomic botany. Now let me turn to somewhat similar problems which have recently been brought to our notice on the Economic side. Very often it will seem that the matters to which I shall refer belong rather to the domain of plant physiology, but since those to which I propose to draw your attention are mainly concerned with plants of economic importance they do, therefore, actually come within the purview of what we generally consider to belong to the realms of Economic Botany.

In the first place I would draw your attention to the interesting observations made by Dr. A. B. Stout and others on the flower behaviour of Avocados,[14] *Persea gratissima* Gaert. (Lauraceæ). These afford an excellent example of the assistance that the botanist can render to the grower and of the practical application of a remarkable botanical phenomenon of great scientific interest.

The Avocado Pear bears hermaphrodite flowers, but they exhibit a daily rhythmic alternation of sexes reaching maturity for the entire plant. This synchronous dichogamy apparently reaches a perfection of physiological regulation to ensure cross-pollination, unknown in any other group of plants.

[13] See *Kew Bulletin*, Appendix I. 1930, p. 44.
[14] 'Memoirs of the New York Botanical Garden,' A. B. Stout, vol. vii. p. 145 (1927).

All the flowers that may be open at any one time, on trees of the same clonal variety, are in either the female or the male condition. If the trees belong to one of the varieties placed in 'Class A' by Stout, of which the Taylor variety is taken as an example, the flowers when they first open in the morning are found to be functioning as females with a receptive stigma, but the anthers are not yet mature.

About midday these female flowers close, for none but flowers in the female state are open on the trees, and another set of flowers then opens in the early afternoon, normally without any overlapping, so that there are never on any tree of 'Class A' flowers in the male and flowers in the female condition open at one and the same time. These afternoon flowers are found to be in the male condition with the stigma withered; the anthers are in an upright position, with their valves open and shedding their pollen.

Careful investigation of trees of 'Class A' has shown that the flowers, when they first open, function as females for some four hours in the forenoon; they then close about midday, remain closed all night and all the following morning, and reopen on the afternoon of the second day in the male condition. Self-pollination of individual flowers is thus rendered impossible by this sex-alternation, and since there is normally a definite time interval, about midday, when no flowers on trees of the same 'Class' are open, cross-pollination on the same tree or between different trees of the same clonal variety can rarely occur.

This rhythmic phenomenon is all the more remarkable because there is an entire reversal of the process just described in other clonal varieties and individual seedlings, which Stout places in his 'Class B.'

In trees belonging to 'Class B' the flowers are in the *male* condition when those of 'Class A' have their stigmas receptive, and are *female* when the pollen of 'Class A' trees is being shed. These reciprocating changes in sex thus provide the opportunity for mutual cross-pollination between the trees of 'Class A' and those of 'Class B.'

The practical application of this discovery hardly needs pointing out, but it is clear that an orchard planted with trees of only one variety is not likely to yield a rich harvest of fruit! That the right selection of the varieties for interplanting can now be made, the grower has to thank the botanist, since it is now possible for him to obtain a maximum yield of fruit from his plantation.

I was interested to learn recently from a former Director of Agriculture, Bermuda, that they could never get Avocados to fruit in the Bermudas. Prizes were offered and cultivation devices, spraying, &c., were tried, all to no purpose. Evidently they were growing trees of only one clonal variety, and had they known of the sex-alternation they would have been able at an earlier stage to develop a profitable industry.

The scientific research which has revealed and elucidated the natural phenomenon exhibited by the Avocado, the full significance of which is a matter of so much consequence in the practice of husbandry, is an interesting example of hidden possibilities being brought to light by scientific research; and suggests comparison with some of the economic problems in the botanical direction, where a demand is made on the scientific worker to produce some economic plant, of a type suited to the requirements of some particular district, in order that it may be capable of commercial exploitation. In the one case the botanist reveals to the commercial grower the secret which will give him success; in the other the commercial grower insists on the botanist providing him with the type of plant he requires in order to make an enterprise successful. Two somewhat different aspects of the relations between pure and applied botany. A few examples of the latter type of problem are worthy of bringing to your notice, as they relate to such important crops as pistachio nuts, limes and bananas.

Pistachio nuts are grown as a crop in California, and the problem facing the plant breeder, if he is to satisfy the grower, is to produce varieties bearing nuts which crack naturally. If varieties are produced, the nuts of which have to be cracked by hand, they are of no value commercially, however good the nuts may be in size or flavour, since the labour cost involved in cracking by hand in the United States is prohibitive if the nuts are to be sold at a profit!

Fortunately scientific research has now produced the desired article, and those who delight in pistachio ices, &c., can rest assured that they are coloured and flavoured by the genuine article and not by some synthetic product.

Limes, again, the staple industry of Dominica, present a curious and difficult problem. The wither-tip disease has made it imperative to carry out experiments with the object of producing races or varieties immune to the disease.

There seems, fortunately, good prospect of success attending these efforts, so far, at any rate, as the production of an immune type is concerned. Dominica, however, is very hilly, and the lime bushes are grown on such steep hillsides that hand-picking of the fruit would be very costly, and in some cases well-nigh impossible. The lime of commerce, as is well known, has the useful habit of shedding its fruit when ripe, so that the Dominican peasant merely has to go and collect the fruit under the trees or bushes. The problem before the plant-breeder working on limes, therefore, is to produce a lime which not only is immune to disease, but which will also shed its fruit when ripe. Unless this second essential can be attained the new variety is of little or no commercial value.

The banana problem, connected with the attempt to produce a strain immune to Panama disease, is also hampered by a somewhat similar economic question. In this case it is necessary that the fruits should be incurved, so that in each 'hand' the apices of the bananas curve inwards towards the stem. In this way the bunches can be easily handled without injury to the fruit, and also there is the further practical advantage—they take up the minimum of space on board ship—two practical points which make all the difference between success and failure in a commercial enterprise—but matters which may baffle the ingenuity of the botanist and geneticist for many a long day before a satisfactory solution, i.e. the successful combining of the two desired characters, can be attained.

Dr. Walter T. Swingle, Principal Physiologist of the U.S. Department of Agriculture, has been writing to me recently about the remarkable researches on the pollination of the date palm (*Phœnix dactylifera* L.) which he and Mr. Roy W. Nixon[15] have been conducting.

'Each species of *Phœnix*,' he writes in his last letter of May 22, 'seems to have determined its peculiar action in ripening the fruit of the date palm. The most amazing thing is that the pollen of the huge Canary Island palm used on the date palm produces a small, peculiarly pointed seed, quite different from the ordinary date seed, and small or medium-sized fruit that ripens late, whereas the tiny palm commonly called *P. Roebelinii*, which has the smallest seeds of any wild form of *Phœnix* known, when used to pollinate the true date palm, causes the formation of large seeds, usually with a curious sunken area about the germ pore and makes large dates which ripen extremely late, nearly two months later than the ordinary crop. Preliminary tests of *Phœnix sylvestris*, from India, seem to give medium-sized seeds and medium to large-sized dates, ripening earlier.'

The economic importance and scientific interest of these discoveries need no comment.

Systematic botanists in the past have, I think, been rather too apt to regard the 'species' they have described as fairly definite units, recognising and recording from time to time 'varieties,' but, as I have said earlier, frequently without sufficient material to enable them to say what such varieties really represent, or how constant and definite they may be. In some cases they may be the so-called 'Jordanons,' while in others, no doubt, as we are beginning more fully to realise, they are the resultants of hybridisation. For the majority of plants the occurrence of such 'varietal' forms appears to be of little more than purely scientific interest, and they may be passed by with only a casual comment.

When, however, almost any plant comes into the limelight of Applied Botany and is found to be of some economic value, then the importance and significance of varietal differences at once becomes apparent. A few cases may be cited in illustration :—

Para rubber (*Hevea brasiliensis*) is considered to be a good botanical species, but a careful examination of the trees now being grown in plantations in the East reveals a number of forms, very similar as regards their morphological characters, but showing marked physiological differences, especially with regard to the yield of latex.

The planter, therefore, who has the good fortune to own a plantation of high-yielding trees is in a favourable position compared with a neighbour whose trees may only yield the minimum quantity of latex. Here again the problem is one for the geneticist to solve, or it may rather prove to come within the province of the horticulturist and involve budding, with the selection of suitable stocks and scions, as is being done in Ceylon, Java and elsewhere, on lines similar to those adopted in relation to comparable problems with apples, pears and plums at home, which are being

[15] Swingle, Walter T. 'Metaxenia in the Date Palm,' in *Journ. of Heredity*, Vol. xix, No. 6 (1928), pp. 257–268. Roy W. Nixon, 'The Direct Effect of Pollen on the Fruit of the Date Palm,' in the *Journ. Agric. Research*, Vol. xxxvi., No. 2 (1928), pp. 97–128; and 'Immediate Influence of Pollen,' in *Journ. of Heredity*, Vol. xix, No. 6 (1928), pp. 241–255.

studied so successfully at East Malling, Long Ashton and Merton; or with cacao, which is engaging the attention of botanists and agriculturists in Trinidad, Ceylon and the Gold Coast.

A similar problem, where the systematic botanist requires the assistance of his economic colleague, has recently been investigated in Australia by Messrs. Penfold and Morrison of the Technological Museum, Sydney.[16] This concerns the oil yielded by *Eucalyptus dives* Schæur.

*E. dives* is a species easy of botanical determination, and is of economic value for its oil, which has a piperitone content of about 45–50 per cent. which is used for the manufacture of thymol and menthol. The recently increased demand for synthetic thymol and menthol has led to fresh areas being exploited, and oil has been obtained yielding only 5–15 per cent. of piperitone—morphologically, however, the trees were true *E. dives*—while some other trees, which Australian botanists referred unhesitatingly to this species, contain oil with under 5 per cent. piperitone and 45-75 per cent. cineol. It might be thought that ecological conditions are concerned in these striking differences—for a typical form and three distinct physiological varieties have been recognised by their oil characters—but near Goulbourn, N.S. Wales, the type form with 40–50 per cent. piperitone has been found growing alongside the variety B, containing only 10–20 per cent. piperitone with 25–50 per cent. cineol. The latter form is of no commercial importance, and it was because oils with a low percentage of piperitone were coming to the distillers, who therefore supposed the oil had been adulterated, that these physiological varieties came to be detected.[17]

Here, then, is an interesting piece of investigation which brings the botanist into alliance with the chemist.

A similar problem exists with regard to camphor, where, as is well known, two, and perhaps more, physiological varieties exist in the species *Cinnamonium Camphora*, which botanists are unable to separate. In the one case solid camphor is yielded on distillation, in the other camphor oil; and it is even stated by the Japanese authorities in Formosa that from one side of the stem of a tree solid camphor may be obtained, while the other side yields only oil. Whether this be true or not, it is the fact that the valuable economic tree is that which yields solid camphor, and that in our Colonies, especially in Mauritius, practically all the trees are oil-yielders, and therefore well-nigh valueless.

Since this is a matter of considerable economic importance, it has seemed desirable to test whether climatic or other conditions in any way influence the character of the product, and in the hope of solving the question layers

[16] The Occurrence of a number of Varieties of *Eucalyptus dives* as determined by Analyses of the Essential Oils, Part I, 1927, Part II, 1928—A. R. Penfold and F. R. Morrison, *Journ. & Royal Society, N.S.W.*, Vol. lxi. and lxii.

[17] Messrs. Penfold and Morrison consider that the varieties, between which there are intermediate forms, may be classed as follows :—

1. *E. dives, Type* . Piperitone 40–50 per cent., Phellandrene 40 per cent.
2. *Var.* A . . Piperitone 5–15 per cent., Phellandrene 60–80 per cent.
3. *Var.* B. . . Piperitone 10–20 per cent., Cineol 25–50 per cent., together with Phellandrene.
4. *Var.* C. . . Cineol 45–75 per cent., Piperitone under 5 per cent., Phellandrene absent or present in small quantity only.

Only the Type and Variety C are of commercial importance.

of authentic camphor-yielding plants from Ceylon have been sent to Mauritius, and layers of oil-yielding plants have been sent from Mauritius to Ceylon.

It is hoped that the experiment may furnish some interesting results, and may also possibly enable us also to formulate some conclusions as to the significance of the physiological varieties of *Eucalyptus dives.* In the latter, known as the broad-leaf peppermint, the proportions of various constituents appear to vary somewhat in some of the varieties depending on the time of year at which material was collected for examination.[18]

I may, perhaps, be allowed to refer to one more instance drawn from the realms of the economic side of Systematic Botany, which, as in the cases to which I have alluded, may prove to be of profound importance.

[18] See Penfold and Morrison in *Journ. & Proc. R.S.*, N.S. Wales, vol. xlii (1928), p. 74. Similar instances were noted at Tumbarumba. A sample of oil distilled from the leaves and terminal branchlets selected from a clump of seven trees was found to contain a small quantity of phellandrene, thus rendering an otherwise excellent oil valueless for medicinal purposes. Moreover, the crushed leaves yielded the excellent aroma of cineol-terpineol-citral.

The medicinal oils for internal use contain as principal constituent a colourless liquid with a camphoraceous odour called cineol or eucalyptol, and the well-known curative properties of such oils for colds, influenza, &c., are generally attributed to this body. . . .

Such medicinal oils must be free from the terpene phellandrene, as this is considered to affect the heart if present in any quantity. Phellandrene, however, is a very valuable oil for industrial purposes, and forms the principal component, or occurs in considerable quantity in the industrial oil group, such as *E. phellandra*, *E. dives* and *E. radiata*.

It was, therefore, very difficult to account for the adverse report. After a special search the trees were located, and were found to be botanically identical. The first six trees examined proved to be *E. dives*, var. 'C'; the seventh tree, however, yielded an oil rich in phellandrene (piperitone and piperitol could also be detected, but very little cineol), and was approximately the variety 'B.' It is a remarkable fact that if the leaves of the first six trees only had been distilled the oil would have been very favourably reported upon, but owing to the inclusion of the leaves from the seventh tree, phellandrene was present in the oil, which resulted in its being condemned.

Again at Mannus Hill it is recorded that on one side of the road a number of trees of the 'Type' were growing distributed in a grove of trees composed of the varieties 'B' and 'C,' whilst on the other side, trees of the 'B' variety were distributed through a belt mainly consisting of variety 'C.' On crushing the leaves of one tree the piperitone-phellandrene odour was pronounced, while crushed leaves of a tree only three feet away exhaled the refreshing aroma of cineol with a little citral.

Messrs. Penfold and Morrison have examined large areas of *E. dives* in New South Wales and Victoria and have shown that in one locality one variety appears to predominate.

They give the following particulars in *Journ. & Proc. Roy. Soc.*, N.S. Wales, vol. lxiii, part iii (1929), p. 84:—

1. In the Braidwood district the 'Type' prevails, with small quantities of variety 'A.'
2. Near Goulburn the 'Type' with variety 'B' are found.
3. In the Tumbarumba district variety 'C' is the predominating form, with very little of the 'Type.'
4. While in Victoria very large areas of variety 'A' are growing in conjunction with the 'Type.'

A similar condition of affairs is also recorded for *Eucalyptus piperita*, in Notes on *Eucalyptus piperita* and its essential oils with special reference to their piperitone content. Part I, A. R. Penfold and F. R. Morrison in *Journ. & Proc. Roy. Soc.*, N.S. Wales, vol. lviii, 1924, where two marked, physiological varieties have been detected by chemical methods.

The Tung oil trees, *Aleurites Fordii* and *A. montana*, whose seed yields a very valuable drying oil, are now being introduced through Kew and the Imperial Institute to all suitable Dominions and Colonies. In these trees the flowers are borne in clusters, and each flower-cluster usually consists of a large number of male flowers surrounding a single female flower. It was noticed some years ago that certain trees under cultivation in America bore two or three female flowers in each cluster or inflorescence. Selected seed from this 'multiple-cluster' type appears to transmit this characteristic, and trees showing this favourable variation may thus be expected to crop more heavily, and yield more oil, than trees with only one female flower in the cluster.

Trees of *A. Fordii* planted in New South Wales are proving very variable in their fruit yield, and Mr. Penfold informs me that the yield of fruit per tree varies from 25-362; unfortunately we do not yet know whether the higher-yielding trees are of the 'multiple-cluster' type or whether they are only 'high-yielders' of the normal form.

The problem, therefore, which may arise is analogous to that which confronts us with Para rubber in the matter of latex-yield; with Cacao as regards permanent poor-yielders and permanent heavy-yielders; or with Cavendish Bananas in the Canary Islands, some forms of which yield bunches of fruit from suckers only 13 months old, while in other cases 30 months elapse before the fruits are ripened. Cases such as these, and there are many others of a like nature, afford an apt illustration that Economic and Systematic Botany can provide romances, possibly of more scientific interest to the botanist than to the commercial planter, but of so great material importance to the latter that the botanist looks to the man of affairs for the financial assistance to help him to discover their solution.

Comparable with what has been described for *Eucalyptus dives* is the case of the Indian grass *Cymbopogon Martinii* Stapf. Two forms of this grass are recognised, which grow on adjoining parts of the hills of the Bombay Presidency and other parts of India. One form, 'Motia,' yields an oil with an average *Geraniol* content of 91.3 per cent., and prefers the drier hillsides—the other, 'Sofia,' with an average of 42.7 per cent. of *Geraniol* in the oil, occurs in the moister localities. In some parts these two physiological varieties grow in contiguous areas and tend to intermix where they grow close together. They can be recognised by the differences in smell, but beyond a slight difference in the pose of the leaves they cannot be separated by any definable botanical characters. From the Economic point of view the essential difference between the two types of oil is that 'Sofia,' or 'ginger-grass' oil, contains a strong-smelling substance called i-carvon, which is not present in 'Motia' or 'Palmarosa' oil; this latter oil is the one which is considered of superior quality and commands a higher price in the markets.

The trees which yield Balsam of Peru and Balsam of Tolu afford a somewhat similar problem. These are regarded as varieties of *Myroxylon balsamum* (L.) Harms, or *Toluifera balsamum* L., the only recognisable difference so far found between them being the structure of the resin cells of the cotyledons in the two cases.[19]

[19] See *Harms, Notizblatt des Kgl. Bot. Gard.*, No. 43 (1908), p. 94.

While on the question of essential oils I may refer in passing to that peculiar and elusive subject, the loss of scent of the common Musk, *Mimulus moschatus* Dougl. I fear there must be some here who have never smelt musk, but I well remember its characteristic odour and how it was grown in pots in almost every cottage in the country, as it was reputed to keep away flies. As some of you will recollect, musk quite suddenly lost its scent a few years before the war, and apparently, though unfortunately we have no exact records, the loss of scent was universal. Despite repeated efforts no scented musk has since been found, though often reported, nor can I get material or seed from Western N. America—the home of the plant—with any trace of the characteristic scent. The plant was introduced to cultivation by David Douglas in 1826, and as far as we know all the wild native plants had the characteristic scent.[20] What has happened ? Is the musk plant now grown exactly the same as the old scented plant, and if so why did all the plants in cultivation as well as those growing wild in British Columbia, almost simultaneously as it would seem, lose their scent. Is this to be regarded as a sudden and universal mutation, and if we assume this, how much nearer are we to an explanation ? It would seem a problem worthy of the attention of the ecologist and chemist to attempt, by cultivating the plant in different soils and under diverse conditions, to try and regain the musk scent.

When we turn our attention to cultivated plants, the innumerable forms and varieties that have arisen in the course of cultivation are almost bewildering. I need only instance such plants as maize, the ground nut (*Arachis hypogæa*), *Voandzeia* or *Ricinus*, the castor oil, whose seeds furnish so remarkable a series of colour and pattern-forms and sizes, constant for each of the many cultivated races.

Or again, I may remind you of the various races or 'cultiforms' derived from *Brassica oleracea* ; cultivation during long ages has resulted in our cabbages, brussel-sprouts, kohl-rabi, cauliflowers and various types of kale. Other striking examples of mutations which have appeared in cultivation, without any possibility of inter-specific hybridisation, are afforded by such well-known 'garden' plants as *Cyclamen persicum* (*C. indicum*), *Primula obconica*, *P. malacoides*, *P. effusa* and our own primrose, *P. veris*. Not only have the plants under cultivation quickly become more robust and the size of flowers greatly enlarged, but marked changes have taken place in the colour and form of the flowers, while fimbriation of the corolla segments and doubling of the flowers has also taken place in the course of a few years. In cyclamen and in the primrose remarkable crestings

[20] Mr. W. B. Anderson of Victoria, British Columbia, who is an authority on the British Columbian flora, informs me, in a letter received in July through the Lieut.-Governor, that he noticed the loss of scent in the native musk plants in British Columbia a good many years ago, before he was aware of what had happened in Great Britain. Years ago at Millstream, where Musk was indigenous, all the plants were scented. Many years later at Comox, where it grows abundantly, a scented plant could not be found. Since then Mr. Anderson has failed to find scented musk plants anywhere in British Columbia. The Millstream locality was far away from any habitation and the scented plants could not have been introduced and were as strongly scented as those growing elsewhere. It seems clear, therefore, that the remarkable phenomenon noticed in Great Britain also occurred in British Columbia, the native home of *Mimulus moschatus*, since very careful search for many years has failed to reveal any scented plants where formerly they were abundant.

on the corolla segments have also been developed. With regard to doubling, it is of interest to notice that in *P. malacoides* and in *P. effusa* this has taken place within some four years after their introduction. Nuclear changes to the tetraploid condition have been found in some of these 'improved' forms, notably in *P. sinensis* and *P. malacoides*, while *P. obconica* has also become tetraploid, but there is no obvious difference between diploid and tetraploid plants.[21] If we invoke 'mutation' we do not seem to have advanced much further along the road, nor does intra-specific hybridisation throw much light as to the commencement of variation, though it may be effective when we have reached some well-marked varietal forms. It has been suggested that the effect of cultivation—i.e. good living and good feeding—has in some way broken down the constitution of the plant and given rise to a tendency to vary, but we have no definite evidence in support of this view; in all such cases we should like to know whether such variations occur among the plants as they grow wild and which may have escaped observation. If this is so, as those most competent to judge consider probable, we must assume that the observation of variants under cultivation is due to the fact that horticulturists are always on the look-out for small varietal differences, which as soon as they are noticed are selected and encouraged.

The explanation of this tendency to vary, displayed by plants whether under cultivation or in nature, should possibly be sought in the realms of chemistry rather than of cytology—we have seen something of the remarkable capabilities of the plant cell as a chemical laboratory in the case of *Eucalyptus dives* and other plants, and it may be that very small additions to, or deviations from, the normal food supply of the plant, as we know, for instance, in connection with the researches carried out with boron and manganese, or with our own transplant experiments with *Plantago major*, may so disturb the composition of the cytoplasm that the whole internal economy of the plant is upset and it becomes 'plastic.' As a result, since the germ-plasm also is affected by the stimulus, the observant cultivator is able to take full advantage of his opportunity, and by careful selection can develop and encourage the production of new and distinct forms.

We now know from Prof. Goodspeed's recent work on the effect of X-rays on the sexual cells of *Nicotiana* [22] that these rays bring about some striking changes in the germ-plasm, and we look forward presently to hearing some further particulars from him on his interesting work.

In all these matters we are still largely in the land of theory, but I think we may say that in our strivings towards the truth we are catching here and there flashes, which encourage us to proceed in our search for the light which will enlighten our darkness.

[21] This has been worked out by Mr. Philp at the John Innes Horticultural Institution.

[22] See Goodspeed, Prof. T. H., 'The Effect of X-rays and Radium on species of the genus *Nicotiana*,' in *Journ. of Heredity*, Vol. xx, No. 6, June 1929, pp. 243-259; 'Cytological and other features of variant plants produced from X-rayed sex cells of *Nicotiana tabacum*,' Bot. Gaz. lxxxvii, No. 5, June 1929, p. 563, and 'Occurrence of Triploid and Tetrapoid Individuals in X-ray Progenies of *Nicotiana tabacum*,' in Univ. Calif. Publ. i., *Botany*, Vol. ii, No. 17, pp. 299-308, 1930. *See also* Muller, H. J. 'The Problem of Genic Modification,' Zeitschr. für Indukt. Abstam. und Vererb., Supplement-band 1, 1928, p. 234.

At present our definite knowledge is fragmentary; we may, if we like, compare it to a few pieces of a 'jig-saw' puzzle. We have discovered a few of the pieces, whose import we do not fully understand, and if we could find some of the missing ones they would help us to visualise the picture. It is for us to try and collect more of the pieces and arrange them as far as may be possible; some undoubtedly fit together, but we do not yet appear to be in a position to make a guess as to what the completed picture may reveal.

You will, I think, have realised that the subjects I have discussed in the course of my address have been based largely on my experiences accumulated during my term of office at Kew, and on the opportunities I have enjoyed, both at home and in various parts of the Empire, of seeing the practical results of our efforts in the directions of Taxonomic and Economic Botany in all their wide and diverse applications.

The opportunity of visiting our Overseas domains, especially when one is in the responsible position of being 'Botanical Adviser to the Secretaries of State for the Dominions and Colonies,' is of immense value, not only because one is thus able to get into personal contact with the botanists, as well as with those working in allied branches, such as agriculture and forestry, but also because one is able to study on the spot the problems and difficulties which present themselves to our Overseas colleagues, and so visualise the directions in which help from the National Botanical Centre or from other institutions can be of the greatest assistance.

Until recently the information required at Kew could only be gleaned either through correspondence, official reports, or from discussions with Governors, Directors of Agriculture or the botanists on their staffs when on leave in England.

In earlier times this sufficed fairly well, and as is well known, was fruitful of many important results, especially during the directorship of Sir William Thiselton-Dyer.

With the growth of scientific activity in all parts of our Tropical Empire and with the necessary development of large and important agricultural and forestry departments out of the original botanic stations, the problems have become so many and so diverse that they could not properly be envisaged, nor could adequate advice be given, by anyone rooted to headquarters.

Thanks to the far-sighted wisdom of the Empire Marketing Board a great change, as you know, has been effected in the last few years, and with the grant given by the Board to Kew it is now possible for the Director or one of his superior officers to visit, on invitation, any part of the Empire where their presence may be required.

Further, a sum was set aside by the Board to allow Botanical Collectors to be sent overseas to study the vegetation of some parts of the Empire, or to collect specimens in some foreign country which might be of economic value for introducing to one of our own Colonies; thus restoring to Kew the privilege enjoyed in the days of Sir Joseph Banks and Sir William Hooker, which led to the introduction of so many plants of scientific and economic value both to the Colonies and to Kew.

The result of this grant has been that on the invitation of the Governments of Australia, New Zealand, the Straits Settlements and Federated Malay States and Ceylon, I was given the opportunity of visiting these countries during the winter and spring of 1927–8. A visit was also paid to Java. At the same time an invitation was received from the Government of the Union of South Africa, which has now been accepted.

The Assistant Director has visited Cyprus and the Sudan.

Mr. H. C. Sampson, the Economic Botanist appointed under the Empire Marketing Board scheme, has undertaken missions, at the request of the respective Governments, to British Guiana and the West Indies, British Honduras, the four West African Dependencies, in connection with the Agricultural Conference held in the Gold Coast last year, and he has recently returned from a visit to the Bahamas.

The Keeper of the Herbarium was also enabled, last year, to visit the various Botanical Institutions in South and East Africa, with which Kew is in intimate correspondence, after attending the meetings of the British Association.

Botanical collectors have been sent to Majorca to obtain graft material of almonds suitable for Cyprus; to the Malay States, Java, Siam, Burma, and Ceylon, to collect wild stocks and cultivated races of bananas to be sent ultimately, after being detained in quarantine at Kew, to the Imperial College of Tropical Agriculture, Trinidad, in connection with the research being undertaken there on the Panama disease.

Another collector, our present Curator, was sent to the East to study tropical vegetation and bring back collections of useful and interesting plants for distribution and to enrich the Kew collections; another, Mr. J. Hutchinson, was sent to South Africa to make careful studies of the flora, and by his collections to widen our knowledge of the types and associations of the vegetation, and he is now engaged on a similar undertaking in Rhodesia on the invitation of General the Right Hon. J. C. Smuts, whom we all look forward to greeting very heartily as President of the Association next year.

Other collecting enterprises on the part of Kew have been undertaken in the Solomon Islands, and on the British-Italian Somaliland Boundary, in connection with the recent joint Boundary Commission, whence Mr. C. L. Collenette has recently returned with a rich and remarkably interesting harvest of material accompanied by ecological information of very great value.

Dr. J. M. Cowan, now Assistant to the Regius Keeper, Royal Botanic Gardens, Edinburgh, who has been a member of the Kew staff for a time, went to Iraq and Persia last year. This was a joint undertaking arranged between Kew and the John Innes Horticultural Institution, Merton, and some very interesting living plants of horticultural and economic importance were brought home, and also a large collection of dried specimens for the Herbarium. Mr. N. Y. Sandwith, a member of the Herbarium staff, accompanied the Oxford University Expedition to British Guiana, and by his careful and intensive collecting has added very greatly to our knowledge of the forest flora of the Colony.

While to complete the story of our major enterprises in this direction, you will be interested to learn that a member of my staff, Mr. Milne-

Redhead, is now at work in Northern Rhodesia on the Aerial Survey that is being undertaken to make a careful study of the vegetation on ecological lines, as a guide to the future development of the country.

This brief summary of the activities of Kew will suffice to show that we are living in an era of progress and development and that we are alive to the opportunities offered of widening our outlook and our interests in the domains of Taxonomic and Economic Botany. As I have hinted earlier, our studies in Taxonomic Botany to be living and of practical value need to be transported from time to time from the Herbarium to the field. In this way only can we realise fully the extent and character of variations, the effects of soils and climates and the prevalence and significance of physiological races.

By the widening of our horizon through travel and by means of vegetational studies in the field, I feel myself on sure ground in maintaining that we are thereby more efficient, more enlightened and more useful Taxonomists, both in the pure and applied directions, than if our studies were strictly confined to the examination of the dried and mounted specimens in a herbarium.

I have attempted to put before you some of the modern problems and some of the recent advances in the realms of Taxonomic and Economic Botany, and have indicated how intimately they are connected with questions of Plant Physiology, Ecology and Genetics, while in many of the problems it is necessary to call upon the chemist for assistance. I hope I may also have succeeded in demonstrating that Taxonomic and Economic Botany, with the new opportunities, provide fields for investigation and research worthy of the attention of the best intellects among our rising generation of students of Natural History. The proper pursuit of these studies in the light of modern developments, demands investigation by experimental methods, as well as the examination of dried specimens, to which full powers of observation and deduction must be brought to bear. Added to this there is the stimulus of romance and the possibility of travel, which make the enterprise worthy of the undertaking. The picture which opens out before us is no new one, for in essentials it is the same as that which stimulated and inspired Charles Darwin, Joseph Hooker, Asa Gray the De Candolles, and other great pioneers in our science.

Yet vast and enthralling as is the prospect we seem somewhat to have failed to attract a sufficiency of able recruits. If this is so then we must needs look for the reason. We may, and in fact I think we are apt to say, like the 'Children sitting in the market-place,' 'We have piped unto you and ye have not danced'; but with whom does the fault lie? May it not be, as regards Taxonomic Botany, that we have piped on a wrong note, that 'we have' in fact 'mourned' in a minor key, and have failed to pitch our tune on the high note of enterprise and endeavour?

If I am not mistaken, and I gather Thiselton–Dyer[23] would have agreed with me, our 'tune' has been marred to our hearers by what I may call our vexatious and often discordant 'Variations on an original theme.' Need I say I refer to the millstone of nomenclature, which encumbers and

[23] Brit. Assn., Ipswich, 1895, Address to Botanical Section, p. 11.

weighs down the neck of the systematic botanist. The theme itself, 'Taxonomic Botany' in its widest sense, is full of charm and interest, but it has been so obscured that many have failed to be attracted by the grandeur and harmonies of its melody. It is to be hoped that as a result of our recent International Conference at Cambridge, many of our nomenclatorial troubles will have been laid to rest, and that we shall now be able to pursue our studies unhampered, being satisfied that the maximum of stability with the minimum amount of change, compatible with progress, is now assured.

Much of our failure to attract disciples is due, I fear, to the misplaced activities of those, whom I might call our Taxonomic 'Scribes and Pharisees,' who have burdened us with 'burdens grievous to be borne,' and have thereby tended to substitute the shadow for the substance.

Be that as it may, though times have changed and circumstances have altered, the spirit of investigation and the interest in Natural History and Natural Phenomena is, I believe, as much in evidence as ever it has been, and it is for us to point the way and bring the labourers into the vineyard.

It is by no means easy to say how this can be done. There have been in the past many who have devoted their lives to scientific research, of whom we are justly proud; to whom material gain counted little and whose 'curiosity' may be said to 'have got the better of their intelligence,' for they consecrated themselves wholly to the search for knowledge.

In these more straitened days, however, not only do we need to be aflame with the same consuming fire, but we have to find the fuel to maintain it so that it may always burn brightly.

To put the matter more directly, we are hampered to-day in our pursuit of scientific research by the all important and interdependent problems of recruitment and remuneration.

With regard to recruitment, and naturally I am speaking only with regard to Botanical Science, are we fully satisfied with the efforts, laudable as they are, that are being made in our schools and universities, for training the rising generation in Biological Science?

A good deal has been said recently about the advantages and disadvantages of early specialisation in Science in the schools, at the expense of a more 'liberal' education. We realise that the last years at school are the time for laying the foundations of a sound education, and it is certainly a debatable matter whether the now prevalent severe competition, I might almost say scramble, for scholarships at the universities among the schools of all types throughout the country, is not after all detrimental to the recruitment of those who should develop into the scientific naturalists for whom we are waiting to solve the problems that confront us both at home and overseas.

There is no question that the scientific training now given in many of our Public and Secondary Schools is of a very high order, and that it is given with the greatest devotion and most splendid enthusiasm. But nevertheless may we not, through force of circumstances which have crept in almost unnoticed, owing to competition between school and school, be unduly forcing the pace and producing a superficial scientific precocity in our youth which will not stand the strain?

In the case of training for a medical career I understand that in at least one of our public schools there is a special 'M.B.' Class for preparing boys to take the first M.B. before they leave school. This seems to be an entirely wrong principle and may result in the cramming in of knowledge, or rather of facts, which are not likely to be retained by a mind that is not sufficiently mature.

Science should not be looked upon as a task, but as a guiding tendency, for it is only by regarding it in this way that we can expect to produce the men with a true interest in and enthusiasm for scientific research.

The flowering stage, so to speak, has been achieved before the roots and leaves have developed sufficiently to bear the fruit, and our young plants, raised from seed which may have fallen on stony places, will be found prematurely to wither away. By some, and I expect by many teachers of Biology, I shall be thought hopelessly out-of-date and old-fashioned, but after all one has had the opportunity of seeing the gradual growth and development of the present scholarship system.

Then again there is a danger of the groundings of science being neglected at the universities, since there is a tendency to assume that the standard of school science teaching is that of the scholarship holder. There are, however, many who turn to science, after they have had the good fortune of receiving a classical education, and I could quote the names of more than one distinguished botanist who only discovered their natural inclination and aptitude was towards Science after they had entered the university.

I am somewhat encouraged in what I have ventured to say by the following statement made early this year by one of our prominent science masters, in which I fully concur :—'Any policy which tends to push Biology back into the earlier years as a special subject, endangers both the ultimate value of the Biology itself and the education of the boy.'

There is no need here for me to emphasise the need for recruits at the present time, this has been done on more than one occasion recently by scientific authorities representing different branches of Biological Science. But there is still need to point out that the services which science can render, and for which there is so great a demand, cannot be obtained without making due provision for the cost.

For the training of men to carry out scientific work in the Colonies valuable provision is now made by the giving of post-graduate Scholarships tenable at the Imperial College of Tropical Agriculture, Trinidad, following on the lines of the similar scheme initiated by the Empire Cotton Growing Corporation, thanks to the wide vision of Sir James Currie.

The fact that we need more scientific research workers at home and also more posts, adequately endowed for them to occupy, I hope I have made evident, and it is significant that this fact is beginning to be realised by some of our big industries and leading firms. Nevertheless, I feel I cannot do better than quote, in concluding my address, a passage from the very interesting book 'The English Tradition of Education,' by the late Master of my old School, Marlborough College, Dr. Cyril Norwood, now Head Master of Harrow, and formerly the distinguished Head Master of Bristol Grammar School. Speaking of 'Things that may be,' Dr. Norwood points out :—

'Agriculture and production in the Dominions, and particularly in the tropical dependencies, require the services of many trained biologists, botanists and zoologists, but they are not to be had. Their work is to apply modern scientific knowledge to the production and protection of crops, and it offers a career of great utility to the community, and some profit. There is no one who is directing the nation's education to meet the nation's need in such directions; we wait always till the gap is there, and the want is felt, and trust to the slow operation of the laws of supply and demand. Meantime wrong crops may continue to be planted, and removable pests play havoc. There would again appear to be somewhere a lack of the sense of the value of knowledge.

'We need in this age the scientific expert, the man who knows, in every branch of activity, and we need to have it driven into the inner consciousness of everybody that to such a person we must in all cases first turn, that knowledge is always to be sought and had, and knowledge used. We need the employer who has imagination, not the man who thinks that the services of an expert can always be bought for a pittance, if you happen to need him, but the man who realises that every business to-day, if it is to be great, requires its General Staff of experts, men possessed of knowledge and capable of research, and that success will go to those who follow the paths which these indicate, and not to those who follow tradition, and ascribe their decreasing returns to every cause save their own failure to appreciate the uses of knowledge.'

SECTION L.—EDUCATIONAL SCIENCE.

# A POLICY OF HIGHER EDUCATION.

ADDRESS BY

THE RIGHT HON. LORD EUSTACE PERCY, P.C., M.P.,

PRESIDENT OF THE SECTION.

My audience will, I hope, forgive me if at the outset I indulge in some platitudes.

Education is not schooling. To approach the problem of higher education from the administrative point of view, to start by planning a school system and a school curriculum, is to begin at the wrong end. The only assumption that the educational administrator has any right to make is that, up to the age of eighteen, a boy or a girl should never be out of touch with educational opportunities, and that it is the duty of all public-spirited citizens to co-operate in providing such opportunities. What the opportunities should be must depend, not upon any preconceived assumptions as to the best and most efficient school system, but on the needs of the individual boy or girl, as expressed in their demand, or the demand of their parents or employers, for a particular kind of education. Our first business is to discover that demand and to make it conscious and articulate. When we have done that, the educational administrator can come in to satisfy it.

The first step towards discovering what is the demand for education is to realise the difference between elementary and higher education. Up to a certain point in a child's life he must be compelled to take what he is given. This is the stage of elementary education. The good teacher will make elementary instruction attractive, but he must avoid like poison the sloppy idea, into which teachers were in danger of falling a few years ago, that the soundness of elementary education is to be measured by its attractiveness. In some degree elementary education must always be forcible feeding. Then comes a transition stage between elementary and higher education when the pupil needs in a special degree the discipline of a good school, but when he is beginning to be a responsible person with a conscious bent of mind and intelligent preferences, to which the wise teacher, not to speak of the wise parent, must attach full value. Last comes the stage of higher education, when forcible feeding becomes impossible and school discipline fades into the background. In that stage the lifeblood of education is the attraction exercised by the free teacher over the free pupil, the willing recognition by the pupil of the teacher's intellectual authority.

These stages run into each other. They vary in length according to the individual. Personality defies all attempts at rigid classification. But, broadly speaking, every boy or girl must progress from each of these stages to the next, and if he cannot do so at a full-time school it is a sign that full-time schooling does not meet his needs. There is a tendency to-day to assume that full-time schooling up to sixteen must be good for everyone, and that all we require is a sufficient variety of schools and curricula. But no one has the least right to make so sweeping an assumption. The more we can vary schools and curricula the better, but at any given moment we must assert that a pupil who fails to progress in this way at the school or schools which are in practice available to him had better leave school. If at the age when he ought to be showing intelligent preferences he continues to require forcible feeding, he had better be moved for a time to an atmosphere where he can become conscious of intellectual hunger. If at the age when he ought to be responding to the attraction of a teacher's intellectual authority, he continues to require the discipline of compulsion, he had better be handed over for a time to the discipline of the factory. Observe that I say, for a time; I will return to that point in a moment. Higher education cannot work by compulsion; if it is forced to do so it will destroy the soul of the society which sets it to perform so uncongenial a task.

At present our attitude towards higher education is vitiated by three unhealthy influences. The first is the superstitious reverence for full-time schooling which we owe to a hereditary governing class. When at about the time of the Reformation the lay servant of the king succeeded the ecclesiastical official in the government of the country, the grammar school, which had been the selective recruiting agent of the ecclesiastic, was gradually expanded into the routine training-ground for all the sons of all the king's servants. Through this parade ground they all passed, with, on the whole, astonishingly good results; but any public school man could draw up a deplorably long list of the misfits of which he had personal knowledge among his contemporaries. The number of these misfits is, I think, growing as the old hierarchical social system of the nation crumbles. The public school boy of to-day surely tends to weary of school at an earlier age than did his father, and an increasing number of 'upper' and 'middle' class parents must experience an uncomfortable feeling that, after all, this or that one among their sons might have developed much stronger intellectual appetites if he had gone through a workshop apprenticeship at a comparatively early age. Yet this is the moment we choose for compelling all parents to burn incense to this aristocratic idol of indiscriminate full-time schooling.

The second unhealthy influence is a corollary of this superstition: the assumption that all education must take the form of a continuous school and university life, that if a boy leaves school he abandons definitely all hope of pursuing any connected course of education. Hence we have despised the idea of part-time education, as if such education were merely a sop thrown to the unfortunate orphans of our civilisation in part compensation for their lack of full-time schooling. Nothing could be further from the truth, as we can see if we glance at the Danish folk school, at the German educational system, or even at our own technical colleges. The truth is, on the contrary, that our secondary schools and universities should be paralleled, throughout their length, by courses of part-time education, and that opportunities should be provided for all students, according to their needs, to change from one to the other at any stage.

The idea that the value and continuity of education depends upon the number of hours spent in school has no basis except the bureaucratic love of a tidy system. May one dare to say, in passing, that the chief danger of a State system of education is that the teaching profession is already peculiarly susceptible to the morbus of bureaucracy without the additional risk of infection arising out of continual contact with civil servants?

The third unhealthy influence, to which we are particularly exposed at the present moment, is the unnatural connection between the ideal of popular education and the idea of statutory compulsion. Compulsion is a necessary ingredient in elementary education, and at that stage, therefore, statutory compulsion has a certain justification, not only in expediency, but in reason. It is at any rate not out of harmony with the atmosphere of the elementary school. The same may be said of the transition stage, though here, if carried beyond a certain age, it should be mitigated by exemptions. But compulsion is utterly alien to the whole conception of higher education, and no sound system of higher education can ever be based upon the expedient of statutory compulsion. For the same reason, the principle, so dear to many of our fellow citizens, of 'no public money without public control' may be applied with some show of reason to elementary education, but is wholly out of place in higher education.

At this moment we are in imminent danger of pushing up the methods of elementary education into the sphere of higher education. If we do this, we fail to secure higher education, by whatever name we call our schools, and we merely keep children in an elementary atmosphere beyond the age at which they should be entering the atmosphere of higher education. We have now reached, or more than reached, the point at which we can no longer work upwards from the elementary school, with our old tools of statutory compulsion and public control. We must begin rather to work downwards from the University, introducing more and more into our education, whether given in full-time schools or part-time classes, the influence of those standards of academic freedom and intellectual authority which it is the peculiar function of the Universities to maintain.

In working downwards from the university, however, we must be careful not to confuse two quite distinct meanings of the phrase 'higher education.' In the sense in which I have been using that phrase, and shall continue to use it, it means the guidance required by all normal boys and girls at a certain stage in their mental development through which they all pass. The guidance they require is almost infinitely varied, according to their bent of mind and the work they are going to do in life, and this higher education must therefore be selective in the sense of being discriminatory. But the phrase 'higher education' also means either advanced studies for which only a minority are fit, or a certain refinement and tempering of the powers of the mind which is not generally necessary for salvation, and may even be harmful to many minds. Not all metals can be ground to a fine edge, not all tools need to be; and to keep a fine edge on a fish knife is a positive waste of metal. This is the higher education of the universities themselves, and it must be selective in the sense of being given only to a comparatively small number of selected students. Confusion between these two meanings of the same phrase leads either, as in America, to the degradation of university education or, as in England, to the treatment of secondary education as if it were primarily a preparatory training for the university. This is, and must continue to be, the primary function of some schools with which the name 'secondary' has become specially identified; but secondary education in its wider sense is not a special training but a general need. That does not make it any the less higher education, and university influence is required throughout its whole range, not because universities are highly select institutions, but because universities, whose business it is as teaching bodies to educate grown-up men and women, are, on the whole, the best guides to the teaching of boys and girls who are growing up.

I will not repeat here what I have said elsewhere as to the administrative steps which the universities should take, as teaching bodies, to make their influence felt in the right way, or as to the need for a close alliance between universities and technical colleges as the joint guardians of the standards of higher education. Universities, however, are not only teaching bodies; and this, therefore, is not the only reason why university influence is essential to any policy of higher education. There is another reason, even more important at the present moment. However wisely we may organise higher education, our success or failure will depend on the extent to which universities, colleges, schools and classes respond to a demand which can only come to them from outside. This is the point I am particularly concerned to emphasise to-day. Because higher education is the meeting between the free pupil and the free teacher, the teacher must know what the pupil demands and what will attract him; and the universities and technical colleges are in a special sense the mediators between the schools and the outside demand which the schools must satisfy.

Let me try to explain what I mean. Many people feel keenly the need for greater variety and inventiveness in our schools, but they seem to rely upon teachers to originate new forms of education out of the mere study of the pupil's mind. This is to regard education merely as a kind of spiritual dietetics, as if the teacher's only problem was to give the pupil the food best suited to his mental constitution. But it is wrong, though at one time it was fashionable, to regard mental health as an end in itself; it is a far higher ideal of education to regard knowledge as an end in itself, and to realise that the teacher's highest function is to pass on to his pupil the knowledge that is the birthright of each succeeding generation. The study of Einstein may be a less healthy mental food than the study of Newton, but the teacher must pass on to his pupil the physics of the present, not of the past. It is the new knowledge that makes the new learning; it is in evolving appropriate methods of teaching new things that the teacher changes and varies education. The motive force of innovation in education must come, therefore, from the discoverer of new knowledge, whether his discovery be a new continent, a dead language, a new gas, a new bacillus, or a new machine. But the discoverer does not, as a rule, transmit this motive force directly to the teacher; he transmits it through other men who make it their business to synthesize new knowledge, to suggest the principles of a new physiology or to assemble new machines into a new factory unit. It is these men who act, or should act, directly upon the teacher and if, in an age of growing knowledge, fresh syntheses and changing industrial organisation, education remains static, it is not because the teacher lacks originality but because he lacks touch with those who are the real originators.

The organic defect in our higher education is that, like our government, it is not harnessed to the life of the society it claims to serve, to the new power and the new opportunities which society is constantly generating from new knowledge. This lack of touch is most clearly seen in our traditional attitude towards industry. The 'upper classes,' though deeply affected by changing economic conditions, still think in terms of the 'liberal professions.' The choice before their sons, in their view, is either to enter a 'liberal profession' in order to serve the community and make a career, or to 'go into business' in order to make money. The 'working classes,' imitating as best they can this aristocratic superstition, assume that their sons must as a rule submit to the drudgery of industry, but their great ambition is that as many as possible should escape from this bondage and become teachers, civil servants or trade union organisers. This is still the atmosphere of both the public school and the secondary school. The idea that industry may be made to offer the most adventurous of careers, that it is the chief, and indeed the only direct, agent of social welfare, and that the liberal professions, including government administration, have at best only the secondary job of diverting some of the wealth produced by industry into particular channels of social welfare which might otherwise run dry—all this is an unfamiliar conception of society to many teachers and to most parents. The key to a new policy of higher education is to make it a familiar conception.

Now, the synthesis of new knowledge is pre-eminently the function of universities. Universities, indeed, have played, and should play, a large part in discovery itself; but all discoveries, whether made within their walls or not, come back to them for formulation and assimilation into the general body of human knowledge. Hitherto the universities have performed this function mainly in the field of the humanities and pure science, but in recent years they have been called on increasingly to perform it also in the realm of applied science and technology. In this latter field their function of synthesis and interpretation is, or ought to be, shared by the technical colleges, particularly in that part of the field which relates to factory organisation and commercial practice. It may, at first sight, seem absurd to include factory organisation and commercial practice among the syntheses of knowledge for which universities are partly responsible, but the fact remains that applied science is never really applied until it is embodied in the most efficient factory unit possible, and the most intelligent methods possible of selling goods in the manufacture of which the latest discoveries of science have been used. The Appointments Board of a great University, rightly understood, is a recognition of this fact, for it embodies the acceptance by the University of the responsibility for supplying to commerce and industry men trained for the practical requirements of manufacturing and trading firms. It is in these practical ways, as well as through the more purely academic formulation and assimilation of new knowledge, that a university interprets the outside demand for education both to its own teachers and to teachers in all schools of higher education, and it is essential to the soundness of all teaching and guidance both in schools and universities that this interpretation should be up to date.

But we must carry this line of thought a step further. When the university professor or research student synthesizes a number of discoveries in physics or biology, he is not merely digesting information received and passing it on to those who will have to teach it; he is also, by his very synthesis, profoundly influencing the direction of further exploration and discovery. Synthesis and discovery react upon each other. It should be the same with the more practical syntheses of industrial practice. Universities and technical colleges should not confine themselves to receiving information as to the type of organisation, both of men and machines, adopted in the most up-to-date factories, and translating that information into a course of training for the men required by such a factory. They should also make a deliberate effort to ensure, so far as possible, that such courses of training react upon industrial practice, and that those responsible for such courses are accepted by industry, not only as subservient trainers but as intelligent advisers. It is perhaps in this respect that the relations between American universities and American industry differ most markedly from the relations which prevail in this country; and our industry suffers in consequence. For instance, education and industrial practice are at complete cross purposes in this country in their treatment of technologists. Industry demands highly trained technologists and the universities supply them, only to find that the road to management in industry does not lie through the technical but through the commercial side. The best university men consequently find that their technical qualifications are rather a handicap to their career, and tend to pass over to the commercial side at the first opportunity. There can be little doubt that our industrial practice in this respect is wrong; it is certainly at variance with the practice in every other great industrial country. It is this sort of maladjustment to outside demand which makes all the difference between good and bad education, and where it exists it cannot be corrected by any initiative or originality in the schoolmaster. It can only be corrected by a persistent effort on the part of universities and technical colleges to come to terms with the outside demand represented by industrial and commercial firms, and a readiness on the part of those firms to take a reasonable amount of educational advice.

I should, perhaps, apologise for having spent so unconscionable a time in packing-up for my journey to a policy of higher education. But it has seemed to me necessary to insist, even to the point of weariness, that such a policy cannot be evolved by educators or politicians out of their inner consciousness, out of any study by the teacher of adolescent psychology, or out of any theorising by the politician about the rights of children or parents. A policy of higher education must be built up in response to the outside demand of the workaday world, that 'fair field full of folk, the rich and the poor, each working and wandering as the world requires,' where men are adding to the sum of human knowledge and human activities. And I have wished, too, to point out that, in interpreting that demand, universities and technical colleges will not be engaging in some new and irksome 'serving of tables,' incongruous with their apostolic functions, but will on the contrary be merely fulfilling their traditional function of synthesizing research into doctrine and keeping new learning up to date with new knowledge.

And now, let us endeavour briefly to interpret this outside demand and suggest the lines of a policy.

The most important fact about it is that it is a demand for mental keenness rather than for physical skill. Broadly speaking, industry is approaching its apotheosis of mechanisation and requires the mind that can marshal machines and can grasp the social purpose of cheap mass production even in the dull routine of repetition work. A generalisation like this is, indeed, no sooner made than it must be qualified. There are signs in some directions that we are reacting away from the machine. The world is probably less content, for instance, with machine-made furniture to-day than it was twenty years ago, and the craft element is increasingly coming back into that industry. The cheap jewellery industry of Birmingham is suffering, not so much from a decline in demand, as from the competition of better taste and greater and more highly organised skill in France and other continental countries. But, while some industries may depend upon craftsmanship, the number of industries which rely on purely mechanical skill acquired at an early age is to-day very small. The textile industries constitute one of the very few exceptions, and it is surely an extraordinary reflection on our social intelligence, that, so far as I know, we should have hitherto failed to make any serious scientific study of the extent to which early apprenticeship is really necessary to efficient production in a cotton or woollen mill equipped with the most modern machinery. But, for our present purpose, we must take the demand as we find it.

This demand for mental keenness means, for most occupations, longer schooling, and schooling directed primarily to the training of the mind. One danger of our present school policy is, I think, that we are tending to fall between two stools. Impressed as we rightly are with the need for training of hand and eye in education, we are putting more and more emphasis on ‘ practical ’ instruction for older children in full-time schools, and we are trying to give this education at the carpentry bench. In this we are, perhaps, inclined to make the same mistake as has been made in the ‘ arts and crafts ’ movement. It is our business to make terms with the machine, not to attempt to promote an ineffective reaction against it by reviving the craft spirit of a past age. The reason why we are being called on to keep children longer in school is not that the boy who is going to be a manual worker, skilled or unskilled, ought to be kept out of the labour market until he is fifteen or sixteen, but that the demand for manual labour, skilled or unskilled, including juvenile labour, is declining every day and is giving place to a demand for labour involving at least some measure of abstract thinking and planning. To a very considerable extent—to what extent it is one of the main duties of our educators to work out in detail—this training of the mind should, no doubt, be carried out in actual contact with the material things upon which the pupil's mind will have to work, but these material things are not hand tools but machine tools. The type of small full-time school to which we are accustomed in this country, and which most of us think infinitely superior to the vast polytechnic-high school of the United States, cannot provide machine shops within its own walls. That fact puts a definite limitation on the function which it should seek to perform and the full-time school should, therefore, not attempt, as it is attempting at present, to cover the whole ground of adolescent education. Its justification lies in the mental training it can give, and it should beware of ineffective compromises with the needs of boys who still require physical rather than mental training.

In my view, therefore, our first aim in higher education should be to develop part-time education in technical schools and continuation classes for all children over the age of fourteen. The reason for this is not that part-time schooling is better than full-time schooling for the mass of our population. The experience of the United States seems to indicate that in the coming machine age, full-time schooling will, under the influence of industry itself, more and more supersede part-time schooling. The reason is rather that we are in a stage of transition when purely manual labour, and therefore juvenile labour, is still required in many industries which will increasingly eliminate it in the future as they reorganise themselves, but when almost all industries desire that their young employees should remain in touch with education in some form. In these circumstances, we should not run the risk of starting the full-time schools of the future on wrong lines, by forcing them to assimilate a mass of pupils who would stay on at school with no clear object either in their own mind or in the mind of their future employers. The various types of full-time schools should, on the contrary, be given an opportunity to detach themselves, as it were, from the background of popular education, to define the kind of mental training they seek to offer and then to draw away from the great reservoir of part-time education an increasing number of pupils who need that kind of training. Moreover, the adequate development of part-time technical education is of the first importance because, if American experience again is any guide, many pupils in full-time schools will, in the future, have to combine their mental training with a considerable amount of practice in the machine shop, and this they will only be able to do if they have at their disposal technical colleges equipped to receive them for a certain number of hours in the week along with pupils attending part-time classes. The part-time technical school or college must, in fact, increasingly occupy the position of a central focus for a large range of full-time schools, who will be grouped round them for 'practical' instruction purposes.

Parenthetically be it remarked, this is, of course, an urban conception of education. The rural problem is, in many respects, a different one with which I have no time to deal to-day ; but, here too, it is to be hoped that we shall see a development of agricultural colleges occupying much the same position as the Danish folk school—the more so because full-time education will never play so large a part in the life of an agricultural as of an industrial community, however much the mechanisation of agriculture may be developed in the future.

I know that these views will be distasteful to a large number of people who, fixing their eyes on the idea of a comprehensive reorganisation of full-time schools associated with the Hadow Report, have no thought to spare for what they regard as the *pis aller* of part-time education. They will find it strange that an ex-Minister, who has himself urged the importance of this reorganisation and who believes that industry is rapidly coming to demand longer full-time schooling and higher mental training, should turn aside from this simple and neat conception of adolescent education in order to plunge into the disordered tangle of part-time continuation schools. To these critics I would reply that, however important the recommendations of the Hadow Report may be, nothing could be more disastrous than prematurely to confine the changing demands of industry and the adventurous tastes of the growing boy within the limits of any nicely-ordered school system. The small full-time school of the English tradition is an inelastic institution, tending constantly to conform, at best, to one of two or three types. The great advantage of the part-time technical school at the present moment is its elasticity, its ability to conform easily and tentatively to the demand of different industries, its power of reacting directly upon that demand, guiding it, modifying it and developing it.

Consequently we ought, I think, to expand the Hadow ideal of four-year courses of full-time schooling for all children from eleven to fifteen, into the wider ideal of five-year courses for all children from eleven to sixteen, beginning for the first three years in full-time schools but completed in the last two years either in full-time schools or in part-time schools according to the pupil's needs. We should seek to ensure attendance at the last two years of such courses, not by compulsion, but by attraction and by arrangements with employers. A boy over fourteen may legitimately be required by the State to show that he is either at work or at school. That is a principle which in one form or another is as old as the reign of Elizabeth, and as new as modern American policy; but that should be the limit of compulsion. The full-time school should, at every stage, work in with the technical school, so that the five-year course is really a continuous one. We shall thus secure, through close co-operation between two distinct institutions, the same result as is secured in America through the rather amorphous polytechnic-high school, without destroying the individuality of our full-time schools.

This should be the foundation and first storey of our policy of higher education. If we want a name for this first storey, better than our present phraseology of senior, central and technical schools, we might consider the general name of junior high schools, full-time and part-time. From this first storey will rise side by side our traditional type of secondary school and our senior technical courses, bringing the pupil up to the college stage of higher education, whether in the technical college or the university.

I need not continue further. My object has been not to sketch a new structure of higher education, but rather to suggest that we should look at our existing structure with new eyes and be prepared to make additions to it, not according to some preconceived plan, but according to the demands of a changing world. In order to do that our educators must look at it from outside, not from inside. They must go out into the highways and byways of our industrial life and mark how meaningless and remote appears much of our educational architecture to the puzzled gaze of the ordinary man and woman at work in the world. My metaphor tempts me to an ecclesiastical analogy. The old English village knows and understands the old English parish church ; it fits into the landscape ; it is a familiar part of the local life. So with the elementary school ; it has grown into, because for sixty years and more it has grown up with, the social life of the masses of the people. The old English town knows, too, and understands its larger churches and its cathedral ; here and throughout Europe the towers and spires of the Middle Ages soar above the huddle of roofs below them and yet seem a natural part of the picture. So with our universities and our grammar and secondary schools, old and new ; England understands them and can labour intelligently to fit them to serve the needs of each succeeding generation. But what of the new and staring churches of many of our great industrial cities, built as it were to order and seeming often to intrude an alien air of middle-class respectability into crowded streets and bustling business centres ? These are like too much of our modern educational legislation and administration, fine, pretentious, roomy, expensive, but representing, not what the man in the street needs, but what other people think he ought to need. Those who labour in these new structures, the teachers like the parsons, are doing a tremendous work, but they are doing it under a severe handicap. Let us resolve in future to plan our education, not on any mere past experience or on any analogies with aristocratic traditions, but in response to a growing demand which we may indeed seek to guide but which it must be our main task to interpret and to satisfy.

SECTION M.—AGRICULTURE.

# VETERINARY SCIENCE AND AGRICULTURE.

ADDRESS BY

P. J. DU TOIT, B.A., Dr. Phil., Dr. Med. Vet.,

PRESIDENT OF THE SECTION.

The mere fact that I have travelled more than 6,000 miles to come and deliver this address here to-day, is perhaps sufficient excuse for me to make a few personal remarks before embarking on the task which I have set myself. When I received the cable from the Secretary of the British Association stating that the Council invited me to become President of Section M for this year, the feeling of bewilderment which I experienced was only partly dispelled by the feeling of deep gratitude for the great honour that had befallen me. I accepted the invitation without sufficiently contemplating the consequences, but I did so with a firm belief in the old German saying:

> 'Wem Gott gibt ein Amt,
> Dem gibt Er auch Verstand.'

The honour which has been conferred upon me is not only personal. In asking me to be President of this Section you have honoured, not an individual, but a country; on behalf of South Africa, therefore, I tender my sincere thanks to you. I realise that it is not customary for the British Association to go beyond the shores of these islands for Sectional Presidents (nor is there any need to do so !) This year you made an exception, and I regard it as a great compliment to the country of my birth.

I am further very sensible of the honour you have done my profession by calling me to this high office. It is the first time that a veterinarian has occupied this chair, and the sincere thanks of myself and my colleagues are due to you for this signal honour.

A year ago this Association met in South Africa and many of you then had an opportunity of getting acquainted with the country, its problems and its people. Agriculturally you saw a country which is rapidly discarding the patriarchal and primitive methods of yesterday for the scientific methods of to-day. You saw our many problems and the necessity of applying all available scientific knowledge to their solution. You saw a people anxious to learn from the old parent countries in Europe or the countries across the Atlantic. Our thirst for knowledge was partly satisfied by your visit last year, and I wish to give you the assurance now that that visit has been of inestimable and lasting value to South Africa in that it stimulated unprecedented interest in scientific work and scientific workers.

Not the least among the problems which you saw were those connected with *stock farming.* It is doubtful whether any country in the world has more problems confronting the stock owner than South Africa. Most of the stock diseases present in European countries are also to be found in the temperate climate of South Africa and, in addition, the majority of the diseases of tropical Africa thrive within its borders.

It is perhaps for this reason that Veterinary Science has made such rapid strides in South Africa. The need for scientific research was first brought home to farmer and statesman alike when rinderpest invaded South Africa 35 years ago and killed off almost the entire cattle population; and the ravages of Horsesickness, Bluetongue, Heartwater, Nagana and scores of other diseases further emphasised the necessity of scientific research. Fortunately for South Africa (and perhaps for Veterinary Science) the right men were forthcoming to undertake this research and to-day, although many problems still await solution, the position of the stock-owner in South Africa is by no means hopeless.

The prominent position which Veterinary Research occupies in the scientific life of South Africa to-day, and the valuable practical results which have been obtained in this field of work, have encouraged me to choose as the subject of my address: the rôle which Veterinary Science plays in the agricultural development of a country. For obvious reasons my remarks will be confined almost exclusively to the Live Stock side of Agriculture in the wider sense. And for equally obvious reasons most of my examples will be quoted from South Africa.

In his brilliant presidential address to this Section in 1928, Dr. J. S. Gordon directed attention to the supreme importance of the live stock industry in the agricultural economy of every unit of the British Commonwealth of Nations. He came to the conclusion that the best means to improve this industry was the increased use of pedigree sires and the elimination of scrub bulls. He also emphasised the need for further research along the following lines: (1) Animal Nutrition, (2) Animal Diseases, (3) Animal Breeding, and (4) Marketing. Now all these subjects, with the possible exception of Marketing, fall within the scope of the veterinarian, and I shall attempt to show how modern veterinary science is actually advancing knowledge along each of these lines.

But before dealing with these specific problems, it is necessary to say a few words about the development which has taken place in veterinary science itself. Looking back over the last 140 years, we see that we have indeed travelled a long way since the first Veterinary College in Britain was established in London in 1791 with the object of placing 'the study of Farriery upon rational and scientific principles.' The gulf which divides the modern veterinarian and the eighteenth-century farrier is at least as wide as that dividing the modern surgeon and the eighteenth-century barber.

During the earlier half of the nineteenth century very little progress was recorded in Veterinary Science. In regard to the origin of disease much ignorance and superstition prevailed. The 'miasmatic theory' was called upon to explain the spread of epidemic and epizootic diseases.

Indeed, it seemed as if little progress had been made since the fourth century after Christ when the Greek writer, Chiron, maintained that glanders and similar diseases were caused by the ' pestiferous hot southerly wind from Africa.'

However, great strides were made during the latter half of the nineteenth century. The work of Louis Pasteur and Robert Koch cleared up the ætiology of some of the most important infectious diseases. And the later researches of David Bruce, Adolphe Laveran, Theobald Smith and others, added further brilliant chapters to our knowledge.

Since the beginning of the present century the growth of Veterinary Science has been phenomenal. In every branch of this science there have been remarkable developments. Indeed, it may be said that a New Veterinary Science has arisen unobserved by the general public. A quarter of a century ago the veterinarian was looked upon as a moderately useful though obscure member of the community, whereas to-day he is regarded as an essential factor in the economic machine of the State. A generation ago the value of a veterinarian was judged by his ability to cure a lame horse or an ailing dog; to-day the veterinary profession is judged by the measure of success which attends their efforts to keep their country free of epizootic diseases. Formerly the work of the veterinarian was individual, to-day it is national.

In this transformation of Veterinary Science the British Dominions and Colonies played no unimportant part. The veterinarians who had migrated to those countries and taken with them the stock of knowledge which they had obtained at the European veterinary schools, found themselves confronted with new problems which required solution. Research work on a large scale became necessary. Novel methods of attacking disease had to be devised. The farmer soon came to realise that his very existence depended on the protective measures devised and enforced by the veterinary staffs.

I propose in the short time at my disposal this morning to review briefly some of the most notable achievements of Veterinary Science in recent years, and to indicate how the work of the veterinarian has become interrelated with that of workers in other branches of science.

It will be convenient to divide our subject into sections and to quote a few examples from each of these.

Let me begin with the largest and most important field of work of the veterinarian, viz. :—

## A. Animal Diseases.

And let me group this subject according to the ætiology of the various diseases:

### 1. *Trypanosomiases.*

Probably no other single group of disease-producing organisms has retarded the agricultural development of the continent of Africa more than that of the trypanosomes. If the cattle population of Africa be estimated at about 40 million head, it is quite safe to say that this number could easily be doubled if the danger of trypanosome infection were removed. In Nigeria, for instance, only a portion of the drier Northern Provinces is suitable for cattle ranching; the much more fertile Southern

Provinces are practically devoid of cattle on account of the ravages of Trypanosomiasis. Similar conditions obtain in almost every territory in Africa (except the extreme south). The soil is fertile, grazing is plentiful, the climatic conditions are favourable, but the presence of tsetse flies and trypanosomes renders cattle farming impossible.

Fortunately, we can record considerable progress in this field of work during recent years. The problem has been attacked along two lines mainly. A direct attack has been launched against the parasite by means of drug treatment; and an indirect attack on the disease has been made through a campaign against the transmitter, the tsetse fly. It may be stated at once that the third line of attack, namely, the immunisation of animals against infection, has not yielded very promising results.

In regard to *drug treatment*, a tribute should be paid to the early pioneers, especially Livingstone, who found that Arsenic had a marked effect on trypanosomes in the blood of animals. Untiring efforts on the part of later investigators (Ehrlich and many others) have brought to light a large number of active preparations, notably Arsenic and Antimony compounds and various dyestuffs. Still more recently further drugs have been added to the list, and these promise to give the stock farmer in infected areas a practical means of combating the disease and keeping his animals in good health and condition in spite of repeated infection. Among these drugs special mention should be made of *tryparsamide*, *Bayer* 205 (Germanin, Naganol) and *Antimosan*. The two former have also given excellent results in the treatment of human sleeping sickness, and the last-named which has quite recently been tried on a fairly extensive scale at Onderstepoort by Parkin, and in Tanganyika Territory by Hornby, has proved to be more effective in the treatment of *Trypanosoma congolense* infection than any drug previously used; at the same time the simple (subcutaneous) administration of this drug renders it more practical than those preparations which have to be given intravenously.

In all this work the veterinarian has kept in close touch with the medical man, on the one hand, and the synthetic chemist on the other.

In the *campaign against the tsetse fly* the basis of co-operation has had to be broader still. Entomologists, botanists, ecologists, medical men and veterinarians have all combined to study this problem. Time and space prevent me from discussing in detail the progress which has been made in this work. But attention should be directed to the very valuable investigations carried out in Tanganyika Territory by Swynnerton and his co-workers. The volume of our knowledge of the life-history and habits of the various species of tsetse flies is being added to year by year, but the rate of progress is not commensurate with the importance of the problem. Governments must realise that the tsetse fly is holding up the advancement and civilisation of Africa. Money and men should be made available for this work, however 'theoretical' it may appear. It is the study of precisely these 'academic' aspects of the bionomics of the tsetse fly, which will probably ultimately lead to the solution of the trypanosome problem.

Only a few words need be added about those trypanosome infections which are *carried mechanically by ordinary biting flies*. The most important of these is '*Surra*' in India and other countries. Great advance has been made since September 1880, exactly 50 years ago, when Evans, the 'Inspecting Veterinary Surgeon' of the Government of Madras, was sent to the Punjab to investigate this disease, and when he succeeded, in a remarkably short time, in discovering its cause. Since then much work has been done, especially by Nieschulz in the Dutch East Indies, on the transmission of this disease, and good progress has also been made in regard to the drug treatment.

It is with great satisfaction that the fact can be recorded that this veteran of science, Griffith Evans, the discoverer of the first pathogenic trypanosoma, is still alive to-day, with more than four score years and ten to his credit, and is able to watch, from his home in Bangor, the progress which has been made in this field of work.

One further trypanosome disease should be mentioned here, namely, *Dourine*. Known for about 150 years, this disease has been responsible for very heavy economic losses to horse breeders in Europe and other countries. With the aid of modern methods the disease was eradicated from most of the closely settled and well organised Western European states. But in the vast open spaces of Canada and other countries, its eradication proved to be a much more difficult problem. It was only when Watson in Canada succeeded in perfecting a delicate diagnostic test for the detection of the infection, that the eradication of the disease could be attempted seriously, and the results of the subsequent campaign in Canada have been entirely satisfactory. It should be added that Watson's success has stimulated further research into the problem of diagnosing other trypanosome infections by serological methods. A fair amount of success has attended these efforts and quite recently Robinson at Onderstepoort has reported further progress in the serological diagnosis of *Trypanosoma congolense* infection.

### 2. *Piroplasmoses.*

Under this heading are included diseases like Redwater or Texas Fever of cattle, Biliary Fever of dogs and horses, 'Gallsickness' or Anaplasmosis and East Coast Fever of cattle.

Their ætiology was completely obscure until Theobald Smith and Kilborne in America, in a series of brilliant researches extending over the years 1888 to 1892, succeeded in elucidating the nature of the first-named disease. Not only did these investigators discover the causal organism in the blood of infected cattle, but they also proved that the disease was transmitted by ticks and that the infection passed through the egg of the tick from one generation to the next. All this was completely new to Science; it was the first time that the transmission of a mammalian disease through an invertebrate host had been proved experimentally. This contribution to science by two veterinarians is worthy of special note.

Theobald Smith, like Griffith Evans, is still able to-day to watch the progress of the work which he initiated many years ago. In the case of '*Redwater*' great advances can be recorded. The direct method of attack is eminently satisfactory, thanks to the discovery by Nuttall and Hadwen in 1909 that the drug Trypanblue has a specific action on the parasite of Redwater of cattle and Biliary fever of dogs. The treatment is so successful that the disease has lost much of its terror since the discovery of the value of this drug.

In some of the other ' piroplasmoses ' (in the wider sense of the term), no such simple treatment is available. As a matter of fact, in the case of Anaplasmosis and East Coast Fever of cattle, no satisfactory method of treatment is known. In these cases, therefore, prevention should be aimed at.

Various methods of *preventive inoculation against Anaplasmosis* have been advocated. In South Africa Theiler, who originally described the parasite (*Anaplasma marginale*) causing Anaplasmosis, found a second species or variety (*Anaplasma centrale*) which differed from the first, not only in regard to its relative position in the red blood corpuscle, but also as regards its virulence. It was found that the injection of blood containing *Anaplasma centrale* invariably produced a mild infection, even in imported cattle, but that this infection conferred sufficient immunity to protect animals against the fatal *Anaplasma marginale* infection. This method of immunisation has been practised in South Africa for nearly twenty years and has been the means of saving thousands of animals. The *Anaplasma centrale* strain has also been sent to other countries where the same method has been used with good results.

Of the diseases mentioned in this section, *East Coast Fever* is the most formidable because of the very high mortality attending it. This disease must have cost South Africa several million pounds since its first appearance nearly 30 years ago. The loss to the country has been partly direct through the death of many thousands of animals, partly indirect through the costly organisation which it is necessary to maintain to fight the disease.

It is impossible in this brief review to discuss the methods employed in the *eradication of East Coast Fever*, or the many practical difficulties encountered in this campaign. For our purpose it is sufficient to state that the *dipping of cattle in an arsenical bath* has proved to be a very valuable aid in the fight against East Coast Fever or any other tick-borne disease.

In South Africa dipping has been practised since the beginning of this century, and has now become an integral portion of the daily routine of farming. No up-to-date stock farm can be found to-day without at least one dipping tank. The dip was originally intended chiefly as a weapon in the fight against tick-borne diseases, but to-day it is largely used merely to keep the cattle free of ectoparasites, quite apart from the fact that some of these parasites may be carriers of disease. The necessity of keeping cattle free of ticks is obvious to anyone who has lived in a tropical or semi-tropical tick-infested country. As an illustration, the fact may be mentioned that on the Natal Coast, before the days of dipping, it was rare to see a cow with more than one or two teats intact, and it was impossible to raise more than about 30 per cent. of the calves; whereas to-day, thanks to the dipping tank, all the udders and teats of the cows are healthy and it is nothing unusual to raise 95 per cent. or more of the calf crop. Even if all the tick-borne diseases should now disappear, the majority of farmers in South Africa would continue to dip their animals regularly.

The extent to which dipping is practised to-day may be gauged by the fact that there were in the Union of South Africa in 1929 more than 13,500 dipping tanks. If we assume that the average dipping interval is eight days (the actual intervals are three, five, seven or fourteen days), and that the average number of animals that pass through a tank on a dipping day is 200 (frequently the number is as high as 4,000), we find (calculating on this very conservative basis) that about 120,000,000 cattle passed through the dipping tanks last year.

Returning now to the campaign against East Coast fever, it may be said that by means of carefully controlled dipping and hand-dressing of cattle, combined with quarantine restrictions, and slaughter of infected herds in the case of isolated outbreaks, it has been possible to keep the disease well under control. The hope seems justified that the disease will be eradicated completely from South Africa and Rhodesia before many years have passed. That this would be a great boon to the cattle industry of these two countries needs no further emphasis.

In the United States of America, where Texas fever (Redwater) is the only serious tick-borne disease, an attempt is being made to eradicate the transmitter, *Boophilus annulatus*, completely by means of dipping. Large areas have already been cleared of these ticks, and the economic advantages to which these areas are entitled after being declared tick free more than compensate for the expenses incurred.

### 3. *Virus Diseases.*

The vast sums of money which have been spent in this country during the last few years on the eradication of *Foot and Mouth disease* should convince even the layman of the importance of this group of diseases.

In the olden days it was *Rinderpest* which caused the severest losses. It has been calculated that the losses in Europe during the eighteenth century amounted to 200 million head of cattle. The disease made its appearance in England in 1865. A Royal Commission was appointed and its report is of value to this day. Later on, improved methods of eradication and prevention were evolved, and to-day most countries are free of Rinderpest. However, in the Far East and in Central Africa the disease is still prevalent, and causes very serious losses.

Two recent outbreaks of Rinderpest, one in Belgium in 1920, and the other in Australia in 1923, both of which were eradicated completely within a few months, have again shown how far Veterinary Science has advanced during the last century and how much a country owes to an efficient Veterinary Service.

South Africa has been free of the two diseases just named for many years. But there are several other virus diseases which play a very important rôle. Among these '*Horsesickness*' and '*Bluetongue*' of sheep are perhaps the most important. An extensive study of the former disease by Theiler and his co-workers has yielded some very valuable results, but the problem of Horsesickness cannot be said to be solved. At present a method of immunisation with hyperimmune serum and virus is practised, and this method has given excellent results in mules. About 4,000 mules are immunised annually, and it has been stated that if the

Onderstepoort Laboratory had produced nothing else except this method of immunising mules, its existence would have been justified.

In horses the method has not been quite so satisfactory. A method of immunisation of horses which would be safer, simpler, cheaper and more reliable than the present method, would be of inestimable value to stock farmers right throughout Africa. Recent work at Onderstepoort with a formalised vaccine (by Du Toit and Alexander), seems to justify the hope that such a method will be found. The work was inspired by the brilliant researches of Dunkin and Laidlaw in England on another virus disease, Distemper of dogs.

The second important virus disease of South Africa is *Bluetongue* of sheep. The disease is of great economic importance and would have been a very serious hindrance to the sheep farmer had it not been for the fact that Theiler discovered a simple method of vaccination by means of which the losses from the disease can be reduced to a negligible quantity. Every year two to three million doses of this vaccine are issued to the farmers, and the ultimate saving to the country must be enormous.

Another African disease, '*Heartwater*' of cattle, sheep and goats, should be mentioned here. Formerly this disease was classified as a virus disease, but a few years ago Cowdry, working at Onderstepoort, found that it was caused by a Rickettsia. It is possible that this discovery may give us a clue to a successful method of combating this serious disease. In the meantime, all that can be done is to dip the animals to eradicate the tick which transmits the disease.

Of the many other virus diseases of animals only one more need be referred to here, namely *Rabies*. This most dreaded of all human and animal diseases has been eradicated from many countries, and is being kept out by strict quarantine measures. In 1918 the disease was introduced into England with a dog which had been smuggled in in an aeroplane. Strict measures were put into force and in a comparatively short space of time the disease was stamped out completely. Methods of preventive inoculation of dogs in countries where the eradication of the disease is very difficult, have been tried on a large scale. The results have, on the whole, been very good, but it is too early to predict the future scope of these methods.

Before leaving this group, it is necessary to point out that virus diseases are not confined to our domestic animals, but are encountered in human beings, on the one hand, and in the lower animals (e.g. insects) and in plants, on the other. Co-operation between medical men, veterinarians and plant pathologists, therefore, becomes imperative. The subject is claiming the attention of many scientific workers and great developments may confidently be expected in the future.

### 4. *Bacterial Diseases.*

Of the host of bacterial diseases only a few need be mentioned here.

The deadly *glanders* which was known before the time of Christ and which, 25 years ago, still caused severe losses amongst horses, and constantly threatened the human population, has now been practically eradicated from all civilised countries, thanks to the accuracy of the diagnostic tests which are used to identify the disease.

Another disease which at one time was responsible for very serious losses and which has now practically disappeared, is *Pleuro-pneumonia* (*Lungsickness*) of cattle. In the year 1860 about 187,000 head of cattle are stated to have died in Great Britain of this disease ; and the mortality in other European countries at that time was correspondingly high. Towards the end of last century the disease was stamped out in Britain and to-day the greater part of Europe is free of the disease. It may be added that South Africa, in spite of the fact that neighbouring countries are still infected, has been free of Lungsickness since 1915.

*Anthrax* is almost universal in its occurrence, but it is much more formidable in the tropics and sub-tropics than in the colder countries of Europe. In the latter countries it appears in sporadic cases, whereas in the former it behaves like any epizootic disease. For this reason its suppression in these countries becomes a matter of great urgency. The problem has been tackled on a big scale in Australia and South Africa by means of preventive inoculation. In the former country the disease has nearly been stamped out, and in South Africa the campaign is carried on with great vigour. The excellent results obtained with the spore vaccine, now employed, promise complete success.

The position in the warm countries in regard to *Quarter Evil* (*Black Quarter*) is much the same as with Anthrax. The disease appears almost in epizootic form and preventive inoculation on a large scale becomes necessary. Very satisfactory results have been obtained in South Africa both with a germ-free filtrate and with a formalised vaccine.

Only one other bacterial disease can be mentioned here, namely, *Tuberculosis*. In 1901, Robert Koch, who about 20 years previously had discovered the cause of the disease, startled the scientific world by announcing to a Tuberculosis Congress in London that human tuberculosis and bovine tuberculosis were two distinct diseases which were not communicable from the one species to the other. Unfortunately, this statement proved to be wrong. We know to-day that human beings do contract bovine tuberculosis, and for this reason most civilised countries adopt measures for the suppression of the disease in cattle. The United States and Canada are leading the world in this respect and have spent millions of pounds in compensation for the destruction of tuberculous reactors. Denmark, Germany, England and other countries are also doing much and have achieved a large measure of success in their efforts to supply to the population milk and beef free of tubercle bacilli. But very much remains to be done. In human beings the mortality from tuberculosis is still high in all countries, and a considerable percentage of the deaths must be ascribed to the bovine strain of the organism. The disease in cattle *can* be stamped out provided enough money is made available.

Recently great interest has been shown in the attenuated strain of tubercle bacilli produced by Calmette and Guerin of the Pasteur Institute. Experiments in which it is attempted to immunise children and young animals, with this strain, are in progress throughout the world. It is sincerely hoped that all this work will prove that the method of Calmette and Guerin has given us yet another weapon against this insidious disease.

### 5. *Internal Metazoan Parasites.*

The only group that need be mentioned in this brief survey are the *Worms.* These parasites have become more and more important and to-day they actually constitute the 'limiting factor' in successful sheep farming in many parts of the world. This subject forms a highly specialised science of its own, the science of Helminthology; in which many notable successes have been achieved in recent years. It is almost unnecessary to add that this subject demands the close co-operation of zoologists, medical men and veterinarians.

Amongst the *Trematodes* the *Liverflukes* and *Schistosomes* of sheep and cattle deserve special mention. The latter group is not of very great economic importance, but is of great interest because of the close relationship between the disease in animals and Schistosomiasis (Bilharzia disease) in man. Le Roux of Onderstepoort has shown recently (1929) how the campaign against the human disease can benefit from further study of the animal disease. This is particularly true of the method of treatment with tartar emetic and other modern drugs.

*Liverfluke* disease of sheep and cattle is a very serious problem in many countries in different parts of the world. In Germany, Nöller has organised what may be termed a national campaign against these parasites, and has achieved a large measure of success. Various preparations have proved to be very efficacious in the treatment of infected animals, and the application of modern principles of hygiene has reduced the incidence of the snails which act as intermediate host of both groups of worms just mentioned.

Generally speaking, the *Nematodes* or round worms are far more serious than either the Trematodes or Cestodes (tape worms). In many countries where sheep farming is conducted on an extensive scale, the infection with various nematode worms seriously threatens the industry. One or two examples may be mentioned.

The ordinary stomach worm of sheep (*Hæmonchus contortus*) is world-wide in its distribution and is the cause of very severe losses. Better farming methods will undoubtedly improve the position, but in the meantime farmers look to the veterinarian to rid their sheep of these deadly parasites. Various chemicals have been tried with varying degrees of success, but perhaps nowhere has the success been so marked as in South Africa, where, as a result of the researches of Theiler, Veglia, Green and others, a method of treatment was recommended which has proved the salvation of many sheep farmers. The method consists of the accurate dosage of a mixture of arsenite of soda and copper sulphate; and the extent to which this method has been applied may be gauged from the fact that at present some 25 million doses of the mixture are issued annually from Onderstepoort. The method is not perfect, but it has certainly been a great factor in making sheep farming a success where otherwise it would have been a dismal failure.

There are many other nematodes which threaten the sheep industry. *Œsophagostomum columbianum* and various species of *Trichostrongylus* are amongst the most important in South Africa. Treatment in these cases is not simple, but it is hoped that the work now proceeding at Onderstepoort

(Mönnig and Le Roux) and in many other parts of the world (notably in the United States of America) will yield practical results.

One further fact must be emphasised here. The menace of worm infection has become so great that no sheep farmer can hope to be successful if he disregards the teaching of modern science. Overstocking of farms must be prevented at all costs; marshes must be drained or the sheep kept away from them; the sheep must be treated regularly according to the best methods known. If these precautions are adopted, the parasites can be kept in check and profitable sheep farming will become possible; if the advice is ignored, then the financial loss to the farmer will be the smaller the sooner he gives up farming.

The great value of hygienic methods in farming has been proved in the case of *Ascaris* infection of pigs. Some years ago the pig breeding industry was seriously threatened by this parasite; whereas to-day, thanks to the researches of Ransom and others, the infection can be eliminated completely.

### 6. *External Parasites.*

The two most important groups of ecto-parasites, the *ticks* and the *tsetse flies* have already been referred to.

A further very important group are the *mites.* These minute parasites are responsible for the diseases known as *scab* or *mange* in animals, and have caused untold losses. In the fight against these diseases, the British Dominions have had very signal success. Australia and New Zealand have eradicated sheep scab completely, Canada is practically free of it, and in South Africa, where the presence of a large native population owning a very inferior class of sheep has made the campaign particularly difficult, the incidence of the disease has been reduced to infinitesimal proportions, and complete eradication within a short time is hoped for.

Another very important ecto-parasite of sheep is the so-called *Blowfly.* The trouble is caused by these flies depositing their eggs in the wool of sheep, especially in the soiled and moist parts, and by the resulting maggots causing serious damage to the wool and the sheep itself. The pest has assumed alarming proportions in Australia and is becoming more and more important in other countries, including South Africa. Determined efforts are being made to combat the pest and valuable progress has been achieved. In this research entomologists and veterinarians are working hand in hand.

### 7. *Diseases due to Poisonous Plants.*

That certain plants are poisonous and may have fatal effects when consumed by animals has probably been known for centuries. However, it is only during recent years that plants have been studied which produce diseases comparable with epizootic diseases. In this field of research South African workers have been prominent.

One of the most remarkable of these diseases is that known in South Africa as '*Gousiekte*' (rapid disease) of sheep, which was studied some years ago by Theiler, Du Toit and Mitchell. The cause of the disease was shown to be the plant *Vangueria pygmæa.* The remarkable nature of this disease may best be illustrated by the following incident: A farmer brought a flock of 1,760 sheep on to a farm where this plant was known to be present, and left them there for less than 24 hours. They were then removed to a clean farm and for about six weeks nothing happened. Thereupon the sheep suddenly started dying and within a few weeks 1,047 sheep had died. This strange occurrence is explained as follows: the poison contained in the plant acts on the heart muscle causing a myocarditis with subsequent dilatation of the ventricles. As soon as the process has reached a certain stage the animal dies of 'heart failure.' To the casual observer the disease presents all the characteristics of an infectious disease; in the case quoted above it certainly seemed as if the disease had 'spread' rapidly among the flock.

The elucidation of the cause of the disease was of great practical importance inasmuch as it enabled the farmer to enclose that portion of his farm where the poisonous plant grew, and to keep his sheep away from it.

Other no less remarkable diseases were studied by Theiler and his co-workers.

A disease called '*Geeldikkop*' (yellow thick head) in sheep was shown by Theiler (1928) to be due to a plant *Tribulus terrestris*, although more recent work by Quin, Steyn and others at Onderstepoort has shown that there are other factors to be considered in the causation of this disease.

'*Vomiting disease*' of sheep was studied by Du Toit (1928) and proved to be caused by *Geigeria* spp. The disease may produce very severe losses in certain years, especially after droughts, when the plant is very widespread.

Many other instances could be cited of diseases which assume great economic importance and which have been traced to poisonous plants. One final example may suffice to demonstrate the peculiar behaviour of some plant poisons. A disease of horses known as '*Jaagsiekte*' was proved by Theiler (1918) to be due to *Crotalaria dura*. When the plant is fed to horses it produces, after a long 'incubation period,' a fever and certain characteristic changes in the lungs. On the other hand, if the same plant is fed to cattle it produces equally definite changes in the liver.

The study of poisonous plants is now being actively pursued in various countries, and further interesting developments may be expected. It is obvious that the co-operation of botanists is essential for the success of this work.

### 8. *Deficiency Diseases.*

Lack of time prevents me from referring in any detail to this interesting group of diseases.

The great importance of the *vitamins* in the nutrition of human beings is so well known that it need not be stressed here. In the case of the common domestic animals (except perhaps the pig, the dog and the fowl) the vitamins seem to be of far less importance than in human beings.

On the other hand, *mineral deficiencies* are, generally speaking, much more important in animals than in human beings. The reason for this is not far to seek: animals, in most cases, derive their nourishment directly from the products of the soil in a limited area; and if the soil should be deficient in any mineral, that deficiency will be reflected in the diet of the animal.

In recent years it has been found that large portions of the earth's surface are deficient in some mineral or other which is essential for the normal health and growth of animals. This subject was discussed by Dr. J. B. Orr, himself a pioneer in this field of work, in his presidential address to this Section five years ago.

In South Africa as well as in other African territories and in Australia the most serious deficiency is that of *Phosphorus*. Theiler and his co-workers have investigated the ill effects of this deficiency on cattle very fully. They have shown that cattle grazing on phosphorus-deficient pastures develops a depraved appetite for bones and other carcase debris, and this may lead to the ingestion of toxic material with fatal results ('lamsiekte' in South Africa); further, that such cattle remain stunted in growth, are late in maturing, are frequently unfertile, produce very little milk, and are very susceptible to various diseases. By the addition of a small daily ration of phosphorus to the diet, they were able to bring about an almost miraculous improvement in the condition of the animals.

As a result of the general feeding of phosphorus compounds in the deficient areas of South Africa, the disease 'lamsiekte,' which a dozen years ago caused enormous losses, has practically disappeared and cattle farming in those areas has again become profitable. The significant fact may be recorded here that the village of Vryburg in Bechuanaland, where ten years ago milk was very scarce, to-day owns a creamery which handles a larger volume of cream than any other creamery in South Africa.

In other countries, where other deficiencies occur, equally striking results have been obtained. Attention need only be directed to the work of Aston in New Zealand on iron deficiency, and the recent brilliant researches of Orr and his co-workers at the Rowett Research Institute on the whole problem of mineral deficiencies.

### B. Other Veterinary Problems.

At the beginning of this paper mention was made of Dr. Gordon's presidential address in which the need for further research into problems of animal diseases, animal nutrition and animal breeding was emphasized.

In regard to *animal diseases* the examples quoted in the preceding section show that considerable advance has been made during the years that lie immediately behind us. Many diseases have been conquered, but many others still await solution. Meanwhile these diseases cause enormous losses and even threaten the adequate supply of the world's markets with meat and other animal products.

Problems in connection with the *nutrition of animals* are now receiving attention in many countries. The vast importance of correct feeding can be illustrated best by referring again to the phosphorus deficiency which exists in the pastures of South Africa and other countries. The astounding results which have been achieved with the addition of a small quantity of phosphorus compounds to the ration of the animals promise to revolutionize the beef and dairy industries in those countries.

*Animal breeding* also presents problems of great importance and these are intimately bound up with the problems of disease and nutrition. In South Africa, as in other countries, there is a constant cry for the replacement of the scrub bull by pedigree sires. This demand would be met to a far greater extent, were it not for the fact that in many parts of the country pedigree bulls cannot live because of disease or nutritional difficulties. In some of the best ranching areas diseases like Heartwater, Piroplasmosis and Anaplasmosis render the introduction of susceptible animals quite impracticable, unless adequate measures for their protection be adopted. And, similarly, the deficient state of the pastures in other areas nullifies all efforts at the improvement of stock, unless the diet be supplemented.

It is pleasing to be able to record progress along both of these lines. In South Africa control over the diseases mentioned above is gradually improving and, in regard to the deficient areas, recent investigations by Du Toit and Bisschop have shown that the grading up of native stock can be carried out with complete success provided the deficient mineral is supplied. Both beef cattle and dairy cattle have been bred on the extremely deficient veld of Bechuanaland without any signs of deterioration, and the cost of the supplementary ration has been negligible in comparison with the material advantage derived from such feeding.

Gratifying though the success which has been achieved may be, the need for further research on live stock problems has never been greater than it is to-day. The development of enormous areas in the British Dominions and Colonies is entirely dependent on the progress of research. With the aid of further scientific measures, these new countries could absorb a very much larger population than they now harbour. The danger of over-population will not make itself felt for generations, nor need the danger of over-production be contemplated seriously. The shortage which has been predicted in the British beef market will have to be met by the Dominions and Colonies. The same applies to the mutton market. And in regard to wool it seems certain that the existing depression is temporary and that, as soon as the present fashions alter, regrettable though such a change may be from other points of view, the wool trade will be restored to its previous healthy state.

The prosperity of a very large percentage of the population, both European and Native, in the Dominions and Colonies depends on the live stock industry (breeding of pedigree stock; beef, mutton or pork production; dairy farming, wool or mohair production; skin and hide trade; poultry farming, &c.). These farmers look to the Veterinary Service of their countries more and more for assistance and protection. Without this assistance profitable stock farming, especially in the tropical and sub-tropical countries, is impossible. The assistance, if it is to be effective, must be based on the latest achievements of scientific research. Rule-of-thumb methods will not suffice. There are fundamental problems which can only be studied at specially equipped institutes; and this is now being done. But, in addition, each country has its own particular problems which it must solve for itself at its own research institutions. Wise governments will support these institutions liberally. Money thus spent will repay itself a hundredfold.

In a humble way South Africa has proved the wisdom of maintaining an adequate veterinary research service. At Onderstepoort the Government, twenty-one years ago established what must be regarded as a fairly large Research Institute, if the size of the population be taken into consideration. This Institute, under the brilliant directorship of Sir Arnold Theiler, soon proved to be not a liability but a valuable asset to the country. The results obtained in any one of its various sections would probably have justified the maintenance of the entire Institution.

At the beginning of this paper it was said that the Dominions and Colonies have played an important part in the recent growth and development of modern veterinary science. The quality of the research work produced by veterinarians in these countries has been of such high order that it soon placed Veterinary Science (which not many years ago was regarded as the Cinderella of sciences) abreast of the other sciences. As a matter of fact, in South Africa it can be said, without disparagement to any other group of workers, that, in research, Veterinary Science occupies a very high if not the leading position. This has had a wholesome influence on the science itself and on the type of worker who was recruited in its service. The stigma of inferiority which for so long was attached to the veterinarian has disappeared. To-day, Veterinary Science is looked upon as a field of work which offers almost unlimited scope for research and which, in its practical application, may bring untold material benefit to a country.

Now that the British Association for the Advancement of Science has honoured the Veterinary Profession by calling one of its members to the chair of a Section, the hope may be expressed that, in future, in this august gathering also, Veterinary Science may continue to occupy a place commensurate with its scientific achievements and with the rôle which it seems destined to play in the development of the British Empire.

# REPORTS ON THE STATE OF SCIENCE,
ETC.

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**Seismological Investigations.**—*Thirty-fifth Report of Committee* (Prof. H. H. Turner, *Chairman*; Mr. J. J. Shaw, *Secretary*; Mr. C. Vernon Boys, Dr. J. E. Crombie, Dr. C. Davison, Sir F. W. Dyson, Sir R. T. Glazebrook, Dr. Harold Jeffreys, Prof. H. Lamb, Sir J. Larmor, Prof. A. E. H. Love, Prof. H. M. Macdonald, Dr. A. Crichton Mitchell, Mr. R. D. Oldham, Prof. H. C. Plummer, Prof. A. O. Rankine, Rev. J. P. Rowland, S.J., Prof. R. A. Sampson, Sir A. Schuster, Sir Napier Shaw, Capt. H. Shaw, Mr. R. Stoneley, Sir G. T. Walker, *and* Dr. F. J. W. Whipple). [*Drawn up by the Chairman except where otherwise mentioned.*]

## General.

The 'Crombie Basement' and the new rooms generally at the University Observatory, Oxford, have greatly increased the facility and comfort of the seismological work done there. But the massive pier, built for the seismographs, has not yet settled down. Changes of weather affect it seriously, as shown by the behaviour of the N.S. component mounted upon it. The E.W. component, mounted in one corner of the basement on a small pier placed directly on the floor, is much less subject to disturbance. Fortunately no important earthquake has as yet been unrecorded because of these troubles. In passing, it may be remarked that attention was recently called to the value of notes made at Irkutsk stating clearly when a seismograph was out of action, and thus explaining the absence of readings which might reasonably be expected for an earthquake not far away. These notes were found of great help in selecting the position of an epicentre which might otherwise have seemed to be excluded as unlikely. It is hoped that the practice of making such notes may be generally adopted.

## Instrumental.

(Notes by Mr. J. J. Shaw)

A new station has been established by Durham University at the Durham Observatory, in the building formerly occupied by the almucantar. A Milne-Shaw seismograph was lent in the first instance in order to test the suitability of the site; and as satisfactory results were obtained the work was put on a permanent footing.

A promising method of forecasting weather from the microseismic movements of the ground is being developed in India by Dr. Banerji, of the Colaba Observatory, in Bombay; and the Government has ordered four seismographs for these experiments. One has already been despatched, two are on the point of completion, and the fourth will follow later.

Two other machines, also for India, are on order for the Jammu and Kashmir Governments; and one for the University of Liverpool for instructional purposes in the Department of Geology.

During the year electric illumination was fitted to the Oxford seismographs in place of gas, and the results have been excellent.

A project for installing a seismograph in South Georgia or the Falklands, as mentioned below, was considered but abandoned as probably unprofitable.

## International.

The International Geodetic and Geophysical Union is to meet at Stockholm August 14–23. A memorandum has been prepared drawing attention to the very unsatisfactory financial position of the Section of Seismology (largely owing to the fall in value of the franc in which contributions to the Union have been paid) and urging that a considerable increase should be made in the grant to the Section in order that the cost of the International Seismological Summary may be completely defrayed as regards printing, and, so far as possible, as regards the preparation.

Copies of the circular were supplied to the members of our national committee on Geodesy and Geophysics, and a sympathetic motion in support was passed. The printing account up to and including the cost of the I.S. Summary for 1925 was given in the last Report. The cost of printing in 1926 was much greater than its predecessors, amounting to £382 as against £280 in 1925. The increase *may* be partly due to temporary causes, but it would be unwise to make this assumption, in the present state of our knowledge; certainly it is in part due to the increase in the number of observing stations which send their readings for collation; and as the science progresses it will almost certainly be necessary to give details additional to those at present given, which may be regarded as an irreducible minimum. Hence the printing bill is not likely to become smaller. In response to an appeal in May 1929, the Royal Society kindly made an additional grant of £150 to meet the deficit on the printing account, with an intimation that it must be the last. They have contributed £525 in all during the last four or five years.

The work of preparing the Summary for printing has hitherto not received any aid from the funds allotted to the Seismology Section; it has been subsidised by the British Association, by the Department of Scientific and Industrial Research, by the Royal Society, by the University of Oxford, and especially by Dr. Crombie, without whose constant generosity the work must have come to an end long ago.

## Bulletins and Tables.

The *International Seismological Summary* for 1926 has been printed and circulated, and the first quarter of 1927, which will be the tenth year of the Summary (1918–1927), is in the press. The work of preparing and printing the Summary has increased so steadily as to balance any gain due to experience and to the use of computations for previous epicentres, so that the idea of revising years previous to 1918 (especially 1916 to 1912, for which much new material has come to hand since the publication of the partial results in the Bulletins of this Committee), long contemplated, has not yet been carried into effect. Nor has it been possible to do much in the way of discussion of results, though the following may be mentioned:—

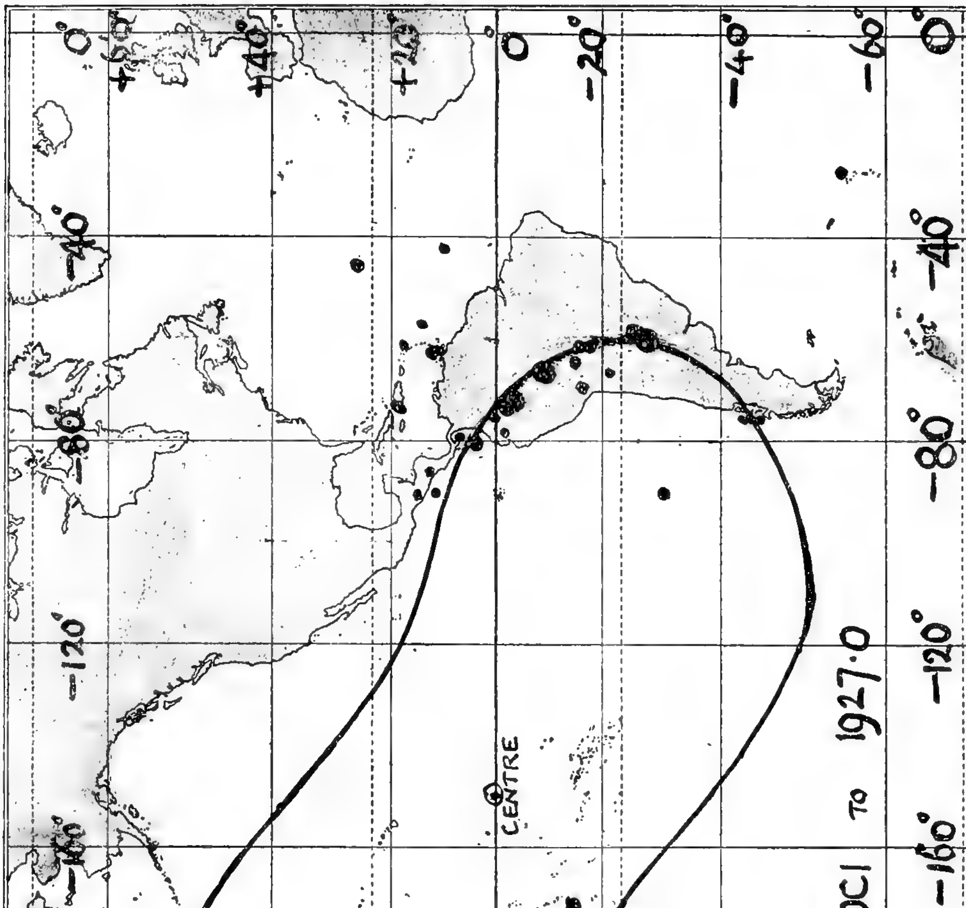
(*a*) The shock of 1926 Oct. 3d. 19h. at 50°·5 S. 161°·0 E. provided a large number of observations of [P], the longitudinal wave which goes right through the earth's liquid nucleus and arrives at stations near the anticentre, many of which in this case were in Europe. The results showed that the empirical formula adopted in March 1922 is substantially correct except for stations not very far from the epicentre (say $\Delta=120°$) where it seems to require correction. But if so, Gutenberg's values, calculated from theory, would also require correction. Our empirical formula (using a simple parabolic curve) agrees well with Gutenberg, except that his theory introduces a slight discontinuity at $\Delta=146°$.

(*b*) The same shock (1926 Oct. 3d.) provided a number of L observations, which were mainly suited by the velocity 1° in 0·405 min., or 4·57 km./sec., though they were apparently divisible into several groups with different starting-points, and some of them were not L waves at all. The details are given in the *Summary* for 1926, October–December. But on trying this speed for other shocks it was found quite unsuitable, except in a few cases. For the great majority the speed is represented by 1° in 0·477 min., or 3·88 km./secs. This seems to suggest that in the majority of cases the waves noted as L by observers are Rayleigh waves, while in some special cases they are chiefly Love waves. The details are not yet quite ready for publication.

(*c*) A number of cases of Gutenberg's S $P_cS$, denoted by [S], and of Gutenberg's $S_cP_cP_cS$, denoted for convenience by $\Sigma$, have been noted in the Summary for certain earthquakes, and found to give residuals from his curves which are satisfactorily small.

## Deep Focus.

The paper sent to Mr. Wadati has now appeared in the Tokio *Geophysical Magazine* and was also printed in the *I.S. Summary*, while its fate was still in doubt. The cases of deep focus were collected by Miss Bellamy in card catalogue form, and also plotted on a map, when it was found that they had a definite local distribution. A rough diagram of this arrangement was prepared for the *I.S. Summary* for 1927, J.F.M., and is reproduced with this report. The epicentres are confined to a comparatively small portion of the earth's surface and arranged approximately in an oval


DEEP FOCI TO 1927·0
CENTRE

curve, with centre on the earth's equator. Is this an old scar representing the detachment of the moon from the earth? The suggestion that the moon came from the Pacific has been made at various times, and the occurrence of many epicentres round the shores of the Pacific lent some support to the idea. Previously, however, there was nothing very definite about the arrangement when we consider earthquakes in general. But the deep focus earthquakes offer a more definite suggestion. Sir James Jeans has kindly offered to re-examine the theoretical possibilities in this connection.

### Some Recent Shocks.

There was a considerable earthquake near New Zealand, 'the worst since 1855,' on 1929 June 16d. 22h. 47m. 10s., followed by another on 1929 June 27d. 13h. 47m. 0s.; a provisional estimate of the epicentre of the first as 43° N. 173° E. was sent to *The Times* on June 18. Fifteen deaths occurred as a result of the first shock, a heavy death roll for Wellington, which has lost only seven lives in all the other earthquakes since 1848. According to *The Times* correspondent, 'Westport was like a town that had been bombarded, and in Greymouth not a building has escaped.'

On the same day as the second N.Z. shock there was an earthquake in South Georgia. A rough estimate of the epicentre as near the South Orkneys was sent to *The Times* on June 28, but the corrected estimate came in a letter from Lt.-Comm. J. M. Chaplin, R.N., dated 1929, Sept. 5, from Grytviken, S. Georgia, kindly communicated to us by the Secretary of the 'Discovery' Committee as follows, in accordance with a suggestion made by the Hydrographer (Admiral Douglas):—

'On [1929] June the 27th, at XII–51–30 G.M.T., a violent earth tremor was felt lasting three minutes and of such intensity as to make lamps swing and glass objects on the laboratory shelves to rattle violently; this is the only earthquake which had ever been felt here as far as can be ascertained, up to that time, although of course further south in the [S.] Shetlands they are quite common. A further shock at about 2 a.m. on the 26th July was noticed in the whaling station. This, however, did not disturb anyone, and was not recorded in my establishment.'

The incident led to a correspondence on the possibility of establishing a seismograph in connection with the 'Discovery' expedition, on S. Georgia or the Falkland Islands, and to a visit to Oxford (on 1930 Feb. 18) by Mr. D. Dilwyn John, one of the officers of the expedition, for full consideration of the matter. But it was decided that, though information from that neighbourhood would be of great value, the conditions did not allow of the proper care of a seismograph.

There was a slight earthquake shock in England on 1929 July 2d. 20h. 30m., felt in Gloucester and the Forest of Dean, but no damage was done. The following note by Mr. R. J. R. Ward, of Denmead, Kettering Road, Northampton, was kindly sent by the B.B.C. to the Meteorological Office, and forwarded by the Superintendent of the Kew Observatory:—

'Tuesday, July 2, between 9.15 and 9.35, during Sir Walford Davies' talk and very nearly towards the end of same (by the way, I was switched on to London direct) the set took on a most awful rumbling sound, entirely blocking out any speech or music that was on at the time. It lasted fully 20–30 seconds, and I was just going to turn off, thinking it might have caught an electrical discharge, as it was very thundery round about, when the speech or music was resumed in the normal. I took no more notice until I heard next evening of earthquake shocks from Gloucester region at the exact time.'

Dr. Whipple adds that 'the time fits in quite nicely.'

On 1929 Nov. 18d. 20h. 31m. 40s. there was a shock near Newfoundland which broke a number of cables. As yet the epicentre is somewhat uncertain; Strasbourg gave 46° N. 54° W.; a preliminary determination at Oxford assigned the position 47°·5 N. 58°·0 W., agreeing with that of the U.S. Coast and Geod. Survey; but a letter from Ottawa dated Jan. 11, 1930, gives 43°·5 N. 57°·3W., and cables were broken a good deal further south still (as well as further north). The Telegraph Construction and Maintenance Company have very kindly supplied information; and Commander Robinson, R.N., of the Eastern Telegraph Company, says that 'the most serious damage occurred in an area between about latitudes 44° N. and 45° N. and longitudes 55° W. and 57° W., and that in an area further south round 40° N. and 53°·5 W. a cable was broken about Nov. 19d. 10h.' There was, however, no aftershock at this time, and though there were aftershocks at Nov. 18d. 23h. and 19d. 2h. they were very slight compared with the main shock. The Hydrographer kindly forwarded the following telegram from the Commercial Cable Company :—

No appreciable change in charted depths along line S. 42° E. from lat. 44°·45 N., long. 56°·09 W. to lat. 43°·58 N. long. 55°·05 W.

A small earthquake on 1930 Jan. 22d. 20h. 44m. 40s. is the subject of an interesting note by Gutenberg and Landsberg (*Beitr. zur Geoph.*, 26.2.1930) who put the epicentre at 50° 6′ N. 8° 8′ E., and trace a probable connection with the shock of 1846 July 29, the epicentre of which was determined macroseismically ; possibly also with older shocks back to 1619.

On 1930 May 5d. 13h. 45m. 50s. there was a destructive earthquake in Burma which cost some hundreds of lives in Pegu and Rangoon, and damaged many buildings, including the famous Shwehmawdaw Pagoda (a shrine of great sanctity said to contain two hairs of Buddha). In the *Rangoon Times* of May 7 we read :—

'It appears that the first shock in Pegu created but little apprehension, and it was only when the fatal second came and houses toppled over like skittles that terror seized the inhabitants. The collapse of the cinema was the most hideous disaster of an appalling visitation.'

As yet we have no seismographical information to hand enabling us to interpret this reference to a preliminary feeble shock.

The earthquake started a destructive fire, and this was followed by a huge seismic wave which overwhelmed the city of Pegu, an ancient seaport whose history goes back to 537 A.D. The epicentre may be put provisionally at 17° N. 95° E.

On the following day, May 6d. 22h. 34m. 10s., there was a severe earthquake in Persia, with a reported death roll of 3,000 ; epicentre about 38°·5 N. 45°·0 E., near the town of Tabriz, which suffered severely.

On July 2 there were a number of shocks near Gauhati in Assam, the severest being at 21h. 3m. 30s. ; epicentre about 25°·0 N. 90°·0 E. Thirty-five railway bridges were destroyed on the Bengal-Dooars railway.

## Explosions.

We are indebted to Messrs. Thom of the Patricroft Canal Works (near Manchester) for responding to a request made by Mr. A. E. Mourant of the Manchester Geological Survey Office, that certain explosions which they were contemplating in the course of their work should be made as far as possible available for seismographic experiments, especially by being made at specified times. Mr. Mourant took a good deal of trouble with the details ; and thanks are also due to the Astronomer Royal and to the B.B.C. for arrangements in connection with time signals. At Stonyhurst the speed of registration was temporarily trebled, and the sensitivity of the seismograph nearly doubled (acting on suggestions made by Mr. J. J. Shaw), in order to see whether such a seismograph could give information of value, without calling portable seismographs into action. Two explosions of 30 and 40 lbs. of dynamite were made on Feb. 21 after some delay owing to fog, which made it undesirable to transport the explosive by car for fear of accident. Stonyhurst (13½ miles away) got records described as 'almost comparable in amplitude with the records of the Jersey earthquakes, and showing two clearly separated phases' : but they were puzzlingly late by about 30 sec. Later explosions made it doubtful indeed whether they were directly connected with the explosion itself ; and Mr. Mourant concludes that we cannot look to our ordinary seismographs to provide information of value. To utilise the explosions, portable seismographs with much higher magnification should be brought into action.

## Kew Observatory, Richmond, Surrey.

The following report has been communicated by the Superintendent, Dr. F. J. W. Whipple :—

During the year 1929 the tabulation of microseisms was extended, the amplitudes and periods recorded by the N. component seismograph being tabulated eight times a day instead of four times. Analysis of the results indicates that there is no definite type of diurnal variation of microseismic activity at Kew. This is contrary to experience in some other parts of the world.

A paper by Dr. Whipple on 'The Great Siberian Meteor and the Waves, Seismic and Aerial, which it produced' has been published by the Royal Meteorological Society. The great meteor fell on June 30, 1908, but it is only recently that its importance has been realised. The shock of the impact of the meteorites on the earth was registered as an earthquake, not only at Irkutsk, Tashkent and Tiflis, but as far away as Jena. On the other hand human beings were much less affected by the earth movements than by the waves of pressure in the atmosphere. There is no other known instance of an earthquake produced by the action of a meteor, as indeed there is no record in historic times of a meteor devastating an area of hundreds of square miles, of a meteor producing airwaves which could travel a quarter of the way round the globe or of a meteor transforming the sky and prolonging twilight in middle latitudes right through the night.

The seismic effects of the meteor have been discussed also by E. Tams (*Zeitschrift der Gesellschaft für Erdkunde zu Berlin*, 1929, pp. 143–5). Tams found that the seismic waves had been registered at Hamburg, and estimated that the movement of the ground north and south had had an amplitude of about $1\mu$. He states also that the vertical oscillations recorded at Jena on the Straubel seismograph had an amplitude of $2{\cdot}3\mu$. From these new data it may be deduced* that the energy of the aerial waves was about 5,000 times that of the seismic ones.

*The Broadcasting of Seismological Information.*

The practice has been continued of broadcasting information with regard to large earthquakes registered at Kew Observatory. Reports are communicated to the Meteorological Office, Air Ministry, and are broadcast from Kidbrooke at 14h. or 19h. G.M.T. with the meteorological data which are normally being sent out at those hours. By special arrangement similar reports of earthquakes registered at Bombay are communicated to London and are also broadcast from Kidbrooke at the first opportunity.

The reports for Strasbourg (broadcast from Eiffel Tower), from Helwan (broadcast from Cairo), and from selected American stations (broadcast from Arlington) are received at the Air Ministry. Persons wishing to have such reports communicated to them regularly should apply to the Director of the Meteorological Office.

During the year ended March 31, 1930, reports of earthquakes registered at Kew Observatory were broadcast on twenty-two occasions. In five cases the estimated position of the epicentre was stated. In the same period Bombay reports were broadcast on eleven occasions. Messages were received from America through the Air Ministry on thirteen occasions.

July 18, 1930.

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* The seismic waves were large enough to be recorded at Hamburg for two minutes. No doubt sufficiently sensitive apparatus would have shown a disturbance lasting much longer. Considering the distance, about 50°, the long wave phase of the earthquake might be expected to last about 40 minutes. Remembering that the energy depends upon the square of the amplitude, an estimate of the total energy as 10 times that passing in the two minutes of greatest disturbance appears reasonable. The expression given by Jeffreys for the total energy crossing a small circle at distance $\Delta$ from the origin of an earthquake is equivalent to

$$1{\cdot}12\times(2\pi)^3\,R\rho\sin\Delta\int_o^t\frac{a^2V^2}{T}at$$

In this expression R is the radius of the earth, $\rho$ is the mean density of surface rocks (3 gm./c.c.), $a$ is the amplitude of the horizontal motion, V and T are the velocity and period of the waves.

Substituting $a=\sqrt{2}\times10^{-4}$ cm., $V=3\times10^5$ cm./sec., $T=15$ sec. and $t=120$ sec., we find $6\times10^{15}$ ergs for the energy passing in two minutes and $6\times10^{16}$ ergs for the total.

The energy of the air waves was previously found to be of the order $3\times10^{20}$ ergs—*Q.J. Met. Soc.*, 56 (1930), 300.

F. J. W. W.

**Calculation of Mathematical Tables.**—*Report of Committee* (Prof. J. W. NICHOLSON, *Chairman*; Prof. A. LODGE, *Vice-Chairman*; Dr. L. J. COMRIE, *Secretary*; Dr. R. A. FISHER, *General Editor*; DRS. J. R. AIREY, A. T. DOODSON *and* J. HENDERSON, Mr. J. O. IRWIN, Profs. A. E. H. LOVE *and* E. H. NEVILLE, DRS. A. J. THOMPSON *and* J. F. TOCHER, Mr. T. WHITWELL *and* Dr. J. WISHART).

*General Activity.*—Nine meetings of the Committee have been held, in London. The resignation by Dr. J. R. Airey of the office of Secretary, which he had held for twelve years, was received with much regret. Dr. D. M. Wrinch, who was unable to attend meetings, has resigned.

The grant of £30 has been expended as follows:—

| | |
|---|---|
| Confluent Hypergeometric functions .. .. .. .. .. | £10 |
| Interpolation of Bessel functions $J_0$ and $J_1$ .. .. .. .. | 8 |
| Preparation of copy for proposed volume .. .. .. .. | 5 |
| Calculation of Hermite functions .. .. .. .. .. .. | 4 |
| Clerical and postal expenses .. .. .. .. .. .. | 3 |

*Volume prepared.*—A volume containing the following tabular matter, with even differences unless otherwise stated, has now been prepared for the printer.

| | Pages |
|---|---|
| Circular functions, without differences:— | |
| Sine and cosine 0·0 (0·1) 50·0 to 15 decimals .. .. .. .. | 5 |
| ,, ,, 0·000 (0·001) 1·600 to 11 decimals .. .. .. | 16 |
| $\frac{1}{2}n\pi$ to 15 decimals, $n=1$ (1) 100 .. .. .. .. .. .. | 1 |
| Hyperbolic functions, without differences:— | |
| Sinh $\pi x$ and cosh $\pi x$ 0·00 (0·01) 4·00 to 15 decimals .. .. .. | 4 |
| ,, ,, 0·0000 (0·0001) 0·0100 to 15 decimals .. .. | 1 |
| Sinh $x$ and cosh $x$ 0·0 (0·1) 10·0 to 15 decimals .. .. .. .. | 1 |
| Sine and cosine integrals:— | |
| 0·0 (0·1) 20·0 (0·2) 40·0 to 10 decimals .. .. .. .. .. | 6 |
| Exponential integral, –15·0 (0·1) 15·0 to 11 decimals .. .. .. | 3 |
| Integral of logarithmic factorial function 0·00 (0·01) 1·00 to 10 decimals | 1 |
| Factorial or Π function, 0·00 (0·01) 1·00 to 12 decimals .. .. | 1 |
| Derivatives of logarithmic factorial function, i.e. di-, tri- and tetragamma 0·00 (0·01) 1·00 and 10·0 (0·1) 60·0, all to 12 decimals .. .. | 24 |
| Integrals of probability integral $Hh_n(x)$, without differences:— | |
| $x=-7{\cdot}0$ (0·1) vanishing, $n=0$ (1) 11; $x=-5{\cdot}0$ (0·1) vanishing, $n=12$ (1) 15; $x=-2{\cdot}5$ (0·1) vanishing, $n=16$ (1) 21 } to 10 decimals | |
| together with differential coefficients of probability function:— | |
| $x=-7{\cdot}0$ (0·1) vanishing, $n=1$ (1) 7, for use to 10 decimals .. .. | 10 |
| Total .. .. | 73 |

The accompanying introductory matter for the various tables will bring the volume up to about 100 pages. It has been ascertained that the cost of printing tabular matter will be approximately £2 a page, and that the cost of the volume will be about £200. The Committee desires that provision should be made for publication at an early date.

*Elliptic Function Tables.*—The following extract is from the Report of the Committee on Mathematical Tables for the year 1873, written by J. W. L. Glaisher:—

'The Account of the Tabulation of the Elliptic Functions.—In September 1872 it was resolved to undertake the systematic tabulation of the Elliptic Functions (inverse to the Elliptic Integrals), or, more strictly, of the Jacobian Theta Functions which form their numerators and denominators.

The formulæ are:— . . . . . . . . .

The tables when completed will give $\theta$, $\theta_1$, $\theta_2$, $\theta_3$, and their logarithms to eight decimals for

$$x=1^\circ, 2^\circ, \ldots 90^\circ, k=\sin 1^\circ, \sin 2^\circ, \ldots \sin 90^\circ.$$

The tables are thus of double entry, and contain eight tabular results for each of 8,100 arguments, viz. 64,800 tabular results.

For the performance of the calculation of $\theta$ and $\theta_3$ ($\theta_3$ being deduced from $\theta$) 8,500 forms were printed and bound up into 15 books (550 in each, with a few over). Each book, therefore, contains forms for the calculation of six nineties, viz. from $k=\sin\alpha^\circ$ (say), $x=0^\circ$, to $k=\sin(\alpha^\circ+5^\circ)$, $x=90^\circ$. Similar forms for the calculation of $\theta_1$ and $\theta_2$ were printed and bound up into 15 other books.

The work has been in active progress since the beginning of October 1872, and eight computers have been engaged from that time to the present, under the superintendence of Mr. James Glaisher, F.R.S., and the Reporter.

It is intended that the tables, which will be completed, it is hoped, by February 1874, shall form a separate work, and that they shall be preceded by an introduction, in which all the members of the Committee will take part—an account of the application of the functions in mathematics generally being undertaken by Professor Cayley, of their application in the theory of numbers by Professor H. J. S. Smith, and of their use in physics by Sir W. Thomson and Professor Stokes, while the account of the method of calculation, &c., will be written by the Reporter.

The magnitude of the numerical work performed has not often been exceeded since the original calculation of logarithms by Briggs and Vlacq, 1617–1628; and it is believed that the value of the tables will be great.

After the circular and logarithmic functions there are no transcendants more widely used in analysis than the Elliptic Functions; and the tables will not only render the subjects in which they occur more complete, but will also, to a great extent, render available for practical purposes a vast and fertile region of analysis. Apart from their interest and utility in a mathematical point of view, one of the most valuable uses of numerical tables is that they connect mathematics and physics, and enable the extension of the former to bear fruit practically in aiding the advance of the latter.'

The only subsequent references to these tables are to be found in the Reports of the Association in the Recommendations of the General Committee, in which sums totalling £809 were granted, of which apparently £259 was for computing and £550 for printing, and in the Treasurer's Reports, from which it appears that the grants were actually drawn. The tables were never published; the reason for this was never made public, although verbal statements have been handed down to the effect that the tables contained systematic errors.

The fact that the grants for printing were actually drawn may be taken as proof that the tables were set in type. Contemporary confirmation is afforded by a review of Cayley's *Elementary Treatise on Elliptic Functions*, by C. W. Merrifield in *Nature*, Vol. xv, p. 252 (Jan. 18, 1877). 'The arithmetical work is quite rightly omitted. That will find a much better place in the hand-book or introduction which will doubtless accompany or follow the great tables of elliptic functions now being printed for the British Association.'

The existence of the manuscript volumes was brought to the notice of the Committee by Prof. G. N. Watson after the death of Dr. J. W. L. Glaisher in December, 1928. An application to the Trustees of Dr. Glaisher's estate resulted in the 30 volumes being handed to the Committee.

The tables have been examined by Drs. R. A. Fisher and J. R. Airey. It appears that no systematic error can be detected, although owing to the method of computation the 10th decimal is subject to somewhat large errors. The tables could be used for the publication of an 8-figure table, and the Committee sets the publication of such a table in the forefront of its future programme.

*Cunningham Bequest.*—By the will of Lieut.-Col. A. J. C. Cunningham, R.E., who died on February 8, 1928, one-twelfth of the residue of his estate was left to the British Association, Mathematical Subsection, 'for preparing new mathematical tables in the theory of numbers.' The amount of the bequest is about £3,000.

The Committee is faced with a question of considerable difficulty in interpreting the terms of the bequest. In order that their future recommendations as to publication of tables may be framed in the most suitable form it is highly desirable that they should have a plain and explicit ruling from an independent authority on the interpretation of the phrase 'new mathematical tables in the theory of numbers,' in its relation to its context in the will. Great diversity of opinions is evidently possible as to the connotation of any term denoting a branch of mathematics, the conventional applications of which may be largely a matter of historical accident, and it is of practical consequence to the work of the Committee whether a broader or narrower connotation be adopted. The following resolution was carried at a meeting on June 16, 1930. 'That the Committee of Section A be requested to take the necessary steps to obtain for the use of the Tables Committee an explicit statement as to the interpretation to be adopted for the terms of the Cunningham Bequest, to which they will be prepared to adhere in recommending the application of funds.'

*Future Programme.*—In addition to the volume now ready for the printer, the Committee has in view the following volumes :—

(*a*) Elliptic functions, based on the tables computed by the late J. W. L. Glaisher. These would occupy at least 200 pages and cost £400 to print. The checking (by differencing) of the functions and the preparation of the printer's copy would have to be provided for.

(*b*) Bessel functions. For over 40 years the Committee has, from time to time, produced tables of Bessel functions, which are scattered throughout the Reports. The greater part of the grants made since 1890 has been spent on these functions. It is highly desirable that a volume of tables on a unified plan should be produced. Much work still remains to be done, and the volume would probably occupy about 300 pages. The work of interpolating Meissel's table of $J_0$ and $J_1$ from interval 0·01 to interval 0·001 has been begun, under the superintendence of Dr. L. J. Comrie.

(*c*) Confluent Hypergeometric Function. A number of tables of this function were published in the Reports for 1926 and 1927, but a much more extensive tabulation is necessary to make the function really useful. The tabulation presents peculiar difficulties, which are now being investigated by Dr. A. J. Thompson.

The Committee, although unable to anticipate the interpretation of the terms of the Cunningham Bequest, expresses the hope that provision will be made for this programme.

*Emden's Equation.*—A request has been received from Sir Arthur Eddington for the tabulation of solutions of Emden's equation

$$\frac{d^2u}{dz^2} + \frac{2}{z}\frac{du}{dz} + u^n = 0$$

for various values of $n$. This request is supported by Sir James Jeans and Prof. E. A. Milne, as the equation is one of great importance in astrophysics. Certain preliminary investigations of method and cost have been made by Drs. J. R. Airey and L. F. Richardson, and the Committee asks for a special grant of £25 to enable the calculation to be undertaken.

The Committee wish to add to their number the name of Dr. L. F. Richardson.

*Reappointment.*—The Committee desires to be reappointed, with a grant of £50 for general purposes.

## Photographs of Geological Interest..—*Twenty-fifth Report of the Committee* (Professors E. J. GARWOOD, *Chairman*, and S. H. REYNOLDS, *Secretary*; Messrs. E. G. W. ELLIOTT, J. F. JACKSON *and* J. RANSON, Prof. W. W. WATTS *and* Mr. R. J. WELCH).

IN the present report 141 photographs are listed, bringing the number in the collection to 8,287. Of these 39 are from the Reader collection of negatives, which has now been completely worked through; 1,133 photographs in all have been added to the collection from this source.

The Committee greatly regret having to record the death of one of their members, Mr. A. S. Reid, whose Scottish photographs, particularly of the Isle of Eigg, are of exceptional interest and excellence.

Mr. C. V. Crook having retired from H.M. Geological Survey, has resigned membership of the Committee, but Mr. E. G. W. Elliott, who succeeds him as librarian, has kindly consented to take his place on the Committee. Prof. A. Morley Davies, Mr. F. Gossling, Miss M. S. Johnston and Dr. L. J. Wills have kindly helped with the description of photographs included in the present list.

In addition to the Reader photographs the present series includes an excellent series by Mr. J. F. Jackson from the Isle of Wight, the landslip of the autumn of 1928 being particularly well illustrated. Dr. T. Franklin Sibly contributes photographs illustrating features in the courses of some of the rivers of South Wales, Dr. A. E. Trueman views of the Swansea raised beach, Mr. J. F. N. Green a set from Islay, and the Hon. Sec. sets from Torquay, Snowdon and the South London district. Mr. A. O. Rowden sends photographs of Lundy Island, Dr. F. S. Wallis and Mr. E. D. Evens of the Hestercombe Syenite, Mr. L. N. Wheaton of the Raised Beach at Hope's Nose, Torquay, and Mr. W. F. Chubb a fine view of the Severn Bore.

Three sets of geological photographs, numbering respectively 22, 25 and 23 subjects have been published by the Committee, and are obtainable through the Hon. Sec. at rates which he will quote on application. Fourth and fifth sets numbering 25 photographs apiece will probably be obtainable before the end of the year. Application should be made to the Hon. Sec. for information concerning these new issues.

The Reader negatives being the property of the Committee, prints (¼-plate) may be obtained through the Hon. Sec. at 4*d*. each, lantern slides at 1*s*.

The Committee recommend that they be reappointed.

### TWENTY-FIFTH LIST OF GEOLOGICAL PHOTOGRAPHS.

FROM JULY 1, 1928, TO AUGUST 1, 1930.

List of the geological photographs received and registered by the Secretary of the Committee since the publication of the last report.

Contributors are asked to affix the registered numbers, as given below, to their negatives, for convenience of future reference. Their own numbers are added in order to enable them to do so. Copies of photographs desired can, in most instances, be obtained from the photographer direct. The cost at which copies may be obtained depends on the size of the print and on local circumstances over which the Committee have no control.

The Committee do not assume the copyright of any photograph included in this list. Inquiries respecting photographs, and applications for permission to reproduce them, should be addressed to the photographers direct.

Copies of photographs should be sent, unmounted, to

Professor S. H. REYNOLDS,
The University, Bristol,

accompanied by descriptions written on a form prepared for the purpose, copies of which may be obtained from him.

The size of the photographs is indicated, as follows:—

| | |
|---|---|
| L=Lantern size. | 1/1=Whole plate. |
| 1/4=Quarter-plate. | 10/8=10 inches by 8. |
| 1/2=Half-plate. | 12/10=12 inches by 10, &c. |
| P.C.=post card. | E signifies Enlargement. |

## ACCESSIONS.

### England.

BEDFORDSHIRE.—*Photographed by the late* T. W. READER *and presented by* F. W. READER. 1/4.

| | | |
|---|---|---|
| **8146** | Shenley Hill, Leighton Buzzard . | Cretaceous Section. 1908. |
| **8147** | Shenley Hill, Leighton Buzzard . | Lower Greensand capped by Gault and Boulder Clay. 1908. |
| **8148** | Shenley Hill, Leighton Buzzard . | Lower Greensand capped by Gault and Boulder Clay. 1908. |

BUCKINGHAMSHIRE.—*Photographed by the late* T. W. READER *and presented by* F. W. READER. 1/4.

| | | |
|---|---|---|
| **8149** | Locke's pit, Hartwell . . | Hartwell Clay capped by Alluvium. 1912. |
| **8150** | Bliss's pit, Stewkley . . . | Kimeridge Clay with septaria. 1914. |
| **8151** | Bliss's pit, Stewkley . . . | Septaria with cone-in-cone structure in Kimeridge Clay. 1914. |
| **8152** | Littleworth brickfield, Wing . | Glacial gravels overlain by Boulder Clay. 1914. |
| **8153** | Littleworth, Wing . . . | Boulder Clay on Gault on Kimeridge Clay. 1914. |

DEVONSHIRE.—*Photographed by* S. H. REYNOLDS, M.A., Sc.D., *The University, Bristol.* $3\frac{1}{4} \times 2\frac{1}{2}$.

| | | |
|---|---|---|
| **8154** | Saltern Cove, near Paignton . | Block of Permian breccia. 1930. |
| **8155** | Saltern Cove, near Paignton . | Block of Permian breccia. 1930. |
| **8156** | Saltern Cove, near Paignton . | Permian breccias on Devonian. 1930. |
| **8157** | Saltern Cove, near Paignton . | Cleaved tuff band in Devonian. 1930. |

*Photographed by* L. N. WHEATON, 27 *Bolton Street, Brixham.*

| | | |
|---|---|---|
| **8158** | Between Broadsands and Elbury Cove, nr. Brixham | Disturbed Devonians. 1928. |
| **8159** | Hope's Nose, Torquay . . | Raised beach on Devonian. 1928. |
| **8160** | Hope's Nose, Torquay . . | Raised beach on Devonian. 1928. |

*Photographed by* A. O. ROWDEN, 15 *Pennsylvania Road, Exeter.*

| | | |
|---|---|---|
| **8161** | Lundy Island, Gannet's rock . | Granite coast. 1926. |
| **8162** | Lundy Island—St. James' stone. | Erosion among joint planes of well jointed granite. 1926. |
| **8163** | Lundy Island, near landing place. | Sea-cave in slate. 1926. |
| **8164** | Hartland Quay . . . | Marine erosion of highly inclined Devonian. 1929. |

DORSET.—*Photographed by the late* T. W. READER *and presented by* F. W. READER. 1/4.

| | | |
|---|---|---|
| **8165** | King's Pit, Bradford Abbas . | Fuller's Earth and top limestones of Inferior Oolite. 1911. |
| **8166** | Bothenhampton, Bridport . . | Weathered slab of Forest Marble. 1914. |
| **8167** | Bothenhampton, Bridport . . | Weathered slab of Forest Marble. 1914. |
| **8168** | Durdle Promontory, Lulworth, E. side | Ripple-marked Cypris freestone. 1910. |
| **8169** | Montacute . . . | Yeovil Sands. 1911. |

*Photographed by* SURREY FLYING SERVICES. 1/2.

**8170** Lulworth Cove from the air.

*Photographed by* G. M. DAVIES, M.Sc., 104 *Avondale Road, South Croydon.*

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|---|---|---|
| **8171** | (29.6) West Bay and East Cliff, Bridport | Cliff of Bridport Sand. 1929. |
| **8172** | (29.4) South of Bothenhampton . | Quarry in Forest Marble. 1929. |
| **8173** | (29.5) Burton Cliff, mouth of R. Brude and E. cliff, Bridport | Cliffs of Bridport Sand and fallen blocks. 1929. |

ESSEX.—*Photographed by the late* T. W. READER *and presented by* F. W. READER. 1/4.

| | | |
|---|---|---|
| **8174** | Albert Docks . . . . | General view of excavations. 1913. |
| **8175** | Albert Docks . . . . | Large tree trunks from the Alluvium. 1913. |
| **8176** | Albert Docks . . . . | Clay filling a wash-out in the peat. 1913. |
| **8177** | Albert Docks . . . . | Peat overlying alluvial clay. 1913. |
| **8178** | Albert Docks . . . . | Alluvial clay with much drifted wood. 1913. |
| **8179** | Albert Docks . . . . | General section; made ground, on peat, on alluvial clay, on gravel. 1913. |

GLOUCESTERSHIRE.—*Photographed by* W. F. CHUBB, *Highgrove,* 25 *Midland Road, Gloucester.* 1/4.

| | | |
|---|---|---|
| **8180** | Severn, near Gloucester . . | The Bore, September 4. 1921. |

HAMPSHIRE (ISLE OF WIGHT).—*Photographed by* J. F. JACKSON, F.G.S., 4 *Linden Road, Newport, I.W., and presented by* Miss C. MOREY. 1/4.

| | | |
|---|---|---|
| **8181** | (221) Yaverland Cliff, N. of Sandown | *Ostrea distorta* limestone in Wealden shale. 1928. |
| **8182** | (222) S. end of Redcliff, Sandown | Weathering of Lower Greensand. 1928. |
| **8183** | (223) Coast section; Redcliff to Culver | Sequence, Wealden to Chalk. 1928. |
| **8184** | (224) Bembridge Ledge, Whitecliff Bay | Marine erosion of limestone and clay. 1928. |
| **8185** | (225) St. Catherine's Point, Niton | Sequence, Upper Greensand to Chalk Marl. 1928. |
| **8186** | (226) St. Catherine's Point, Niton | Details of Chloritic Marl. 1928. |
| **8187** | (227) Woody Point, St. Lawrence | Weathered surface of conglomerate bed at base of Chloritic Marl. 1928. |
| **8188** | (228) Ventnor, between the Esplanade and Steephill cascade | Block showing section from Upper Greensand to Chalk Marl. 1928. |
| **8189** | (229) Windy Corner, Gore Cliff . | Road blocked by rock-fall of July 26, 1928. 1928. |
| **8190** | (230) Windy Corner, Gore Cliff . | Near view of great rock-fall of September 22, 1928. 1928. |
| **8191** | (231) Windy Corner, Gore Cliff . | Near view of the rock-fall from the Niton side. 1928. |
| **8192** | (232) Windy Corner, Gore Cliff . | General view of the fall of September 22, 1928. 1928. |
| **8193** | (233) Windy Corner, Gore Cliff . | Lakelet at foot of landslip of September 20–22, 1928. 1928. |
| **8194** | (234) Windy Corner, Gore Cliff . | Slipped masses and striated surface of Gault. 1928. |
| **8195** | (235) Windy Corner, Gore Cliff . | Destruction of road by landslip of September 20–22, 1928. 1928. |
| **8196** | (236) Below Windy Corner, Gore Cliff | Mass of fallen material and lakelet. 1928. |

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| **8197** | (237) Undercliff below Windy Corner, Gore Cliff | Detail of landslip of September 20–22, 1928. 1928. |
| **8198** | (238) Undercliff at Windy Corner | Cracks due to movements of September 20–22, 1928. 1928. |
| **8199** | (239) Undercliff below Windy Corner | Pressure ridge in grassland due to the landslip. 1928. |
| **8200** | (240) Shore at Rocken End . | Shore forced up by pressure of landslip of September 20–22, 1928. 1928. |
| **8201** | (241) Shore at Rocken End . | Details of shore forced up by pressure of landslip. 1928. |
| **8202** | (242) Chale Bay and Blackgang bluff seen from Rocken End | Tongue of slipped material pushing out into Chale Bay. 1928. |

*Photographed by* S. H. Reynolds, M.A., Sc.D., *The University, Bristol.* 1/4.

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|---|---|---|
| **8203** | E. of Brook Point . . . | Foundering down of cliff. 1929. |
| **8204** | The Crackers, Atherfield . . | Concretions in Ferruginous Sands. 1929. |

Herftordshire.—*Photographed by the late* T. W. Reader *and presented by* F. W. Reader. 1/4.

| | | |
|---|---|---|
| **8205** | Mimm's Hall brook, Warrengate, Potter's Bar | Peat and clay in gravel. 1908. |

Kent.—*Photographed by the late* T. W. Reader *and presented by* F. W. Reader. 1/4.

| | | |
|---|---|---|
| **8206** | Stone Court Chalk Pit, Stone near Greenhithe | Drift-filled channel in Chalk. 1919. |
| **8207** | Greenhithe (New globe pit). . | Pipe of brickearth in gravel. 1912. |

*Photographed by* S. H. Reynolds, M.A., Sc.D., *The University, Bristol.* 1/4.

| | | |
|---|---|---|
| **8208** | (28.58) Herne Bay . . . | London Clay on Oldhaven Beds. 1928. |
| **8209** | (28.57) Herne Bay . . . | London Clay on Oldhaven Beds. 1928. |
| **8210** | (28.60) E. of Herne Bay . . | Fossils in Thanet Sands. 1928. |
| **8211** | (28.59) W. of Reculvers . . | Section London Clay to base of Woolwich Beds. 1928. |
| **8212** | (28.61) E. of Reculvers . . | Woolwich Beds on Thanet Sands. 1928. |
| **8213** | (28.62) Reculvers . . . | Groynes. 1928. |
| **8214** | (28.41) Plumstead, Tuff and Hoar's Pit | Blackheath Beds on Woolwich Beds on Thanet Sand, on Chalk. 1928. |
| **8215** | Plumstead, Tuff and Hoar's Pit | Blackheath Beds on Woolwich Beds. 1928. |
| **8216** | (28.43) Plumstead, Tuff and Hoar's Pit | Blackheath Beds on Woolwich Beds. 1928. |
| **8217** | (28.42) Tuff and Hoar's Pit . | Base of Thanet Sand on Chalk. 1928. |
| **8218** | (28.52) Southborough Pit . . | Tunbridge Wells Sand on Wadhurst Clay. 1928. |
| **8219** | (28.55) Tunbridge Wells Common | Weathering of Tunbridge Wells Sandstone. 1928. |

Leicestershire.—*Photographed by the late* T. W. Reader *and presented by* F. W. Reader. 1/4.

| | | |
|---|---|---|
| **8220** | Harby, near Belvoir . . . | Quarry in Marlstone. 1908. |
| **8221** | Harby, near Belvoir . . . | Quarry in Marlstone. 1908. |

MIDDLESEX.—*Photographed by the late* T. W. READER *and presented by* F. W. READER. 1/4.

| | | |
|---|---|---|
| **8222** | N. London Ballast Co.'s pit, Edmonton | Low level terrace gravel of Lea Valley. 1914. |
| **8223** | N. London Ballast Co.'s Pit, Edmonton | 'Washout' in Lea Valley gravels. 1914. |
| **8224** | N. London Ballast Co.'s Pit, Edmonton | Section of Lea Valley gravel. 1914. |
| **8225** | Hedge Lane, near Edmonton . | Lower Terrace gravels. 1914. |
| **8226** | Southgate Council Gravel Pit, Edmonton | Middle and Lower Terrace Gravels. 1914. |

SOMERSET.—*Photographed by* E. D. EVENS *and presented by* F. S. WALLIS, D.Sc. $8\frac{1}{2} \times 5\frac{1}{2}$ (enl.).

| | | |
|---|---|---|
| **8227** | Hestercombe, near Taunton . | Diorite intrusive in Morte Slates. 1929. |
| **8228** | Hestercombe, near Taunton . | Diorite. 1929. |

SURREY.—*Photographed by* S. H. REYNOLDS, M.A., Sc.D., *The University, Bristol.* 1/4.

| | | |
|---|---|---|
| **8229** | Parkwood Fuller's Earth Pit, Nutfield | Section of Sandgate Beds. 1928. |
| **8230** | Parkwood Fuller's Earth Pit, Nutfield | Section of Sandgate Beds. 1928. |
| **8231** | (28.54) S. Merstham Nursery Pit. | Sandy Gault on Folkestone Beds. 1928. |

*Photographed by the late* T. W. READER *and presented by* F. W. READER. 1/4

| | | |
|---|---|---|
| **8232** | St. Catherine's, Godalming . | Lane in Lower Greensand. |

SUSSEX.—*Photographed by the late* T. W. READER *and presented by* F. W. READER. 1/4.

| | | |
|---|---|---|
| **8233** | Waterloo Rocks, Tunbridge Wells | Weathering of massive Tunbridge Wells Sandstone. 1909. |
| **8234** | Waterloo Rocks, Tunbridge Wells | Weathering of massive Tunbridge Wells Sandstone. 1909. |
| **8235** | High Rocks, Tunbridge Wells . | Honeycomb weathering of Upper Tunbridge Wells Sandstone. 1909. |
| **8236** | High Rocks, Tunbridge Wells . | Vertical joints enlarged by weathering in Upper Tunbridge Wells Sandstone. 1909. |
| **8237** | Cliff at East Groyne, Hastings . | Wealden section—Wadhurst Clay to Fairlight Clay. 1907. |
| **8238** | Cliff at East Groyne, Hastings . | Wealden section—Wadhurst Clay to Fairlight Clay. 1907. |
| **8239** | Near Hastings . . . . . | Bedding and jointing in Ashdown Sand. 1907. |
| **8240** | Bucks Hole, near Hastings. . | Wadhurst Clay overlying Ashdown Sand. 1907. |
| **8241** | Hastings . . . . . . | Slab of Hastings Sand with scales of *Lepidotus*. 1909. |

YORKSHIRE.—*Photographed by the late* W. H. BANKS, *Hergestcroft, Kington, Herefordshire.* 1/2.

| | | |
|---|---|---|
| **8242** | Gordale Scar, near Malham . | Ravine in horizontal Carboniferous Limestone. 1889. |

## Wales.

BRECON.—*Photographed by* T. FRANKLIN SIBLY, D.Sc., LL.D., *Vice-chancellor's Lodge, Reading.* 1/2-pl. enl.

| | | |
|---|---|---|
| 8243 | Porth-yr-ogof . . . . | Entrance to the underground channel of the Mellte. 1914. |
| 8244 | Near Porth-yr-ogof . . . | The Mellte emerging from its underground channel. 1914. |
| 8245 | Upper Clun-gwyn fall . . | Waterfall over fault-scarp. 1914. |

CARNARVONSHIRE.—*Photographed by* S. H. REYNOLDS, M.A., Sc.D., *The University, Bristol.* 1/4.

| | | |
|---|---|---|
| 8246 | Snowdon, S. slopes of Lliwedd . | Cleaved rhyolite tuff. 1930. |
| 8247 | Snowdon, Lliwedd from the Watkin track | Precipice of rhyolite tuff. 1930. |
| 8248 | Snowdon, Lliwedd from the Watkin track | Composed of cleaved tuffs of the Lower Rhyolitic series. 1930. |
| 8249 | Snowdon summit and Glaslyn . | Crags formed of the Bedded Pyroclastic series. 1930. |
| 8250 | Snowdon summit . . . | Crags formed of Bedded Pyroclastic series. 1930. |
| 8251 | Snowdon summit from the N. . | Tuffs with interbedded rhyolites and thin dolerite sills. 1930. |
| 8252 | Snowdon, Crib Goch . . . | Formed of rhyolite and tuff capped by acid intrusion. 1930. |
| 8253 | Snowdon, Bwlch Goch, above Glaslyn | Banded acid intrusion. 1930. |
| 8254 | Snowdon, near Rhyd-ddu . . | Quarry in Llandeilo slate. 1930. |
| 8255 | Beddgelert, near Rhyd-ddu . | Nodular base of Snowdon rhyolites. 1930. |
| 8256 | Snowdon, distant view of Cwm Clogwyn du'r Arddu | Moraine barrier. 1930. |
| 8257 | Snowdon, Clogwyn du'r Arddu . | Moraine-dammed lake. 1930. |
| 8258 | Snowdon, Llyn du'r Arddu. . | 1930. |
| 8259 | Snowdon, Cwm Clogwyn du'r Arddu | Lower Rhyolitic series with interbedded linestone. 1930. |
| 8260 | Snowdon, Afon Trewennydd . | Dolerite scree. 1930. |
| 8261 | Tryfan from Capel Curig road . | Formed of rhyolite of the Capel Curig series. 1930. |
| 8262 | Capel Curig, near Royal Hotel . | ?Strain-slip fracture or jointing in grit block. 1930. |
| 8263 | View from above waterfall Llyn Ogwen | Bwlch-y-Cywion 'granite' intrusion. 1930. |
| 8264 | Llyn Idwal, Nant Ffrancon. . | Moraines. 1930. |
| 8265 | Llyn Idwal and Craig ddu . . | Ice-worn rock and silting up of Llyn. 1930. |

DENBIGH.—*Photographed by the late* T. W. READER *and presented by* F. W. READER. 1/4.

| | | |
|---|---|---|
| 8266 | Pen-y-bont, near Cefn . . | Ruabon Marls. 1919. |
| 8267 | Oernant, near Pentre dwfr, Llangollen | Slate quarry. 1919. |
| 8268 | Near Horseshoe Pass, Llangollen. | Distant view of Carboniferous Limestone escarpment. 1919. |

GLAMORGAN.—*Photographed by* A. E. TRUEMAN, D.Sc., F.G.S., *University College of Wales, Swansea.* 1/4.

| | | |
|---|---|---|
| 8269 | Swansea Bay, about 500 yards E. of Black Pill | Submerged Forest looking E. 1929. |

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|---|---|---|
| **8270** | Swansea Bay, about 500 yards E. of Black Pill | Submerged Forest looking S. 1929. |
| **8271** | Swansea Bay, about 500 yards E. of Black Pill | Submerged Forest looking N. 1929. |

*Photographed by* S. H. Reynolds, M.A., Sc.D., *The University, Bristol.* 1/4.

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|---|---|---|
| **8272** | Caswell Bay, Gower . . . | C-beds. 1927. |
| **8273** | Caswell Bay, Gower . . . | Jointing in S-beds. 1927. |
| **8274** | Mumbles, Gower . . . | Pseudobreccia. 1927. |
| **8275** | Broadslade Bay, Mumbles . . | 'Stylolite.' 1927. |
| **8276** | Caswell road quarry . . . | 'Stylolite.' 1930. |
| **8277** | (6.27) Oystermouth, Gower . | 'Black Lias' quarry. 1927. |

## Scotland.

Argyll.—*Photographed by* J. F. N. Green, B.A., 51 *Alexandra Grove*, N. 12. 1/4.

| | | |
|---|---|---|
| **8278** | (a) S. of Ardnoe Point, near Crinan | Squeezed concretions in calcareous flaggy sandstone. 1923. |
| **8279** | (b) Beannan Dubh, Islay . . | Escarpment of bedded conglomeratic arkose. 1923. |
| **8280** | (c) Point E. of Bonahaven Bay, Islay | Worm burrows in flaggy dolomitic sandstone. 1925. |
| **8281** | (d) N. of Rudha Buidhe, Islay . | Bowmore flags. 1923. |
| **8282** | (e) S. of Tamhanachd, Islay . | Overfold in calcareous passage beds. 1925. |

## Ireland.

Dublin.—*Photographed by* S. H. Reynolds, M.A., Sc.D., *The University, Bristol.* 1/4.

| | | |
|---|---|---|
| **8283** | Portraine . . . . . . | Disturbed Ashgillian. 1928. |
| **8284** | Portraine . . . . . . | Contorted Ashgillian limestone. 1928. |
| **8285** | Portraine . . . . . . | Crushed Ashgillian limestone. 1928. |
| **8286** | Portraine . . . . . . | Crushed Ashgillian limestone. 1928. |
| **8287** | Portraine . . . . . . | Crushed Ashgillian limestone. 1928. |

## Animal Biology in the School Curriculum.—*Report of Committee* (Prof. R. D. Laurie, *Chairman and Secretary*; Mr. H. W. Ballance, Dr. Kathleen E. Carpenter, Prof. W. J. Dakin, Mr. O. H. Latter, Prof. E. W. MacBride, Miss M. McNicol, Miss A. J. Prothero *and* Prof. W. M. Tattersall) *appointed to consider and report upon the position of Animal Biology in the School Curriculum and matters related thereto.*

The Committee reports that it is continuing its investigations on the lines of the scheme adopted in its Report to the Glasgow Meeting in 1928. It proposes to present at some future date a report covering a series of years from 1929 onwards, but desires to put certain matters on record now.

1. The general demand for, and interest in, Biology as a subject of educational value continues to grow. The matter is engaging the attention of the Board of Education. A Committee of the Economic Advisory Council has been appointed 'to consider the obstacles which stand in the way of the education and supply of biologists for work in this country and overseas, and to submit recommendations for the removal of such obstacles.' The Association of British Zoologists has published a Report of its Sub-committee on the Teaching of Biology in Schools, adopted January 11, 1930, in which it holds that it is essential to insist that the education of every school child of either sex 'should include a course of general biology lasting for at least two years'; it suggests that the series of animal forms studied should include Amœba, Hydra, Earthworm, an Insect, and Frog, and adds that 'the list of animals is not to be regarded as a mere series of types, but as material exemplifying the problems of life.' The British Social Hygiene Council is pressing for the teaching of Biology in the Schools and has made representations to the Board of Education to that effect. In the Council's Twelfth Annual Report, 1927, it is demanded that 'greater attention should be given to the biological sciences in educational systems' as being 'in fact, a fundamental necessity to any real improvement in the general standard of personal and public health.' Biology is taking an increasingly prominent place in the activities of Summer Schools. Overseas also the movement is gaining strength. A strong movement is on foot, organised by the British Social Hygiene Council and backed by an Advisory Committee of the Colonial Office towards the introduction of Biology as an important part of the educational system of India and of the Crown Colonies. The position in the South African Schools was given some attention at the meeting of the British Association in South Africa last year, and Prof. H. B. Fantham's publications in the South African Journal of Science call for attention. The following publications of value to educationists interested in the school teaching of Biology are additional to those given in the 1928 Report :—


Association of Assistant Masters in Secondary Schools. *Report on the Conditions of Science Teaching in Oxfordshire.* Compiled by a Committee of the Oxfordshire branch of the Incorporated Association of Assistant Masters in Secondary Schools, 1929.

Association of British Zoologists. *Report of a Sub-committee on the Teaching of Biology in Schools.* Adopted by the Association, January 11, 1930. Obtainable from Prof. Frank Balfour-Browne, Hon. Secretary of the Association of British Zoologists, Winscombe Court, Winscombe, Somerset.

British Association for the Advancement of Science. *Report on Animal Biology in the School Curriculum.* Bound in Rept. Brit. Ass. for 1928. London, 1929. Obtainable also separately as Reprint No. 24, from the Office of the Association, Burlington House, London, W. 1.

British Association for the Advancement of Science. *Report on Science in School Certificate Examinations.* Bound in Rept. Brit. Ass. for 1928. London, 1929. Obtainable also separately as Reprint No. 23, from the Office of the Association, Burlington House, London, W. 1.

Fantham, H. B. *The Teaching of Biology in High Schools.* South African Journal of Science, vol. xxvi, Johannesburg, December, 1929.

Pinsent, A. *The Teaching of Science and the Training of Science Teachers.* Forum of Education, vol. vi, No. 3. November, 1928.

Rasmussen, Vilhelm. *Nature Study in the School.* Gyldendal, Copenhagen; Brentano's Ltd., 31 Gower Street, London, W.C. 1. Reprinted 1929. First published in Denmark in 1909. (A pioneer book dealing with Nature Study Method.)

Science Masters' Association. *General Science: introduction, outline of a course, suggested practical work, and specimen papers.* Compiled by a Committee of the Science Masters' Association. Murray, 1924. 1/1 post free. Obtainable from Canon T. J. Kirkland, King's School, Ely.

Wyss, C. von. *The Teaching of Nature Study.* Black. 1927. 3/6.

Some further references will be found in *The Teaching of the Life Sciences* published by the Friends' Guild of Teachers in 1927 (undated) and obtainable from the Secretary of the Guild, Bootham School, York, 7d. post free.

2. Biology, Botany and Zoology Syllabuses in School-leaving and Matriculation Examinations.

BIOLOGY.

(*a*) First Certificate and Matriculation.

Since the 1928 Report syllabuses in Biology have been provided for the First School Certificate Examination by Bristol, by the Cambridge Local Examinations Syndicate, and by the London Matriculation Board. The Oxford and Cambridge Schools Examination Board is thus the only examining body which has not as yet provided a syllabus in Biology at the School Certificate stage, though the Committee understands that a Committee of the Board has the matter under consideration at the present time; it has, however, in recent years examined a few schools on their own syllabuses.

(*b*) Higher Certificate.

Durham remains the only examining body without a syllabus.

ZOOLOGY.

(*a*) First Certificate and Matriculation.

For First School Certificate provision remains as in the 1928 Report, that is to say, 'Syllabuses are provided by Durham, London and Cambridge Local Examinations Syndicate; in the case of the latter under the title 'Natural History of Animals.' A few schools offer their own syllabuses for the Oxford and Cambridge Schools Examination Board.'

(*b*) At the Higher Certificate level a syllabus has been provided by Durham which first examined candidates in 1929. All examining bodies therefore now provide syllabuses at this stage.

BOTANY.

As indicated in the 1928 Report, syllabuses are provided at both stages by all examining bodies.

3. The substitution of Biology for Botany is continuing steadily in the schools, as appeared would probably be the case from the figures given in the 1928 Report. To make this clear Table XI of that Report is repeated here and brought to date by the addition of the percentages for 1928 and 1929.

(Footnote: The actual figures for the various Examination Boards have been collected and may be obtained from the Chairman of the Committee, Dept. of Zoology, University College of Wales, Aberystwyth.)

*Relative Numbers of Entries in England and Wales for Botany, Biology and Zoology respectively, expressed in percentages.*

SCHOOL CERTIFICATE OR MATRICULATION.

| Years. | Botany | Biology. | Zoology. |
|---|---|---|---|
| | % | % | % |
| 1918 | 98·2 | 1·3 | 0·5 |
| 1919 | 98·0 | 1·1 | 0·9 |
| 1920 | 98·2 | 1·1 | 0·7 |
| 1921 | 98·2 | 1·0 | 0·8 |
| 1922 | 97·7 | 1·8 | 0·5 |
| 1923 | 97·6 | 1·7 | 0·7 |
| 1924 | 97·3 | 2·1 | 0·6 |
| 1925 | 96·6 | 2·8 | 0·6 |
| 1926 | 95·7 | 3·6 | 0·7 |
| 1927 | 94·8 | 4·6 | 0·6 |
| 1928 | 93·0 | 6·3 | 0·7 |
| 1929 | 91·0 | 8·3 | 0·7 |

HIGHER CERTIFICATE.

| Years. | Botany. | Biology. | Zoology. |
|---|---|---|---|
| | % | % | % |
| 1918 | 85 | 8 | 7 |
| 1919 | 84 | 3 | 13 |
| 1920 | 87 | 3 | 10 |
| 1921 | 81 | 4 | 15 |
| 1922 | 82 | 3 | 15 |
| 1923 | 74 | 9 | 17 |
| 1924 | 77 | 7 | 16 |
| 1925 | 77 | 9 | 14 |
| 1926 | 72 | 9 | 19 |
| 1927 | 71 | 9 | 20 |
| 1928 | 71 | 9 | 20 |
| 1929 | 69 | 10 | 21 |

4. The number of candidates, boys and girls together, presenting biological subjects (Botany, Zoology, Biology, and the increasingly popular schemes of General Science including Biology) expressed as a percentage of the total number of entrants for all subjects has remained at about the same figure of 25 per cent. during the last twelve years, the total entrants having risen from 33,563 in 1918 to 80,388 in 1929. It would appear that the great majority of these biological candidates are girls.

5. The Committee is not able to give figures indicating the number of boys taking the biological subjects at the First School Certificate stage, but it is known to be an almost negligible quantity. It would appear likely, however, from cases of schools known to members of the Committee as introducing Biology now for the first time, that a slow movement may be commencing towards its more liberal introduction into the boys' schools.

6. Recommendations.

(*a*) It was urged in the 1928 Report that Biology should be included as a fundamental subject in the curriculum of all schools. The Committee re-affirm this recommendation.

(*b*) The four School Certificate and Matriculation Examination Bodies which did not provide syllabuses in Biology were invited in the 1928 Report to consider the desirability of doing so. The subject now is recognised by all the examination bodies at this stage, and a syllabus provided by all except the Oxford and Cambridge Schools Examination Board, which nevertheless examines a few schools on their own syllabuses. At the Higher stage Durham remains the only Examining Body which does not provide a syllabus. The Committee invites the University to consider the provision of one.

(*c*) Attention was called in the 1928 Report to the shortage, particularly among men, of teachers with biological training. The Committee calls attention to the fact that the position has not materially altered.

(*d*) It was submitted in the 1928 Report that there was need in the Universities for the provision of schemes of study related more definitely to the needs of science students intending to become teachers. It was suggested that there should be a more general recognition of General schemes of study as alternative to the present Special schemes as a path to a good degree. One way of meeting the situation would be by the institution of General Honours as has already been done by the Universities of London, Manchester, Leeds and Reading; another would be by instituting General and Special schemes in either of which Honours would be awarded to the better candidates. Suggestions along some such lines are, the Committee understands, the subject of discussion at present at several of the other Universities. The provision of such courses as alternative paths to a good degree would, the Committee feels, help most materially towards the introduction of Biology into the Boys' Schools.

7. Books suggested for School Libraries.

The following are supplementary to the list given in the 1928 Report. It must be clearly realised, however, that the list is a selection only and that, notoriously, the same book does not appeal with equal force to different people.

Atkinson, George Francis.—'First Studies of Plant Life.' Edited by E. M. Wood. (Ginn. 4/6.)

Berks, Robert.—'Garden Science: a three years' course of practical science based on experiments in garden, field, and classroom.' (Nelson. 2/–.)

Blomefield, Leonard.—'A Naturalist's Calendar.' Second edition, edited by Francis Darwin. (Cambridge University Press. 1922. 3/6.)

Borradaile, L. A.—'The Animal and its Environment.' (Oxford University Press. 1923. 18/–.)

Dell, J. A.—'Animals in the Making.' (Bell's Natural Science Series. 2/6.)

—— 'The Gateways of Knowledge.' (Cambridge Nature Study Series, Cambridge University Press. 1912. 3/6.)

Godwin, H.—'Plant Biology, an outline of the principles underlying plant activity and structure.' (Cambridge University Press. 1930. 8/6.)

Green, E., and E. A. Potter.—'Biology by Discovery.' (Modern Science Series, Dent. 1929. 5/–.)

Green, J. J.—'A First Book of Rural Science.' (First Books of Science. Macmillan. 2/6.)

Holmyard, E. J.—'Biology for Beginners.' (Modern Science Series, Dent. 1930. 2/–.)

Locy, W. A.—'The Growth of Biology.' (Bell. 1925. 16/–.)

Maris, K. E.—'Introductory Science for Botany Students.' (Murray. 1928. 3/–.)

Nicholson, E. M.—'How Birds Live.' (Williams & Norgate. 2nd Ed. 1929. 3/6.)

Shuttleworth, Margaret A.—'The Wonders of the Human Body.' (London University Press. New and Revised Edition, 1928. 2/6.) (Suitable for Upper III.)

Thomson, A. Landsborough.—'Problems of Bird Migration.' (Witherby. 1921. 18/–.)

Walker, Norman.—'An Introduction to Practical Biology: a course of work based chiefly upon the plant and arranged for use without special apparatus in either the classroom or the home.' (Pitman. 1926. 5/–.)

Wayside and Woodland Series (Warne):—

Coward, T. A.—'Life of the Wayside and Woodland: When, Where and What to Observe and Collect.' (7/6.)

Russell, F. S., and C. M. Yonge.—'The Seas: our knowledge of Life in the Seas and how it is gained.' (1928. 12/6.)

Smith, B. Webster.—'The World in the Past: a popular account of what it was like and what it contained.' (1926. 10/6.)

Wells, H. G., Julian Huxley and G. P. Wells. 'The Science of Life.' (Amalgamated Press. 1930. Vol. I, 19/–; Vol. II, 19/–; Vol. III, 20/3.)

Whipple, A. H.—'The Teaching of Hygiene: illustrated by Simple Experiments.' (Gill. 1928. 1/6.)

Wyss, C. von.—'Living Creatures: Studies of Animal and Plant Life.' (Black. 1927. 12/6.)

8. Supply of Trained Biologists for posts at home and overseas.

There has been now for some time a shortage of trained biologists for vacancies overseas. It is on grounds of general education and culture and as a background for citizenship, that the Committee is most concerned to press for the introduction of Biology into all schools as a subject to be taken by all scholars. But with Biology so recognised the supply of Biological experts required for posts at home and overseas would be forthcoming.

## **Stresses in Overstrained Materials.**—*Report of Committee* (Sir HENRY FOWLER, *Chairman*; Mr. J. G. DOCHERTY, *Secretary*; Prof. G. COOK, Prof. B. P. HAIGH, Mr. J. S. WILSON).

THE following interim report is submitted.

The programme of work set out in the report for 1929 has been commenced.

An investigation of the stress-strain relation up to and beyond the yield point is being carried out in the Engineering Laboratory of the Royal Naval College, Greenwich, by Mr. J. G. Docherty and Mr. F. W. Thorne, but the tests are not yet complete. It is hoped to publish the results soon.

Professor G. Cook, in the Engineering Department, King's College, London, is investigating (*a*) the effect of non-uniform stress produced by torsion, flexure, and internal pressure, on the initial yield point in mild steel, (*b*) the stress distribution in the initial stages of plastic strain for these cases. It is hoped that the results of these experiments will be published in the autumn.

It is hoped that a full report of these experiments, and also the other papers proposed in the 1929 Report, will be presented along with the report for 1931.

The Committee ask to be reappointed for another year.

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## **Earth Pressures.**—*Fifth Interim Report of Committee* (Mr. F. E. WENTWORTH-SHIELDS, *Chairman*; Dr. J. S. OWENS, *Secretary*; Prof. A. BARR, Prof. G. COOK, Mr. T. E. N. FARGHER, Prof. A. R. FULTON, Prof. F. C. LEA, Prof. R. V. SOUTHWELL, Dr. R. E. STRADLING, Dr. W. N. THOMAS, Mr. E. G. WALKER, Mr. J. S. WILSON).

SINCE its last report the Earth Pressures Committee has met once, viz., on May 29, 1930, at the Building Research Station at Garston, Watford, by the kind invitation of Dr. Stradling, to see the research work which is being carried out there by Prof. C. F. Jenkin, whose report is attached, and who explained to the Committee the nature of the research and the apparatus he was using. Those present were impressed with the fact that the investigation of the actual pressure and resistance of clay is very much more complex than previous investigations have led us to suppose, and indeed that the same may be said about sand. Professor Jenkin is on his guard against the many factors, which may lead to quite wrong conclusions in this kind of research, and although the results of any research cannot be foretold, it seems likely that his work will lead to information and practical results. The Committee recommend that his and their work be carried on for a further period.

### *Progress Report on Earth Pressure Experiments.*

Since my last report my apparatus has all been moved to the Building Research Station, Watford. Considerable delay has occurred in building my new laboratory, but it is now complete and I have started working in it.

In August last I visited the large research laboratory, Berlin, known as Versuchsanstalt für Wasser und Schiffsbau (Testing Station for Water and Shipbuilding). This establishment is carrying out regular tests of soils encountered in connection with the new dock works at Bremerhaven. The soil samples are packed in air-tight cases to ensure against loss of moisture on the journey. Briefly, the following tests are carried out:

The moisture content is determined by drying.

The grain size is determined by sieving, and in the case of finer grains, by water separation.

The specific gravity is determined by removing the air with carbon tetrachloride, and then measuring the volume with water.

The proportion of carbonate of lime is determined by measuring the CO given off when the grains are treated with sulphuric acid.

The nature of the grains is determined by microscopic examination.

The acidity is determined by measuring with an electric bridge, giving the hydrogen-ion content.

In addition to these tests, another is carried out, which is no doubt the important one from an engineer's point of view, to ascertain the shearing strength of the soil. This is effected by means of a machine designed by Prof. Terzaghi (a description is given in Krey's *Erddruck und Erdwiderstand,* published by W. Ernst & Sohn of Berlin). The soil is not tested exactly as it arrives, but it is diluted with excess of water and then put in a thin layer between sand-filtering discs under a known load. The water is thus squeezed out until a stable condition is reached, which takes about a fortnight. It is considered that the clay then contains the maximum amount of water possible under that particular load. The load usually selected is that to which it is subjected *in situ,* but the clay is sometimes tested after having been subjected to other selected loads, and therefore contains more water, or less, as the case may be. It is important to note that the shearing strength obtained represents the result of combined cohesion and friction.

While my new laboratory was being built I concentrated on the solution of problems presented by a very simple model of sand. As I have previously stated, I believe that sand pressures are essentially connected with the packing of the grains. It appeared to be worth while to investigate the pressure of ideal sand made up in uniform spheres. Rough experiments with Hoffmann steel balls showed that they behaved very like sand. The mathematical solution of the equilibrium of steel balls in *three* dimensions appeared to be very difficult, so the two-dimensional problems were attacked. The spheres (in two dimensions) can be replaced by cylinders and a model was made with about 100 discs 1 in. dia. by ¼ in. thick. These can be piled on edge and their motion examined as they slip.

The results of the mathematical investigation of the forces between the discs proved to be most interesting. This simple two-dimensional model will reproduce all the phenomena of 'arching.' The arching over a hole in the bottom of the bin is reproduced. The arching which supports sand in a silo (or vertical tube) is reproduced. The arching which is used in the latest Swedish method of building retaining walls, anchored back at the top, is reproduced. The model shows all these actions and the graphical solution of the forces gives numerical results.

I regard arching as the most fundamental property of sand and, therefore, the model appears to be valuable.

The next problem was to find out how to use the model: how to apply the results to real problems. One use of the model has already been found, which may be of great importance. The model shows that there are limits which cannot be passed by arching; thus no arching can occur over a hole in the bottom of a box unless the depth of sand is greater than a certain proportion of the diameter of the hole. This appears reasonable. The same reasoning shows that sideways arching cannot occur in sand if the depth of sand is small compared with the width. This at once indicates how accurate measurements may be made on sand pressures—*i.e.* how arching may be eliminated—viz., by keeping the width of the wall on which the pressure is measured large compared with its depth. All previous experiments have, I believe, been made with approximately cubical boxes, and attempts have been made to avoid arching by using larger and larger models. The solution appears to lie in the proportions, not in the size of the model.

A measuring apparatus was at once put in hand on these lines and is now nearly completed. Preliminary experiments show that it acts satisfactorily, so far as can be judged at present. The apparatus is designed to measure the force on a wall (vertical or battered) and to give the horizontal and vertical components and the point of application: in other words, to give the resultant force in magnitude, position and direction. The 'wall' is about two feet long and three inches deep.

Since I reported to the Committee the fact that sand would bear loads proportioned to the *cube* of the dimension of the bearing surface (not *square*), Mr. Oscar Faber has repeated my experiments with rather more accurate apparatus and has confirmed my results.

The clay experiments are held up at present till the new 3-ton testing machine, now nearly ready, is available.

I should welcome a visit from the Committee to see my calculations and new apparatus.

C. F. JENKIN.

## Notes and Queries on Anthropology.—*Report of Committee* (Dr. A. C. Haddon, *Chairman*; Mr. E. N. Fallaize, *Secretary*; Mrs. Robert Aitken, Mr. H. Balfour, Capt. T. A. Joyce, Prof. J. L. Myres, Mrs. Seligman, Prof. C. G. Seligman).

The four previous editions of *Notes and Queries on Anthropology* were published respectively in 1874, 1892, 1899, and 1912. The present Fifth Edition, 1929, follows the general lines of the last edition, but a re-arrangement was deemed necessary and little of the old wording has been retained; owing to recent investigations, the section dealing with Social Anthropology, of which Magic and Religion now form an integral part, had to be entirely re-written. The General Committee appointed at the Oxford Meeting of the British Association, 1926, to compile this edition delegated the writing of various sections to five Sub-Committees and in addition to the members of these a number of specialists in different departments of Anthropology have aided the Committee by their advice and criticism and in writing on certain subjects. The names of all these have been acknowledged in the book and we would like to take this opportunity to thank them for their willingness to help and for their valued co-operation. It was found in 1928 that it was necessary to engage an Assistant Editor and we were fortunate in obtaining the services of Mr. L. J. P. Gaskin, then Librarian to the Royal Anthropological Institute, who did his work with zeal and entirely to the satisfaction of the Committee. The book is published by the Royal Anthropological Institute at the price of 6*s*.

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## Kent's Cavern.—*Report of Committee appointed to co-operate with the Torquay Natural History Society in investigating Kent's Cavern* (Sir A. Keith, *Chairman*; Prof. J. L. Myres, *Secretary*; Mr. M. C. Burkitt, Dr. R. V. Favell, Mr. G. A. Garfitt, Miss D. A. E. Garrod, Prof. W. J. Sollas).

The Committee has received from the excavators the following report:—

Excavations have been continued in the 'Wolf's Cave,' and along the foot of the 'Sloping Chamber,' from October 1929 to the end of May 1930. The heavy rains of January caused considerable interference in the work. Last year's trench was extended to 60 feet in length, and deepened to 7 feet below the upper or 'granular stalagmite' floor, except at a point near the entrance to the 'Wolf's Cave,' where bedrock was reached.

Previous explorers had dumped their tip all along the trench, and it is doubtful if some of the finds were in their original position. These include a quartzite pebble of the Budleigh Salterton Pebble Bed type, four inches long, which had apparently been used as a hammer-stone; three flints of no very definite character, but showing signs of utilisation; and an interesting bone implement, shaped to a much-worn and polished point, suggesting its use as an awl or borer. All of these tools were found in the 'Sloping Chamber.'

The fauna continues to be entirely of the usual Late Pleistocene type, with Horse still predominating over Hyena, followed, at some distance, and in numerical order, by Rhinoceros, Stag, Mammoth, *C. Megaceros*, Bear, Bos, and Wolf. A few remains of microtine fauna have not yet been under expert examination.

The deposit, although not easily distinguishable from the cave-earth elsewhere found immediately below the 'granular stalagmite' floor, contained a considerable number of rolled and sub-angular pieces of grit from the Lincombe Hill; a test sample, upon washing, revealing a residue consisting as to two-thirds of this rock as against only one-third from the limestone. Occasional small pieces of concreted grit, and of a crystalline stalagmite, presented themselves, but neither the corresponding earthy deposit, called by Pengelly the 'breccia,' nor its covering floor of 'crystalline stalagmite,' has presented itself *in situ*. Nevertheless, the composition of the deposit points to a mixture of material of different ages, and the absence of the 'middle' or 'crystalline stalagmite' floor renders such a mixture by no means surprising.

In the course of deepening the deposit in the 'Bear's Den,' in order to give head-room for the passage under an archway, the proprietor of the cavern reached the base of the concreted 'breccia,' and revealed below it a fine silt apparently identical with the silt at the base of the deposits in the 'Gallery,' referred to in the Committee's report for 1928. A sample of this deposit led to the discovery of a bone, probably of fox.

It is intended to deepen the trench throughout the 'Wolf's Cave' and the 'Sloping Chamber,' from end to end, during next season's work.—(Signed) F. BEYNON. A. H. OGILVIE.

The Committee asks to be re-appointed with a grant of £10 to meet the cost of unskilled labour for removing debris after examination.

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**Sumerian Copper.**—*Reports of Committee* (Mr. H. J. E. PEAKE, *Chairman*; Mr. G. A. GARFITT, *Secretary*; Mr. H. J. BALFOUR, Mr. L. H. DUDLEY-BUXTON, Prof. GORDON CHILDE, Prof. C. H. DESCH, Prof. H. J. FLEURE, Prof. S. LANGDON, Mr. E. MACKAY, Sir FLINDERS PETRIE, Mr. C. LEONARD WOOLLEY) *appointed to report on the probable source of the supply of copper used by the Sumerians.*

(By Prof. C. H. DESCH, F.R.S., University of Sheffield.)

*Third Interim Report.*

THE grant from the Association has made it possible to continue the employment of Mr. E. S. Carey during a part of the session, and many specimens have been analysed. Further, the writer has been engaged on a metallographic examination of such specimens as consist largely of uncorroded metal, with a view to determining the nature of the metallurgical processes employed in their production, and also of corroded specimens, as part of a study of the process of corrosion in the soil. This question is of some archæological importance, as it may happen that one constituent of an alloy is removed by corrosion more rapidly than another, so that the analysis of a highly corroded object may not give the same ratio of constituents as the original alloy. These investigations will be reported on separately. In the analyses which follow, the composition of highly corroded objects has been calculated back to the unoxidised metal.

Some fragments from El Obeid were received from the British Museum, described as probably of the first dynasty, but not earlier. A nail and a lion fragment proved to be of practically pure copper, with no tin, the nickel content being 0·109 per cent. and trace respectively. A plate from the same source, but probably of later date, contained 7·95 per cent. of tin and a trace of lead, but was free from nickel.

The following specimens from Ur were received from Mr. Woolley, being of the first dynasty date or earlier.

| | Tin. | Nickel. | Lead. |
|---|---|---|---|
| Spear. U 11,886 | 0 | 0·165 | 0 |
| Axe. No number | 15·1 | 1·11 | tr. |
| Nail with hole. U 12,229 | 1·45 | 0·34 | 0 |
| Rim of bowl. No number | tr. | 0·35 | tr. |
| ?Nail. U 12,672 | 8·80 | 0·27 | tr. |
| Knife. U 11,436 (Sargonid period) | 0 | 0·44 | 0 |
| Bowl. No number. (Date uncertain) | 14·3 | 0·10 | 0 |

A further consignment consisted of five numbered specimens from the British Museum.

| | Tin. | Nickel. | Lead. |
|---|---|---|---|
| 1. Plates and nail. Sumerian. (Hall) | 0 | tr. | 0 |
| 2. Pot and lamp. Sumerian. (Woolley) | 0 | 0·84 | tr. |
| 3. Trowel. Sumerian. (Woolley) | 0 | 1·61 | 0 |
| 4. Bowl. Possibly Sumerian. (Woolley) | 0 | tr. | 0 |
| 5. Fragments. Probably post-Sumerian. (W.). | 14·3 | 0 | tr. |

Specimens 1 to 4 are therefore copper, whilst 5 is bronze, apparently produced from copper from a different source.

The finds at Ur during the excavations of the last season include a spear head from the stratum just above the flood deposit, described as the earliest metal object so far found in the work. This has proved on analysis to be of copper, with no more than traces of foreign elements. In the later deposits objects of both bronze and copper have been found, but have not yet been analysed. Mr. Woolley is of opinion that the finding of hammered copper axes of later date than the cast axes of bronze, and to some extent imitating them in design, is evidence of a failure of the supply of tin between 3200 and 2700 B.C. This clue appears to be worth following up. So far, the source of the tin used in the making of these bronzes remains unknown.

In view of the suggested connection between Sumerian and South African metals, a series of specimens was obtained from Mr. H. B. Maufe, of the Geological Survey of Rhodesia. Three metallic objects were examined :—

| | Copper. | Iron. | Nickel. | Tin. | Lead. |
|---|---|---|---|---|---|
| 1. Native copper. Falcon mine, Chilimanzi district . . . . . . . . | 97·50 | 0·35 | 0 | tr. | 0 |
| 4. Partly smelted furnace product; same source . . . . . . . . | 6·45 | 25·79 | 0·37 | 0 | tr. |
| 8. Native copper. Silverside mine, Lomagundi district . . . . . . . | 98·01 | tr. | 0 | tr. | 0 |

Specimens 2, 3, 5, 6 and 7 were ores, both pyritic and carbonate. None of these proved to contain more than minute quantities of nickel. They do not, therefore, enter into consideration as possible sources of Sumerian copper. The furnace product 4 is interesting, the ratio of nickel to copper being very high. In this respect it resembles some of the South African alloys which have been described previously, apparently made by the mixing of ores of copper and nickel. No information as to its probable date is available.

Mr. W. F. Collins also supplied a fragment of an ingot of copper found on Athlone Farm, near Buiduva, S. Rhodesia, together with remains of crucibles. This proved to be copper containing only traces of tin, iron and nickel. The date of these workings is unknown.

A large spear head from Zimbabwe, lent by the South African Museum, was composed of bronze with 12·26 per cent. of tin, with only traces of iron and lead, and no nickel. Many analyses of South African objects, bearing on this question, have been published by Prof. G. H. Stanley (*S. African Journal of Science*, 1929, 26, 732).

On account of the connection between Indian and Sumerian metals already recorded it has been thought worth while to analyse two fragments of ancient celts from Burma sent by Mr. T. O. Morris. Both of these are cast material. No. 1, from Gyogya Village, Thayetmyo district, contained 1·02 per cent. tin and a trace of iron, with no lead or nickel. The other, from Thaminthal Village, Lower Chindwin district, contained 2·77 per cent. tin, a trace of lead and no nickel.

The opportunity has been taken to analyse a number of Chinese bronzes of dates varying from the Chow to the T'ang periods. These are tin bronzes, most of which contain relatively high percentages of lead. They are mostly free from nickel, and appear to be derived from entirely different sources from the Sumerian material.

Although there have been several recent papers on the subject, the cause of the passage from copper to bronze remains completely obscure. The analysis of the early bronzes offers no support for the suggestion that they were obtained accidentally by smelting minerals which contain both copper and tin. Such minerals are always of a complex character, and would not give rise to such pure alloys as the early bronzes are found to be. It would appear, therefore, that these bronzes have been made by mixing oxide ores of copper and tin, which must have been done deliberately. It would be desirable to obtain further specimens of ores from various parts of Asia within the possible range of Sumerian trade, in order to throw some light on this question.

**Distribution of Bronze Age Implements.**—*Report of Committee* (Prof. J. L. Myres, *Chairman*; Mr. H. J. E. Peake, *Secretary*; Mr. A. Leslie Armstrong, Mr. H. Balfour, Prof. T. H. Bryce, Mr. L. H. Dudley Buxton, Prof. V. Gordon Childe, Mr. O. G. S. Crawford, Prof. H. J. Fleure, Dr. Cyril Fox, Mr. G. A. Garfitt).

Since last year all the remaining specimens from England and Wales in the Ashmolean Museum have been sketched and entered on cards, as well as a number of recent accessions to other museums, a number in private collections and some offered for sale by Messrs. Sotheby. A large series of sketches of early bronze implements in the Copenhagen Museum, made by Mr. Clark, have also been placed on cards.

The specimens in this country from sites in England and Wales are now completely dealt with, except for the late Bronze Age hoards in the British Museum, a few in Yorkshire museums, and some in private hands. Fresh discoveries are, however, being made from time to time.

Of the balance of £39 14*s.* 10*d.*, a sum of £24 16*s.* 6*d.* has been paid to Miss Chitty for work done during the year, leaving a balance of £14 18*s.* 4*d.*; a further sum of £8 5*s.* will shortly be due to her, when the available balance will be £6 13*s.* 4*d.*

The Committee asks to be reappointed with balance in hand and a fresh grant of £50; which, it is estimated, will provide for recording the British Museum hoards above mentioned.

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**South African Archæology, etc.**—*Report of Committee appointed to consider the lines of investigation which might be undertaken in Archæological and Anthropological Research in South Africa prior to and in view of the meeting of the Association in that Dominion in* 1929 (Sir H. A. Miers, *Chairman*; Dr. D. Randall-MacIver, *Secretary*; Mr. H. Balfour, Dr. A. C. Haddon, Prof. J. L. Myres).

The excavations of Miss Caton-Thompson and her party at Zimbabwe Ruins were completed in the summer of 1929, and a preliminary summary of the results was presented to the Anthropological Section at Johannesburg. Two large parties of members of the Association visited the site in August. In accordance with agreement, the trenches were filled, and the stability of neighbouring walls assured, under the supervision of the resident Curator of Ruins, and to his satisfaction, before the first rains; and the portable antiquities were put in his charge, pending the provision of a permanent museum-building by the Government of Southern Rhodesia.

The more important objects from these excavations were shown in the Zimbabwe Loan Exhibition at the British Museum (see *Report of Council*), and returned to the site; all expenses of transport being generously borne by the Government of Southern Rhodesia.

Miss Caton-Thompson's account of her excavations is ready for printing, and will be published shortly by the Clarendon Press.

The Committee desires to express its appreciation of the willing and generous assistance rendered by everyone concerned, to Miss Caton-Thompson and her party, throughout these excavations and the numerous journeys incidental to them in Southern Rhodesia.

# **Vocational Tests.**—*Report of Committee* (Dr. C. S. Myers, *Chairman*; Dr. G. H. Miles, *Secretary*; Prof. C. Burt, Mr. F. M. Earle, Dr. L. C. Wynn Jones, Prof. T. H. Pear, Prof. C. Spearman).

## A. Introduction.

In the Report of last year it was suggested that the general absence of correlation between 'motor' tests might be partly due to (1) the low reliability of the tests employed, (2) the disturbing effects of 'practice' and 'fatigue,' and (3) the disturbing influence of other 'abilities' which the tests employed to measure 'motor' functions are not devised to measure, but which nevertheless may affect the scores. It was further suggested that this absence of correlation might also be accounted for by the specific nature of the movements commonly involved in the tests, and that when such relatively simple and specific movements were combined into the more complex operations employed in assembling work these operations might exhibit a much closer relationship than do their simpler components.

In the present report it is impossible to do more than describe, briefly, the general lines along which work has been carried out in the light of these suggestions and to indicate the main conclusions.

## B. The Main Problems.

The following are the main problems with regard to which the experiments have been planned.

1. *The problem of Accuracy.* How far does each of the tests employed in the research afford a reliable measure of 'ability'?

2. *The nature of the Abilities involved.* How are the abilities measured by the various assembling tests related to one another and to other abilities, in particular to 'general intelligence' and to 'mechanical aptitude'?

3. *The effect of Practice, Improvability.* (i) How far can we predict from the scores made initially at the various assembling tests the ability which an individual may attain after practice? (ii) What is the character of the practice curve? (iii) How far does practice at one operation influence ability at another?

4. *The nature of the mental processes involved in Assembling.* (i) What are the processes operative at first? (ii) How do these change as practice continues?

## C. General Plan.

The assembly operations were divided into two classes, viz. (i) operations in which the subject is first required to *think out how* to put together the parts of the object he is called upon to assemble, and (ii) operations in which the mode of assembling is *already known* and where therefore the subject has only to perform the actual assembling. These have been termed, provisionally, 'intelligent assembling,' and 'routine assembling,' respectively.

The principal tests employed have been tests of these two types of assembling, together with tests of 'general intelligence' and tests of 'mechanical aptitude.' Spearman's 'tetrad-difference' criterion has been employed to determine the presence of factors involved in these different groups of tests.

The tests have been chosen so as to include a variety of movements, both in type and in complexity; and the subjects include an 'adult' group of men and women, an elementary school-boy group, and an elementary school-girl group. An investigation of 'age' and 'sex' differences is thus made possible. A 'backward' class of school girls has also been tested (for details see later).

To investigate the influence of early practice on 'reliability,' each of the routine tests was repeated several times (usually five—each 'test' itself consisting of, usually, ten repetitions of the operation to be tested), and the intercorrelations of these trials were calculated.

To investigate the effects of more prolonged practice, the subjects were then divided into (*a*) two (or, in the case of the adult subjects, four) practising groups, who practised certain of the routine operations daily, and (2) a control group who 'rested' while this practice was in progress. On the termination of practice both 'practisers' and 'controls' were re-tested on all routine tests. The practice period consisted of a fortnight's daily practice with the omission of Saturdays and Sundays in the case of adults, and of a daily practice on the five school days of the week in the case of school boys.

The resulting data offer the opportunity of (i) tracing the 'reliability' of each test through the various stages of practice, (ii) investigating how the relations between the several tests practised by the same group may change with practice, (iii) ascertaining how the rate of improvement, as shown by the practice curves, may vary from one subject to another and from one test to another, and (iv) determining how far improvement in one test may 'carry over' to another test.

In the output records of the subject throughout his practice period there is a more comprehensive measure of 'ability' than a simple sitting is likely to yield. The scores obtained at the mental tests ('general intelligence' and 'mechanical aptitude') render possible a comparison with 'ability' and 'improvability' at the assembling tests, and give the means of ascertaining how far these tests are dependent on mental factors and how far on more purely 'motor' factors.

To throw light on the mental processes involved in assembling work, introspections were made by the adult subjects who were supplied with a list of points upon which information was especially sought. The tests were also practised by the writer.

The adult subjects undertook the tests voluntarily and no special incentive was offered. The general conditions at the schools were such as to lead us to believe that every effort was being made by the school children to do their best at the tests. Nevertheless, in case interest might flag during the practice period a monetary incentive was given, based on (i) the subject's total score, (ii) the number of times the subject beat his own best previous record, and (iii) the number of times his section beat a rival section into which the practising group was divided. The addition of a monetary and competitive incentive to those already operative establishes conditions approximating to those obtaining in industrial work.

## D. Data Collected.

*Tests*: Briefly enumerated, they were:

(1) *Intelligent Assembling.*

(*a*) *Porcelain Test.* In this the subject was required to assemble, without previous knowledge, the various parts attached to the interior porcelain portion of an ordinary electric lampholder.

(*b*) *Container Test.* Here the metal exterior into which the above-mentioned porcelain fits was required to be assembled without previous knowledge.

(*c*) *Wiring Test.* This was done after the subject had learnt how to assemble the above-mentioned parts. In it he was required to 'wire up' the electric lampholder—i.e. attach it properly to the end of a wire, the other end of which was inaccessible.

(2) *Routine Assembling.* These tests employed the various parts of the lamp-holder as follows—

(*a*) *Screw Test.* The insertion of ten small screws into ten metallic blocks, with the fingers, constituted one 'trial.' A 'test' consisted of five such trials.

(*b*) *Porcelain Test.* Assembling the parts of the porcelain interior (*cf.* 1*a*) of the lampholder—repeated five times with adults, fifteen times with school groups.

(*c*) *Container Test.* Assembling the parts of the container (*cf.* 1*b*)—repeated five times with adults, fifty with children.

(*d*) *Wedges Test.* Assembling the two wooden wedges into the top of the lamp holder—repeated five times with adults, fifty times in groups of ten, with school children.

(*e*) *Wiring Test.* Wiring up the lampholder (*cf.* 1*c*)—repeated five times with adults, fifty with children.

(*f–j*) *Stripping Tests.* These employed the same material as the five routine assembling tests; but here the subject was required to take apart the pieces previously assembled, under similar and standard conditions as for assembling.

(3) *General Intelligence.* All subjects took a comprehensive test of general intelligence of one hour's duration, a different test being used for adults and for school children respectively.

(4) *Mechanical Aptitude.* The school groups took two 'mechanical' tests, viz. (*a*) the 'models' type, and (*b*) the 'mechanical explanation' type. A 'star' puzzle,

in which a star shaped piece of metal had to be disengaged from a pair of 'horse-shoes,' was also introduced in the boys' groups.

(5) *School Examination Records* were obtained for the boys and girls. In the latter case it was possible to divide these into (*a*) ability at English, (*b*) ability at other classroom subjects, and (*c*) ability at handwork.

*Subjects :*

(*a*) *An adult group* numbering forty-seven subjects drawn from members of the staff of the National Institute of Industrial Psychology and of the City of London College, and from senior students at the College. Thirty of these, divided into four groups, were 'practising' subjects, taking first all the tests, then practising one of the routine tests (both the 'assembling' and the 'stripping' variety) which differed with the group, and finally being re-tested on all of the routine assembling tests. The practice was carried out daily under test conditions for a period of a fortnight, omitting Saturdays and Sundays. The remaining seventeen acted as 'controls,' taking initial and final test only.

(*b*) *A schoolboy group* consisting of the top two classes of a Tottenham elementary school, numbering seventy boys. Thirty-eight of these were 'practisers,' divided into two groups of eighteen and twenty respectively. Each group practised two of the assembling tests (both 'assembling' and 'stripping' in each) daily for five consecutive school days ; they were given all the tests before practice and were re-tested on the unpractised routine tests after the practice. The remaining thirty-two acted as controls.

(*c*) *Schoolgirl* groups drawn from the top two classes of a London elementary school and from a backward class of girls of similar age in the same school. These took the initial tests only. They total fifty-nine 'normals' and twenty-two 'backwards.'

## E. Chief Conclusions.

The following conclusions emerge clearly from a preliminary examination of the results of the investigation.

I. *Relating to accuracy of measurement in Routine Assembling.*

(*a*) A single 'trial,' i.e. the assembling of a single object, such as one 'container' or one 'porcelain,' affords *some* indication of 'ability,' as shown by its correlation with other single trials at the same operation. Its 'reliability,' however, although tending to be 'significant' (about thrice its probable error) is so low (about ·3) as to render it *entirely untrustworthy as a measure.* When the scores made at several trials are added together a much more reliable measure is obtained. Thus on adding together the five trials made by adults at the same routine test ('porcelains,' 'containers,' 'wiring'), the reliability rises to over .70. Similarly, the 'reliability' of ten trials at the screwing test is, for screwing in, .63 and for unscrewing, .66 ; and that of the 'wedges' rises from .18 for a single trial to .52 for the sum of five trials.

(*b*) If the measure must be confined to a single trial, it is much more reliable to choose the best, or next best, or third best, &c., trial, than to choose the first, or second, or third, &c., trial. *The best, next best, &c., trials are almost as reliable as the sum of all five trials, and there is little to choose between them on this score.*

(*c*) It follows from the foregoing observation that the disturbing influence of random errors on the reliability of a short test of the kind here referred to is greater than that exerted by systematic factors, such as practice or fatigue incurred during the sitting—and this in spite of the fact that such factors were discernible in the curves.

(*d*) The *reliability* of these routine tests *depends upon the number of repetitions* included within the measure *rather than on the length of time required for each repetition.* Thus the 'reliability' of a single trial at the 'porcelain' test, occupying several minutes, is lower than that of the sum of ten trials at the 'screw' test, occupying a few seconds. When, however, we take as our measure the sum of ten trials at porcelain assembling the reliability rises to a somewhat higher figure than that of the screws (.86 as against .63)—and similarly for the other more complex assembling tests.

(*e*) If we include the same number of repetitions in our measures of 'ability,' the routine assembling tests possess much the same degree of 'reliability' when employed with adults as with children.

(*f*) *The more prolonged period of practice has no clearly marked influence on* '*reliability*'—the coefficient differing little from day to day during the practice period.

II. *Relating to accuracy of measurement in intelligent assembling.*

(*a*) The average inter-correlation of the three 'intelligent' assembling tests is .33 for the boys and .38 for the girls.

(*b*) The two tests of 'mechanical aptitude' correlate .72 (boys) and .59 (girls) with one another, and yield correlation coefficients of similar magnitude when compared with themselves by determining the correlation between the pool of 'odd' sub-tests with that of 'even' sub-tests. It is shown later that these involve the same 'mechanical' factor as that in the intelligent assembling tests.

III. *Relating to Influence of Practice on Ability.*

(*a*) The general influence of practice is to draw individuals closer together with respect to 'ability.'

(*b*) *There is a well-marked tendency for individuals to maintain, during practice, the rank order with which they begin.*

(*c*) Generally speaking, those weaker at assembling (routine) exhibit greater variability from day to day, i.e. their practice curves are less smooth.

(*d*) They also effect more improvement during the period of practice, whether this be measured 'absolutely' or in relation to their 'ability.'

(*e*) There is a small positive correlation between 'general intelligence' and 'ability.'

(*f*) *The correlation between 'general intelligence' and 'improvability' is, if anything, negative.* This does not mean that, given equal initial ability, those who are less intelligent will tend, on this account, to improve more with practice, but that those who initially rank low at assembling have an easier task to effect a given amount of improvement.

(*g*) *It follows from the foregoing observations that the scores made initially at a routine assembling test provide a better criterion as to the ability to which an individual may subsequently attain, through practice, than do either his 'general intelligence' or his 'improvability.'*

IV. *Relating to the Factors involved in 'Intelligent Assembling.'*

(*a*) The various tests of 'intelligent' assembling correlate (i) with one another, (ii) with tests of 'mechanical aptitude' and (iii) with 'general intelligence.'

(*b*) The application of Spearman's 'tetrad-difference' criterion to these inter-correlations denotes the presence of (i) a small factor running through all of these tests, and (ii) a group factor common to the 'intelligent' assembling and 'mechanical aptitude' tests.

(*c*) The inter-correlations indicate no new 'mechanical' factor peculiar to the (practical) 'intelligent' assembling tests, over and above the 'mechanical' factor in the 'mechanical aptitude' tests.

(*d*) The best tests of this mechanical factor are the more difficult assembling tests and the mechanical aptitude tests. The simplest assembling tests provide little, if any, measure of the mechanical factor. This is probably due to the facts (i) that in such simple tests the scores of those who possess good mechanical ability is almost wholly determined by the time taken to effect the practical (motor) work of assembling rather than that taken to think out *how* to assemble, and (ii) that such thinking out as occurs is based on previous experience rather than on original thought.

V. *Relating to the factors involved in 'Routine Assembling.'*

(*a*) There is a general positive correlation running through all the routine assembling tests and the tests of general intelligence.

(*b*) The application of the 'tetrad-difference' criterion indicates the presence of a group factor common to the routine assembling tests.

(*c*) The more difficult motor processes involved in 'assembling' appear more highly saturated with this factor than do the easier 'stripping' processes.

VI. *Relating to Transfer of Practice Effects.*

(*a*) There is little evidence of transfer of practice effect from one routine operation to another.

(*b*) The mere acquisition of speed in effecting a series of movements, as involved in this practice, must, however, be distinguished from the kind of 'training' which might be given by a competent psychologist, after having practised the operations himself. Such training would involve—(i) the determination of the best method of arranging the parts to be assembled, (ii) the determination of the best order in which to assemble the parts, (iii) the elimination of needless movements, (iv) the avoidance of awkward movements, (v) the determination of 'accident' points, i.e. points at which it is imperative to 'slow down' if error is to be avoided, and (vi) instruction in the principles involved in the foregoing points.

### F. Work in Progress.

Work in progress includes—

(1) An examination of the relation between the abilities measured in the one group of 'backward' school girls.

(2) An analysis of the processes involved in 'intelligent' and in 'routine' assembling.

### G. Further Work Suggested.

The results so far obtained suggest further work in the direction of—

(i) Investigating the relation between the most complex assembling operations and the simpler 'motor' tests, such as those referred to in the Report on Manual Dexterity recently issued by the National Institute of Industrial Psychology.

(ii) Investigating the effect of training, as suggested in E.VI (*b*), on the ultimate ranking of a group of subjects previously graded by assembly tests.

The Committee ask for a renewal of the grant.

*September* 1930.

## **Mycorrhiza in Relation to Forestry.**—*Report of Committee* (Mr. F. T. Brooks, *Chairman*; Dr. M. C. Rayner, *Secretary*; Dr. H. M. Steven). *Drawn up by the Secretary.*

The grant was asked for as a contribution towards expenditure incurred in :

A. The collection of data and material from forestry stations in various parts of the British Isles.

B. The planning and starting of field experiments in a selected area, supplemented and extended by pot cultures and laboratory observations and experiments.

It must be understood that any scheme of research likely to yield results of value in this subject must be planned to cover a period of several years, for which reason the present report necessarily takes the form of a general account of the scheme of work with details of those parts of it actually under way, rather than a description of results already achieved.

The main purposes of the research may be summarised as follows :—

(1) To ascertain whether the formation of normal mycorrhiza may fairly be regarded as a causative factor in the healthy growth of young trees. It may be assumed as already established that it is an invariable concomitant of such growth.

(2) To determine whether the absence of mycorrhiza associated with unsatisfactory growth is related to :—

(*a*) absence of the mycorrhiza-forming fungi appropriate to the trees ; or,

(*b*) the existence of soil conditions inimical to the formation of functional mycorrhiza of a type beneficial to the trees.

(3) To ascertain whether it is practicable to supply deficiencies or ameliorate existing soil conditions by experimental treatment of nursery stock or in other ways.

At this stage the enquiry has been limited to members of the genus *Pinus* in common use for afforestation in Great Britain and the Dominions ; in particular, Corsican Pine (*P. laricio*), Maritime Pine (*P. pinaster*) and *P. contorta*, with co-ordinated observations on Scots Pine (*P. silvestris*) and Mountain Pine (*P. uncinata*). In the three species first named, no data are at present available with respect to the structural types of mycorrhiza normal to the trees in their natural habitats, or the fungi responsible for its appearance in each case. Incidentally, the formation of a type collection of mycorrhizas from the native habitats of these and other species of *Pinus* for purposes of reference is urgently required as a preliminary step in research on the subject, and would undoubtedly be of practical service to foresters both in this country and the Dominions.

The work carried out since allocation of the grant is as follows :—

(A) Visits have been made to a number of forestry stations and experimental plots in Great Britain and Ireland, and comparative observations made and filed on the types of mycorrhiza formed by species of *Pinus* under various soil conditions.

Incidentally, reports have also been filed describing the condition of the roots of seedlings of the same and other species growing in the nursery attached to the Imperial Institute of Forestry and of material of *P. canariensis* sent from Teneriffe by an officer of the Australian Forestry Service.

(B) An intensive study of a newly afforested area has been undertaken by means of field experimental plots supplemented by pot experiments with transported soil. The area selected is on the Wareham plantation of the Forestry Commission, where considerable difficulty has been experienced in establishing *Pinus* sp. from seed. Here, on an area of one-tenth of an acre fenced and surrounded by a fire screen, experimental plots have been sown and subjected to various treatments: e.g. cultivation, application of manures, and inoculations with humus from various sources. Considerable time and labour have been expended in obtaining what is regarded as suitable samples of humus for soil inoculations, and these experiments have been checked and supplemented by pot cultures. For comparative purpose a series of control plots has been treated similarly at a forestry station in the north of England on peaty soil of a different type from that at Wareham.

From the nature of the case, these experiments cannot yield results capable of interpretation for several years. In the meantime, observations have been made on the reaction of seedlings in pot culture to soil from the Wareham area and data collected bearing on the presence of a factor toxic to vegetation.

(C) Marked differences are known to exist between geographical races of *P. silvestris*. Attention having been drawn to this matter by comparative observations on the mycorrhiza of such forms, sowings have been made of seven geographical races of *P. silvestris* in three different localities. Observations on the mycorrhiza-forming capacities of the resulting plants, may, it is thought, provide data throwing light on the determining influence of soil conditions on mycorrhiza formation in general.

One conclusion of a positive kind may be already placed on record. It is clear that certain exotic Pines, e.g. *P. laricio* and *P. pinaster,* find in this country fungi capable of forming mycorrhizal associations of a favourable type. Whether these are identical with those formed by the trees in their native habitats or are representative of all the structural types formed under these conditions has yet to be learned.

The present report offers an opportunity to acknowledge the unfailing help and co-operation received from officers of the Forestry Commission in this country and from the Director of Forestry in the Irish Free State, without which this work could not profitably be undertaken.

Experiments with transported soil have been rendered possible by the exceptional facilities possessed by the Department of Botany, Bedford College, University of London, without which such work would be impossible in London. Acknowledgment for the use of these facilities is due to the Council and Principal and to the Head of the Department of Botany.

For the prosecution of these researches, funds are required :—

(*a*) To carry on the work already undertaken and extend its scope.

(*b*) To provide a small shelter house of a type different from the ordinary greenhouse and more suitable for growing pot cultures of Conifers on a larger scale.

(*c*) To cover expenses relating to the collection of data respecting the types of mycorrhiza formed by various exotic species of *Pinus* in their native habitats and thus provide the nucleus of a type collection for purposes of reference.

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**Breeding Experiments on Plants.**—*Report of Committee* (Sir DANIEL HALL, K.C.B., F.R.S., *Chairman*; Mr. E. M. MARSDEN-JONES, *Secretary*; Dr. K. B. BLACKBURN, Prof. R. R. GATES, Dr. W. B. TURRILL, Mr. A. J. WILMOTT) *appointed to carry out breeding experiments as part of an intensive study of certain species of the British Flora.*

RESEARCH aided by the grant of £50 made at the 1928 meeting of the Association, permission to use the unexpended balance of £32 2*s*. 6*d*. having been granted at the 1929 meeting.

This report is a continuation of that published in the Report of the Brit. Assoc. 1929, p. 267.

*Silene.*—The year's work included the crossing of a large number of newly obtained varieties of *Silene vulgaris* and *S. maritima* and the scoring of the $F_1$ generations. Parts three and four of the series of papers on this genus have been published in the *Kew Bulletin* 1929, pp. 145, 197, and the fifth part is nearly completed for press. Many hundreds of plants are being grown in connection with these studies and extensive field work is being continued.

*Centaurea.*—The scoring and describing of the large generations referred to in the last report was completed, with a few exceptions.

*Saxifraga.*—The $F_3$ generation of the tetraploid *S. potternensis* has been scored and the papers on the genetics and cytology of the three generations has been accepted for publication in the *Journal of Genetics.* $F_1$ generations of numerous back-crosses have been successfully made and scored.

*Ranunculus.*—Two papers on the inheritance of sex and colour have been published in the *Journal of Genetics* xxi (1929). The discovery of more sex forms in the wild and the making of more $F_2$ generations will, it is expected, lead to the need of extending research on this genus.

*Anthyllis.*—Additional generations from selfings have been scored.

*Finance.*

The balance of £32 2*s.* 6*d.* has been spent on wages for labour and sundries connected with breeding work on the genera mentioned above. Vouchers for this amount have been forwarded.

## **Transplant Experiments.**—*Report of Committee* (Dr. A. W. Hill, *Chairman*; Dr. W. B. Turrill, *Secretary*; Prof. F. W. Oliver, Prof. E. J. Salisbury, Prof. A. G. Tansley).

The transplant experiments initiated in 1927 by the British Ecological Society are being carried out at Potterne, near Devizes, Wilts, in the grounds of Mr. E. M. Marsden-Jones, with assistance from Kew. Five species of British plants are being grown in five different soils at Potterne and also in ordinary garden soil at Kew. Every species is represented by 25 plants of known origin and similar genetical constitution in every one of the soils. Four large enclosures, 35 feet long, 10 feet broad and 3 feet deep, were erected to contain the imported soils, sand, calcareous sand, clay, and calcareous clay. The species already transplanted are: *Centaurea nemoralis* Jord., *Silene vulgaris* Garcke, *S. maritima* L., *Anthyllis vulneraria* L., and *Plantago major* L.—Material of *Fragaria vesca* L. is being prepared for use as the sixth species. Soil analyses have been made by Mr. C. G. T. Morison, M.A., of the School of Rural Economy, Oxford. Meteorological records have been kept since April.

A full report of the results obtained up-to-date has been prepared by Mr. E. M. Marsden-Jones and Dr. W. B. Turrill, and has been presented to the Transplant Committee, and accepted for publication in the *Journal of Ecology* for August, 1930. The results of periodic records for the five species transplanted for the 5 to 18 months the plants have been in the beds are embodied in the report. The chief facts emerging to date are:—

The most obvious changes are taking place in *Silene vulgaris*, *S. maritima*, and *Plantago major.*

*Centaurea nemoralis* has shown little change, but the general tone is better on the clays than on the sands, though flowering commenced first on the latter. The mean number of stems per plant was higher on the clays than on the sands. The main seedling germination was in the spring.

In *Anthyllis vulneraria* morphological changes of a qualitative nature have not occurred, but interesting facts regarding selection are apparent from the data obtained. The species is known to be calcicolous, and this is in agreement with the high death rate on the sand and on Potterne soil. It is, however, certain that absence of lime is not the sole factor involved. The death rate at Kew, following frosts, was very much higher than on the clay (or other soils) at Potterne, though the $CaCo_3$ content is lower in the clay than in the Kew soil. Attack by wilt-causing fungi (*Verticillium*), blocking the vascular bundles of the root is often an immediate cause of death. Secondary flowering, stem heights and heads per stem, and seed germination showed the same sequence as tone and deaths. Edaphic factors are obviously important in causing the known natural limitation of this species and should be considered when it is proposed to cultivate *Anthyllis* as a forage plant. The Transplant results suggest that on suitable land it would be a valuable and relatively persistent crop.

*Plantago major* has proved, even within five months, exceedingly plastic. The original plant was a dwarf form and this habit has been very nearly retained on the sand and to a less degree on the calcareous sand. The measurements and figures given show the marked deviation in luxuriant growth which has occurred on the clay and amongst the Potterne reserves, and somewhat less on the chalky clay. Further developments in this species will be watched with interest.

*Silene vulgaris* on the calcareous sand is developing a marked 'strict' habit, similar to that which has been many times observed in individuals among wild populations. It is just possible that this change may prove to have a genetical basis. On the calcareous sand the foliage developed a lighter green colour, on the clay a more yellowish-green colour, and on the chalky clay, a more blue-green colour than in the parent. Secondary growth occurred especially on the sand. General tone was best on the clay and worst on the calcareous sand. In pure *S. vulgaris* germination was restricted to the spring on all the soils and was best on the clay and worst on the calcareous sand and chalky clay.

In *S. maritima*, at the last record, there was a marked though irregular tendency for the plants on the sand to change to plants with smaller leaves and with more anthocyanin and a flatter habit than in the parent. On the calcareous sands the leaves were narrower and smaller, the plants were flattened, and the calyces had much more red anthocyanin than in the parent. On both clays there was little change from the parent. General tone at the last record was best on the chalky clay and worst on the clay (these records were made before the September drought). Seeds of *S. maritima* have marked autumn and spring discontinuous germination on all the soils. Spring seedlings were most numerous on the clay and least numerous on the calcareous sand. Autumn seedlings were most numerous on the chalky clay and least numerous on the clay. Autumn seedlings survived the 1928–29 winter only on the chalky clay (and at Kew).

To sum up: *Centaurea nemoralis* does not at present appear to be plastic, but is capable of survival under a wide range of edaphic conditions. *Silene vulgaris* is slowly plastic under certain edaphic conditions. *S. maritima* is decidedly more plastic than its congener. *Anthyllis vulneraria* is not plastic and is not capable of survival under a wide range of edaphic conditions. *Plantago major* is exceedingly plastic.

It is obvious that the experiments, if they are to yield a maximum of conclusive facts, will have to be continued for a long period of years. It is important to remember that the making of soils from raw materials can be slowly followed in the beds. It is hoped that periodic analyses, for which arrangements have been made, will yield useful pedological data.

In *Centaurea nemoralis*, *Silene vulgaris*, *S. maritima*, and *Anthyllis vulneraria* extensive genetical research is being continued at Potterne and Kew, and this involves the use of lines from which the Transplant materials originated. All experimental work is being correlated with field, laboratory and herbarium studies.

Apart from actual changes in the plants and from stages in soil-making, many interesting biological facts are being observed and some are embodied in this report. The plants are grown in the absence of competition, and differences between all or some of the plants on the different soils must, on the whole, be due to edaphic factors. It is, however, necessary to emphasise that plants have an individuality, that the history of a given individual is never exactly like that of any other individual, that 'accidents' happen to individuals, and that, therefore, records have usually to be of a statistical nature with the limitations of this method.

Kew, Dec. 24th, 1929.

ARTHUR W. HILL,
*Chairman.*
W. B. TURRILL,
*Secretary.*

## **Chemical Analysis of Upland Bog Waters.**—*Report of Committee* (Prof. J. H. PRIESTLEY, *Chairman*; Mr. A. MALINS SMITH, *Secretary*; Dr. B. M. GRIFFITHS, Dr. E. K. RIDEAL).

THE figures of the complete analyses of the water of Miles Rough bog, Bradford, taken in April and June, 1928, showed no significant differences between inlet and outlet except in nitrate content. The complete analysis of the water of Heather Glen bog, near Bingley, taken in May, 1929, gave a similar result. No further water analyses could be made in 1929 owing to the drought.

As the results of the previous complete analyses had pointed to nitrate, as the significant variable, the analyses of 1930 were confined to this constituent, except that in April figures for Calcium were also obtained, as it was deemed of possible significance. The results of two sets of analyses, taken in April and June respectively, point to the importance of nitrate supply in algal growth. In April the freely inflowing water is charged with considerable amounts of nitrate, and this is the time of the greatest growth of algæ. In June the incoming supply is cut off and the nitrate of the water standing in the bog is reduced to a very low figure. This is followed by the lowest ebb of algal life. The general indications, therefore, are that nitrate is the limiting factor in the growth of algæ in this situation. Details will be given in a separate communication.

The grant is now expended, and as the two most significant periods only of the algal year have been investigated, it would be useful to supplement previous work by nitrate analyses in October and February. It is, therefore, requested that the Committee be reappointed with a grant of ten pounds.

**Formal Training.**—*Final Report*[1] *of Committee* (Dr. C. W. Kimmins, *Chairman*; Mr. H. E. M. Icely, *Secretary*; Prof. R. L. Archer, Prof. Cyril Burt, Prof. F. A. Cavenagh, Miss E. R. Conway, Sir Richard Gregory, Prof. T. P. Nunn, Prof. T. H. Pear, Prof. Godfrey Thomson and Prof. C. W. Valentine) *appointed to consider the Bearing on School Work of recent views on Formal Training.*

The object of the Committee is to prepare an authoritative statement as to the disciplinary value of various elements in the curricula of the schools, about which much confusion of thought exists.

It is evident that, in the light of modern research, the extravagant claims made in the past for the unique value of certain subjects from the point of view of mental discipline, apart from their intrinsic value in the scheme of education, cannot be sustained.

In many schools an erroneous and extreme doctrine of Formal Training is still resulting in much wasted time and effort. An excessive amount of the time-table is frequently devoted to subjects of relatively little importance with the main object of securing that mental discipline for the production of which they are imagined to possess special qualifications.

The following special papers have been prepared which have been approved by the Committee:—

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|---|---|
| 'Formal Training: the Psychological Aspect' ... | Prof. Cyril Burt. |
| 'Some practical Applications' ... ... ... | Prof. F. A. Cavenagh. |
| 'Latin and Formal Training' ... ... ... | Prof. R. L. Archer. |
| 'General and Special Training in the Application of Skill' ... ... ... ... ... ... | Prof. T. H. Pear. |

If a reliable statement can be formulated of the disciplinary value of certain subjects in the curriculum and the conditions in which, by improved methods of teaching, this can to a limited extent be secured, it is impossible to over-estimate the influence it should have on educational procedure. The purpose of this report is to place the subject of Formal Training in its true perspective.

## Formal Training: The Psychological Aspect.

By Prof. Cyril Burt, M.A., D.Sc.

The traditional view, known as the doctrine of 'mental discipline' or 'formal training,' assumes that the effects of mental exercise are general. It maintains that, by practising a mental capacity on some particular subject, we strengthen that capacity as a whole, and so improve its efficacy for any subject on which it may be employed in future. Thus, it has been claimed that the teaching of mathematics trains the powers of reasoning, so that the child becomes more logical, not only in dealing with other branches of the curriculum, but also in dealing with the problems of everyday life.

In the past this doctrine has been widely held among teachers and educationists; but during the last twenty years it has been severely criticised on the basis both of general theoretical principles and of experimental results.

The theoretical objections run briefly as follows: Mental processes, (those of memory, for example) do not depend upon simple capacities —'faculties' lodged in some phrenological organ of the brain; and, even

[1] This report repeats, with emendation, the interim report printed last year.

if they did, we should have to conceive those capacities as already determined at birth and unalterable in after-life. Rather, it is said, the processes of the mind consist essentially in specific associations between definite situations and definite responses, these associations being due to the formation of particular nerve-paths within the brain. Clearly, the formation of one set of nerve-paths cannot influence the formation of another set of nerve-paths, unless they themselves are linked by similar associations.[2]

The experimental studies have been numerous. They have dealt chiefly with such mental functions as memory, discrimination and manual dexterity, and with such educational subjects as arithmetic, grammar, geometry, science and Latin.[3] Two restrictions should be noted. First, in the experiments which endeavoured to isolate elementary psychological functions, it is important to realise that the investigators deliberately simplified the situation, and eliminated, so far as possible, emotional factors like interest or ambition: hence it is not always fair to apply direct to classroom conditions results obtained in the psychological laboratory. Secondly, the experiments on educational subjects have dealt almost exclusively with the influence of one school study upon another: they have not attempted to evaluate the effect of such studies upon the learner's interest, enjoyment and efficiency in after-life.

The position reached may be stated thus: No psychologist would doubt that under certain circumstances something very much like transfer of improvement undoubtedly takes place. Accordingly psychologists are now concerned rather to criticise the popular explanations of such transfer, and to deny that it occurs so freely and so widely as has previously been assumed. Hence in the most recent investigations the object has been, not so much to discover whether there is any such thing as a transfer of

[2] Compare Thorndike, *Educational Psychology*, vol. ii, pp. 359 and 418. Here I am concerned only to state the argument in its most definite form. Probably few psychologists in this country would accept the extreme associationist position as stated in the text. English psychology now teaches us that, if situation S is associated with reaction R, then, when a similar situation occurs in the form of $S_2$, it calls up not R but $R_2$, where $R_2 : R :: S_2 : S$. Thus, if the first bar of 'God Save the King,' heard in the key of B flat, becomes associated with the second bar in the same key, then, if a week later I hear the first bar in the key of C natural, I call up, not the original continuation in the original key, but an analogous continuation unconsciously transposed to the new key. This process, variously known as 'relative suggestion' or the 'eduction of correlates,' already implies a kind of transfer of training in the very process of association itself.

[3] For a recent summary of these experiments, see Whipple, *Twenty-seventh Year Book of the National Society for the Study of Education*, Part II, pp. 186–197; Sandiford *Educational Psychology*, pp. 279–289. Thorndike's elaborate investigations ('Mental Discipline in High School Studies' and 'A Second Study of Discipline in High School Studies,' *Journ. Educ. Psychol.*, xv. 1924 and xviii, 1927) deserve special mention as among the most elaborate and the most recent. His conclusion may be quoted: 'By any reasonable interpretation of the results, the intellectual values of studies should be determined largely by the special information, habits, interests, attitudes and ideals which they demonstrably produce. The expectation of any large difference in general improvement of the mind from one study rather than another seems doomed to disappointment. The chief reason why good thinkers seem superficially to have been made such by having taken certain school studies, is that good thinkers have taken such studies, becoming better by the inherent tendency of the good to gain more than the poor from any study.'

improvement, but rather to discover what are the factors and conditions that mediate or promote it.

The current view can be summed up as follows: Transfer of improvement occurs only when there are *common usable elements*, shared both by the activity used for the training and also by the activity in which the results of that training reappear. The more the influenced and the influencing activities resemble one another, the greater the influence is likely to be. Practice in subtraction will improve accuracy in division, because the latter involves the former, but it may have little or no effect on accuracy in multiplication. The study of Latin will aid the study of French, because many French words are derived from Latin roots, and because many of the methods of work used in learning Latin—*e.g.* the use of a dictionary—will also be required in learning French.

The 'common elements' may be elements of (i) material, (ii) method, (iii) ideal; they are most 'usable' when they are conscious.

In the laboratory experiments, it would seem that these common usable elements consist in a partial identity of material rather than of mental function. The fact that items of information, acquired during the training, can be usefully applied again in the subsequent tests is quite likely to produce an improvement in those tests as a result of the preceding training. On the other hand, the fact that the functions employed in both training and test are popularly called by the same name—'imagination,' 'observation,' 'memory,' or the like—is no guarantee that general improvement will be secured.

In the more concrete experiments dealing with school studies, it seems clear that the common usable elements may arise not only from partial identity of material, but also from a partial identity of method or procedure, and sometimes from a partial identity of ideal and aim. Hence it appears probable that in the schoolroom the most important agencies in transfer are such things as generalised attitudes and interests, generalised modes of attacking mental problems, generalised schemes of thinking, useful moral habits and serviceable maxims of logic.

These conclusions have been succinctly expressed by Prof. Godfrey Thomson and Professor Nunn: 'Transfer of training appears, to put it cautiously, to be much less certain and of much narrower spread than once was believed. Subjects of instruction will not therefore be included in the curriculum lightheartedly on the formal "discipline of the mind" argument. Other things being reasonably equal, useful subjects will have the preference.'[4] 'We conclude that the training produced by an occupation or a study consists primarily in a facility in applying certain ideas and methods to situations of a certain kind, and in a strong tendency to bring the same ideas and methods to bear upon any situations akin to these.'[5]

The influence of conscious recognition has been made amply clear by recent experimental work. Here lies a principle which is of special interest to the teacher. A common element is more likely to be usable if the learner becomes clearly conscious of its nature and of its general applicability: active or deliberate transfer is far more effective and

[4] Thomson, *Instinct, Intelligence and Character* (1924), pp. 144–5.
[5] Nunn, *Education: its Data and First Principles* (1920), pp. 210–211.

frequent than passive, automatic, or unintentional transfer. This seems especially true where the common element is an element of method rather than of material, an ideal rather than a piece of information. Accordingly, when practice in reasoning about physical sciences improves the child's power to reason in biological sciences, this occurs not because his reasoning faculty as such has been strengthened, but because the habits and general notions of procedure which he has learnt in the first field are again consciously brought into play in the second field. Merely to practise a child in accuracy of scientific reasoning by quietly correcting his errors and merely repeating the exercises will not of itself produce any generalised power of reasoning logically ; but if incidentally the child is encouraged to form an ideal of accuracy in reasoning, and to study its implications, he may try to live up to that ideal in every department of life.

It follows, therefore, that what chiefly assists the spread of training is not the mere perception of facts, but the perception of relations between facts : and this is something more than mere mechanical association. 'There comes first an unconscious employment of certain principles or ideals. These gradually become clearer and more definitely outlined. They are recognised by their owner and named, and thereby gain tremendously in effectiveness and in transfer-power. This recognition must, however, await the slow growth of the idea to be recognised. The teacher cannot put the words into the pupil's mouth—or rather, unfortunately, he can do so, but if he does it too early he will give mere words. . . . In general, the rule appears to be that any teaching which makes the pupil more conscious of how successful results are obtained is likely to assist transfer.' [6]

The practical corollary is obvious. Teachers should arrange the work of their pupils and their own mode of teaching so as to lead their pupils to recognise clearly the methods by which efficient work is done. Further, it follows that the intelligent child, who can perceive relations spontaneously, who can generalise his methods and re-apply them on his own initiative, is likely to show a wider transfer than the dull child. With the dull the teacher can hope to do little more than implant specific memories and specific habits that will be definitely useful in and for themselves, and, so far as possible, impress upon the child how these memories and these habits may subsequently be applied.

*Note.*—Sir Percy Nunn and Prof. Godfrey Thomson add the following statement :—

'We are in agreement with Professor Cyril Burt's contribution to this Report, which, in fact, incorporates the views we have expressed independently in the books which he has cited.

## Some Practical Applications.

By Prof. F. A. Cavenagh, M.A.

The educational implications of the foregoing paper are clear. We can no longer retain any school subject solely on the ground that it provides 'mental discipline,' nor should we speak of the 'educative value' *of a subject*. Educative value exists not in the subject *per se*, but in the way

[6] Thomson, *ibid.*, p. 143.

in which it is studied. It consists (to use another favourite expression) in 'learning how to think,' in forming interests or sentiments about a subject, and in building up such habits as perseverance, independent attack of problems, application of previous knowledge, etc. Any teaching which fails to foster such mental processes is uneducative, however much information it may succeed in driving in, and whatever examination results it may gain. Indeed, these considerations provide an additional argument against the dominance of examinations, since they tend to encourage either cramming, which can only induce the habit of further cramming, or spoon-feeding, which will produce habits of mental dependence, credulity, and inertia.

It cannot, however, be denied that a good deal of knowledge has to be acquired at school, either for the needs of life or as a basis for higher study, which depends largely on rote memory, and which cannot be taught in an 'educative' way. That is inevitable. But we should at least reduce such work to a minimum, and we should take care that everything which admits of intelligent teaching is so presented. Under present conditions teachers often avoid what they know to be the right method because it would take too long, and because the use of it would prevent them from covering the examination syllabus. After the examination the knowledge frequently vanishes; and as the children have not gained the desirable sentiments and habits their schooling avails them little in after life. It is no exaggeration to say that the modern mania for examination results not only wastes thousands of pounds of public money, but renders many recipients of secondary education less cultured and efficient than they might have been without it.

The same holds at the University level. An Honours graduate, if he is superior to a pass man, is superior not because of his greater knowledge, but because he has had less inducement to cram and more opportunity to get genuinely interested in his subject and to form conscious ideals of method. But a student who comes to the University with all the interest knocked out of him and with no habits of independent study, will certainly not take a high place in an Honours degree; hence the very disappointing record of many who enter with a 'brilliant' school record. And those who deal with post-graduate students must regretfully admit that the lecture-plus-examination methods of the modern university can be no less stultifying. And further, as teachers are largely recruited from the victims of these methods, the evil is perpetuated.

It thus appears that this generalised 'transfer' exists, and that it can cut both ways. If education consists in 'what remains after we have forgotten all we learnt,' it may be no more than a dislike and contempt for any serious mental pursuit, for anything 'high-brow.' On the other hand, it may mean activity of mind and the capacity for finding interest in any task and for constantly increasing the circle of one's interests.

If, as seems true, every subject can be studied in such a way as to create the right habits, then every subject can, on general formal grounds, claim a place in the curriculum. Selection will then depend on the intrinsic value of each subject; and this depends in turn on the proclivities and future needs of individuals. There are no absolute values, and it is waste of time to argue as if there were. While everybody needs a certain humanistic basis, many cannot travel far on this line, but require scientific or technical subjects. The actual subjects are comparatively unimportant; any subject well taught will provide some beneficial 'transfer,' any subject badly taught will do harm.

## The Disciplinary Value of Latin.

By Prof. R. L. Archer, M.A.

The traditional claim of Latin to a large place in the curriculum for its disciplinary value has often suffered from three defects:—

(i) The range of the effect has not been defined. It has been alleged that it produces all-round 'accuracy,' whereas, if 'common usable elements consist in a partial identity of material,' the improvement must be largely confined to the use of language.

(ii) The stage which must be reached before the effect is produced has not been sufficiently defined.

(iii) The importance of the pupil's emotional attitude has been ignored.

If these points be taken into account, we believe that a limited but important claim for Latin can still be substantiated.

(i) Thomas Arnold, unlike many of his contemporaries, limited the effect to the use of language and particularly of the pupil's own language; and he attributed it largely to translation from the vernacular into Latin. Owing to the superior exactitude of Latin, it is claimed, a pupil who is intent on so translating a passage into Latin as to bring out its exact force has a stronger motive for analysing its precise meaning than can be secured by any other device. Such analysis becomes an unconscious habit, and, however dissimilar may be other situations which require an analysis of an English passage, the material (the English language) is the same. Latin prose thus isolates an element which appears in many situations and secures a definite objective in teaching, and it is this which is meant by formal training.

The history of modern languages further suggests that familiarity with Latin literature has affected their style. Sometimes this effect has been bad, *e.g.* when it produced excessive imitation of Cicero; but on the whole it has made for desirable elements in 'form,' such as the absence of exaggeration and emotionalism.

More doubtful is the claim put forward for Latin at its early stages that, as in deciding the form of a Latin verb you have to consider its conjugation, voice, mood, tense and person, and a mistake in any of them vitiates the result, a habit is set up of considering all relevant factors, in deciding *any* issue. One could not affirm that this result never occurs, but, as there is no identity (or even similarity) between the situation in which the habit is acquired and that in which it is hoped that it will operate, it is possible only if such care becomes a conscious ideal with considerable emotional strength.

(ii) Of the three possible effects which have been considered in the last section, only the unsubstantiated third could affect the early stages. The first appears valid, but the benefit begins only about the matriculation stage and applies to the abler pupils; that of the second begins even later. The second is thus for specialists; but the first establishes a claim for

Latin as a part of general education for those who are likely to continue the study to the matriculation stage, if the answer under the next heading is favourable.

(iii) So little was the third point recognised that old-fashioned arguments almost suggested that it was the very uncongeniality of striving to be accurate which constituted the value. If the modern point of view be adopted, it becomes probable that many pupils have not enough of the general factor in ability (which appears to enter into the learning of Latin very considerably) to make sufficient progress to acquire the necessary desire for accuracy and the readiness to make the effort willingly. But this objection does not apply to matriculation candidates taught by an inspiring teacher.

The claim has thus been greatly reduced, but does not altogether disappear. Although much of our reasoning has taken place intuitively and without words before we throw it into language, throwing it into words is usually essential for ourselves and always for conveying the argument to others; and the untrained mind often works out its arguments entirely in verbal terms and is at the mercy of language. Thus, in most subjects, accurate analysis of language is an indispensable factor, though only one factor, in accurate thought. On the other hand, it cannot be maintained that it is only through Latin that such power of analysis can be trained.

*Note.*—The claims of Latin for inclusion in the curriculum on the ground of its intrinsic value as distinct from its value as formal discipline are not considered in this paper. They constitute its strongest claim.

## Relation between General and Special Training in the Acquisition of Skill.

By Prof. T. H. Pear, M.A., B.Sc.

Popular views concerning the relations existing between different skills are varied and conflicting. On the one hand a person is spoken of as 'clever with his hands,' 'good at games,' 'an all-round athlete.' On the other hand, the world's best exponents of any complex skills are usually very careful to be specialists, and there are even good grounds for the fear that the learning of a new complex skill, which superficially resembles a skill in which one is highly proficient, may actually be detrimental to the latter.

It is possible that both these views are justified. If we take the summary given below, as approximately representing the state of our knowledge concerning formal training, this may be illustrated.

No serious student of the subject denies that transfer from one acquisition to another *may* occur. But generally (*a*) the amount of transfer found in experiments is very much less than might have been naïvely supposed and (*b*) such transfer occurs in certain conditions which can be approximately specified. They are :—

(1) When common factors, of matter, of method, or of ways of approach exist, have been analysed out and recognised by the learner.

(2) Where an attitude of liking or disliking, welcoming or fearing, the new task, set up strongly in the one type of learning, has been transferred to the other.

(3) Where a sentiment (*i.e.* a relatively permanent organisation of tendencies to emotion) has been acquired towards the work, or towards some person connected with the work, and is then transferred to the other type of learning.

It becomes clear that if this analysis be approximately correct, any two skills which superficially resemble each other (*e.g.* tennis and badminton, ski-ing and figure-skating), may contain (*a*) similar, even identical, elements which can be transferred bodily, and (*b*) completely antagonistic elements. From this it follows that habits learnt in the one type of skill may transfer 'positively' or 'negatively.' For example, while a figure-skater learns always to lean towards the direction of his turns, a ski-runner may have to lean either towards or away from the direction. An expert skater would find this very difficult to unlearn, while a novice at both sports would find less difficulty. Similarly, the oral learning of two foreign languages in the same year may cause interference.

Very little experimental work appears to have been done upon transfer as it relates to complex skills, *i.e.* 'integrations of well-adapted performances,' or even to the simplest cases of dexterity. But the results of experimental work carried out during the last few years in the Manchester laboratory [7] seem to fit very well into this suggested view of transfer in general. In the experiments, there is an almost spectacular lack of transfer between habits which appeared to be very similar indeed. In some cases there is negative transfer owing to the interference of habits. Where transfer occurs it seems to be in terms of a general mental attitude.

If these results are confirmed by others, it would seem that we can never, on the basis of superficial inspection, believe that, because two skills look similar, acquisition of proficiency in one will transfer to the other. Where it does transfer, it may be as a result of common habits (though in such cases the risk of negative transfer is great), or of common material. Where positive transfer takes place it is more likely to occur through the agency of emotional attitudes, sentiments and ideals. The attitude of analysing movements, of demanding to know the reasons for them, the sentiments and ideals formed in connection with a particular teacher, or his method of regarding a certain skill, are probably the most powerful vehicles of transfer.

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[7] By Dr. C. E. Beeby, Mrs. L. Henshaw, Mr. J. N. Langdon and Miss P. Holman.

**Educational and Documentary Films.**—(Sir RICHARD GREGORY, *Chairman*; Mr. J. L. HOLLAND, *Secretary*; Mr. L. BROOKS, Miss E. R. CONWAY, Mr. J. S. DOW, Mr. G. D. DUNKERLEY, Dr. B. A. KEEN, Dr. C. W. KIMMINS, Mr. R. S. LAMBERT, Mr. A. E. MUNBY, Prof. J. L. MYRES, Mr. G. W. OLIVE, Mr. G. N. POCOCK, Dr. T. SLATER PRICE, Prof. C. SPEARMAN, Dr. H. HAMSHAW THOMAS) *appointed with the following reference:—Educational and Documentary Films: To enquire into the production and distribution thereof, to consider the use and effects of films on pupils of school age and older students, and to co-operate with other bodies which are studying those problems.*

IN view of the exceedingly wide terms of reference assigned to them, the Committee have had to consider whether on the one hand they should commit themselves to enquiries which would take a considerable time and would ultimately cover all aspects of the educational use of the cinematograph, or on the other should confine themselves to a branch of the subject in which immediate practical advantage may be reaped. In deciding to adopt the latter course, the Committee have been influenced by the fact that there are already in existence many reports and other documents dealing with the subject, among which they would specially refer to the Report of the Committee of the Imperial Education Conference of 1923 on the use of the Cinematograph in Education, to the Educational Survey published last year by the Secretariat of the League of Nations and to the monthly International Review of Educational Cinematography published at Rome by the Educational Cinematographic Institute under the auspices of the League. The comparison and classification of this large amount of matter will obviously be one of the first tasks of any far-reaching Committee or Commission which may be set on foot to promote the use of the cinematograph in British educational institutions. Such a representative Commission is now in being in the shape of the Commission on Educational and Cultural Films which has this year been established with the assistance of the Carnegie United Kingdom Trustees, and to that Commission the Committee feel that the task of watching over the development of educational cinematography can safely be left.

The first conclusion at which the Committee of the Imperial Education Conference of 1923 arrived was 'that a strong *prima facie* case has been established in support of the view that the cinematograph can be of real value as an adjunct to present educational methods, that properly used it may be of great assistance by way of illustration, and that it should accordingly be recognised as part of the normal equipment of educational institutions.'

The experience which members of the Committee have had in their several capacities leads them to think that the position of the cinematograph in education is very much where this conclusion left it. The case for its use is still a *prima facie* one. Individuals here and there have explored and, may be, strengthened the case, but it is true to say that the number of schools and other institutions which are experimenting with this instrument is very small, and that the pioneer work is as yet having no general and extended influence upon methods of instruction. It is useless to lay the responsibility for this state of things upon producers, commercial and other, of educational or semi-educational films. In comparison with other educational equipment, films are costly to produce, and in the absence of a widespread demand it is not reasonable to expect that they will be produced and put on the market. Moreover, until the stage of individual experiment is passed and a large number of workers in the realm of education have adopted the instrument and have tested its possibilities in their particular fields of instruction, it is impossible for would-be producers to ascertain the precise nature and extent of the demands to be made upon them.

In comparison with the gramophone and wireless, the cinematograph has made disappointingly little headway as an instrument of education during the last few years, particularly in schools, both Secondary and Elementary, from which, if at all, the main demand must come. The Committee know that teachers are not backward in recommending the provision of new educational aids, nor are Educational Authorities generally backward in supplying them. What appears to be lacking, however, is a knowledge of the technique of the instrument, such as can be readily acquired in the case of the gramophone and of wireless with little expenditure of time and trouble, but which in the case of the cinematograph has to be sought for in highly technical papers and other publications.

The Committee have, therefore, come to the conclusion that since they must limit their aim, and leave it to more representative and more permanent bodies to survey and advise over the whole field of cinematography, they can most usefully direct their efforts to elucidating, with the help of well-qualified experts, three or four outstanding technical questions with which the teacher who desires to make use of cinematography is faced. And since, as it seems to them, the early realisation of the possibilities of cinematography and the creation of an adequate demand for educational films depends upon the introduction of the cinematograph projector into the schools, they propose in the main to deal with the technique of the instrument under school conditions and with special reference to classroom use.

They note that the Commission on Education and Cultural Films has instructed its third Committee on Film Production and Technique, Distribution and Circulation to co-operate with them, and they understand that this Committee agrees that their co-operation can usefully take this form.

For the suggestions which follow upon

(*a*) Cinematograph Film ;
(*b*) Cinematograph Apparatus ;
(*c*) Illumination and Eye Strain ; and
(*d*) Structural Conditions

the Committee as a whole accept responsibility, but they desire gratefully to acknowledge that the memoranda upon these subjects were originally drafted by Mr. J. S. Dow, B.Sc., A.C.G.I., Honorary Secretary of the Illuminating Engineering Society, Mr. A. E. Munby, M.A., F.R.I.B.A., Dr. T. Slater Price, O.B.E., D.Sc., F.R.S., Director of the British Photographic Research Association, and by Mr. W. Vinten, Manager of the London Branch of the Society of Motion Picture Engineers.

## A. Cinematograph Film.

Safety is the first essential of any cinematograph projection outfit intended for use in schools. It is common knowledge that a certain amount of danger must arise when nitro-cellulose is used as a carrying medium for the photographic emulsion, and special precautions must be observed and properly carried out, not only when the film is on the projector, but also during transit, storage, inspection and re-winding. So long as this element of danger exists and special precautions—personal and structural—have to be taken to avoid it, there is little possibility that such films will become part of the apparatus of instruction in schools.

There is practically no risk of fire when 'safety base' films are used. Their wearing qualities are, however, only about 60 per cent. of those of the nitrate film, and they are at present slightly more costly than the usual films. Therefore, the professional use of this 'non-inflammable' film is limited, but it is very much safer than nitrate film. Although there is no legal compulsion in the matter, there is a recognised practice in the film industry that all sub-standard film (16 mm. and below) shall be made from safety base only. Hence, in a performance where one of these sub-standards is used, there is no risk that some of the film is nitrate, *i.e.* 'inflammable film.'

For these reasons the Committee have come to the conclusion that there is little hope of an early and widespread use of cinematograph films in schools unless they are of the 'non-flam' or 'safety base' type. As this report will show, the acceptance of this principle will not add greatly to the difficulties of the teacher who wishes to make his own motion pictures for classroom use, and will certainly encourage the professional picture maker to add to the stock of educational films on a safety base, both by the production of new films and by the reproduction and reduction of films which have already been taken on the ordinary inflammable material. These processes are to him comparatively simple, but reduction may result in a certain loss of effect, since scale is of the essence of good picture making.

Practically speaking, all safety base at present available consists of cellulose acetate—ordinary film is made from nitro-cellulose—the difference between one safety base and another consisting mainly in the methods adopted to overcome the defects of the plain acetate. It is non-inflammable in the sense that it does not flare up when a light is applied, but slowly chars away so long as the high temperature is maintained.

It has the further advantage that while the heat of a normal beam of light in a projector is sufficient to cause ordinary film to burst into flames, if kept there without movement or special means of protection, this is not the case with safety base, excessive heat produced in this way merely producing charring. There is also the difference between the two bases in that at a comparatively low temperature nitrate base decomposes with the production of dangerous decomposition products. This is not the case with acetate base.

At the Seventh International Congress of Photography, held in London in July 1928, there was a considerable discussion on the definition of safety film, but it was found impossible to arrive at any agreed conclusions. The matter is now under consideration by the various national committees, which are expected to report at the next Congress at Dresden in 1931.

A film, after exposure, has to go through the processes of development, fixing, washing and drying, after which it must return to its original dimensions, within very narrow limits of tolerance: otherwise difficulties would occur in its passage through the gate of the projector. During these processes it is subject to various strains which may affect the dimensions. The chief strain is due to the fact that the base itself is not entirely impervious to moisture, and alters, therefore, to some extent in size according to its moisture content. This creates some difficulties in the use even of the nitrate base, but they are very marked in the case of the acetate base. Minor strains may also be caused by the swelling of the sensitive layer during development, variations in this swelling during fixing and washing, and then de-swelling (contraction) on drying.

When passing through the projector the film is subject to considerable strain, of an alternating character, and yet, after being used many times, its dimensions must still remain within the limits of tolerance allowed. It must also withstand, as much as possible, marking due to scratching, abrasure, etc.

It is found that the mechanical properties of the nitrate base are such that it stands up to the above treatments far better than the acetate base. The latter has the further disadvantages that it is flabby in handling (in the developing, etc., processes) in comparison with the former, and also that when dry it tends to become brittle and break easily.

At the present time the nitrate base film is the only one which satisfies the call on it when made of the standard size, 35 mm., though cellulose acetate base film of standard size is usable and will be improved as time goes on. When, however, the size of the film is reduced to 16 mm. (sub-standard), the differences between the two bases are not so marked. The strain of wear and tear is not so great in the sub-standard film, and consequently the non-inflammable base is found to satisfy all requirements within reasonable limits.

### B. Cinematograph Apparatus.

Acetate film of 16 mm. or less in width is suitable for use by the amateur and for educational (classroom) purposes. There is a large selection of reliable apparatus for producing such films made by firms of repute, and there are also libraries of well selected, though not specifically educational, films. This 16 mm. standard equipment will give excellent results in a classroom not exceeding a seating capacity of 80, provided the room can be well darkened (down to one desk light for the master and a red lamp over exit doors). The projector is easy to manipulate by the master or an assistant. It is quite safe if reasonably used, and can be coupled up to any electric light supply. It is easily portable, and several makers fit their projectors with a stopping device so that any one frame (picture) can be held stationary on the sheet without heat from the light source giving trouble. Care must, however, be taken to make sure that the rigidity of the projector under operative conditions has not been sacrificed to portability. It is known that demand is causing concentration on improvements that will increase the size, definition and brilliancy of the projected picture and decrease the tendency to flicker at low speeds. Lecture rooms of from 120 to 150 seats will then be catered for, but this capacity is the limit that can be expected from 16 mm. outfits.

For lecture rooms of from 200 to 250 seats, the professional sized film of 35 mm. is advisable to-day. In existing schools and colleges where structural alterations are not intended or are difficult, safety base 35 mm. film combined with a projection outfit which complies with the special Home Office regulations for portable projectors gives satisfactory results. These regulations are the outcome of a committee of the film trade working in collaboration with the Home Office officials, instituted to provide for exhibitions in schools and parish halls, etc. It was considered by the trade that if some such regulations existed, education committees and societies might provide a good market. Several makers of apparatus produced quite low-priced outfits of good quality, but even so, the outfit was found to be too costly for the parish hall, and education authorities apparently still have the matter under consideration.

For larger audiences, specially constructed theatres should be provided, with a properly constructed operating room equipped with two projectors, the whole outfit complying with the regulations made by the Home Secretary under the Cinematograph Act and the particular local authority. The seating should be inclined and a good lighting system should be provided throughout the theatre.

The old difficulty of lack of electric current is quickly disappearing, and the standardisation of current generally under the Central Electricity Board will also be of great advantage. Where electric current is not available, the 16 mm. outfit may be used with a 12 volt bulb running from batteries, the batteries to be of the motor car type which can be charged at the local garage. The use of oxygen gas from cylinders, and coal gas for lime lighting is quite safe when certain precautions are taken.

The incandescent focus lamp is being very rapidly improved on account of the demand for it in the production of talking pictures. The 30 volt 900 watt lamp has been found very satisfactory for demonstrations up to 200 people on the 35 mm. film, and this type of lighting can be recommended for use wherever possible. It is simple, satisfactory and safer than any other light source.

A specification for a 16 mm. projector will be found in the Appendix. Such a projector can be purchased nowadays through the ordinary channels for about £20. A projector outfit to take standard (35 mm.) safety base films and complying with the Home Office regulations for portable projectors will cost about four times that amount. For a stationary projector to take standard films and suitable for professional use the cost will be from £135 upwards.

### C. Illumination and Eye-strain.

*Choice of Screen.*—The size of screen adopted naturally varies according to local conditions. A screen 5 to 6 ft. in width is usually ample for school use, but folding screens of somewhat smaller dimensions are supplied with some well-known types of portable projectors. When the film is shown in a long and narrow room, aluminium-painted screens may be used with advantage. The brightness of the image will then vary with the angle of view. The ratio of the brightness on the axis to that obtained when the film is viewed from the extreme angle of observation should not exceed 4 to 1. The limiting angle corresponding with this ratio may vary from 25° to about 40° according to the nature and size of the screen. In all cases where observation at a somewhat oblique angle is likely, dead-white screens are preferable. In this case the brightness on the axis is less than with an aluminium-painted screen, but the effect, as viewed from different angles, is more uniform.

*Nature of Illuminant and Intensity.*—In practice the illuminant is usually a focus type electric incandescent lamp, which may consume 200–250 watts, and is run off 50 volts. When alternating current is available, the pressure may be conveniently reduced by a transformer: otherwise a suitable resistance in series with the lamp is necessary. Lamps capable of being run direct from the supply voltage are sometimes used, but the effect is less satisfactory. With such a lamp a mean beam intensity up to 2,800 candles may be attained, corresponding with a screen illumination of about seven foot-candles at a distance of 20 feet (with no film in the gate and the shutter open).

*Conditions of Display.*—Apparatus of the above type may be used in rooms from which daylight cannot be entirely excluded, but naturally no direct daylight should reach the screen. The blinds should be drawn: blinds which pull up from the bottom, running in channels, are advantageous in preventing troublesome streaks of light. The screen may with advantage be surrounded by a curtain extending vertically 2–3 ft. from the screen so that the latter is protected from all but weak diffused daylight. Provided the picture is not impaired, the presence of diffused daylight in a room may be considered an advantage, as it lessens the contrast between the bright picture and the surroundings, and thus diminishes liability to eye-strain. Such supplementary illumination also aids the supervision of audiences of children. Small projectors have been shown satisfactorily to audiences of 150 and even 200, provided the room can be darkened and the seats suitably arranged.

*Eye-strain.*—Suggestions in regard to conditions to be observed with a view to diminishing eye-strain were embodied in the Report of a Joint Committee appointed in 1919 by the Illuminating Engineering Society at the request of the London County Council. Great importance was attached to undue elevation of the eyes as a cause of eye-strain. It was recommended that the angle of elevation to the top edge of the picture should not exceed 35°. With a screen 3½ to 4 ft. square, this angle would probably not be exceeded, provided no seat was nearer than about 7 ft. from the screen. The Committee also advised that the lateral angle of view to the extreme edge of the screen should not exceed 25°. This condition would usually be complied with, assuming a screen of the size indicated above and a minimum distance therefrom of about 7 ft., if the width of the space occupied by the audience did not exceed 20 to 25 ft. Compliance with the above conditions would in itself probably prevent undue proximity to the screen rendering difficult the following of movements in the picture. The Committee suggested that the distance of the most remote seat should not exceed twelve times the height of the picture. With a 4 ft. screen this would mean a limiting distance of about 50 ft.

Attention was also drawn to the importance of avoiding defects in films (scratches or holes in the gelatine), and vibrations due to light and portable apparatus not being securely fixed—all of which may act as possible sources of eye-strain.

In conclusion, it may be remarked that long-continued observation of moving pictures is liable to impose some strain on the eyes of children. Any danger in this respect may be materially lessened if stationary pictures are shown at suitable intervals. Effective 'colour-interludes,' based on the display of scenes which undergo gradual changes in colour, have been devised for this purpose.

## D. Structural Conditions.

In considering the structural conditions desirable for the use of films in schools, the Committee limit themselves to safety base (non-flam) films.

As the danger in the use of non-flam films appears to be no greater than in the use of a lantern for other purposes, the suggestions merely amount to precautions which seem desirable when any audience of young people is brought together in a building.

It should be remembered that until non-flam films have been solely in use for some time the risk of panic will be greater than with most other public displays. This could be combated by the exhibition of a notice that the films are non-inflammable.

*Exits.*—The room used should have at least two exits which should not be near the lantern and access to which should be maintained without obstruction. The doors of these exits should open out and be secured by panic bolts which are released by pressure on the doors from within.

The tops of the doors should bear the word Exit to be visible from all parts of the room, and this should be illuminated by a source of light independent of the lantern.

*Gangways.*—Sufficient gangways leading to the exits should be provided free from obstruction.

*Electric Leads.*—The leads to the lantern should run so that interference, intentional or accidental, by the audience is not possible. For example, if they have to cross the floor a suitable trench with cover should be provided in the floor. These leads should have their own double pole fuses of the enclosed type in a cast-iron box. The arrangements should be such that if the lantern fuse 'blows,' the fuses controlling the lights of the hall will not be affected. If this presents difficulties in a small installation, as an alternative sufficient low voltage lamps served by portable accumulators should be provided to give enough light for emergencies.

*Space.*—While no special enclosure for the lantern or operator is necessary, sufficient space which cannot be encroached upon in the dark should be reserved round the lantern for easy operation.

It might occasionally happen that the whole of a small room was required for an audience with a vacant room adjoining. In such circumstances a hole might be made in the end wall for the lantern objective and the lantern and operator placed in this adjoining room.

## APPENDIX.

### *Specification of a* 16 *mm. Projector.*

The machine should be of sound construction as generally accepted in cinematograph apparatus.

The machine must be rigid under operative conditions.

All parts subject to wear should be so designed that replacements can be obtained and fitted to the machine by the operator.

The 16 mm. standards as fixed by the Society of Motor Picture Engineers should apply, and the machine must effectively operate film that has shrunk 1·5 per cent. below the standard of new film. English standard dimensions should be used and the decimal system used to show limits.

Where British Engineering Standards Association standards apply they are to be used, and all screws and threads are to be of British Association Standards. A hand drive should be incorporated.

All terminals or connections in the electric circuit are to be enclosed by a suitable insulating material and the machine 'earthed' by a third wire in the 'leads' when intended for use on a circuit over 50 volts.

A switch for the motor and one for the lamp should be fitted in a convenient position, and where this is intended for over 50 volts 'earthed' switches must be installed.

A suitable means of holding any one 'frame' or picture stationary in the gate for five minutes, without damage to the film from the heat of the light source, is necessary.

The metal parts of the gate that are in contact with the film should be either of rustless steel or a hard non-ferrous metal.

Simplicity of threading the film into the machine is very essential in order to avoid delay between reels.

Where possible, dust-proof lubricators should be used for parts requiring lubrication, and should be of such construction or colour as to define its use.

A means of winding film from one reel to another should be incorporated in the machine.

The general finish of the machine should be a matt black enamel, and all plated parts should be dull plated, with the object of avoiding any reflection in the eyes of the operator.

A strong dust-proof cover of wood or metal to enclose the whole of the machine and motor should be part of the equipment, and the cover should be fitted with a suitable handle and fasteners for hand transport. In the lid of such cover, or other suitable place, should be fixed an instruction chart on the oiling and operating of the machine, and means for holding securely two film reels.

Each machine should have a serial number for reference purposes.

# SECTIONAL TRANSACTIONS.

(For reference to the publication elsewhere of communications entered in the following lists of transactions, see end of volume, preceding index.)

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## SECTION A.

## MATHEMATICAL AND PHYSICAL SCIENCES.

### Thursday, September 4.

**Discussion** on *The Meteorological Relations of Atmospherics.* (Mr. R. A. Watson Watt; Prof. E. V. Appleton, F.R.S.; Monsieur R. Bureau; Mr. M. A. Giblett.)

Mr. R. A. Watson Watt.—The immediately active work on the origin of atmospherics appears to be associable with the names of Appleton, Austin, Bureau, Dean and Harper, Lugeon, Paolini, Schindelhauer and Watson Watt. The author's conception of the present working hypothesis of each worker is stated in order to de-limit the scope of the present discussion. The experimental methods of each group are summarised and criticised. The examination is illustrated by discussion of typical cases. An attempt is made to state the most definite conclusions as to the meteorological relations of atmospherics which a normally critical physicist would regard as justified by the existing experimental evidence. Suggestions are made for critical tests to resolve some of the differences of view.

M. R. Bureau.—'The recording of the Number of Atmospherics per minute.'

1. The principle of the method of recording atmospherics.
2. The different processes employed to arrest the transmissions in order to allow the atmospherics only to pass.
3. Choice of scale for the recorder. Detailed and condensed curves. Rapid publication of curves.
4. Recording of different categories of atmospherics. Separation of feeble atmospherics from strong atmospherics.
5. Registration on different wave-lengths. Simultaneity and non-simultaneity of individual atmospherics on different wave-lengths. Influence of wave-lengths on diurnal variation. The part played by propagation in these phenomena.
6. Comparison of results obtained at Mt. Valerien and St. Cyr.
7. Examples of long range and of local atmospherics.
8. The amplitude of the night maximum. Variations in the same latitude and along the same meridian.

Prof. E. V. Appleton, F.R.S.—In a discussion with this title it seems desirable to ascertain whether a case for a predominantly terrestrial origin for atmospherics can be made out. The various extra-terrestrial sources which have been suggested are, therefore, considered. The thunderstorm mechanism seems to be a more likely source than the extra-terrestrial sources proposed.

Attention is called to the experimental fact found by Appleton, Watt and Herd that, for atmospherics of local origin, negative electrostatic field changes are about 1·5 times as frequent as positive, while for those of distant origin positive radiation field changes are about 1·5 times as frequent as negative. The possible significance of this is briefly discussed.

Mr. M. A. Giblett.—Observations of the sources of atmospherics made at the Radio Research Station, Slough, Bucks, at 13·00 G.M.T. daily have been transmitted immediately in an experimental code to the Meteorological Office, Royal Airship Works, Cardington, Beds, where they have been plotted on and studied in connection with the current synoptic weather charts. The results of this experiment in including such observations in a daily weather service will be outlined.

Prof. M. Siegbahn.—*The Highly Ionised Spectra in the Extreme Ultra-violet.*

Mr. W. Sucksmith.—*The Gyromagnetic Effect in Paramagnetic Substances.*

An apparatus for the measurement of the gyromagnetic ratio for paramagnetic substances has been designed. The angular momentum produced by a change of magnetic moment is extremely small, and low frequency resonance is utilised for building up the resulting impulse to a measurable magnitude. Results on the $Dy^{+++}$ ion give $g$, the Landé splitting factor, equal to $1.28 \pm .07$, which indicates that the magnetic moment is due to both orbital and spin contributions. Measurements on other paramagnetic substances are in progress.

Prof. J. H. van Vleck.—*Some Problems of Magnetism.*

## Afternoon.

Inspection of the Henry Herbert Wills Physical Laboratory, and of apparatus referred to in the following papers (taken as read).

(*a*) Prof. A. M. Tyndall and Dr. C. F. Powell.—*The Mobility of Ions in Pure Gases.*

Measurements of the mobility of ions in purified nitrogen and helium have been made in all-glass metal apparatus which can be subjected to rigorous heat treatment and by a method which has a high resolving power for ions of different mobilities. The results disclose the very great importance of small traces of impurity on the mobility of positive ions. In helium, for example, values of mobility for the positive ion as high as 17 and 14 were found instead of the value 5 obtained originally by Franck. Slight contamination of the gas on standing for a week transformed the ions into a mixture of widely differing mobilities with a predominance at about 10. The mere removal of a liquid air trap inserted to prevent any access of vapour from mercury or tap grease to the gas produced a further marked effect. In both gases the negative carriers were electrons and no detectable number of ions were found. Such marked effects are to be expected from the principle of electron capture according to which a positive ion on impact with a molecule of lower ionisation potential can capture an electron from a neutral molecule with a consequent change in the nature of the ion.

In order to obtain the true mobility of a positive ion moving in its own gas at pressures of 100 mms. or more it will be necessary to reduce the impurity to a few parts in one million. As the matter stands at the present time, no significance can be attached to any of the values of positive ion mobility recorded in the literature.

(*b*) Mr. W. Sucksmith.—*Apparatus for determining the Gyromagnetic Effect for Paramagnetic Substances.*

(For abstract, see Morning Session above.)

(*c*) Dr. G. F. Brett.—*The Synthesis of Ammonia by controlled Electron Streams.*

It is known that a beam of electrons passing through a mixture of nitrogen and hydrogen causes these gases to combine, giving ammonia. It is also known that some electron speeds are more efficient than others in effecting this synthesis.

The present experiments are concerned with measuring accurately these critical speeds, and with the search for new ones.

The method consists in freezing out the ammonia as it is formed, and measuring the resulting fall in pressure; the rate of pressure change measures the rate of reaction. A special Pirani gauge is used for this purpose.

Five definite electron speeds between 15 and 30 volts are found at which the reaction rate increases sharply; four of these correspond to ionised or excited states of nitrogen, and the fifth to ionised hydrogen.

(*d*) Dr. H. W. B. Skinner and Dr. S. H. Piper.—*Angular Distribution of Photoelectrons from a Copper Crystal.*

This experiment is an attempt to obtain evidence of the coherence of the photoelectrons which are ejected from a crystal when radiation strikes it. If the photoelectrons are the conduction electrons and form a coherent wave in the crystal, the work of Rosenfeld and Witmer has made it seem probable that some coherence should occur. The theory is the exact analogue of the Duane Theory of X-ray (or electron) diffraction. We may therefore hope under suitable conditions to obtain maxima in the angular intensity distribution of the photoelectrons. But the theory is subject to the condition that the mechanism responsible for the electrical resistance of the metal does not cause excessive damping of the electron waves in the lattice. If the maxima are obtained we should have evidence of the lattice-binding (as opposed to atomic-binding) of the conduction electrons.

To realise the best experimental conditions we are forced into the region of very long X-rays, and this makes the technique complicated. The K radiation of Lithium is used for the light source and falls on a copper crystal round which a Faraday collecting box can be rotated. A retarding voltage analysis is made.

The X-ray tube is incorporated in the Pyrex bulb which contains the whole apparatus. The anticathode is of copper let in with a copper-pyrex seal for water cooling and Lithium can be distilled on to the copper. The apparatus is treated with the methods of modern high-vacuum technique.

Apart from the main question, the apparatus will be used for experiments on the general problem of the production of photoelectrons by soft X-rays.

(*e*) Mr. E. T. S. Appleyard.—*Absorption of Light in Cæsium Vapour.*

The Bergen-Davis experiment on the capture of electrons by $\alpha$ particles suggests that when electrons move with respect to $\alpha$ particles there is a very high probability of capture of the electron into one of the orbits of ionised helium. If, following Bergen-Davis, we calculate the energy change which occurs by recapture into the ground state of ionised helium it proves to be equal to twice the ionisation energy of ionised helium. Hence, if this energy reappears as radiation the emitted radiation must have twice the frequency corresponding to the series limit of ionised helium.

If the results of Bergen-Davis are correct, the converse of capture should be absorption of this frequency and ejection of an electron with a definite energy, a process which should occur with great probability. Cæsium vapour is experimentally convenient to study on account of its low ionisation potential.

A study of absorption of cæsium in the ultra-violet is therefore being made using a flourite spectrograph and a high power hydrogen discharge tube to provide a continuous spectrum.

(*f*) Mr. S. H. Piper and Dr. T. Malkin.—*X-ray Investigations with Long-chain Compounds.*

Routine investigations of the composition of various natural fats and waxes by a comparison of their long spacings with those of pure synthetic standards and mixtures. Intensity distributions in the various orders of reflection allow the position of a side chain to be determined (Shearer. P.R.S. 1925, 2194).

(*g*) Dr. L. C. Jackson and Dr. C. F. Powell.—*An Attempt to detect Metastable Molecules in Active Nitrogen.*

When metastable excited atoms or molecules impinge on a metal surface, they set free electrons therefrom, the maximum energy of which is given by the difference between the energy of the metastable atom or molecule and the work function of the metal surface. This effect is being employed in an attempt to detect the $^{3}\Sigma$ metastable $N_2$ molecules in active nitrogen predicted by Cario and Kaplan's theory. A molecular beam of active nitrogen falls on a nickel target and sets free electrons from it. These electrons are collected on a nickel box surrounding the target and a velocity analysis of them is made. Preliminary results have been obtained.

### (*h*) Dr. H. W. B. Skinner.—*A Two-Metre Vacuum Spectrograph.*

The objects kept in view in the design of this instrument are (1) ease of adjustment and (2) range of utility. The first point is attained by taking all the adjustments out through the wall of the main tube of the spectrograph by means of small ground joints and arranging a lighting system within for observing the various scales; also by arranging that the plate may be removed through the side of the main tube through a large ground joint without the necessity of removing the heavy end-plate. The second point is satisfied mainly by using as large a photographic plate as convenient, and allowing range of adjustment so that the spectrograph may be used up to the visible region. Actually the plate is 27·5 cm. in length and the wave-length regions 0–2000 Å.U. 2000–4000 Å.U., &c., may be photographed on separate plates. This, of course, implies a large spectrograph tube (35 cm. dia.) and a correspondingly fast pump for evacuation.

The spectrograph is carried entirely on a steel girder which lies within the copper spectrograph tube. At the one end the girder carries the grating mount which is carried on three levelling screws with tangent screw adjustment and length adjustment. The plate-holder and slit are carried on the other end of the girder. The whole may be removed as a unit from the copper tube. The plate-holder may be raised or lowered from outside the main tube for the purpose of taking a number of exposures on a single plate without re-evacuation. The plate is bent into a circle of two metres diameter (this is possible with plates of special thin glass). It can be rotated by means of a screw and bearing about an axis which coincides with the slit. Thus different spectrum regions may be focussed with one adjustment of the plate (two adjustments of the grating are also necessary). The slit is carried on a tube which projects through the end-plate into the main tube. It is thus buried, but since it must lie on the Rowland circle, this is unavoidable unless we adopt the rather complicated device of fixing a subsidiary slit-tube inclined at a small angle to the main tube. It was thought better to retain the slit as part of the girder unit in spite of the slight inconvenience due to its burial. It can be adjusted from outside the spectrograph and can be removed easily for cleaning by means of a slip-tube.

The end-plates are castings held on to the main tube by bolts and made vacuum tight by rubber and grease. A large Siemens-Schuckert pump with a speed of about 2 litres/sec. is used for evacuation. It will pump the 200 litres volume from atmospheric pressure to 1 mm. in about 10 minutes.

The detail design can be seen from the drawings. A Blythwood metal grating of 14,000 lines per inch and of size 8×5 cm. is at present in use.

### (*i*) Mr. G. Herzberg.—*Investigations of the Band Spectra of* $P_2$, $C_2$ *and* CH *in the Ultra-violet and in the Vacuum Region.*

Though there is quite a good deal known about the structure of the nitrogen molecule, so far hardly anything is known about the Phosphorus molecule ($P_2$) which ought to be analogous to $N_2$. Therefore the band spectra of Phosphorus are studied here in greater detail. An extended band system of $P_2$ (below λ 3500) has been found, part of which was measured 20 years ago by Geuter. There are some peculiarities in the vibrational states of the two electronic levels involved which make an analysis rather difficult. It is hoped to get plates with the two-metre vacuum grating in order to resolve the fine structure which seems to be rather simple. In the long wave-length region the bands extend only to a certain value of the rotational quantum number (predissociation). Another band system, the origin of which is not yet quite certain, has been found. The conditions of excitation point to CP as carrier. The fine structure of the bands of $P_2$ would also be interesting in relation to nuclear spin and statistics.

Until now only very few band systems of $C_2$ and CH have been found. In order to test the validity of recent theories of molecular structure it would be highly interesting to get knowledge about the higher electronic levels of these molecules. Therefore, their spectra are being investigated in the vacuum region.

### (*j*) Mr. G. Herzberg and Mrs. L. Herzberg.—*An attempt to obtain Bands of* $Be_2$ *and* $B_2$.

According to the quantum mechanics of homopolar binding the formation of molecules of Be and B should be possible though they have not yet been observed.

An attempt is being made to get band spectra of these molecules by investigating different sources of light containing various compounds of these atoms. So far bands definitely ascribed to these molecules have not yet been found. The investigation is being continued also on account of the knowledge such molecules would yield about the nuclear structure of these atoms.

### (*k*) Mr. J. H. Lees.—*A Self-recording Photographic Photometer.*

A Cambridge Instrument Co. non-recording photometer was adapted, using the original photographic plate movement, the light source, and the slit system. To obtain an accurate magnification of the plate motion, a fine steel wire wrapped round the screw controlling it passes over the drum carrying the bromide paper, giving in this instance a magnification of 20.

The light passing through the plate falls on a General Electric Co. cæsium on copper vacuum photoelectric cell. The current from this is amplified by a Phillip's electrometer triode, and finally a galvanometer reflects a spot of light on to the bromide paper.

### (*l*) Mr. W. R. Harper.—*The Probability of modified X-ray Scattering.*

By the choice of a suitable wave-length the ionisation due to the passage of X-rays through light gases is caused almost entirely by recoil electrons if the usual laws of photoelectric absorption can be extrapolated to this region. An experiment is being made to verify this and to measure the energy associated with the recoil electrons. Hence, the probability of the occurrence of modified scattering may be compared with theory, and further development should enable the method to be used for X-ray intensity measurements.

### (*m*) Dr. H. H. Potter.—*Some Experiments on Magnetic Alloys.*

A ternary alloy possessing pronounced ferromagnetism has been prepared by melting together silver, manganese and aluminium. A maximum magnetisability is obtained when the atomic proportions are four parts silver, one part manganese and one part aluminium.

An attempt is now being made to obtain single crystals of this alloy—which has a face-centred cubic structure—with the object of studying the directional magnetic properties.

The system silver-manganese-tin has also been examined but shows no maximum of magnetisability in the ternary system. The manganese-tin binary system, however, contains two ferromagnetic compounds, one of them being strongly magnetic at low temperatures.

### (*n*) Dr. G. I. Harper.—*The Absorption of X-rays in the Region of 1 to 10 Å.*

Hitherto the chief errors in such measurements have arisen from the difficulty of obtaining thin films of sufficient uniformity and purity. These difficulties are being avoided by the use of gaseous absorbers, the absorption coefficients being measured by an ionisation method using a vacuum monochromator, and a Phillip's Electrometer Triode which has been adapted to measure ionisation currents down to $10^{-14}$ amp. with an accuracy of 1 per cent., the ionisation currents being recorded on a table galvanometer. It is hoped that these measurements will serve as an accurate test of the validity of Jönsson's law of absorption, which is not in agreement with later measurements.

### (*o*) Mr. J. H. Burrow.—*Demonstration of Glass Metal Seals and Glass-blower's Lathe.*

## DEPARTMENT OF MATHEMATICS (A*).

### (MORNING SESSION.)

### Mr. E. H. LINFOOT.—*A Problem in the Analytic Theory of Numbers.*

Waring's problem, perhaps the most famous in the modern theory of numbers, deals with the representation of an integer as a sum of $k$th powers, $k$ being fixed. Evidently it is one of a whole series of problems ; given any class $M$ of positive integers, we may investigate the representation of an integer as a sum of '$M$ numbers.' Mr. Evelyn and I have considered* the problem which arises when $M$ is the class of *numbers not divisible by* $k$th *powers*, $k$ as before being fixed for all. We ask : In how many ways can a large number $n$ be represented as the sum of $s$ $M$-numbers ? The answer is that the number of representations is asymptotic to

$$\frac{1}{\zeta^s(k)}\,\frac{n^{s-1}}{(s-1)!}\prod_{p^k \nmid n}\left(1+\frac{(-1)^{s+1}}{(p^k-1)^s}\right)\prod_{p^k|n}\left(1+\frac{(-1)^s}{(p^k-1)^{s-1}}\right)$$

as $n \to \infty$. We prove this when $s \geqq 3$ by an application of the powerful Hardy-Littlewood method, which consists in investigating the function

$$f(x) = \sum_{M} x^M \qquad (|x| < 1)$$

near its barrier of singularities $|x|=1$. Here $M$ runs through the '$M$-numbers'; the power series, which clearly has $|x|=1$ as its circle of convergence, cannot be continued beyond this circle. When $s=2$ the analytic method fails, but in this case we are able to establish the result by an elementary though difficult argument.‡

* *Math. Zeitschrift* 30 (1929), 433–448. ‡ *Journal für Math.* 164 (unpublished).

### Dr. L. S. BOSANQUET.—*The Summability of Fourier Series.*

Let $f(t)$ be integrable—Lebesgue and periodic with period $2\pi$, and let

$$\varphi(t) = \tfrac{1}{2}\left\{f(x+t)+f(x-t)-2s\right\}.$$

Hardy and Littlewood and their pupils have proved : *

I. If $\varphi(t) \to 0$, (C, $\alpha$), then the Fourier series of $f(t)$ is summable (C, $\alpha+\delta$) to $s$, for $t=x$, where $\alpha \geq 0$ and $\delta > 0$.

II. If the Fourier series of $f(t)$ is summable (C, $\alpha$) to $s$, for $t=x$, then $\varphi(t) \to 0$, (C, $\alpha+1+\delta$), where $\alpha \geq -1$ and $\delta > 0$.

There are similar results for Allied series and Power series. The theorems remain true if the Denjoy integral is employed, provided in I that $\alpha+\delta \geq 1$. They are false in certain cases with $\delta = 0$.

A problem arises of defining a scale of two-parameter summability, reducing to Cesàro summability when the second parameter is zero, and satisfying conditions of consistency.

A typical result is

III. If $\varphi(t) \to 0$, (C, 0), then the Fourier series of $f(t)$ is summable $(0, 1+\delta)$ to $s$, for $t=x$, where $\delta > 0$.

This is false with $\delta = 0$.

* The following definitions are assumed known: $\varphi(t) \to l$, (C, $\alpha$), as $\to 0$, where $\alpha > 0$, means

$$\alpha\int_0^1 (1-u)^{\alpha-1}\varphi(tu)\,du \to l \text{ as } t \to 0;$$

$\varphi(t) \to l$, (C, 0) means $\varphi(t) \to l$ in the elementary sense;

$\sum a_n$ is summable (C, $\alpha$) to $s$, where $\alpha \geq 0$, means

$$\sum_{n<w}\left(1-\frac{n}{w}\right)^{\alpha} a_n \to s \text{ as } w \to \infty.$$

Prof. E. C. TITCHMARSH.—*Functions which are their own Fourier Transforms.*

The Fourier reciprocity between two even functions $f(x)$, $g(x)$, is expressed by the formulæ

$$g(x) = \sqrt{\left(\frac{2}{\pi}\right)}\int_0^\infty \cos xy\, f(y)dy \quad , \quad f(x) = \sqrt{\left(\frac{2}{\pi}\right)}\int_0^\infty \cos xy\, g(y)dy.$$

In some cases $g(x) = f(x)$; this is true for example for the functions

$$\frac{1}{\sqrt{x}} \ , \quad e^{-\frac{1}{2}x^2} \qquad \operatorname{sech} x \sqrt{\left(\tfrac{1}{2}\pi\right)}.$$

We find that there are formulæ which give general expressions for all functions which have this property. The simplest such formula is

$$f(x) = \frac{1}{2\pi i}\int_{c-i\infty}^{c+i\infty} 2^{\frac{1}{2}s}\,\Gamma\left(\tfrac{1}{2}s\right)\psi(s)x^{-s}ds,$$

where $\psi(s) = \psi(1-s)$.

Mr. T. W. CHAUNDY.—*A Note on the Hypergeometric Equation.*

Equations of the generalised hypergeometric type :—

$$f(x\mathrm{D})\, y = x^m g(x\mathrm{D})\, y$$

are classified by the residues (mod $m$) of the zeros of $f$, $g$. Conditions are obtained that this equation be soluble by algebraic and logarithmic functions, and that the equation

$$f(x\mathrm{D})\, y = x^m y$$

be soluble (1) by exponential functions and polynomials,
(2) by Bessel functions.

The theory, due to Mr. J. L. Burchnall and the author jointly, yields various well-known results and also certain formulæ (possibly new) for the solutions $P_n$, $Q_n$, of Legendre's equation.

Dr. D. M. WRINCH.—*Recurrence Relations and some Definite Integrals involving Legendre Polynomials.*

It is shown that the integrals, with regard to $\mu$ of the functions

$$\frac{P_n(\mu)}{\nu-\mu}\ ,\quad \frac{P_n(\mu)P_m(\mu)}{\nu-\mu}\ ,\quad \frac{\mu P_n(\mu)P_m(\mu)}{\nu-\mu}\ ,$$

between the limits $-1$ and $+1$ follow readily from the recognition that these definite integrals, as functions of $n$, satisfy the familiar recurrence relation of $P_n$. The values of the integrals of the functions

$$\frac{P_n'(\mu)P_m(\mu)}{\nu-\mu}\ ,\quad \frac{\mu P_n'(\mu)P_m(\mu)}{\nu-\mu}\ ,\quad \frac{(\mu^2-1)P_n'(\mu)P_m'(\mu)}{\nu-\mu}\ ,$$

between the same limits, and of some allied integrals, are then inferred almost without calculation.

Miss R. C. YOUNG.—*The Algebra of Infinities.*

This paper is concerned with the manipulation of ( positively and negatively 'infinite values' as they occur in analysis (*i.e.* by passage to a limit).

With the ordinary conception of an 'infinite value,' only the crudest algebraic combinations are allowable; and results proved for finite values have to be discussed separately for infinite ones, even when the final statement is formally the same in both cases; or again cases of non-finiteness are avoided altogether, and the treatment left incomplete. Hence, some more refined and inclusive mode of definition is a desideratum.

The possibility of such a definition of wider scope appears bound up with the *algebra of finite many-valued limits*, in whose light an infinite limit, or *complete infinity*, is seen to have some of the properties of a many-valued quantity; and such a 'complete infinity' may be analysed into constituents (*simple infinities*) which have themselves 'not quite' the character of numbers.

The ordinary rules of arithmetic are discussed with reference to these various 'infinities' and illustrations given.

Prof. W. E. H. Berwick.—*The Complex Multiplication of Elliptic Functions.*

Application of the addition theorem in trigonometry leads to formulæ expressing tan $2\theta$, tan $3\theta$ . . . in terms of tan $\theta$. Similar formulæ in elliptic functions express $F(mu)$ in terms of $F(u)$, $F^1(u)$ for all integral values of $m$.

When the modulus is such that the primitive periods are $2c$, $2c\sqrt{-n}$ the function $F(u\sqrt{-n})$ is also doubly periodic in $2c$, $2c\sqrt{-n}$ and so, by a well-known theorem on functions of a complex variable, is expressible rationally in terms of $F(u)$, $F^1(u)$. A method of obtaining this rational expression is given when $F(u)$ is Weierstrass' elliptic function of the second order. Numerical examples are included, one showing $F(u\sqrt{-5})$ in terms of $F(u)$. The paper ends with a statement of four arithmetical problems on complex multiplication which are not yet entirely settled.

## Friday, September 5.

Sir E. Rutherford, Pres.R.S., Mr. F. A. B. Ward, and Mr. C. E. Wynn-Williams.—*New Methods of Analysis of $\alpha$– Particles.*

Papers dealing with Aspects of the Solid State:—

Considerable progress has been made recently towards an understanding of the physical properties of the solid state, due, on the one hand, to the advent of the wave mechanics and on the other to the development of X-ray technique. The object of the discussion is to review recent advances from the theoretical and experimental points of view.

Prof. J. E. Lennard-Jones.—*The Nature of Cohesion.*

The object of this discussion is to review the progress which has been made within the last three or four years towards an understanding of the nature of solid bodies. This has been due to the happy interplay of theory and experiment, for both have made important contributions to our knowledge. This first paper attempts to review some of the principles which have been established recently with regard to the nature of atoms and their bearing on the all-important question of their reaction with one another. We cannot hope to understand or influence the physical properties of solid bodies until we understand the nature of the cohesive forces which hold solids together.

There are three main new ideas which have become established within the last few years. Firstly, there is the main idea of the wave mechanics that it is impossible to follow the electron in all its ways. All that we can hope for is a knowledge of the probability that an electron shall do this or do that. The result of this is that we now deal in patterns instead of orbits. An electron in the presence of a positive nucleus may have this or that pattern, each continuous about the nucleus and occupying the whole of space. To each pattern corresponds a definite energy, and the electron normally stays in that pattern which has the lowest energy.

Secondly, there is the principle, first enunciated by Pauli, that there are never more than two electrons which take up the same pattern, and of these pairs one has one kind of electron spin and the other the opposite kind.

Thirdly there is the *exchange principle.* Electrons may interchange their patterns, and as all electrons are identical, it is impossible to know which electrons occupy which patterns. This exchange phenomenon has important consequences. The charge distribution of an atom can only be expressed by the use of six dimensions, and this six-dimensional character is important in calculating the energy of atoms or the energy of interaction of electrons.

To these new principles must be added an old one, viz., the principle of minimum energy: an atomic or electronic system tends to assure the state of lowest energy.

Cohesive forces may be classified under five headings, viz.: (i) Van der Waals; (ii) homopolar; (iii) ionic; (iv) metallic; and (v) adamantine.

The principles enumerated above may be applied to an interpretation of these types of cohesive forces. It is becoming clear that the first may now be calculated owing to the smearing-out process described above. Van der Waals' fields seem to be due to a *dynamic polarisation.* Homopolar binding, on the other hand, seems to be due to the exchange phenomenon, and ionic binding due to the principle of minimum energy. Metallic cohesion, though not yet investigated in detail, seems to be due to the interpenetration of atomic electric charge, which leads to greater electrostatic attraction, and due also to the exchange principle. The question as to whether atoms will form a metal-like sodium or a collection of molecules like hydrogen seems to be determined by the principle of minimum energy. Adamantine cohesion is probably due to the exchange phenomenon and thus the same as homopolar binding, but this is not yet definitely established.

It seems as though the principles underlying the nature of cohesion are now understood, and the immediate need of the future is the discovery of a mathematical technique, which will permit of their application to particular cases.

## Prof. W. L. Bragg, F.R.S.—*The Structure of the Solid State: Inorganic Compounds.*

Recent years have witnessed a great advance in our understanding of the way in which the physical properties of matter are explained by its atomic arrangement. Progress has been made possible both by the closer understanding of interatomic forces which we owe to recent developments in mathematical physics, and by X-ray analysis in revealing crystalline arrangement. The latter in particular has passed from a technique which could only deal with the simplest compounds, to one by which the most complex inorganic crystals can be analysed. The present paper attempts a general survey of inorganic compounds.

Of the four main types of interatomic binding, ionic, homopolar, molecular and metallic, the first two are of prime importance in typical inorganic compounds, though examples can be given showing a continuous transition towards the other types. A great deal of experimental material has now been collected, and presents very interesting problems for further theoretical treatment. The simplest compounds are the associations of ions forming structures of great regularity. A number of workers, in particular in the school of V. M. Goldschmidt, have shown how the compounds fall into types determined by the factors of ionic charge, size and polarisation. In the salts with complex acid radicles, composed of outer electronisation atoms surrounding an inner acid-forming atom, it would be very interesting to know the type of binding within the group. So many of its physical properties can be given a semi-quantitative explanation on the assumption that outer and inner atoms are charged ions, yet this can hardly be the true picture. Much experimental work remains to be done on the configuration of the more complex groups, most of those so far examined being the simple tetrahedral or threefold types. The silicates present an interesting intermediate stage between the simple compounds with continuous ionic lattices, and the salts with independent complex ions. SiOy groups can be independent (orthosilicates), or link in chains (pyroxenes and amphiboles) or in sheets (micas) or in three-dimensional complexes (silica, zeolites). They can be regarded as acid radicles with indefinite extension in one, two, or three spatial dimensions.

The laws of co-ordination in simple compounds outlined by Goldschmidt can be extended in a very satisfactory way to the more complex types. Pauling has framed a striking series of general rules for such structures, which goes far to explain their forms as representing an atomic arrangement which gives a minimum value to the potential energy.

Another interesting field for theoretical investigation is provided by the transition from one type of binding to another shown by the oxides, sulphides, selenides, tellurides, arsenides of certain divalent elements. Some have the sodium chloride structure, others the opaque metallic nickel arsenide structure which has properties more like those of an intermetallic compound. Again, there is the so-called 'adamantine' compounds of formula AX, with their characteristic tetrahedral arrangement in which the binding appears to be homopolar throughout.

The pressing need at the present juncture is for a theoretical technique which can treat these complex interatomic bindings so as to obtain quantitative results for comparison with experiment.

Dr. J. D. Bernal.—

Discussion on the previous papers.

Prof. E. A. Milne, F.R.S.—*Stellar Structure and the Origin of Stellar Energy.*

## Department of Mathematics (A*).

Dr. J. Henderson.—*The Methods of Construction of the earliest Tables of Logarithms.*

The paper gives a brief description of the methods adopted by Napier and Briggs in the construction of their original logarithmic tables. Napier constructed the first logarithmic table in the general sense, while Briggs followed with the first logarithmic table with 10 as the base. In Napier's method the calculations do not depend on the idea of a base; they depend essentially on the association of two progressions, one arithmetical and the other geometrical. Briggs, however, used methods which depend on the fact that the base is 10; he formed a skeleton table and then interpolated by the method of differences, which he apparently understood completely.

Dr. R. A. Fisher.—*Inverse Probability.*

The controversy over 'inverse probability' seems to be unique in the history of mathematics. The reasons for the rejection of the classical theory are obvious and need only be stated. Its retention in mathematical text-books is to be explained by the fact that until recently no alternative method was available to give an account of inductive reasoning.

The method of maximum likelihood has no logical connection with inverse probability, although it has been associated with it historically. Its derivation by this path involves the introduction of arbitrary functions at two distinct stages, which can be made to cancel each other. Likelihood is not a synonym for probability; it is a quantity, which, like probability, measures the degree of rational belief, but it does not obey the laws of probability. Statements about unknown samples of known populations are made in terms of probability, statements about the unknown populations from which known samples are drawn are made in terms of likelihood. Likelihood serves all the purposes necessary for the problem of statistical estimation.

The invariant character of the percentile values does, however, make possible certain statements in terms of probability respecting the values of the parameters of populations. Statements of this type, which have very strangely been overlooked, are available only when the observations are of quantitative variates, and not merely of frequencies. They differ from the statements of inverse probability, both numerically and logically; the statements of inverse probability are absolute in the form, based on a hypothetical super-population of an absolute character, but can never be verified, for any further samples from the same population will alter the content of the statements. The statements of the percentile method are relative in form and rigorously demonstrable without any assumption as to the *a priori* distribution of the parameters.

Dr. J. Wishart.—*Combinatorial Methods in Problems of Sampling.*

In the statistical theory of sampling, we are concerned with finding the mean values of powers and products of series of symmetric functions of the observations of the sample. Practical convenience dictates the exact nature of the symmetric functions which should be calculated, and the derivation of the final result involves the methods of combinatorial analysis, and in particular the theory of separations. The nature of earlier work on this subject, involving knowledge of the products of the elementary symmetric functions, is sketched, and the way in which the most modern methods have been evolved is thereafter described, with appropriate illustrations.

### Mr. J. O. Irwin.—*The Approximate Numerical Evaluation of Single and Double Integrals.*

(A) The principal methods of quadrature for one variable: (*a*) The Euler-McLaurin formulæ; (*b*) Other formulæ dependent on equidistant ordinates; (*c*) Formulæ based on Everett's central-difference interpolation formulæ; (*d*) Gauss' method.

(B) The extension of A (*c*) to double integrals. Other formulæ for cubature.

### Dr. L. J. Comrie.—*Modern Babbage Machines.*

Babbage, over 100 years ago, was the pioneer in efforts to build a machine that would calculate a function from its higher order differences. His partially completed difference-engine, as well as that of Scheutz, whom he inspired, is now in the Science Museum at South Kensington. Other spasmodic attempts to build machines for mechanical integration are on record. About three years ago three commercial machines appeared—the Nova Brunsviga Model IVA, the Brunsviga-Dupla, and the Burroughs Class 11 machine—all of which are capable of integrating from second finite differences, the latter with printing of argument, function, first and second differences. They will be described and demonstrated.

## Monday, September 8.

### **Presidential Address** by Dr. F. E. Smith, C.B., C.B.E., Sec. R.S., on *The Theories of Terrestrial Magnetism.* (See p. 15.)

### Dr. P. A. M. Dirac.—*The Proton.*

It is believed that all matter is built up from two elementary kinds of particle, the electron and the proton. Recent theoretical work seems to show, however, that these two kinds of particle are not independent, but are connected in such a way that actually there is only one fundamental kind of particle in nature.

The kinetic energy $W$ of an electron is given in terms of the momentum, according to the principle of relativity, by an equation which is quadratic in $W$, allowing of negative roots as well as of positive ones. Ordinarily the negative roots are discarded as being unwanted and physically meaningless. This is not permissible in the quantum theory, however, since there transitions can take place from states of positive energy to states of negative energy. It then becomes necessary to give a physical meaning to the negative-energy states. This we can do only by assuming that nearly all the negative-energy states are occupied by electrons, with just one in each state in accordance with the Exclusion Principle of Pauli. *We can then interpret the unoccupied negative-energy states as protons.* Elementary considerations show that they will appear to us as things having a positive energy, and also a positive charge.

There are certain difficulties in the theory that have not yet been removed. They are (1) the great difference in the masses of the proton and the electron, and (2) the fact that the theory predicts that electrons and protons will annihilate one another at a rate which is much too great to be correct. These difficulties will perhaps be removed by a better understanding of the interaction between electrons.

### Dr. F. W. Aston, F.R.S.—*Some New Mass-Spectra.*

### Mr. R. Stoneley.—*The Identification of the Phases of Earthquake Shocks.*

It has been shown by Jeffreys, Conrad and others that for the different waves recorded in the P and S phases of earthquakes, the distance-time curves approximate closely to straight lines for epicentral distances up to 12° or more. In addition to P, P*, $P_g$, S, S*, $S_g$, discussed in detail by Jeffreys for the Hereford and Jersey earthquakes of 1926, Conrad has found several other waves for which no immediate explanation is forthcoming, and has given a very full account of one earthquake for which P and S are sub-divided into $P_x$, $P_n$, and $S_x$, $S_n$ respectively; Dr. Jeffreys notes on some of the records discussed a wave which is possibly a compressional wave $P_s$ in the sedimentary layer, and Mr. E. Tillotson finds both $P_s$ and the corresponding $S_s$ in the records of a Balkan earthquake. All this work, however, has involved careful scrutiny and measurement of the actual records. It is interesting to examine the wealth of observations given by the International Seismological Summary. The epicentres of the I.S.S. are sufficiently accurate for a preliminary examination, and no adjustment of epicentre will explain away some of the very large residuals. Earthquakes in the Balkans are particularly favourable for investigation in view of the large number of European observatories.

If the usual method is followed and the time of transit is plotted against epicentral distance for all the observations printed (including the 'Additional readings,' which are specially valuable) there is no difficulty in picking out the P, $P_g$, S, $S_g$ graphs. and P* and sometimes S* may be drawn in, with close agreement between the measured velocities and those found by Jeffreys and Conrad. Most of the large residuals are at once seen to arise from wrong identifications: for instance, in 1926, September 3rd. 21 h. 59 m. 50 s., the residual of +28 s. in P for Athens arises because $P_g$ has been read for P, and the residuals of +55 s. for Lemberg, and 70 s. for Rocca di Papa are attributable to the same cause. Likewise, the +70 s. for S at Zagreb arises from the use of $S_g$; the correct S wave is quoted as '$ePR_1$ ?' in the additional readings. Vienna mistakes S* for S, and most of these incorrect identifications could be inferred from tables for times for $P_g$–P, $S_g$–S, &c. Naturally, all this could best be done from the original records, but it is possible to obtain fairly rapidly quite a lot of geophysical information, not from a laborious examination of seismograms, but from the readings and *misreadings* available in the I.S.S. It is because of these incorrect identifications that the published observations are so informative!

With the rather wide scatter of the plotted observations one might be tempted to infer large errors of readings, to the extent of 20 s. or 30 s. This would be unwise: with fairly large paper speeds observations can be trusted to a few seconds, and timing errors are probably negligible in most cases, apart from mistakes of one whole minute. With the large number of phases that are recorded up to 15° it is better to assume provisionally that all observations are significant until examination of records disproves it. The preliminary examination outlined here may indicate (*a*) the proper identification of a given reading, (*b*) an improved time of starting and epicentre, by the use of tables of $P_g$–P, $S_g$–S, and others, (*c*) an occasional large clock error, (*d*) an epicentre of an earthquake for which readings are very few, (*e*) special earthquakes that merit detailed study (in fact, one feels that every earthquake deserves special study).

It was certainly surprising that the readings in the I.S.S. gave for certain earthquakes values of the velocity of P, P*, $P_g$, S, $S_g$, and perhaps S*, that were close to Jeffreys's values. In identifying a given wave it would be sufficient to assume these values as a first approximation. All these six were found in the first shock examined, viz. 1926, August 18 d. 17 h. 4 m. 52 s. (epicentre in or near Cephalonia, 38° N., 20°.5 E.), and most of the large residuals had evident explanations. $P_s$ and $S_s$ were suspected, but not confirmed, but there seems to be a wave of velocity 6.9 km./sec. and another of about 4 km./sec. In the Zürich (E) record of the Jersey earthquake, reproduced by Dr. Jeffreys in 'The Earth' (second edition), these latter waves are definitely present, with velocities 7.1 and 4.1 km./sec. approximately. If these waves are confirmed they would be particularly interesting, as the velocities fit the data for crystalline basalt, which has not so far been shown to be present as one of the layers of the earth's crust.

The wave with a velocity of about 7 km./sec. appears to be present in the shocks of 1926, August 17 d. 1 h., September 3 d. 21 h., December 17 d. 6 h. and 17 d. 11 h., and it is proposed to examine the actual records of one of these earthquakes; further investigation is certainly necessary before this suggestion can be advanced seriously. The reason for associating these P and S waves together is that the ratio of the velocities P/S is about 1.7, which is what is found for the pairs $P_g/S_g$, P*/S*, P/S and $P_s/S_s$, and corresponds fairly closely to the case of an elastic solid for which $\lambda=\mu$ in the usual notation, or $\sigma$, Poisson's ratio, $=\frac{1}{4}$, when the ratio of the wave velocities is $\sqrt{3}$.

The compilers of the International Seismological Summary are to be congratulated on the way in which they have published the 'additional readings' at the end of the classified observations, and it is to be hoped that, so far as the limitations of space, and ultimately finance, permit, they will continue to print all the information at their disposal.

## Department of Mathematics (A*).

Prof. A. C. Dixon.—*Integral Equations.*

## Tuesday, September 9.

Prof. R. S. Mullikan.—*Molecular Spectra and Chemical Binding.*

**Discussion** on *Flow in Gases.* (Mr. E. Ower; Mr. F. C. Johannsen; Mr. E. C. Bilham; Mr. M. A. Giblett.)

Mr. E. Ower.—*The Standard of Measurement of Gas Flow.*

Hitherto no instrument for measuring gas flow has been devised which does not require experimental calibration. Therefore, if an instrument is to be adopted as a standard the desirable features to be sought are that its calibration shall not be liable to change and shall vary as little as possible with the speed of the gas. Only one instrument suitable for general use is known which possesses these characteristics—the pitot-static tube.

The properties of this instrument are described, attention being drawn to the fact that the pressure at the orifices of the static tube is the resultant of two opposite effects, a suction due to the flow round the nose and a pressure due to the presence of the stem of the instrument. It is only when the holes are in such a position that these two effects balance that true static pressure is recorded. Dimensions of an instrument fulfilling this condition are given. It has a hemispherical nose and is thus more robust than the N.P.L. standard type.

The question of the use of the pitot-static tube for the measurement of low speeds is next discussed, and a short account is given of a research by which it is hoped to establish the calibration of the instrument down to an air speed of about 2 feet per second. For this purpose a special manometer has been designed, which has a sensitivity somewhat better than one hundred thousandth of an inch of water.

Mr. F. C. Johannsen.—*The Measurement of Air Speed.*

The precise calibration of a standard pressure-tube anemometer, with which anemometers for ordinary service may be compared, involves the rotation of the standard instrument on a whirling arm at known speeds over the ground. The true speed of the instrument through the air is less than that over the ground as a result of the swirl, or velocity in the orbit of rotation, imparted to the air by the motion of the instrument under calibration. The discrepancy, of the order of 10 per cent. of the true speed, introduces a serious difficulty into the calibration of a standard anemometer, since no instrument exists whereby the swirl may be measured with certainty, and since the swirl velocity is so small, at low speeds of the standard instrument, as to be below the range of an anemometer for which an approximate calibration is available.

Apart from the difficulties of calibration, anemometers of the pressure tube type are unsuitable for the measurement of low air speeds in consequence of the extremely small pressures involved. In common, moreover, with vane anemometers their dimensions are too large to permit of their being used for explorations in regions of rapid velocity gradient. For such purposes, hot wire anemometers have many advantageous features, notably a high degree of sensitivity at low air speeds, and a rapid response to speed fluctuations. They are thus peculiarly well adapted for investigation of the eddying air flow associated with numerous aerodynamic problems. Over a low speed-range much of the uncertainty arising from their liability to change of calibration may be obviated if the heat-loss : wind-speed relation is expressed as a function of the heat loss in still air.

Mr. E. G. Bilham.—*Recent Improvements in Meteorological Anemometry.*

A brief description is given of the modifications introduced into the design of the standard Dines Tube Anemograph as a result of investigations carried out at the

National Physical Laboratory between 1926 and 1928. As compared with earlier patterns the modified instrument represents a substantial gain in accuracy in recording both gusts and mean wind velocities.

Details are given of the present distribution of anemographs from which data are regularly published, with special reference to instruments of the new pattern. The introduction of a non-liquid type of recorder has made it possible to obtain records of relative wind velocity at sea in recent years.

Particulars are also given of the three-cup Robinson anemometer designed by J. Patterson of the Canadian Meteorological Service, and a comparison is made between it and four-cup systems formerly used by the Meteorological Office, London, on small anemometers.

Mr. M. A. Giblett.—Reference is made to full scale experiments in the measurement of wind speed and direction at the Meteorological Office, Royal Airship Works, Cardington, Beds, on a very open time scale so revealing short period fluctuations and typical records are shown.

## Dr. W. S. Tucker.—*The Screening of Southend from Gun-Fire.*

The variability of ranges of audible sound in the open air was made the subject of special study by Tyndall, who conducted exhaustive experiments for Trinity House. The relationship between range of audibility and meteorological conditions was worked out by Stokes, Osborne Reynolds, Rayleigh and Milne in terms of variation of the velocity of sound with height above the earth's surface, the velocity being affected by wind and temperature. Recent advances in meteorological observation have made it possible to forecast to some extent the refraction of sound in any given direction, using aeroplane observation for upper temperatures and pilot balloons for wind.

For explosions produced at a given point on the earth's surface it is possible from the meteorological data so obtained to discover if any other point on the surface is within sound shadow and to determine with some degree of approximation the minimum depth of that shadow.

The region surrounding Southend-on-Sea, which is five miles from the firing point of heavy guns, is severely exposed to sound and low frequency disturbance under those conditions under which there is an increase of velocity of sound with height, towards the point of observation.

This velocity gradient is not in general constant as height increases, but for the purpose of forecasting, the nearest linear relationship of velocity with height has been adopted. The depth of sound shadow over the Southend area, with the gun as a source of sound, has then been calculated out for various directions of wind and various wind and temperature gradients, and where this depth becomes zero the point of observation is subject to the full violence of the disturbance.

As an aid to the rapid forecasting of the extent of the region in shadow, a number of celluloid discs have been produced which are capable of rotation about the gun as centre. The depth of sound shadow for a range of five miles has been indicated by lunar shaped areas extending half round the circle, and of maximum thickness increasing with increase of wind gradients.

Another set of discs deals with shadows produced by temperature refraction and consists of circles thicked into black rings for negative temperature gradients and red rings for positive gradients. These discs can be superimposed on the wind refraction discs so that the blackened area due to wind can be extended or diminished by the black or red areas due to temperature.

This model is in actual use and is employed before the firing programme is carried out, the meteorological data above referred to being obtained just prior to the occasion.

The efficacy of this method of selecting suitable times of firing to give immunity to the Southend area from gun disturbance, has been proved by microphone and other observation on the days in question.

The paper is illustrated by records of microphone disturbance obtained under various types of weather condition.

## Report of Committee on *Seismological Investigations.*

## Department of Mathematics (A*).

### Mr. H. S. M. Coxeter.—*Regular Polytopes.*

Every regular polytope can be represented by a symbol of the form

$$\{k_1 , k_2 , \ . \ . \ . \ k_{m-1}\} ,$$

$m$ being the number of dimensions. *E.g.* $\{5\}$ is the pentagon, $\{\frac{5}{2}\}$ the pentagram, $\{4, 3\}$ the cube, and $\{3, 4\}$ the octahedron. The problem before us is to find what values of the $k$'s are admissible.

In this paper, it is shown that every regular polyhedron (*i.e.* three-dimensional polytope) corresponds to a rational solution of the equation

$$\cos^2 \frac{\pi}{k_0} + \cos^2 \frac{\pi}{k_1} + \cos^2 \frac{\pi}{k_2} = 1.$$

Then this criterion is extended to more dimensions, thus leading to a complete enumeration of the regular polytopes.

Finally, some account is given of 'density' (*i.e.* the number of times the boundary of a polytope encloses its interior). This quantity takes the values 1, 3, 7 in three dimensions; and 1, 4, 6, 20, 66, 76, 191 in four. Certain five-dimensional polytopes, although they satisfy the above-mentioned (extended) criterion, have to be excluded on account of infinite density.

### Mr. P. Du Val.—*Some Relations between the Theory of Polytopes and Algebraic Geometry.*

The insight into the structure of certain groups (namely, the groups of linear fractional transformations of a complex variable) afforded by the remark that they are simply isomorphic with the groups of rotational symmetry of the regular polyhedra in three dimensional space, suggests the enquiry whether any other groups arising in analysis or algebraic geometry are amenable to a similar treatment. The purpose of the paper is to answer this question in the affirmative by means of a set of examples.

The *uniform polytopes* of higher space have been completely enumerated by Elte and Coxeter. It was remarked by Schoute that the 27 vertices of a certain polytope in six dimensions correspond in a special way to the 27 lines on the general cubic surface, and the group of symmetry of this polytope is simply isomorphic with the group of the 27 lines. It can be found that similar results hold for the finite systems of lines on all the surfaces of del Pezzo, as well as for many other finite systems of entities occurring in geometrical configurations, such as :

The 28 bitangents of the general plane quartic curve ;

The 120 tritangent planes of the twisted sextic of genus 4, which lies on a quadric cone ;

The 15 planes and 10 nodes of Segre's cubic primal in four dimensions ;

The 16 nodes and 16 tropes of the Kummer surface ;

The finite systems of rational curves of various orders on the surfaces whose prime sections are hyperelliptic.

Infinite polytopes (or space fillings) can be found whose groups of symmetry are simply isomorphic with certain enumerably (discretely) infinite groups of transformations connected with the figures of nine associated points in a plane, and of eight associated points in space.

### Mr. W. V. D. Hodge.—*Topological Methods in Algebraic Geometry.*

The importance of the concept of the Riemann surface in the theory of algebraic curves is well known to all mathematicians. Owing to certain superficial difficulties the use of topological methods has received comparatively little attention in discussions of varieties of more than one dimension, though Picard has shown that such methods can be very powerful in the theory of surfaces. In recent years, however, Lefschetz and other workers in America have developed the theory of the analysis situs of algebraic varieties and have shown that many problems in algebraic geometry can be simplified greatly by considerations of topology. In this paper a survey is given of recent work in this connection, and it is shown how Riemann's relations between the periods of Abelian integrals and the inequalities satisfied by the real and imaginary parts of the periods can be generalised to apply to Abelian integrals of any multiplicity.

Mr. L. C. Young.—*Continuous Groups and the Foundations of Geometry.*

Dr. H. W. Richmond.—*A problem on Cubes of Rational Numbers.*

The object of this paper is to call attention to some work, published half a century ago and apparently overlooked, upon an arithmetical problem closely allied to problems which have recently been discussed at considerable length. With a few unimportant exceptions, all rational numbers are expressible as a sum of cubes of two rational numbers either in an infinity of ways or not at all. Sylvester and Pepin discovered many types of numbers of the latter class; but of rules for recognising numbers of the former class or of methods for resolving them into a sum of cubes, very little is known. Sylvester, however, made the statement that he had succeeded in resolving all the whole numbers up to 100, other than those excluded by his rules, with the single exception of 66. He did not publish his results; and so the problem has been left.

Prof. P. J. Daniell.—*The Mathematical Theory of Flame Motion.*

A new formula for the speed of propagation of flame into unburnt gas is obtained by considering the rate of variation of combustion with temperature. The resulting differential equation leads, after somewhat intricate approximations, to a formula

$$V^2 = k\,(T_o/T)\,S^2\,exp\,(-E/RT)$$

where S is the speed of sound at the initial temperature $T_o$, while T is the final temperature; $k$ is a pure constant depending mainly on the proportions of the mixture.

# SECTION B.—CHEMISTRY.

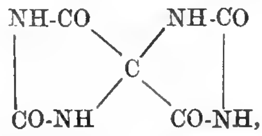
## Thursday, September 4.

**Presidential Address** by Prof. G. T. Morgan, O.B.E., F.R.S., on *A State Experiment in Chemical Research.* (See p. 38.)

Prof. Sir William Pope, K.B.E., F.R.S., and Dr. J. B. Whitworth.—*The Resolution into Optically Active Components of the Spiro*-5:5-*Dihydantoin.*

Several spiranes of enantiomorphous molecular configuration which contain no asymmetric carbon atom in the molecule have been resolved into their optically active components; the spiranes thus hitherto studied have been of considerable molecular complexity.

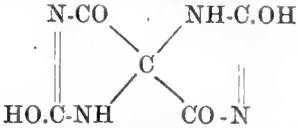
It seemed to us desirable to attempt the resolution of an enantiomorphous spirane of very simple constitution in the hope that the examination of simple substitution derivatives of such optically active substances would yield further information concerning the stability of molecular complexes. For our purpose we chose the spiro-5:5-dihydantoin




described by Biltz and his pupils in 1917. On fractional crystallisation of the brucine salt of the synthetic material from water, *l*-brucine-*l*-spiro-5:5-dihydantoin is readily obtained in clusters of thin, colourless needles melting at 260°; from this the lævo-spirodihydantoin is separated in the usual way.

The specific rotatory power of *l*-spiro-5:5-dihydantoin for the mercury green line is, in ethyl alcohol solution, $[\alpha] = -113°$ and, in aqueous solution, $[\alpha] = -115°$.

In dilute aqueous ammonia, however, the specific rotation changes widely with the concentration of the alkali; thus, in a 1·75 per cent. solution in 12 per cent. aqueous ammonia solution gave a specific rotatory power of $[\alpha] = +8{\cdot}7°$. This curious reversal of the sign of the rotatory power suggests that the constitution of the dihydantoin changes in alkali solution to the tautomeric form




Biltz recognised that the spiro-dihydantoin exists in this form in its diacetyl-derivative, and it is generally agreed that uric acid also assumes the enolic form in its metallic salts. The present work furnishes strong confirmation of the view that the numerous different types of compound containing the group -CO-NH-, which yield metallic derivatives, do so in the enolic form.

Whilst externally compensated spiro-5:5-dihydantoin is very sparingly soluble in alcohol and water, its optically active components are readily soluble; so great is the difference in solubility that *d*-spiro-5:5-dihydantoin can be separated in an almost pure state from the optically impure material recovered after part of the *l*-isomeride has been removed as brucine salt. On washing this mixture with cold alcohol, the *d*-compound dissolves whilst the *dl*-substance remains undissolved. Further, on mixing cold concentrated alcoholic solutions of *d*- and *l*-isomerides the *dl*-substance separates in small colourless needles; it is thus proved that the externally compensated substance is a true racemic compound.

We are now studying the substitution derivatives of the spiro-5:5-dihydantoin, in connection with which interesting problems in stereochemistry and tautomerism obviously arise.

Prof. F. E. Francis, Dr. T. Malkin, and Mr. S. H. Piper.—*Natural Fatty Acids.*

Dr. M. Nierenstein.—*Pyrylium Series.*

Afternoon.

Visit to The Bristol Potteries.

Visit to the works of the St. Anne's Board Mill Co.

## Friday, September 5.

**Discussion** on *The Present Position of the British Dyestuff Industry.* (Prof. A. G. Green, F.R.S.; Prof. J. F. Thorpe, C.B.E., F.R.S.; Mr. T. Taylor; Sir William Pope, K.B.E., F.R.S.; Prof. G. T. Morgan, O.B.E., F.R.S.; Mr. C. J. T. Cronshaw; Mr. J. Rogers; Mr. J. Morton; Sir Joseph Turner; Mr. W. J. U. Woolcock, C.B.E.)

In opening this discussion on the present position of the British dyestuffs industry Prof. A. G. Green, F.R.S., continues his survey of the progress of this industry since 1901 when he spoke on a similar topic at the Glasgow Meeting of the British Association. He outlines the deplorable position of the British enterprise in dyes at the outbreak of war in 1914, and describes the emergency measures which then had to be taken to ensure adequate supplies for our country and its allies. The renaissance of the British industry dates from that period. He then refers to the changes in organisation which occurred during the latter part of the war period, and thus leads up to the state of affairs which immediately preceded the legislative measure known as the Dyestuffs (Import Regulation) Act which came into force in January 1921 to continue until

January 15, 1931. During the lifetime of the Act, British dye manufacturers have continued to make commendable progress, and the author compares their productive capacity with those of their rivals in the other principal dye making countries. The speaker deduces from these recent developments certain inferences which should influence Governmental action in the immediate future.

Prof. J. F. Thorpe, F.R.S., President of the Chemical Society, and Member of the Dyestuffs Industry Development Committee, refers to the marked progress in dyestuff production during the last ten years and to the beneficial effect of the Dyestuffs Act on this development. He instances several important technical and scientific problems which still remain to be solved. Changes in modes and fashions must be expected; the introduction of artificial textile fibres is in itself a revolution. Dyes in considerable vogue at present will be obsolete in a few years, and to meet these inevitable changes strong and well-equipped schools of organic research are required in close touch with a progressive industry capable of making use of trained chemists not only in dye production but in all other branches of the fine chemical industry.

Mr. Tom Taylor, a member of the Licensing and Development Committees appointed under the Act, discusses the manufacture of lake pigments as an important addition to the production of dyestuffs. During the last seven years importation of lake colours has almost ceased, thus enabling the British lake makers to establish an important trade to the advantage of the lake consuming industries. The production of British-made printing ink is now three times greater than it was in 1913, and the total output of British lake colours is ten times that of the same year. This improved position the speaker attributes almost entirely to the Act.

Among the subsequent speakers representing the scientific side of the chemistry of colouring matters are Sir William Pope, Prof. Armstrong and the President of the Section, and on the technical and manufacturing side Messrs. C. J. T. Cronshaw and J. Rogers of the Imperial Chemical Industries and Mr. J. Morton (Scottish Dyes, Limited).

The discussion is summed up by Mr. W. J. U. Woolcock, Chairman of the Dyestuffs Industry Development Committee.

AFTERNOON.

Visit to The Broad Plain Soap Works, Bristol.

## Monday, September 8.

**Discussion** on CHEMOTHERAPY.—

(*a*) Dr. F. L. PYMAN, F.R.S.—*Antimalarials and Antiseptics.*

(*b*) Dr. T. A. HENRY.—*Derivatives of Quitenine.*

(*c*) Prof. R. ROBINSON, F.R.S.—*New Antimalarials.*

(*d*) Dr. H. KING.—*Mechanism of the Action of Arsenicals.*

(*e*) Dr. JESSIE STEWART.—*New Arsenicals and Antimonials.*

(*f*) Prof. G. T. MORGAN, F.R.S.—*The Composition of the Antimony Analogue of Tryparsamide.*

(*g*) Prof. C. S. GIBSON.—*Recent Investigations of Heterocyclic and Homocyclic Arsenicals.*

(*h*) Prof. G. BARGER, F.R.S.—*The Action of Chemical Substances on Teredo.*

Prof. E. C. C. BALY, C.B.E., F.R.S.—*Further Studies of Photosynthesis of Carbohydrates.*

As has already been described, the photosynthesis of carbohydrates can be achieved by the irradiation of carbonic acid adsorbed on a suitable surface in the form of a very fine powder. The material used in the earlier experiments was nickel carbonate, which, however, required previous activation and was only effective for about two hours.

Considerable advantage is gained by the use of supported $Fe_2O_3$ as catalyst, the oxide, containing some $ThO_2$ as promoter, being deposited on kieselguhr which has previously been coated with Al $(OH)_3$. It has been found that the activity of the powders is proportional to the magnitude of their electropositive charge in water saturated with carbon dioxide. This fact has rendered possible determinations of the rate of poisoning by the oxygen set free during the photosynthesis and the rate of de-poisoning by carbonic acid. This has enabled the optimum intensity of the light to be measured for the process to become continuous.

The activity of the powders varies in a remarkable way with the amount of $ThO_2$ present in the $Fe_2O_3$, sharp maxima being observed when the $ThO_2$ content is about 1·31 and 2·12 per cent., with minima on either side of these amounts. The maximum yield yet obtained of carbohydrates is 0·256 grm. with 100 grms. of powder in two hours.

Afternoon.

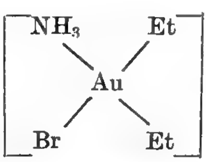
Visit to The Bristol Potteries.

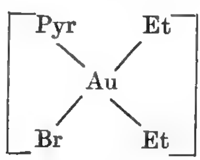
## Tuesday, September 9.

Prof. M. W. Travers, F.R.S.—*New Experimental Methods for the Study of Gas Reactions.*

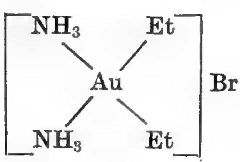
Prof. C. S. Gibson and Prof. J. L. Simonsen.—*Recent Investigations of Organic Compounds of Gold.*

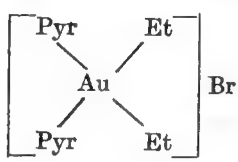
Diethyl gold bromide, originally investigated by Pope and Gibson (*Journ. Chem. Soc.*, 1907, 2061) readily forms co-ordination compounds. Of these, *monoammino-bromodiethylgold* and *monopyridinobromodiethylgold* :



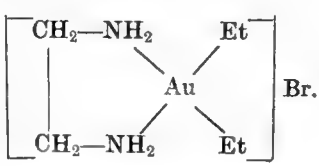




are colourless, stable, non-ionisable compounds produced from the unstable salts :



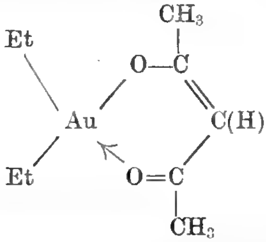




first formed by the action of ammonia and pyridine respectively on diethylgold bromide. *Ethylenediaminodiethylgold bromide* is a colourless, stable salt having the constitution




In all these compounds, gold has a co-ordination number of four.

*Diethylgold acetylacetone* formed by the interaction of diethyl gold bromide on thallous acetylacetone is a colourless liquid, m.p. 9–9·5°, having the constitution




its physical properties being in marked contrast to those of dimethylthallium—and trimethylplatinum acetylacetone, which are colourless, high melting solids recently described by Menzies. Diethylgold acetylacetone undergoes spontaneous decomposition by light and also on being gently heated, beautiful gold mirrors of varying thicknesses being obtained. From this compound, by the action in alcoholic solution of sodium iodide, *diethylgold iodide* has been obtained.

The investigation is being extended in various directions.

## Dr. R. C. Menzies.—*Some Organic Compounds of Thallium.*

The thallous atom, in virtue of its strongly electropositive nature, replaces the hydrogen atoms of many hydroxy compounds—including simple and polyhydric alcohols of the aliphatic series—with formation of stable compounds, which are generally less soluble than those of the alkali metals and consequently more easily prepared.

In many cases, all the hydroxyl hydrogen atoms of polyhydric alcohols can be thus replaced, dithallium ethylene glycol, trithallium glycerol, tetrathallium erythritol, and hexathallium sorbitol being cases in point.

The hydroxyl hydrogen of hydroxy acids can also be replaced by thallium as well as the carboxyl hydrogen, dithallium glycollate, tetrathallium d-tartrate, tetrathallium meso-tartrate, and hexathallium gluconate having been prepared by treating the corresponding acids or their normal thallium salts with the necessary excess of aqueous thallous hydroxide solution.

Well-defined thallium salts of the reducing sugars have not yet been prepared owing to the reduction of the metal with oxidation of the sugars and consequent formation of mixtures of oxidation products.

Methyl arabinoside, however, and α and β-methyl glucosides and sucrose in which the reducing groups are protected, all give well-defined thallium derivatives, forming respectively trithallium methyl arabinoside, trithallium α- and trithallium β-methyl glucoside, and tetrathallium sucrose. It will be noticed that while all three of the replaceable hydrogens in methyl arabinoside can be exchanged for thallium, in the case of the methyl glucosides one of the four available hydrogens escapes replacement, while sucrose on treatment with varying quantities of thallous hydroxide yields tetrathallium sucrose in which four only of the eight available hydrogen atoms are replaced.

An explanation of these observations, which it is hoped to extend to other sugars, has not yet been found, although it is significant that while tetrathallium tartrate is easily prepared, the dimethyl thallium radical, which must take up more space than the thallium atom only forms a tridimethyl thallium tartrate.

Beginning with the simplest cases, attention may be called to Sidgwick's and Sutton's recent paper showing that thallous ethoxide has in benzene solution a fourfold molecular weight, and to their suggestion that the thallium and oxygen atoms may be arranged at alternate corners of a cube, the ethyl groups being attached, naturally, to the oxygen atoms. If one of the β-hydrogen atoms in each ethyl group be now replaced by OTl, a possible configuration for the thallium salt of ethylene glycol is seen to be an arrangement of cubes having oxygen and thallium atoms at alternate corners, each oxygen atom being joined to the oxygen atom at the corner of the adjacent cube by the group—$CH_2.CH_2$—. This suggestion is, of course, put forward with all reserve.

But the writer is of opinion that before any explanation of the results outlined above can be forthcoming, it is necessary to explain the differences between thallous ethoxide and dithallium ethylene glycol, which are striking. He would welcome further suggestions.

Dr. H. T. S. Britton; Dr. R. A. Robinson, and Mr. W. L. German.—*Complex Acids of the Rarer Elements.*

After briefly referring to electrometric titrations—potentiometric and conductometric—of normal monobasic and polybasic acids, it is shown how these methods have been applied to the study of tungstic, molybdic and vanadic acids. The work so far done reveals that these acids are by no means similar to the corresponding acids formed by the earlier members of groups 5 and 6 respectively of the periodic classification as regards their electrolytic dissociation and chemical behaviour. It is a surprising fact that the tungstic and vanadic oxides are not readily precipitated from solutions of their salts on acidification in spite of their almost complete insolubility in water. The observations indicate that there are present in acid solutions alkali metal salts of ill-defined polyacids, which tend to resist decomposition except by a large excess of acid.

Dr. F. G. Soper.—*The Effect of the Solvent on Reaction Velocity.*

The relation of reaction speed in solution to the cohesion of the solvent and to the relative cohesions of the products and reagents (Richardson and Soper, J. C. S., 1929, 1873) may be explained in terms of a solvent effect on the critical complex, controlling the proportion of critical complexes breaking up to form the reaction products.

By using the velocity equation for a homogeneous gas reaction to express the rate at which critical complexes are produced and by assuming that the fraction of complexes breaking up to form the products is connected with the relative entropies of reagents and products, a reaction velocity equation is obtained which is in conformity with the equilibrium requirements and which also expresses the connection between reaction speed and the cohesion of the solvent.

Afternoon.

Visit to the National Smelting Co., Ltd., Avonmouth Factory, Avonmouth.

## SECTION C.—GEOLOGY.

### Thursday, September 4.

Prof. S. H. Reynolds, Mr. J. W. Tutcher, and Prof. L. S. Palmer.—*The Geology of the Bristol Area.*

Mr. E. H. Davison.—*The Granite of Western Cornwall, its Alteration and Contact Metamorphism.*

In the paper West Cornwall is taken as that part of the county from Land's End to east of the Hensbarrow or St. Austell granite outcrop. The various granite outcrops inside this area are considered to be the denuded cupolas of an underlying mass of potash granite which, though showing a certain amount of variation in composition, is believed to be one intrusion.

The sequence of events was :—

(*a*) emplacement of the granite mass,
(*b*) intrusion of pegmatite, aplite and 'elvans' (quartz porphyries of various types),
(*c*) uprise of fluids charged with compounds of boron and fluorine.

The alteration of the country-rocks (chiefly slates) is due to (*a*) and (*c*). In the aureoles the slates show the following changes as the granite is approached :

(*a*) spotting,
(*b*) development of chiastolite, andalusite or ottrelite,
(*c*) development of micas,
(*d*) conversion to hornfels.

Basic dykes and sills have also been made into epidiorites.

The fluids, hot and chemically active, converted the more argillaceous slates into tourmaline rocks, while more calcareous types became calc-silicate hornfels with the development of axinite, epidotes and garnets. The granites and porphyries were also attacked and gave rise to tourmaline-bearing rocks, greisens or china-clay.

The mineral lodes also belong to this period of activity, and their contents vary with their position relative to the granite surface. Certain zones can be established :—

i. iron-manganese zone,
ii. lead-zinc-silver zone,
iii. copper arsenic-wolfram tin zone.

i and ii are found above the granite surface, whilst iii, which can be further subdivided, passes from just above the surface to well below it. Two types of veinstone are common 'Peach,' a chlorite-quartz veinstone, rich in cassiterite, and 'Capel,' a brecciated tourmaline-quartz rock.

### Dr. G. Slater.—*The Glaciated Rock-surfaces of Nooitgedacht, and the Upper Dwyka Boulder-shales of Griqualand West.*

I. *Nooitgedacht* is the name of a farm situated on the left bank of the River Vaal, a dozen miles north-west of Kimberley. A small area of the farm is occupied by a subsidiary valley a mile in length, which exhibits striking evidence of a 'fossil Glacial Landscape' of Carboniferous age. The valley is a rock basin with a glaciated floor, subsequently converted into a glacial lake with deposition of varve-shales. The surface of the floor is now a limonitic pseudomorph, which has preserved all the evidence of abrasion and polishing by land ice such as :—Striations, chatter-marks, differential abrasions, grooves, and glacial shelves. Isolated bosses of rock exhibit examples of crag and tail associated with *roches moutonnées*.

This area has now been protected, thanks to the public spirit of the De Beers Corporation, Kimberley.

II. *The Upper Dwyka Boulder-shales.* The line marked by the Harts-Vaal-Orange Rivers from Vryburg to Prieska marks the drainage system of the pre-glacial Kaap Valley which guided the Transvaal Ice Lobe south-west. Retreat of the ice was followed by invasion of the sea or lake, resulting in the deposition of the Upper Dwyka Boulder-shales to a maximum depth of about 200 feet.

These bedded shales are interspersed with numerous erratics of all sizes dropped from floating ice. Near the base of the shales are lenticles of tillite-like material, microscopic examination of which throws new light on glacial sedimentation.

At Douglas the shales contain 'boulders' of tillite-like material, and these beds are overlain by varve-shales with cone-in-cone structure in the calcareous layers.

*Infra-formational Folding.* At De Kalk on the Riet River a fine section of the Upper Dwyka Boulder-shales exhibits infra-formational folding in the lower part of the exposure, apparently due to the stranding of ice-floes or pack-ice possibly associated with pressure-ridges.

The author expresses his thanks to the British Association and the Royal Society for grants in aid of this investigation in the summer of 1929.

### Prof. J. W. Gregory, F.R.S.—*The recent Cable Fractures in the Western Atlantic.*

### Mr. S. H. Straw.—*On the Fauna of the Palæozoic Rocks of the Little Missenden Boring.*

The boring put down at Little Missenden (Bucks.) reached Palæozoic rocks at a depth of 1,200 feet, and remained in them until it was discontinued at 1,264 feet.

The fauna of these rocks, though resembling that of the Upper Ludlow, has few species identical with those of the latter formation. On the other hand, the fish remains as well as certain of the invertebrate species possess undoubted Lower Devonian affinities.

Up to the present, however, it has not been found possible to make a direct correlation with any of the known Downtonian or marine Lower Devonian faunas of Western Europe. The Missenden fauna does not appear to have been recognised *in situ* elsewhere in Europe. It may be present in certain East Prussian drift blocks whose exact source of origin is unknown, but which may have been derived from the floor of the Baltic Sea.

In North America several elements of this fauna are common in the highest beds of the Stonehouse formation at Arisaig, Nova Scotia.

AFTERNOON.

Excursion to Portishead and Clevedon.

## Friday, September 5.

**Discussion** on *The Validity of the Permian as a System.* (Ordered by the General Committee to be printed in extended form.)

Dr. BERNARD SMITH.—The validity of the Permian as a system, or as to what its limits should be, even if accepted as a system, has already given rise to much discussion and bids fair to cause more controversy and expenditure of energy than that attaching to the limits of the Cambrian and Silurian Systems in the days of Murchison and Sedgwick.

The question appears to depend largely upon two points :—

1. The definition of a system.
2. The determination of the vertical range of the (so-called Permian) rocks that naturally fall into such a system.

Geikie has laid it down that a system or period should contain a number of Series, Formations or Epochs ; a Series a number of Groups or Stages ; and a Group, two or more Beds which in turn may be split into Zones. Again, a System should show, *in general*, a fauna and a flora that is distinct from that above or below and characteristic in certain striking cases.

If the stratigraphical record, or even large parts of it, were perfect, and represented by a series of rocks deposited (in the sea or even in land-locked continental areas) in uninterrupted sequence, geological history would show a blending and transition from period to period, and no sharp division could be made. But the record has been constantly interrupted, and these interruptions have served two purposes :—

(1) Firstly, the geographical and topographical changes introduced by widespread earth movements have *either* given a spurt to *or* put a check upon evolutionary processes—affecting in the course of time even those areas where sedimentation was continuous.

(2) Secondly, have caused in certain areas a suppression—by denudation or non-deposition—of parts of the record.

It is interruptions of the second type that have been seized upon most eagerly by stratigraphers in the past for drawing their boundaries between one formation, or one system, and another, and for arranging their history into chapters.

This works very well locally, but since Systems have been named, and limits have often been given from localities in which a series of rocks has been first studied in detail and presumed to be typical but which may be curtailed by a considerable hiatus at the base, and even at the top also, the geologist begins to find difficulties when he tries to push his systems farther afield, and has to take, in part or whole, of what may be termed the transition or 'liaison' deposits between his system and the one above or below in cases where the succession has been virtually unbroken. He then finds that he must limit his system by faunas and floras and not by unconformities. We have well-known instances of this in our attempts to separate Cambrian from Ordovician, Silurian from Devonian, Cretaceous from Eocene, and so on in different parts of this country and of the world.

The question before us to-day is due to similar causes. In some parts of the world diastrophic movements have caused the suppression of basal beds which if deposited would have been claimed by certain geologists as Permian ; in other districts either there is or it is claimed there is a stratigraphical break between Permian and Trias. On the other hand, there is in some districts an apparent perfect passage up from the Carboniferous, or up into the Trias.

A much thicker belt of rocks is now claimed by certain geologists as Permian than Murchison and his colleagues ascribed to that formation when it was first established.

The Permian of Murchison begins with the Kazan group and closes with the lower part of the Tartarian, both correlated with the Upper Permian or Zechstein of Germany. These are underlain by the Kungur and Artinsk beds which overlie unmistakable Upper Carboniferous rocks and which are now claimed by many as Permian.

Again, marine developments in other areas containing Ammonites, as does the Artinsk, are considered to be of slightly older Permian age than the latter, namely, the Lower Wolfcamp formation of Texas and the lowest zone of Timor (Somohole). 'The fauna of Timor,' says J. P. Smith [1927], 'is the richest in the world in genera, species and number of individuals collected. . . . The forbears of all the stately and beautiful genera of the Triassic may be seen in the simple and unpretentious forms of the Permian.'

With regard to the limits of the rocks at present claimed as Permian, there is within them ample room for a system. Schuchert, for example, who in 1928 published a review of the Permian with special reference to the glaciation in Permian times (and who, if he stands alone in his total claim, is supported by various individual claims), has no doubt whatever that the system is valid, and concludes that all the tillites—whether considered by others to be Permian, Permo-Carboniferous, or Carboniferous—are not only of Permian age but Middle Permian. This is a bold statement which will probably be combated strongly in various quarters.

To mention only two cases: he attributes from 4,000–6,000 ft. of sediments, more than one-half limestones, in the Glass Mountains of Texas to the Lower and Middle Permian, and these are surmounted by 2,500–3,000 ft. of Upper Permian equivalent to the Zechstein. In the Karroo he classifies the Lower Beaufort Series as Upper Permian, averaging about 8,000 ft.; on the Basal Upper Permian or Ecca Beds, from 2,000–3,000 ft. The Dwyka Series, up to 2,900 ft., he ascribes to Middle Permian.

With regard to the Permian fauna, 'Diener (1921) tabulates 193 forms of Ammonites, to which 17 others have been added (1928), making 210. These are grouped in 41 genera: 11 come up from the Upper Carboniferous, 10 going into the Trias, leaving 22 restricted to the Permian.'

In the American Permian the reptiles form a striking and characteristic group.

Some such enlargement of the 'System' as originally defined, although perhaps not acceptable in its entirety, is no argument against acceptance of the Permian as a 'System' so long as it fulfils the other requirements as defined above. Do the rocks which are nowadays ascribed to the Permian by Schuchert and others contain, in general, organic remains of types showing distinctive characters which link them together as a whole and which distinguish them from those above and below? This implies more than normal evolutionary changes throughout.

Let us now consider the other side of the question, and this brings us nearer home.

In England a series of breccias, conglomerates, sandstones, marls or shales and limestones, overlying Coal Measures of normal aspect, used to be termed the Poikilitic rocks or New Red Sandstone. Later they were divided into two systems, Trias and Permian. As time went on more of the lower part of the 'Permian' (supposed to correspond to the Rothliegende) was absorbed into the Upper Coal Measures—so much so that in the Midlands very little of what was once designated Permian is left.

On the eastern flank of the Pennines the higher parts of our representatives of the Permian (correlated with Zechstein and part of the Rothliegende) are considered by my colleague, Dr. Sherlock, to be homotaxial with part of the Bunter, and he—considering not only this country—would abolish the Permian altogether, classifying all rocks above the Hercynian diastrophism in this country up to the Rhaetic as an 'Epeiric' System at the base of the Mesozoic. There are indeed greater signs of unconformity in places within the Trias itself in this country than between Permian and Trias.

For my own part I may state that in W. Cumberland and part of the Vale of Eden the Permian deposits appear to pass up conformably into those classed as Trias. So gradual is the change and so interrelated do the deposits seem to be that the beds as a whole were formerly termed the New Red Series (which, however, included some Upper Coal Measures). Later they were separated into Permian and Trias, then the St. Bees Sandstone (supposed Bunter) was put back into the Permian, and finally everything above the Magnesian Limestone was classed as Trias.

Now it is considered that the St. Bees Shales overlying the Magnesian Limestone and passing up into the St. Bees Sandstone are 'liaison' deposits between the Permian and the Trias, but it is still difficult to accept this horizon as the boundary between the Palæozoic and Mesozoic eras, as it is bridged by the deposition of breccias formed under similar climatic conditions throughout Permian and early Triassic times.

It would be, indeed, a great convenience to British geologists if we could consider the rocks deposited after the zenith of the Hercynian movements in this country as belonging to the Mesozoic, and if we could combine the Permian and Trias into one system; but in this we might be ignoring the significance of the great unconformity due to the movements. Our Permian rocks may be but poor representatives of the Upper and a little of the Middle Permian.

In dealing with continental and non-fossiliferous deposits, one is apt to fall easily into traps. Dr. Sherlock thinks that the St. Bees Shales represent the Middle Permian Marl. This cannot be proved; but since we now consider the Magnesian Limestone of Whitehaven to be the Middle Limestone—the St. Bees Shales *may* represent the Upper Limestone—the Upper Permian Marls and part or the whole of the Lower Mottled Sandstone—who can tell?

To my mind the evidence for establishing the validity or otherwise of the system does not occur in this country. To attempt to tackle the problem at this end appears to me to be like grasping the wrong end of the stick.

Prof. D. M. S. Watson, F.R.S.—It is the commonly accepted belief of geologists that Systems should be defined as the periods of time between great diastrophic events, which will produce either marked unconformities or great changes of fauna, effects visible over a large part of the world. Thus the rocks which belong to a definite system should not be connected by passage beds to those of the next and should in general be unconformable to those below.

Application of these criteria to Britain would lead to the abolition of the Devonian System, the Lower Old Red Sandstone being added to the Silurian and the upper to the Carboniferous, of the Permian System whose lower half would form the top of the Carboniferous, whilst the rest would form a beginning of the Trias, and of the Jurassic which passes down conformably and through passage beds into the Trias. Thus British geologists have not in fact delimited Systems by the criteria which are assumed to be used.

The validity of a system depends on its usefulness; and this on the length of time it represents as judged from thickness of strata and from the extent of evolutionary change which takes place within it.

The Amphibia and bony fish of the Lower Carboniferous of Scotland do not differ recognisably in their evolutionary grade from those of the Middle Coal Measures of Europe and N. America, which represent the same facies.

The Amphibia from the Stephanion of Nyram are clearly somewhat more advanced than those of Linton, Ohio, and other Middle Coal Measure faunas.

The Lower and Middle Rothliegende Amphibian faunas of Europe clearly represent in part a further development of those of the Coal Measures and of Nyram, but they include a few rare forms which agree with the far larger dry land fauna of the Artinskian of Texas, whose age is fixed by interbedded ammonites.

The Tetrapod fauna of the Copper-bearing Sandstones of the Urals, which are supposed to be about of the age of the Kupferschiefer, contains some animals (Zygosaurus) which tie it to the Texan fauna and others which are morphologically intermediate between the Texan Pelycosaurs and the S. African Tapinocephalus zone Deinocephalia.

The Karroo System exhibits the steady evolution of a fauna from the Tapinocephalus zone M. Permian, through the Cisticephalus zone whose fauna agrees with the Zechstein fauna of the North Dwina to the L. Triassic Cynognathus zone.

This series is then interrupted by the Coal Measures of the Molteno series which yield no identifiable reptiles, and the fauna of the overlying Red Beds, though theoretically capable of derivation from that of the lower zone, is far more advanced and agrees with those of the Upper Trias of the Northern Hemisphere.

The evolutionary change which is shown by a comparison of the Reptiles and Amphibia of the U. Permian Cisticephalus zone with those of the L. Permian of Texas is very great indeed, vastly more important than that which distinguishes the Stephanian from a Lower Carboniferous fauna and at least as great as that which occurs during the Trias.

A comparison of the U. Permian fish from Madagascar with a fauna of the Rothliegende shows a similar change.

Thus it is probable that the Permian System represents a space of time as long as the Carboniferous, and it is therefore convenient to maintain it.

Dr. R. L. Sherlock.—Ever since its foundation the existence of the Permian System has been a matter of dispute. Many geologists would place the strata so called in the Carboniferous, others in the Triassic System, and these opposite views have resulted in the retention of the Permian. The reason for the different views is that the Permian consists of a lower division normally conformable to the Carboniferous and containing Palæozoic forms of life; and an upper division conformable to the Trias and containing, besides residual forms, Mesozoic animals and plants. The supporters of the Permian System believe it to contain a fauna and flora intermediate in character between Palæozoic and Mesozoic types. This is mainly due to adding together the totally different lists of forms from the lower and upper divisions and so arriving at a passage fauna and flora.

The lower and upper divisions have different distributions in space and if found together are often unconformable. In Britain the lower division is generally recognised as Upper Coal Measures, while the upper division passes not only upwards but laterally into Trias.[1] The Lower Permian should be added to the Carboniferous and the Upper Permian to the Trias. As the Rhaetic, at least in Western Europe, is separated from the Trias and closely connected with the Jurassic, it is suggested that the Upper Permian (Zechstein) and Trias, with the Rhaetic taken away, form a natural system which may be called the Epiric (continental) System.[2]

In Lancashire both Lower (Collyhurst Sandstone) and Upper (Marl with Limestones) Permian pass westward into 'Bunter'; in West Lancashire the Bunter represents the Permian as well as the Lower Trias.

The type of the Permian System is Russian and is continental in character. The life is scanty and exceptional. It appears that, in Russia, there is a lower (Artinskian) division succeeding the Carboniferous conformably, and cut off by a break from red beds which are the top of the System and apparently represent the Zechstein. The Artinskian may well be attached to the Carboniferous and the unconformable red beds to the Trias. In Germany the lower division is divided into two very unequal parts, the Lower and Upper Rothliegende. The lower part is always conformable to the Carboniferous while the thin upper part is unconformable to the lower, but conformable to the Zechstein, of which it is probably merely the base (as in Yorkshire). Probably the division between Carboniferous and Epiric falls between the Lower and Upper Rothliegende. During the Lower Rothliegende there were earth movements and volcanic activity which ceased before the Upper Rothliegende and Zechstein were laid down.

In North America red beds are found in places as an addendum to the Carboniferous and are followed by great earth movements. It is agreed that the Upper Permian (Zechstein) is absent in North America. Dr. A. S. Romer informs me that the vertebrate fauna of North America is not opposed to my classification.

In South Africa there is an apparently unbroken succession of strata from Carboniferous to Rhaetic, and there is no positive evidence of a Permian. The Dwyka ice-age was called Permian largely because of the supposed glacial deposits in the British Enville Beds and these are now known to be non-glacial and probably Carboniferous. Du Toit appears to have proved definitely the Carboniferous age of the glacial deposits.[3] The red beds are terminated by volcanic rocks, which, if Rhaetic, as supposed, form a convenient termination to the Epiric System. The Beaufort and Stormberg Series are together approximately equivalent to the Epiric.

In Australia also the ice-age is now regarded as definitely Carboniferous. Consequently the ice-age in India also is probably Carboniferous and not Permian as usually stated. The same argument applies to the ice-age of South America.

[1] Sherlock, R. L.—Relationship of the Permian to the Trias in Nottinghamshire. *Q.J.G.S.*, Vol. lxvii., 1911, pp. 75-117 and pl. v.

[2] —— A Correlation of the British Permo-Triassic Rocks. *Proc. Geol. Assoc.*, Part I, Vol. xxxvii., 1926, pp. 1-72 and pls. i-ii; Part II, Vol. xxxix., pp. 49-95 and pl. iv.

[3] Du Toit, A. L.—Geology of South Africa, 1926, pp. 279-280.

The Middle Gondwana of India[4] is unconformable to the Lower Gondwana with the glacial deposits and may be taken as representing the Zechstein-Trias or Epiric System.

In India it seems probable that the Chideru Beds of the Salt Range may be the base of the Epiric.

As regards the Permian flora, according to E. Weass,[5] if the division between Palæozoic and Mesozoic was made in the fossil plants it would be placed between the Rothliegende and the Zechstein.

Dr. C. T. Trechmann.—The Permian Magnesian Limestone of North-East England should be regarded as one formation along its whole extent from Cullercoats in Northumberland to Nottingham. It is as distinct and self-contained as, for instance, the Lias or the Chalk.

One of the chief points of evidence for questioning the validity of the Permian in England seems to be the apparent direct upward passage of the Middle Permian Red Marls into Lower Mottled Sandstones of the Bunter along a stretch of country about Bulwell and Kirkby-in-Ashfield, some ten miles north of Nottingham. I have for several years held the opinion, following observations in Durham, that this upward passage must be more apparent than real and that the red beds after the close of Magnesian Limestone times transgressed widely over the exposed edges of the Permian and came to rest successively upon Upper Magnesian Limestone, Middle Red Marls, and Lower Magnesian Limestone. Only Lower Magnesian Limestone and Middle Red Marls occur in the immediate vicinity of Nottingham; the Upper Magnesian Limestone does not appear till one reaches Collingthwaite, some 17 miles north and east of Nottingham. The section in the Annesly—Kirkby-in-Ashfield railway cutting contains a more or less concealed break or non-sequence, the lower part of the section of red beds being very argillaceous; and passes up quite suddenly into red sands of Triassic aspect.

In Durham the Magnesian Limestone is thickly developed and dolomitic, that is to say, it consists of material derived from minerals dissolved in the waters of the sea. The Middle Magnesian Limestone in Durham, comprising the Shell Limestone Bryozoa Reef and its eastern and western bedded equivalents, is always found to the east of the outcrop of the lower beds. The Upper Magnesian Limestone, comprising in ascending order the Flexible Limestone, the Cannon Ball concretionary, and the Hartlepool and Roker oolitic Dolomites; rests in places upon the Reef, but chiefly to the east of it or down its eastern slope, but has never been seen to the west. This and other evidence, such as the gradual extinction of the fauna as one ascends in the Reef, points to a sea shrinking eastwards, desiccating and throwing down its dissolved salts.

That this eastward retrogression of the beds in Durham is not due to subsequent denudation is shown by borings beneath the Trias south of the great concealed fault that lies between Seaton Carew and Hartlepool. Here passing eastwards from the Lower Magnesian Limestone outcrop at Aycliffe the hidden Permian increases in thickness from 299 feet at Whitehouse Farm near Norton, to 878 feet at Seaton Carew, some seven miles to the north-east. Borings on the Tees near Middlesbrough brought up Dolomitic Oolites of the Hartlepool type full of gypsum.

Along its stretch in Yorkshire the Upper Magnesian Limestone keeps well away to the east of the outcrop of the lower beds. Rather north of Knaresborough the Middle Red Marls disappear beneath Triassic red beds and are replaced in Durham by the Middle Permian Bryozoa Reef.

There seems no reason to do away with the Permian so far as north-eastern England is concerned, as it is a formation quite distinct from the Carboniferous below and the Trias above. The Permian fauna is a very distinct one and has apparently only one fossil, Spiriferina cristata, common to the Carboniferous, and none to the Trias. The Permian and Trias beds of England are continental in character and deposition must have been intermittent, and there must be many instances of non-sequence to correspond with the Marine Permian and Trias of other regions.

Prof. J. W. Gregory, F.R.S.—The continuity of Geological Evolution is accompanied by the absence of definite boundaries to the Geological systems. Hence, their validity has been repeatedly questioned. A crusade against the Permian System

[4] Wadia, D. N.—Geology of India, 1910.

[5] Kayser, E.—Lehrbuch der Geologie. Sechste und siebente Auflage, I. Band, 1923, p. 409.

forty years ago by influential members of the British Geological Survey, including H. B. Woodward, J. B. Jukes-Browne, and J. G. Goodchild, was based on the views that most of the rocks were iron-stained Carboniferous, and that the fauna was a stunted survival from that system; those authors adopted Conybeare and Phillips' term Poikilitic for both Permian and Trias. The rejection of the Permian was then supported by de Lapparent, Waagen and others; it is still urged in America by C. R. Keyes and in Russia by Noinsky and Jakolev, who regard the Permian as the littoral representative of the Carboniferous. Under the influence of Goodchild, in 1896 (in Great Rift Valley) I included the East African Permian in the Carboniferous.

The question does not depend on British stratigraphy but on that of countries where the System is better represented and especially by marine faunas. Modern opinion appears in favour of the System from three main lines of evidence. First, the great advance in life. The flora is marked by a great development of gymnosperms, but is nevertheless pre-Triassic. The fauna shows a great advance in several groups: thus, among the Foraminifera, the Carboniferous *Fusulina* and *Schwagerina* are followed by *Neoschwagerina*, *Doliolina*, and *Verbeekina*. The Cephalopods contain the first true Ammonites with the Medlicottidæ restricted to the Period. The Permian is characterised by the appearance of the main division of Acanthopterygian Fish—the Protospondyli. The Reptiles, as Prof. Watson in his speech has shown, are alone sufficient to mark the Permian as a valid and important System.

Its fauna was affected by the extinction of many Palæozoic types. Some Rugose Corals and some typically Palæozoic genera such as *Spirifera* lived on; but they also survived the Trias, and the corals of this type were still locally important in the Portlandian.

The Permian is also memorable as one of the greatest mountain-building eras in the earth's history. During it, was upraised the great Altaid Mountain System, of which the Hercynian, Armorican and Appalachian are local representatives. The most important stratigraphical break was the Saalian of Stille, between the Lower and Middle Permian, and not that (Pfalzian) between the Middle and Upper Series.

The inclusion of the Permian in the Carboniferous would make that System too cumbrous. Between the Devonian and Trias six series are generally recognised—Lower, Middle and Upper Series of both Carboniferous and Permian. At one time the palæontological separation of the Uralian (Upper Carboniferous) from the Artinskian (Lower Permian) was doubtful; but further knowledge of the Artinskian fauna justifies its separation.

The objection to the Permian as a System on the ground that its upper and lower boundaries are indefinite would apply also to the Cambrian, Ordovician, and Devonian.

The overwhelming balance of current opinion, especially in countries where the Permian is well developed, adopts it as an important and well-based system. The present tendency in America is not to drop the name Permian but the Carboniferous by dividing the Upper Palæozoic into three systems, the Mississippian, Pennsylvanian, and Permian.

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Prof. G. A. Hickling.—The supposed relation of the Permian and Triassic rocks has undoubtedly been influenced by the placing of the boundary of the Palæozoic and Mesozoic between them, with a supposed physical and palæontological break. Consequently the evidence advanced some years ago by Dr. Sherlock indicating that some of the Magnesian Limestone of Yorkshire was contemporary with part of the Bunter Sandstone of Nottingham was received with some scepticism. Nevertheless, subsequent investigation has tended to confirm the general conformity of the Trias with the Permian and to show that some of the sandstones formerly mapped as Bunter pass laterally into rocks containing Permian fossils. In the neighbourhood of Manchester the Bunter Sandstones are clearly separated from the underlying Permian Sandstones by some 200 to 240 feet of fossiliferous Permian marl with thin limestones. Followed westward, within 25 miles, the marls then lose most of their fossils and limestones, become sandy and finally diappear. The underlying Permian Sandstones are then inseparable from the Bunter, and are more or less represented by the Lower Mottled Sandstone division of that formation. This view was held many years ago by Binney, and is fully substantiated by recent borings. In Cumberland, the recent mapping has failed to show any evidence of a break between the beds above and below the thin band of Magnesian Limestone which is accepted as the local top of the Permian. Dr. Bernard Smith has shown that the whole Permo-Triassic sequence of Cumberland is closely similar to that in south Durham, except for the great expansion of the Permian sandy beds in the former area and the corresponding reduction of the limestones. It will be shown in a subsequent paper at this meeting that there was no physical barrier between these areas in Permian and Triassic times, and in these circumstances the replacement of a great thickness of limestone by sandy deposits suggests lateral passage. At least, there is nothing to indicate a Permo-Triassic break. In my opinion the essential continuity of the Permian and Trias in this country must be accepted as a fact in the discussion.

Since they are continuous, and the sandy facies of both groups is usually quite unfossiliferous and lithologically indistinguishable, it is no matter for surprise that mistakes have been made in correlation, and it may be affirmed with little hesitation that many isolated sandstones in this country can never be strictly correlated. This affords no argument either for or against the abolition of the Permian system; it does urgently demand the adoption of some such term as 'undifferentiated Permo-Trias' (or New Red Sandstone) for beds which cannot be definitely correlated—especially by the official Geological Survey.

The above are matters which have led to this discussion—but the subject is much wider, namely, whether the Permian fulfils our conception of a system. Examination of the well-established systems readily shows that their delimitation rests on no logical basis whatever, other than that they cover a substantial portion of the geological record, and are more or less naturally bounded in the region where they have been originally defined. They are commonly stated to be bounded by physical or palæontological breaks. The two supposed criteria are fundamentally one. Without physical interruption, either locally or elsewhere, there can be no palæontological break, and it is yet far from being established that any but possibly the very greatest unconformities are of world-wide effect. Experience proves repeatedly that the systems which are naturally bounded in one region are difficult of application elsewhere. Under these circumstances, close adhesion to the original definition of any system is the only alternative to chaos.

The Permian and Trias together cover one great epoch of widespread continental extension. The great length of this interval is amply attested by the enormous evolutionary progress which occurred within it, the development of nearly every great group of reptiles and of the larger stegocephalia; wide changes in the fish fauna; the major portion of the evolution of the ammonites; and many more changes of similar magnitude. It is clearly the enormous contrast between the floras and faunas above and below these rocks which led to the conclusion that the break between the Palæozoic and Mesozoic must lie within them. The contention that these rocks should form one system would be logical, and open to objection only on the ground that the system would be unwieldy. Their union, however, would do nothing to diminish the difficulties of correlation which gave rise to this debate, but would merely hide them under a comprehensive name. It should be recognised that the Permian and Triassic rocks are very incompletely represented in this country and in Central Europe. The Lower Permian in particular appears to be very slightly represented. The Zechstein alone (probably mostly Upper Zechstein) is represented by adequate fossil evidence.

Prof. W. S. Boulton.—No system, perhaps, has given rise to more discussion than the Permian. Difficulties and problems have arisen wherever its rocks have been investigated. The Period bridging the gap between the Palæozoic and Mesozoic eras was marked by powerful crustal movements, notably in Europe and North America, and its rocks accumulated for the most part in continental areas with an arid climate. In consequence the rocks are abnormal, fossils are absent or scarce, and correlation of deposits in separate or distant regions becomes difficult or impossible.

I would urge, however, that these Permian problems ought not to be shirked because they are difficult and not immediately resolvable. By abolishing the *name* 'Permian' and substituting some other, such as Permo-Carboniferous or Permo-Trias, or by linking up the whole or part of its deposits with the Carboniferous or with the Trias, we do not thereby solve the Permian problems.

Further, I would stress the unwisdom of attempting to settle the validity of the Permian as a system chiefly from a study of British stratigraphy. To decide whether or no there shall be a separate system which we call Permian, it is not sufficient to go back to the time of Murchison and his contemporaries, and discuss the propriety of adopting the old name New Red Sandstone or some other in order to rid us of the troublesome name Permian. The Permian system is now recognised by most foreign geologists—the name is well established. And whatever its obscurities and difficulties nobody denies that the system is represented, as in India, South Africa, Texas and elsewhere, by rocks many thousands of feet in thickness, or that its fauna and flora are distinctive and important despite the fact that they may show gradual transitions into faunas and floras which are older and younger than Permian. The period of time, implicit in its rock accumulations, in its stratigraphical breaks and in the evolution of its fossil organisms, is comparable in extent with that of other well-established systems.

The question of retaining the Permian as a separate and distinct system is international, and can only be satisfactorily decided by some representative international body of geologists, after an exhaustive analysis of all the evidence, more especially in those areas where the fossil faunas and floras are preserved most completely.

In such an international symposium, what would be the attitude of British geologists ? Dr. Sherlock, apparently, would advocate the abolition of the term Permian, and after linking up the so-called Permian of the Midlands, for example, with the Carboniferous he would unite the Permian of the north of England with the Trias under a new system—the Epiric. Most British geologists, however, would probably advocate the retention of the name Permian, so that the problem for us is to decide what deposits must be included, how they are interrelated in different parts of Britain, and what are the upper and lower limits of the system.

In attempting to answer such questions I can here only refer briefly to the rocks in the Midlands of England, to which Murchison gave the name Permian and which were mapped in detail by Hull. Of Hull's three divisions of 'Salopian Permian,' the lowest (Keele Beds) is generally agreed as belonging to the Carboniferous on palæobotanical and other grounds. The Middle or Calcareous Conglomerate and the Upper or Trappoid Breccia divisions have recently been grouped under the term Enville Beds by the Geological Survey, and provisionally put in the Carboniferous. At the same time they class the Hopwas Breccia, which occurs at many places in the Birmingham district immediately below the Bunter, with the Trias. So that, according to the Geological Survey recent mapping, there remains no visible Permian in the Midland area.

I have much sympathy with the Survey in their anxiety to settle the systematic position of these puzzling Midland rocks, for it is obviously necessary to have them indicated on the official maps. It is to their credit, if I may venture to say so, that they expressly admit the doubtful age of these Enville Beds in their recent maps and memoirs dealing with the Birmingham district. But unfortunately in some more recent Survey publications, and elsewhere, it has become the fashion already to refer to the Enville Beds as definitely of Carboniferous age. Which only shows how great is the responsibility of an official Geological Survey. What the Survey have done has been to work upwards from proved Coal Measures until arrested by what they regard as a considerable stratigraphical break, which happens to be at the base of the Bunter, or in places at the base of the Hopwas Breccia.

Yet we should always remember that up to the present there is no palæontological evidence for the Carboniferous age of the Enville Beds. What fossil evidence they do yield in the shape of vertebrate footprints and skeletons points rather against than for a Carboniferous age of these rocks. Moreover, an unconformity, however useful and satisfying in map construction, is not necessarily a limit to a system, as that term is generally understood. If it were, the Old Red Sandstone of Scotland, for example, would stand little chance of a separate existence.

I do not believe, however, that the Enville Beds represent a continuous series of deposits. There are probably many non-sequences in them, and at the base of the Clent and Northfield Breccias, with their accompanying marls and sandstones, a considerable overstep or unconformity exists.

The period that intervened between the Upper Coal Measures and the Bunter was probably of vast duration, and marked by the culmination of the great Hercyian and Armorican mountain-making movements, accompanied throughout by erosion and deposition of enormous masses of coarse red sandstones, conglomerates and breccias, with local marls and fine sandstones in the shallow inland waters of low-lying basins. Many thousands of feet of rock were removed by erosion during this period and much of it must have been deposited in the Midland area. We now see exposed in outcrop a mere vestige of it, for the greater part lies buried beneath the mantle of Trias and other Mesozoic strata which floor the Midland Plain.

Does the unconformity at the base of the Bunter in the Midlands, or at the base of the Hopwas Breccia where that occurs, represent the time-equivalent of the whole of these vast processes of mountain-building, erosion and deposition, and the whole of the Permian as it occurs in the rest of Britain and abroad ? It would seem so if we are to believe those who roundly assert that the whole of the Enville Beds are Carboniferous, and the Hopwas (Nechells) Breccia is Trias, and that no Permian rocks are present in the Midlands.

I have shown elsewhere that there exists under a large area in Birmingham a mass of coarse breccia and sandstone (Nechells Breccia), proved to be upwards of 400 feet thick in places, known to rest at Nechells discordantly on the Calcareous Conglomerate and immediately overlain in different places by the Keuper Sandstone, Upper Bunter Sandstone or Bunter Pebble Beds. It has affinities with the Hopwas Breccia and is probably part of the same group. Some of these Nechells Breccias, especially in their lower portions, are indistinguishable from the Warley Breccias which lie to the west of Birmingham and which are classed by the Survey and Wickham King with the Clent Breccia. Again, typical Bunter Pebble Beds at Hockley in West Birmingham rest upon Warley Breccia at a depth of 610 feet, while only a mile to the east the Bunter is resting upon the Nechells Breccias, which contains material of the Warley type. Further evidence bearing on this is now in my possession and will be shortly published.

It will thus be seen that if the present Survey reading of the general succession, which is admittedly tentative, were to be taken as final, it would mean that we should have a great breccia-sandstone-marl group, passing up from the Carboniferous to the Trias, with no considerable break so far determined. We should have in fact a state of things not unlike that in West Cumberland and the Vale of Eden, described by Dr. Bernard Smith, where there appears to be a transition breccia series linking up the Permian with the Triassic deposits. But in the case of the Birmingham district, the linking up is between the Carboniferous and Trias, with no room for a Permian System. On the other hand, if a satisfactory dividing line can eventually be fixed between the Nechells or Hopwas Breccia and the Warley or Clent Breccia to the west, then the break between the Palæozoic and Mesozoic eras would occur in the immediate neighbourhood of Birmingham within the limits of a great Breccia group.

In other words we should have very similar breccia groups, indicative of similar physiographic conditions, and containing materials derived from much the same source, both below and above the unconformable junction.

Without going further into details, enough has been said to show that the Permian problem of the Midlands has not yet been cleared up, and except for the convenience of map display the grouping of the Enville Beds with the Carboniferous does not carry us very far, and in any case should be regarded as tentative and open to revision whenever new facts emerge.

In my opinion, the wise and safe course to pursue is to keep the Permian System in being, at any rate until there is a demand among the Permian specialists of the world for its suppression. In the meantime let us be frank and admit that there are certain deposits in this country bridging the Carboniferous and the Trias which at present are incapable of exact classification, neither provably Carboniferous nor provably Trias. To detach them and link them up with either is apt to produce, after a while, self-deception and a false sense of security. There would always be the probability that they are the time-equivalent of Permian rocks elsewhere. Patient research in the future will reduce the number of doubtful and unknown elements in the problem.

Dr. H. C. Versey.—It is suggested that the difficulties in fixing limits to the Permian System and in correct correlation between marine and continental developments can be overcome by adopting a new basis of correlation and re-defining the system accordingly. The various episodes or phases of the Hercynian revolution are all of sufficiently widespread character to afford reliable criteria for correlation, accompanied, as they are, by important faunal and floral changes. The Sudetian and Asturian phases are both used as limits to major divisions in the Carboniferous. The generally accepted beginning of the Permian period, i.e. at the Autunian, is not accompanied by any earth movement while the Autunian is so closely allied, lithologically and florally, to the Stephanian that it seems desirable to group them together as the highest major division of the Carboniferous. After the Autunian (=Lower Rothliegende) the Saalian episode is shown by extensive unconformities and floral changes, so that a natural base is provided for a new System, with a flora of more Mesozoic aspect. In the marginal tract of the Urals the Saalian phase may be recognised by strand-line changes in the Kungur Beds which are followed by the transgressive Kazan Beds. The Artinskian would thus be placed in the Carboniferous, and D. White's discovery of Autunian plants with Artinskian mollusca strengthens this allocation. Correlation with the marine facies can be made via the Kazan Beds.

A further episode of movement (the Pfalzian or Palatine) occurred after the Upper Permian, and where this occurs, a natural boundary between Permian and Trias. This is found in the Urals, Donetz Basin, Vosges, Pyrenees, Northern England, but is everywhere weak. In England it was accompanied by important changes in drainage direction, and for such changes some interval of time must be allowed. Such a weak movement is unlikely to have produced marked strand-line changes, but it is noteworthy that the base of the Marine Trias is marked by a sudden incoming of new elements in the fauna. The Anisian division of the Marine Trias is correlated with the Muschelkalk, and the Scythian division with (at least) the uppermost Bunter. According to Stille, the oscillations in the continental area of Germany may be correlated with transgressions and regressions in the Tethyan province. It seems probable that the accepted base of the Marine Trias is roughly equivalent in time to that of the Continental Trias.

The Permian System is thus re-defined to include only Upper Rothliegende and Zechstein (Saxonian and Thuringian).

Prof. P. F. Kendall, F.R.S.—While there appears to be no unconformity between Permian and Trias, sections in East Lancashire and Cheshire afford clear proof of a formidable inter-Permian unconformity. Borings at Stockport show a conglomerate which rests on various members of the Stockport Marls and Sandstone and even on the Collyhurst Sandstone. This conglomerate has been shown by Dr. Versey to comprise a suite of rocks including types suggesting derivation from the Wrekin-Caradoc area. Close comparison may be drawn between this conglomerate and that discovered in the Middle Permian Marls at Harworth, near Doncaster, which also contains a large proportion of Pre-Cambrian rocks of Uriconian or Charnian types. It seems likely that these two conglomerates are the records of some uplift or physiographical change that took place after the deposition of the Lower Magnesian Limestone and Middle Marl. If the two conglomerates are identical in age, it would bring the Western succession into parallelism with the Eastern and would further render Dr. Sherlock's correlation superfluous.

Dr. E. Neaverson.—Whatever controversies may rage around the British strata, hitherto referred to as the Permian System, everyone is agreed that these rocks (together with those of N.W. Europe generally) are of unusual type.

As a matter of general principle, correlation within the present stratigraphical classification is made by means of the marine invertebrate faunas, which, being most uniform and widespread, have become the standard of reference for the Systems, even where the latter were first defined by non-marine faunas and floras. It can safely be said that if vertebrate or plant assemblages had been taken as the basis of stratigraphical subdivision throughout the geological column, a very different classification would have resulted. Hence, the grouping must be made on a consistent plan if confusion is to be avoided, and the Permian System should be redefined in the first instance by means of the marine invertebrate faunas which are known chiefly from Sicily, India, Timor and Texas.

The fossil assemblages from these places are sufficiently distinct from those of the Carboniferous and Triassic Systems to justify the retention of the 'Permian System.' For example, the ammonoids first exhibit, predominately, the frilling of the sutural lobes, and many genera are restricted to Permian rocks. Moreover, the evolution of the faunas and the thickness of the strata in some places necessitate such subdivisions as would make the contiguous systems unwieldy, if the Permian rocks were added to either, or even divided between the two.

In the case of Tertiary strata which yield only land-vertebrates, the rocks have been fitted with considerable success into the classification based on marine faunas, and there seems to be no adequate reason why the Permian continental deposits should not eventually be similarly adjusted.

The established name 'Permian' may well be retained. Compound terms like 'Permo-Carboniferous' and 'Permo-Trias,' do not help, but only create confusion. The term 'Epiric,' recently proposed for the combined Upper Permian and Trias, is already used in another connexion; moreover, other stratal groups could equally well be described as 'epiric.'

The whole question is not one for British geologists alone; it would be better considered by an international committee who could define, in the first place, the limits of the Permian marine sequence, and then possibly obtain some measure of agreement concerning the position of the equivalent continental deposits.

AFTERNOON.

Excursion to Burrington and Cheddar.

## Saturday, September 6.

Excursion to Cattybrook, Aust, Damery, and Chipping Sodbury.

## Sunday, September 7.

Excursion to Stanton Drew, Stoke Lane, Priddy, and Frome.

## Monday, September 8.

**Presidential Address** by Prof. O. T. JONES, F.R.S., on *Some Episodes in the Geological History of the Bristol Channel Region.* (See p. 57.)

Prof. A. H. COX and Mr. D. A. BRYN DAVIES.—*The 100-foot Base Level at Cardiff.*

Well-marked terraces and base-levels occur on the eastern side of the Taff Valley in Cardiff City and its immediate neighbourhood. The lowest is a gravel terrace on which the central parts of Cardiff are built. Its southern or seaward margin is about 30 feet above O.D., rising inland to 50 feet.

A higher platform is cut across gravelly drift and occasional solid at a height of 100 feet above O.D. in Cardiff, rising to 130 feet further north.

Near Cardiff these platforms are only developed to any extent on the drift-covered crop of the soft red marls of the Old Red Sandstone and of the Keuper Marl Series. On the west side of the Taff their development has been hindered by the outcrop of harder beds including the Radyr Stone, the local basement bed of the Keuper Series. Their relation to the present Taff River is being further studied.

The platforms are sufficiently extensive to represent considerable pauses in the process of base-levelling and accordingly corresponding platforms might be expected to reappear locally in some at least of the areas along the Severn Estuary.

Prof. G. DELÉPINE.—*The Dinantian Zones of Goniatites in the North of France and Belgium.*

I would like, in the first place, to pay tribute to the memory of the late Arthur Vaughan, whom I met in Bristol twenty-two years ago. He devoted much of his time to showing me over the fine sections of this country around Bristol and in South Wales.

Everyone knows the Zones he established in the Dinantian, on Corals and Brachiopods. They were used not only in the British Isles, but also on the Continent, in Belgium and in the north and west of France, and proved successful in establishing more accurately the connections between the Dinantian of this country and of the other parts of north-western Europe.

The difficulty, however, was to obtain satisfactory results when one comes to the formations called Yoredale and Millstone Grit in the north of England, and Namurian in Belgium. In this case, Shales and Grits come in; Corals are often practically absent, Brachiopods are few and show little change on a large range. Thus, for zoning, one wants to use other groups. Amongst these, the Ammonoids are particularly suitable, because, generally speaking, their species have a wide geographical distribution combined with a short vertical range.

During these last years Mr. W. S. Bisat has successfully worked the Carboniferous Goniatites in Yorkshire and Lancashire, and has established a fine succession based on their species and varieties; he has shown that one might distinguish no fewer than thirty-six zones and sub-zones of Goniatites between Upper Dinantian and the Middle Coal Measures.

However, his classification did not extend downwards beyond the Middle and the Lower Dinantian, because these levels (I mean the Lower Viséan and the Tournaisian) are reduced or even, in some places, not represented at all in the north of England.

In Germany, Hermann Schmidt worked also the Carboniferous Goniatites and published a succession of zones, of which the main lines correspond pretty well with those established in England. In Coal Measures he was able to extend his divisions a little higher. Moreover, he extended his zones downwards to the base of the Dinantian.

He distinguished in the Dinantian three main divisions:

III. Glyphioceras { *granosum.* *striatum.* *crenistria.*

II. Pericyclus { *Kochi.* *plicatilis.* *princeps.*

I. Protocanites { *geigenensis.* *Lyoni.*

According to H. Schmidt, his *Glyphioceras Stufe* should equal the Viséan, his *Pericyclus Stufe* the Tournaisian, and his *Protocanites Stufe* corresponds to the *Etrœungt Zone* of French authors (=K2). But if one looks to the species he refers to, his *Glyphioceras Stufe* represents only D2–3, not the whole Viséan. In Tournai, *Pericyclus princeps* is in C1, and is nowhere known lower than this level. That shows immediately that this succession has been established by Hermann Schmidt on a non-complete sequence.

The sequence is complete in the Bristol area and South Wales, and it is known in detail, thanks to the Geological School of Bristol University. It is also complete in the north of France and Belgium. These are the places where one might expect to get a chance to establish a succession of zones of Goniatites, as A. Vaughan succeeded in establishing the succession of Corals and Brachiopods.

In the first place, it is essential to have a well-defined base. In the north of France this lower limit has been established by J. Gosselet, both on petrographical and palæontological evidence. He called the base *Zone d'Etrœungt* (=K2); it is formed by shales interbedded with limestone containing the first carboniferous species (*Spirifer tornacensis*, semi-reticulate *Producti*), still a reduced lot of Devonian forms (such as *Spirifer Verneuili*), together with a number of species limited to that level (*Clisiophyllum Omaliusi*).

The Goniatites of Upper Devonian have been accurately determined and their zones established in Germany by Wedekind and Schindewolf. I only mention here the uppermost zones:

VII. *Gattendorfia.*
VI. *Wocklumeria.*
V. *Lævigites.*

In brief, we look to find in the *Etrœungt Beds* of Avesnes (north of France) *Cymaclymenia camerata*, which is a well-defined species of the *Wocklumeria Zone* (see Annales de la Société géologique du Nord, **54**, p. 99, pl. vi., figs. 1–3, 1929). That settles the question about the bottom of the Dinantian: the so-called *Zone d'Etrœungt* (=K2) corresponds to the *Hangenberg-Schichten* of German authors, and, palæontologically, to their *Wocklumeria Zone*.

The *Lævigites Zone* is Upper Famennian.

The *Gattendorfia Zone* (with *Aganides intermedius* and different species of *Gattendorfiæ*) probably corresponds to the Z Beds, although, until now, no Goniatites have been found from that level in the north of France nor in Belgium.

A very rich fauna of Goniatites is known from the C1 Beds (=Upper Tournaisian) in Tournai and also in the east of Belgium. Another one has been gathered in

Maredsons by Father Grégoire, from the C2–S1 Beds; another one also has been collected by the late Dupont, from the D1 level; and then we reach, with the numerous Goniatites from Visé, the well-defined zone of *Goniatites s. str.* of Bisat and the restricted *Glyphioceras Stufe* of Hermann Schmidt, which correspond to D2–3.

The following table gives a resumed scheme of the succession of Goniatites in Dinantian:—

| Stratigraphical Divisions. | Zones. | Succession of Goniatites in Belgium and the North of France. |
|---|---|---|
| Viséan | D3 | *Goniatites spiralis* + *subcircularis.* |
| | D2 | *Goniatites crenistria* + *striatus,* &c. |
| | D1 | *Beyrichoceras hodderense* + *Beyrichoceras* n.sp. (cf. *redesdalense*). |
| | S2 | No Goniatites known. |
| | C2–S1 | *Pericyclus* sp.; *Prolecanites* sp. (*compressus* group); *Nomismoceras frechi.* |
| Tournaisian | C1 | *Pericyclus princeps*; *Munsteroceras complanatum, rotella, inconstans, sphæroidale*; *Aganides rotatorius*; *Protocanites clymeniæformis,* &c. |
| | Z1–2 | No Goniatites known in Belgium. (*Gattendorfia* + *Aganides intermedius* known in Germany.) |
| | K2 | *Cymaclymenia camerata* (Wocklumeria Zone). |
| Famennian | K m–1 | (Lævigites Zone.) |

## Dr. A. E. Trueman.—*The Classification of the Upper Carboniferous.*

Any scheme of classification of the Upper Carboniferous rocks must have regard to the marine and non-marine faunas and to the flora. The work of Mr. W. S. Bisat has gone far towards providing a basis for classifying the lower part of the Upper Carboniferous, where marine faunas are dominant, but marine bands with goniatites are so infrequent in the upper portion of the Upper Carboniferous that they afford little more than a check on conclusions arrived at from studies of other fossils.

In Britain the divisions of the Upper Carboniferous put forward by the late R. Kidston have for many years been widely employed. While the two uppermost divisions (Radstockian and Staffordian) are well marked off from the lower ones, there is no justification for retaining the classification into Lanarkian and Westphalian (or Yorkian) proposed by Kidston. The Lanarkian as used in Scotland included nearly all the pre-Staffordian measures, and is approximately equivalent to the greater part of the so-called Westphalian (Yorkian) of England. Recent work by Miss E. Dix suggests that more precise correlation by means of fossil plants may be possible when more collecting has been done at known horizons. The results obtained at present accord with those based on the fauna, whereas it had previously been supposed that a correlation based on floral evidence was not in agreement with that determined by certain faunas. Similar results have been obtained on the Continent by Bertrand, Jongmans, Renier and others.

The non-marine shells have been used to divide the Coal Measures into zones, most of which have now been recognised by various workers in almost every important coalfield in Britain, the same general sequence being present in Scotland and in the north of England as in South Wales. Certain types have a longer range in some areas, while others are more abundant in some coalfields than in others. Recent work indicates the possibility of subdividing certain of the zones already defined. It is suggested that the zones afford at least an approximate correlation. The changes between successive zones are due mainly to the disappearance of certain groups and the incoming of others, which may partly result from the peculiar conditions under which the Coal Measures were deposited, the formation of coal seams leading to the repeated withdrawal of animal life and to the subsequent entry of a new population. In this connection it is significant that some of the greatest faunal changes coincide with the thickest and most widespread coal seams. It is probably due to these conditions, and to their apparent uniformity over relatively wide areas at certain periods, that such organisms as sedentary Lamellibranchs can be employed for correlation.

It is undesirable to attempt to provide more than a tentative classification of the Upper Carboniferous until more information is available, especially concerning the position of its base (which appears to have been fixed at different horizons in Scotland, and in England and Wales, and which has been taken at different horizons by workers on fauna and flora[1]), and concerning the relation in Britain of the Westphalian (as used by de Lapparent) and the Stephanian, which probably embraces some part of the measures now referred to the Radstockian.

## Dr. D. A. Wray and Dr. A. E. Trueman.—*The Sequence of non-marine Lamellibranchs in the Upper Carboniferous of Yorkshire.*

Non-marine lamellibranchs have been systematically collected from upwards of sixty horizons in the Upper Carboniferous of Yorkshire, and their study has shown that the sequence is very similar to that already described by Dr. W. B. Wright from Lancashire,[2] and also by Messrs. Clift and Trueman from the adjoining coalfield of Nottinghamshire and Derbyshire.[3]

The earliest forms so far recorded come from shales below the Huddersfield White Rock, the uppermost member of the Middle Grit Series in the Millstone Grits; or about 250 feet below the base of the Rough Rock. Forms of *Anthracomya* from the shales at the base of the Coal Measures, which appear to be related to *Anthracomya bellula* Bolton are very distinctive, and constitute a readily recognisable horizon. Several new species of *Anthracomya* and *Carbonicola* occur in the lower part of the Coal Measures; these have recently been recognised at comparable horizons in other coalfields. Bands characterised by non-marine lamellibranchs are very numerous in the measures between the Silkstone and Barnsley coals, and include forms distinctive of the corresponding horizons in the adjoining North Midland coalfields. A factor which is likely to prove of considerable economic importance is the distinctive fauna in the measures overlying the Barnsley coal, and the Mansfield Marine Band respectively.

The following divisions can be recognised:—

*Phillipsii* Zone.—Measures above the Shafton or 'Top' Marine Band.

*Similis-Pulchra* Zone.—Measures between the Shafton Marine Band and the Barnsley coal.

*Modiolaris* Zone.—Measures between the Barnsley coal and the Green Lane, Thorncliffe Thin or Middleton Little seam.

*Ovalis* Zone.—Measures below the Green Lane coal. Fauna well developed above the Wheatley Lime or Silkstone Four-foot coal.

Several interesting conclusions can be drawn from a comparison of the succession of the faunas in the North Midland coalfields. Thus the Better Bed coal of Yorkshire, the Kilburn seam of Derbyshire and the Arley Mine of Lancashire which have already been correlated with one another by one of us,[4] on stratigraphical considerations occur in closely corresponding positions within the *Ovalis* Zone. In a similar manner the Deep Hard or Parkgate coal of the York, Derby and Nottingham coalfield corresponds in the succession with the well-known Trencherbone seam of Lancashire; while the famous Barnsley or Top Hard seam would appear to be almost certainly represented in Lancashire by the Furnace or Rams Mine.

## Dr. D. A. Wray.—*The Succession of Marine Bands in the Coal Measures of Yorkshire.*

It was not until the development of the concealed coalfield of South Yorkshire and Nottinghamshire within the past thirty years that any close attention was devoted to the numerous marine fossils which occur at several horizons in the Coal Measures of Yorkshire. In 1845, Phillips recognised the wide persistence of the marine band

[1] See, for example, J. Pringle and J. W. Jackson, *The Naturalist*, 1928, p. 377.
[2] *Summary of Progress of Geol. Survey*, 1928, Part ii., p. 36.
[3] *Quart. Journ. Geol. Soc.*, Vol. lxxxv., p. 77.
[4] *Proc. Yorks. Geol. Soc.*, Vol. xxi., 1929, p. 272.

overlying the Halifax Hard Bed or Bullion coal; while Green in his comprehensive memoir on the Yorkshire coalfield, records the presence of other marine horizons.

Dr. Gibson, formerly of the Geological Survey, first directed attention to the relative importance of these bands from an economic point of view; and the most persistent of these, the Mansfield Marine Band, is now the index horizon sought for in every new sinking. Assiduous collecting from cores enabled Culpin, Dyson and others to extend considerably our knowledge of these bands, while recent discoveries suggest that there are at least ten distinct horizons at which marine fossils occur within the Coal Measures of Yorkshire. Some are now known to be only local in occurrence, while others are not only persistent throughout the coalfield, but are represented in the Coal Measures in areas as far apart as Scotland and South Wales. The outcrop of the Mansfield Marine Band has been traced in the Wakefield and Barnsley district, and its relative position in the succession clearly determined.

By means of the several marine bands, combined with a study of the non-marine lamellibranch fauna of the intervening measures, it is now possible to correlate with considerable confidence details of the succession throughout the Yorkshire coalfield.

Mr. W. S. Bisat.—*The Major Subdivisions of the Carboniferous of Western Europe.*

The Continental terms in use for major subdivisions of the Carboniferous, such as Westphalian and Viséan, have long been in use by British geologists, the only name not generally used by us being Namurian. Exposures in the Namur area of equivalent age to our Lancastrian are apparently not good, but as the adoption of the name Namurian would lead to uniformity with our Continental confrères, I here advocate its use as the name for one of the major divisions. A symmetrical division of the Carboniferous is then possible, using Continental names for eight major divisions, the secondary divisions being based on the dominant goniatite genera (except at the base and summit) and the tertiary divisions based where possible on goniatite species, or where these fail, on corals or non-marine forms of life. The eight major subdivisions would be Stephanian, Upper and Lower Westphalian, Upper and Lower Namurian, Upper and Lower Viséan, and Tournaisian. The floral break between Upper and Lower Carboniferous forms then comes halfway up the series at the junction of Upper and Lower Namurian, and the ten secondary divisions founded on dominant goniatite genera (represented by the symbols in descending order A, G, $R_2$, $R_1$, H, $E_2$, $E_1$, $P_2$, $P_1$, B) completely cover the major portion of the Carboniferous, thus:—

| | | | | |
|---|---|---|---|---|
| Lower Westphalian | A | (Anthracoceras) | = | Mansfieldian |
| | G | (Gastroceras) | = | Halifaxian |
| Upper Namurian | $R_2$ | (Reticuloceras) | = | Marsdenian |
| | $R_1$ | | = | Kinderscoutian |
| | H | (Homoceras) | = | Upper Sabdenian |
| —floral break | | | | —floral break |
| Lower Namurian | $E_2$ | (Eumorphoceras) | = | Lower Sabdenian |
| | $E_1$ | | = | Grassingtonian |
| Upper Viséan | $P_2$ | (Goniatites s. str.) | = | Upper Bollandian |
| | $P_1$ | | = | Lower Bollandian |
| | B | (Beyrichoceras) | = | Cracoean |

Afternoon.

Excursion to Dundry and Keynsham.

## Tuesday, September 9.

**Discussion** (Sections C, E, H) on *The Relation between Past Pluvial and Glacial Periods.* (Prof. H. J. Fleure, Chairman; Prof. J. W. Gregory, F.R.S., and Dr. K. S. Sandford for Section C; Miss G. Caton-Thompson, Miss D. A. E. Garrod, Mr. L. S. B. Leakey, Dr. L. Woolley, for Section H.) (See p. 371.)

AFTERNOON.

Excursion to Avon Gorge.

## Wednesday, September 10.

Dr. G. W. TYRRELL (Glasgow) and Dr. K. S. SANDFORD (Oxford).—*Tectonic Relations and Petrography of Spitsbergen Dolerites.*

In early Cretaceous times the eastern part of the Spitsbergen region was the theatre of great igneous activity which took the form of dolerite intrusions, both as dikes and more massive vertical masses, and as sills of considerable extension, thickness and uniformity, injected into horizontal sediments of Late Palæozoic and Mesozoic ages. Vertical intrusions occur on the grand scale in the central and southern parts of the Hinlopen Strait, and in the northern part of the Stor Fjord region. Sills are also very frequent in these localities, and extend far south in the Stor Fjord, west in the Sassen Bay region, and east into North-east Land.

A close relationship subsists between the fjords and the dolerites. The lines of weakness which delimit the Kainozoic fjord-blocks were already in existence as fissures and fractures in Lower Cretaceous times, and provided channels for the ascent of dolerite magma. But not till long after the dolerite had solidified did the fracture-lines develop as fjord-faults. The Kainozoic crustal instability in this region was probably caused by the enormous displacement of doleritic magma at an earlier period, the latter itself probably being due to the overloading of the East Spitsbergen continental shelf by sedimentary accumulations. The basalt floods of the North Atlantic or Thulean region in early Kainozoic times may represent a later expression of the same crustal instability that produced the Spitsbergen dolerites and the correlated fjord-faulting.

Petrographically the dolerites are of a common type, ranging from gabbroidal varieties in the larger intrusions to basaltic varieties in the smaller dikes and on the edges of larger masses. The majority of the rocks are of medium grain, and may contain olivine, or quartz, or both, in addition to labradorite, enstatite augite, and titaniferous iron ore. An unusually interesting petrographical feature is the occurrence of olivine side by side with quartz in the form of a micropegmatitic mesostasis. The best example is a gabbroidal type from the Alk Range in Hinlopen Strait, in which fresh olivine partially or wholly enclosed in plates of augite co-exists alongside an abundant mesostasis of micropegmatite. Somewhat similar rocks occur in the sills of the Stor Fjord region. These rocks therefore provide good examples of Bowen's reaction relation operating in a doleritic magma.

Dr. H. C. VERSEY.—*The Speeton (pre-glacial) Shell Bed.*

Mineralogical analysis reveals a large percentage of minerals which may be attributed to Scandinavia and the rock is considered equivalent in age to the transported masses of sand in the Yorkshire Drift, i.e. Upper Pliocene. It possibly represents a period of relatively genial climate preceding the Basement glaciation and succeeding an earlier glaciation which did not reach the British coast. The bed is regarded as in situ, owing its present elevation to subsequent uplift. The local geography at the time is also discussed.

Dr. A. HEARD and Mr. J. F. JONES.—*Eohepatica dyfriensis, a Liverwort-like Plant from the Lower Downtonian of the Llandovery District.*

This peculiar organism occurs as a thin dorsiventral thallus with an apparently gregarious habit. No complete specimens have been obtained, but the largest fragment collected exceeds 10 x 8 cms.

Chemical and mechanical methods of treatment have shown the thallus to be highly differentiated.

The plant bears many ovoid bodies about ·4 cm. in diameter, from which numerous disc-like spores (?) have been recovered.

The dorsal surface bears groups of numerous air-pores, the cells of which contain plastids; and are separated from each other by only one or two epidermal cells.

These air-pore groups appear to be segregated in depressed polygonal areas which occur over the whole of the upper surface of the plant.

The inner cells of the thallus, which do not underlie the polygonal areas, consist of fairly uniform collenchyma; below the air-pores the cells appear to consist of irregularly sized thin-walled parenchymatous cells associated with large air-spaces.

Spirally thickened elements have been observed in several 'treated' fragments, but their exact position in the thallus has not yet been determined.

The ventral layer has not yet yielded much information of morphological interest. Intertwining filaments recovered from below the lower surface are interpreted as rhizoids.

In several features, ***Eohepatica dyfriensis*** bears some general resemblances to ***Marchantia.***

## Prof. H. G. A. Hickling.—*The Geological Structure of the English Pennines.*

A contoured map of the post-Coal Measure folding of the Carboniferous rocks in the Pennines and adjoining coalfields was produced by taking the existing levels of various Carboniferous strata throughout the area, and reducing them to the common datum of the Barnsley coal-seam of Yorks and Notts, the Trencherbone of Lancashire, Banbury 7-feet of North Staffs and Hutton or Low Main of Northumberland and Durham, which have been chosen as approximately corresponding horizons. The central area of the northern Pennines was contoured on the Great or Main Limestone, which can be traced throughout nearly the whole area.

The general character of the folding thus represented strikingly confirms the distinctness of the northern and southern Pennines, and shows the break at the Craven faults to be even more important than has been usually supposed. The area south of the break is somewhat strongly folded about an irregular dome which is elongated N.N.W.–S.S.E. This is flanked on the east by the synclines of the Yorks and Notts coalfields whose axes trend N.W.–S.E. (i.e. 'Charnian'), while to the west the folding is much stronger and its dominant lines clearly N.E.–S.W., or 'Caledonian.' The 'Caledonian' folding attains its climax in the many strong echeloned anticlines which lie between the Burnley Coalfields and the Craven faults, where it abuts sharply and discordantly against the almost unfolded northern Pennines or 'Rigid block.'

The northern 'Rigid block,' extending from the Craven faults to the Tyne, shows moderately strong transverse folding in the Stainmore gap and the Tyne valley. The former folds divide it into a southern 'Ingleborough' block and Northern 'Cross Fell block,' while the latter separates it from the more folded area of Northumberland. The whole block shows evidence of a slight tilt towards the north-east at an early date, extending from the Lake District to Durham, where it terminates in the Sunderland syncline, a fold of definitely 'Charnian' trend.

Though the main folding of the Pennines is of Hercynian age, there is thus almost no indication of Hercynian or Armorican trends in the folds produced, the lines of movement being evidently influenced by older structures. Comparison of the present slight easterly tilt of the Carboniferous of the northern Pennines with the tilt of the Permian rocks shows them to be nearly the same, and demonstrates conclusively that no tectonic ridge can have existed in this area in Mesozoic times. The very intimate relation of this easterly tilt to the existing surface features suggests a relatively modern movement, to which the 'Pennine Chain' as a topographic feature is mainly due.

The Pennines afford a splendid example of the relation between earth-movement and sedimentation in Carboniferous times. Over much of the northern Pennines subsidence was small and consequently the whole thickness of Carboniferous sediments deposited possibly never exceeded 3,000 feet. South of the Craven faults subsidence exceeded 15,000 feet, resulting in an equal thickness of sediments. The former area of small movement remains still unfolded, while the latter shows intense folding. The warping of the base of the Carboniferous series which took place while the remainder of those rocks were being deposited was approximately equal to all the warping of the area which has taken place subsequently.

## Dr. A. Raistrick.—*The Moraines of Western Durham.*

Miss E. M. Lind Hendriks.—*The Stratigraphy of South Cornwall.*

The so-called Ordovician sequence of South Cornwall resolves itself into two series of Devonian age, the Gramscatho overlapped by the Grey-Black Beds. The beds are subdivided and the distribution indicated from Porthleven to Pentewan.

A fossiliferous quartzite of Caradoc age, previously regarded as dating the entire succession, is shown to be restricted to repetitions of a crush-belt produced by lateral movement operating above the plane of weakness provided by the overlap. The oldest rock is therefore never 'in situ,' and always associated with the highest beds. Fossiliferous Upper Silurian limestones similarly occur. Quartzite fossils from Gerrans Bay include *Orthis altera* Barrande, characteristic of Barrande's $Dd_2$ beds of Central and S.W. Bohemia. New limestones from the same locality match an '*Orthoceratenkalk*' in Barrande's $Ee_2$ beds of Bohemia and Bavaria.

Barrande's stages F–G (Bohemia) are represented at the base of the overlapping Grey-Black series in Gerrans Bay by fossiliferous pebbles including a Lower or Middle Devonian fish, and fragments of the underlying Gramscatho Beds. The Grey-Black series is therefore not inverted, and cannot safely be placed lower than Upper Devonian. The spilite near its top is identical with that of Mullion Island. The Gramscatho Beds beneath the overlap contain throughout a new species of *Dadoxylon* wood of a type not known below Middle Devonian, with fragments suggestive of *Asteroxylon Mackiei* indicating that age. Tiny coal layers in the lowest division compare with those in Barrande's stage H of Bohemia (Hostim, &c.).

The South Cornish beds below the overlap are thus of French, Alpine, Central or South European type, the North Europe (Rhine, &c.) facies of the North Cornish Devonian being well known.

Tectonic breccias within the crush-belt account for most of the beds mapped as 'unconformable basal Devonian conglomerate'; the remainder comprise natural breccias on the horizon of the Gramscatho tuff, and in East Meneage a small brecciated complex. This is an intrusion of serpentine, continuous with a sheet in Veryan Bay, injected contemporaneously with, and altered by, the movement, which is continuous with the Lizard boundary-fault. At an intermediate point in Gerrans Bay the same serpentine, less altered and resembling that of the Lizard, overlies the thrust and is the plutonic equivalent of a spilite not older than Upper Devonian. Both serpentine and crush-belt are therefore closely post-spilite, and earlier than the post-Carboniferous stresses by which they have been repeated. The crush-belt is thus referable only to a period between Upper Devonian and Carboniferous, and therefore connected with the Variscian folding which, with similar strike, accompanied the injection of serpentines to gabbros and granites in Central Europe. The Lizard serpentine thus belongs to a Bohemian province of Variscian (Lower Carboniferous) date. It further follows that the spilites of South and North Cornwall are of one age (Upper Devonian); hence the structure of the entire county is the relic of a pinched-up fan with the southernmost syncline replaced by a Lower Carboniferous thrust.

The beds are correlated as in the table on the opposite page.

## September 11—18.

### Excursion to West Cornwall.

After the meeting closed at Bristol, some twenty-five members moved on to Camborne, where a series of visits to mines, quarries and pits had been arranged by Mr. E. H. Davison, of the Camborne School of Mines. The party was very warmly welcomed, and all possible facilities were afforded both for geological study and also of manufacturing and mining processes.

September 11.—The Carnmenellis Granite and the Polhigey Tin Mine. The Anglo-Oriental Corporation entertained the visitors to lunch.

In the evening a formal welcome was given at a dinner on the invitation of the Cornish Institute of Mining Engineers.

September 12.—Underground in the East Pool and Agar Mine. Messrs. Holman Bros., Ltd., invited the party to lunch, and then showed the manufacture of mining machinery.

September 13.—Castle-an-Dinas, Zennor, Gurnard's Head, Porthmeor Cove.

September 14.—Porthleven, Megilliggar Rocks, and Prah Sands.

| France, Alps, Central and S. Europe. | | | Rhine and N. Europe. |
|---|---|---|---|
| | *Barrande.* | *S. Cornwall.* | *N. Cornwall.* |
| | | ?Dodman phyllite. Pillow-lava in Grey-Black shales with F–G pebbles (remanié fossils) at base. Thrust. | Purple, Green and Black slate. Pillow-lava. |
| | | OVERLAP | Limestones and Upper Devonian Slates. |
| | | Upper Gramscatho Sandstones with plants. | |
| | Top removed by denudation. | Middle Gramscatho chert, limestones, spilite, tuff, breccias, &c. Plants and marine fossils. | 'Basal tuff.' |
| H 700 ft. | $h_3$ Clay-slates. $h_2$ Clay-slates and thin quartzites. $h_1$ Clay-slates, Plants and coal. } Upper-Middle Devonian | Lower Gramscatho:— (*c*) Gunwalloe Sandstone, Coal, *Dadoxylon*, &c. (*b*) Jangyeryn Grits. | Middle Devonian Slates. |
| G 1000 ft. | $g_3$ Limestones, Lavas, slates. $g_2$ cherts, &c., with goniatites. } couches, bigarrées } Lower-Middle Devonian | (*a*) Buff and Green Beds, *Dadoxylon*, &c. Basal (transition) slate—Green and purple. | Staddon Grit. |
| F 350 ft. | $g_1$, $f_2$, $f_1$ } Limestones with fish and marine fossils. | Mylor Slate (altered and unfossiliferous). No Base. | Meadfoot Beds, Calcareous Slates with fish and marine fossils. Dartmouth Slates (Pteraspis Beds). } Dittonian. No Base. |
| E | $e_2$ Orthoceratenkalk. | In Thrust. | |
| D | $d_2$ Quartzite (Caradoc). | In Thrust. | |

September 15.—Mount St. Michael (by kind permission of Lord St. Levan), Penlee Quay. The Royal Geological Society of Cornwall invited the visitors to lunch in Penzance.

September 16.—South Terrass Mine. Messrs. Loverings, Ltd., entertained the members to lunch and conducted the excursion over their china-stone and china-clay properties.

September 17.—Tremorebridge Quay. Very unfavourable weather caused the abandoning of the day's programme at noon.

# SECTION D.—ZOOLOGY.

## Thursday, September 4.

**Presidential Address** by Dr. W. T. Calman, F.R.S., on *The Taxonomic Outlook in Zoology.* (See p. 82.)

Miss S. M. Manton.—*Anaspides and Paranaspides.*

*The Habits and Feeding Mechanisms of Paranaspides and Anaspides.—Anaspides* is plentiful on Mount Wellington, Tasmania, but *Paranaspides,* formerly abundant in the Great Lake, Tasmania, is now very scarce, and may be approaching extinction.

*Paranaspides* resembles the more primitive members of the Malacostraca, such as most Mysids and Euphausids, in that it filters the water in which it swims, and so obtains part of its food requirements. The filtratory mechanism consists of the maxilla and adjacent mouth parts, and is operated much as is the maxillary component of the filtratory mechanism of filter feeding Malacostraca. However, in *Paranaspides* the maxillary filter functions alone, unaided by a stream of water from the thorax. *Paranaspides* provides a living example of an animal with a type of filter feeding mechanism which, as has already been suggested, probably preceded the *Hemimysis* type. The movements of the thoracic exopodites is simpler than that of other Malacostraca already investigated, with the possible exception of some of the most primitive living Mysids.

*Anaspides,* found in mountain pools with flowing water, is predominantly a bottom living form, walking or swimming over the substratum. In swimming and walking the thorax and abdomen are used as a single functional region, and the pleopods and thoracic exopodites beat in series. This is quite unusual for the Malacostraca. *Anaspides,* although it possesses a maxillary filtering mechanism of the same type as that of *Paranaspides,* does not filter the water in which it swims, the mouth parts being then stationary. The maxillary filtering mechanism, however, plays an important part in collecting particles scraped off large masses of food which would otherwise be swept away by the flowing water without an effective method for their collection. The use of the primitive filtering mechanism is neatly correlated with the conditions of life of the animal, little morphological change being involved.

Both *Paranaspides* and *Anaspides* sweep up algal slime off weeds and feed to a variable extent on large food as does a Mysid.

The persistence at the present day of Crustacea as unspecialised in habits as *Paranaspides* and *Anaspides* is associated with their habitat. *Paranaspides* has been well protected from predatory fish by weeds, and *Anaspides* has no competitors in its mountain pools. Both forms appear incapable of meeting competition and are very inefficient hunters of live prey or of food of any size.

*Paranaspides* has almost disappeared as a result of the artificial raising of the level of the Great Lake by about 32 feet for the purposes of water power, and the consequent dying off of nearly all the old weeds in the lake.

Prof. H. Graham Cannon.—*On the Internal Anatomy of a Marine Ostracod, Cypridina (Doloria) levis* Skogsberg.

Hitherto very little has been known with certainty about the internal anatomy of Ostracods owing to the extreme difficulty of fixation. However, some material collected by the 'Discovery' Whaling Expedition was found to be so well fixed that it has been possible to work out in detail the anatomy of the chief systems of organs.

The chief interest in the blood system lies in the presence of a muscular pericardial floor and a system of blood vessels provided with muscles by which they can be distended or collapsed. In addition, there is a pair of muscular valves of peculiar structure at the point where the main vessels open into the body cavity. It is suggested that this muscular system is an accessory circulatory mechanism, and from a study of the arrangement of the muscles it has been possible to suggest the actual mechanism by which the blood is forced round the body. The heart does not appear to be essential and this is significant from the fact that in many Ostracods the heart is absent.

The compact nervous system is peculiar in that a basal ganglion occurs in each limb. In addition, a well-defined system of giant nerve fibres occurs, most of which are, as usual, associative elements, but some of which are apparently motor neurones running to the powerful muscles of the swimming antennæ. The structure of the protocerebrum and deuterocerebrum has been worked out in detail. The tritocerebrum is peculiar in that a 'central body' occurs in the tritocerebral commissure. A well-defined 'sympathetic' system is present.

The segmented excretory organs consist of antennal glands and not maxillary glands as in the Cypridæ. The glands show a typical structure, and there is a well-defined sphincter between end-sac and duct.

### Prof. H. Graham Cannon.—*On an Undamaged Specimen of Nebaliopsis typica* G. O. Sars.

*Nebaliopsis typica* was originally described by Sars from a few damaged specimens collected by the Challenger Expedition. Thiele subsequently described the limbs in detail from a crushed specimen collected by the German Deep Sea Expedition. The Swedish Antarctic Expedition of 1898-9 was the first expedition to obtain a complete and undamaged specimen. This was briefly described in a posthumous note by Ohlin, from which it appears that the animal was still alive when it reached the surface. The specimen, however, has been lost, and it is presumed that it went to the bottom with the 'Antarctic.' The 'Discovery' Whaling Expedition collected eight specimens in 1926-7, one of which is complete and undistorted. Photographs of this specimen will be exhibited.

### Prof. F. A. E. Crew.—*The Effects of Density on a Mouse Population.*

### Reports of Committees.

## Afternoon.

### The Work of the Great Barrier Reef Expedition :—

### Mr. A. P. Orr.—*Conditions in the Sea-water bathing Coral Reefs.*

In temperate waters much information has been gained on the relation between physical and chemical conditions in the sea and the animals and plants which live in it. The larger animals are susceptible to differences in temperature and salinity, while the minute plants, besides being affected by these, depend on traces of various nutritive salts. In temperate waters these nutritive salts show a seasonal cycle, being abundant in winter and disappearing in the surface water when the minute plants remove them in the early spring. Later in the summer they are regenerated again and currents in the sea distribute them throughout the water again.

In the sea lying between the Barrier Reef and the mainland there is comparatively little seasonal change except in temperature and salinity. The necessary nutrient salts are nearly always present, but invariably in such minute quantities that a large growth of minute plants is not possible. Fluctuations in the degree of oxygenation and in the alkalinity of the sea are thus not marked either.

In the sea water actually over a coral reef, however, conditions are very different. The intense metabolism going on in coral pools results in enormous fluctuations. The rich animal and plant life causes diurnal variations which are so great that they obscure seasonal variations. The oxgyen content, for example, may vary from less than 1 c.c. per litre of sea water at night to over 9 c.c. per litre during the day. The temperature changes are also very considerable. It is probable, however, that large as they are, these changes have no harmful effect on corals.

One condition which has been supposed to have a considerable influence on coral growth, is the degree of turbidity. Contrary to what is generally found in coral seas, the sea between the Barrier Reef and the mainland is not clear. On an average it is not more clear than the English Channel. As a result of this and also because of the movement of sediment by wave action, corals are subjected to a continuous rain of sediment. It has been found, however, that even quantities of sediment greatly in excess of that moved by the waves can easily be removed by the common species of corals.

Mr. F. S. Russell and Miss S. M. Marshall.—*The Plankton of the Seas round the Great Barrier Reef.*

In view of the importance of plankton organisms as the main link between the potential food reserve of dissolved nutrient salts in the sea and the reef building corals themselves, it was necessary that a very complete survey of the plankton should be part of the programme of the Great Barrier Reef expedition. Collections were made weekly to obtain information on the changes in abundance and composition of the plankton throughout the year. The study of the centrifuge plankton was undertaken by Miss S. M. Marshall, and that of the zooplankton by Mr. J. S. Colman and Mr. F. S. Russell. The results indicated that although poor compared with the great abundance to be found in our northern seas, the plankton of the Barrier Reef lagoon channel was a true coastal plankton and considerably richer than that of the open ocean. The quantities of diatoms present showed no seasonal changes comparable with the great outburst in our own seas known as the spring and autumn increases. There was some slight indication that the richest period of the year coincided with the periods of strongest winds, possibly caused by the stirring up of the nutrient salts from the sea bottom. The animal plankton showed no really great changes in total bulk, but its composition changed markedly at times. There were, for instance, sudden outbursts of Salps at certain times of the year, whose appearance coincided with certain phases of the moon. Most of the species of Siphonophores also disappeared during the rainy season when there was a definite decrease in the salinity of the sea water.

The most numerous group in the animal plankton was the copepods which appeared in great numbers but were mostly very small. The difficulties of drawing a true comparison between the abundance of plankton life in the barrier reef waters and that of northern seas is increased when the difference in rates of development between cold and warm water living animals is taken into consideration. Owing to high rate of development in tropical waters periodic collections probably give too low a figure.

Dr. C. M. Yonge.—*The Food, Feeding and Digestive Processes of Corals.*

Dr. S. M. Manton and Dr. T. A. Stephenson.—*The Growth, Breeding and Life Conditions of Corals.*

## Friday, September 5.

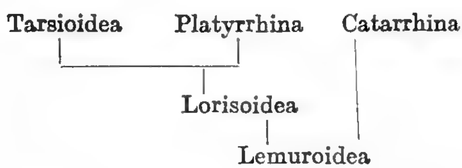
Dr. D. de Lange.—*Some Remarks on the Phylogeny of the Placenta.*

All criteria used for classifying the different forms of the placenta have a descriptive and, strictly speaking, not a morphological significance. The same end stages can be reached by different ways of development and need not be homologous. Probably primary ovoviviparous conditions have given rise to the formation of the egg-membranes (chorion and amnion). In young stages the exocoelom forms the protective water-cushion; in later stages this function is performed by the amnion cavity. The chorion (diplotrophoblast) originally possessed respiratory, and afterwards acquired digestive significance. The trophoblast corrodes the maternal wall and an ectoplacenta is formed which shows a histiotrophic (=lytic) character. This type of temporarily attached ectoplacenta seems to me the starting-point of the other placental forms. In large animals with spacious uteri and a large production of glandular secretion the attachment is delayed (Carnivora) or does not occur at all (most Ungulates). The placenta of *Manis* is completely hæmochorial; still, I have found some traces of a former attachment, i.e. two vestigial placental cushions. Opposite one of these, an ephemeral plasmodiblast develops. Thus, there is a slight indication of a former more intense interaction between maternal wall and trophoblast. In the endotheliochorial placenta, the border hæmatome of the placental girdle may be considered as a derivate of the primitive ectoplacenta, while next to it develops a placental labyrinth by the intermingling of maternal and fœtal capillaries. The maternal capillary system is first to develop and determines the pattern of the labyrinth. In Orycteropus a very low type of endothelial placenta occurs, which lacks a real labyrinth, because the fœtal capillary system only shows a slight development. On the other hand, the border hæmotomes are strongly developed as is the trophoblast at the same spot. In the Bradipodidæ the placenta does not possess the girdle-form, and extensive hæmotomes are lacking. On the contrary, there is the beginning of labyrinth-formation and the maternal capillaries show a tendency to lose their endothelial wall. This leads to the conditions in Chiroptera, where a hæmochorial, labyrinthiform placenta is formed by the breaking down of the maternal endothelium in a younger typically endotheliochorial stage. In the case of *Miniopterus* the two stages are found in the ripe placenta. In the olliform placenta, on the other hand, the stage of an endotheliochorial labyrinth is never passed. The lacunæ of the ectoplacenta are filled with maternal blood but they have never been maternal vessels. The confluence of these spaces gives rise to the intervillous space. The placental development of *Galeopithecus* characterised by the lack of a connective stalk and by the large extent of the allantois is an example of the formation of an olliform placenta in a way independent from that of the Primates. The olliform placenta of the Dasipodidæ deviates in a still more obvious manner from that of the Primates. Here the fœtal trophoblastic villi corrode large spaces in the maternal stroma, but the roofs of these spaces and the uterine epithelium of these roofs remain intact. So intervillous spaces are formed bordered on all sides by maternal connective tissue and not by a trophoblast layer.

Finally, most Rodents and Insectivores show a labyrinthiform, hæmochorial placenta with important remains of ectoplacental tissue. Here the hæmochorial condition arises partly by the transformation of the ectoplacenta, partly by the changing of a previous endotheliochorial stage. Thus, there are at least four types of hæmochorial placenta which have probably arisen quite independently from a more primitive attached, histiotrophic stage. I think these examples are sufficient to make it probable that each of Grosser's placental types has a polyphyltic origin.

## Dr. C. Tate Regan, F.R.S.—*The Evolution of the Primates.*

Five sub-orders of the Primates are recognised, the relationship of which may be expressed diagrammatically thus :—




The resemblances between the Platyrrhines (American Monkeys) and the Catarrhines (Old World Monkeys, Apes and Man) are due to parallel evolution. *Tarsius* is remote from the ancestral line of man, whose nearest living relative is the Chimpanzee but whose ancestor probably did not belong to the same family as this ape.

## Prof. F. H. Edgeworth.—*On the Musculature for opening and closing the Mouth in Vertebrates.*

The jaws are raised and the mouth shut by the masticatory muscles in all Vertebrates. They are depressed and the mouth opened by various groups of muscles.

In Dipnoi and Elasmobranchii the mouth is opened by the action of the Genio-coracoideus or Genio-thoracicus.

In Ganoidei the mouth is opened by the action of the Genio-branchialis and Rectus cervicis. In Lepidosteus and Teleostei the Genio-branchialis is absent and its place is taken by the Intermandibularis posterior (Siluroids) or by the Protractor hyoidei.

In Amphibia and Sauropsida the mouth is opened by the Depressor mandibulæ. In Amphibia this muscle is a secondary stage of the Levator hyoidei. In Siren and *Ichthyophis* only a partial transference of insertion takes place, and both a Levator hyoidei and a Depressor mandibulæ are present.

A Levator hyoidei is present in Dipnoi, where it is separated from the dorso-anterior part of the Constrictor hyoideus. In Amphibia only the ventral part of the Constrictor hyoideus is present (Interhyoideus posterior).

In Sauropsida the Constrictor hyoideus is developed in its whole dorso-ventral extent. Its dorso-anterior fibres form either a Levator hyoidei and a Depressor mandibulæ (*Sphenodon*, some Lacertilia, Crocodilia, Birds) or only a Depressor mandibulæ (Chelonia, Ophidia).

The Levator hyoidei is present in embryonic stages of all Mammalia. It either persists as such (Monotremes) or develops into the Stapedius (higher Mammalia). It does not develop into the secondary condition of a Depressor mandibulæ.

In Monotremes the mouth is opened by the Detrahens mandibulæ, in higher Mammalia by the Digastricus mandibulæ, which is formed by fusion of the Intermandibularis superficialis and the whole or a part of the Interhyoideus.

## Miss D. E. Sladden.—*The Adaptation of Alytes to Warmth.*

The Midwife toad (*Alytes obstetricans*) under normal conditions spends the greater part of its life on land, living in underground holes during the day, and becoming active at dusk, when it leaves its retreat to search for food. Occasonally during these nocturnal ramblings it takes to the water.

If the larvæ are kept in a higher temperature than that to which they are accustomed in their normal environment—from an early stage in their development—the metamorphosed young, when kept at the same temperature will frequently repair to any water available. By subjecting the young toads to dry as well as warm conditions, they can be forced to enter the water even during the day.

This experiment, which induces Alytes to change its habits, appears to be most successfully carried out on the young larvæ, the adult toads not being able to survive the violent change of temperature.

## Miss D. E. Sladden.—*The Production of Defects in the Frog.*

Eggs of the common frog (*Rana temporaria*) when subjected to the action of various agents such as sugar—which may possibly act as an absorbent of oxygen in solution—or exposed to a reduced air pressure at an early stage in development, gave rise to tadpoles showing various defects.

These defects were of the following nature:—

(*a*) Distention of body-cavity;

(*b*) Rupture of gut-wall caused possibly by the absorption of water and consequent swelling of the yolk, which extruded into the body-cavity;

(*c*) Flexure in tail due to a defect in the myotomic formation;

(*d*) Distortion of sacral region in the metamorphosed frogs resulting from the defect in the tail;

(*e*) Suppression of hind limb due to compression at an early period of development.

The larvæ showing defects at the time of hatching, in most cases with extreme abnormalities, died after a short period of larval life, and only apparently normal larvæ remained. From among these, however, after a period of 8 weeks or over, tadpoles showing flexure in tail and various consequent distortions in the region of the sacrum commenced to appear. These later larvæ were successfully reared through metamorphoses.

## Prof. J. Graham Kerr, F.R.S.—*John Samuel Budgett, a Bristol Naturalist.*

AFTERNOON.

## Dr. H. W. MILES.—*On the Diversity of Habit of three Sawflies (Tenthredinidæ) infesting Gooseberry.*

The sawflies are *Pteronus ribesii* Scop., *Pt. leucotrochus* Htg. and *Pristiphora pallipes* Lep.; *Pt. ribesii,* a multivoltine species, is sharply contrasted with the closely allied *P. leucotrochus,* a univoltine species. The former oviposits on the undersurfaces of the leaves, the eggs being inserted on the veins, and the subsequent larvæ are more or less gregarious. The latter oviposits on either surface of the leaf, the veins being scarcely ever chosen as oviposition sites; the eggs are laid singly, and the larvæ show little tendency towards gregarious habits. *Pristiphora pallipes* oviposits within the leaf tissue, and tends to place its eggs singly scattered over the host plant. The larvæ feed singly. This species is multivoltine and parthenogenetically thelytokous.

*P. ribesii* presents the interesting phenomenon of retarded development in the prepupal stadium, and this tends to modify the usual alternation of sex predominance which is associated with parthenogenesis in species which are arrhenotokous.

## Dr. F. A. DIXEY, F.R.S.—*Some new points in the development of Insect Wings.*

## Mr. J. V. PEARMAN.—*The Natural History of the Psocoptera.*

Short description of Psocids given.

An attempt has been made to supply a reasonably complete account of psocid life history based on fresh observations and on the few recorded observations.

It is found that the insects may be grouped roughly in three categories:—(i) surviving representatives of the more primitive stock, (ii) foliage dwellers, and (iii) bark dwellers.

A particular method of oviposition is characteristic of each group. The primitive species deposit their somewhat boat-shaped, sculptured eggs at random; the foliage dwellers lay groups of smooth, oval eggs and cover them with a sheet of web, while the bark dwellers encase their eggs in a coating of comminuted fragments with or without the further protection of a spun web. With regard to the last mentioned, it is of interest that the chewed particles forming the ensheathing layer are swallowed and pass through the alimentary tract, being deposited in a fluid medium simultaneously with the egg.

At hatching the egg shell is cut by a knife-like structure attached to the embryonic cuticle above the head of the larva and controlled by two firm strands of membrane. This oviruptor is operated by the rhythmical depression and inflation of three cephalic 'pulsatory areas.' The embryonic cuticle is shed after partial emergence of the larva.

In ecdysis, the sloughing of the nymphal integument is a repetition of the process of shedding the embryonic cuticle. There appear to be six pre-imaginal instars.

Prior to mating the males perform a courtship dance. In pairing the female takes the superior position.

Food in the majority of cases consists of micro-fungi and algæ (principally Pleurococci). Some species devour dead organic matter and a few are entomophagus to some extent and are thus, in a slight degree, of economic value.

Certain of the more primitive forms have been proved to make a sound by tapping with the abdomen, and in the higher forms structures have been found for which a stridulatory function has been postulated.

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Pearman, J. V.—'Life Histories': *Proc. Bristol Nat. Soc.*, 1927, VI, pt. v, p. 384; *Ent. Month. Mag.*, 1927, LXIII, p. 197; 1928, LXIV, p. 209, 239, 263; 1929, LXV, p. 89. 'Sound Production': *Ent. Month. Mag.*, 1928, LXIV, p. 179.

## Saturday, September 6.

Excursion to Dunster.

## Monday, September 8.

Mr. C. B. Williams.—*Migration among the Lepidoptera.*

Mr. N. N. Murti.—*The Physiology of the Heart of larval Starfish and Sea-Urchins.*

The larval heart of *Echinus* and *Asterias* makes its appearance first as a clump of two or three cells on the dorsal aspect of the larva a little above the junction of the œsophagus and the stomach, immediately to the right of the pore-canal. These cells are budded off from the anterior coelom and later they form a vesicle. At a more advanced stage, in *Echinus*, the ventral wall is invaginated into the vesicle and therefore bears to the vesicle the same relation as the heart of a vertebrate does to the pericardium.

Rhythmical contractions of this sac can be clearly seen when the larva is placed with its dorsal surface up, in a drop of water, on a slide under the microscope. There are as many as 35 pulsations per minute in the case of *Echinus* and only about 12 in the case of *Asterias.* The pulsations travel from the floor of the sac upwards exactly like the peristalsis of the intestine. The effect therefore is to drive the contained fluid upwards. This fluid, however, is the ordinary blastocoelic fluid and the heart is in communication with the blastocoele. The fluid contained especially in the heart of *Echinus* stains more deeply than that in the rest of the blastocoelic ground substance, and the suggestion seems justified that this difference in the staining capacity is due to the exudation of digested material, from the stomach cells. If this suggestion is justified the object of the heart is to drive the nutritive material forwards towards the region of the larval mouth and larval brain in *Echinus*, and to the apical end and tissues removed further away from any other nutritive fluid, in *Asterias.*

The heart certainly persists in the adult life of the star fish and the sea urchin. But owing to the growing opacity of the body wall during the metamorphosis of the larvæ its movements cannot be seen. The structure and relationship of this organ bear a close resemblance to the pericardium of *Balanoglossus.*

Mr. W. E. Swinton.—*The Plesiosaurs in the Bristol Museum.*

The Bristol Museum contains a collection of Plesiosauridæ second only to that of the British Museum. Eight specimens of particular interest are exhibited and seven of these are types. Interest also attaches to the fact that the first description of four of these was made to the British Association in 1839 by Sir Richard Owen. The specimens are :—

Genus *Plesiosaurus* (*sensu strictu*).

*Plesiosaurus conybeari*, Sollas. Skeleton and skull from the Lower Lias of Blackven Water, Charmouth. Original description, Sollas, 1881, Q. J. G. S., xxxvii., pp. 440-81. pls. 23 & 24. Lydekker cast doubt on the skull belonging to the skeleton but there appears no good reason for the belief that they belong to different species or individuals.

*Plesiosaurus brachycephalus.* The type skeleton (part and counterpart) collected from the Lower Lias of Bitton, Glos., in 1830, and described by Owen in the Report of the British Association for 1830, p. 69. This species was originally separated from *P. macrocephalus*, Owen, on the characters of its cervical vertebræ. The distinctive features are almost certainly due to age and the species are now grouped together under the latter name. This particular specimen, never adequately described, gives an excellent idea of the whole structure of the creature, including the shoulder girdle.

Genus *Eretmosaurus.*

*Eretmosaurus rugosus* (Owen). The type vertebræ of *Plesiosaurus rugosus* described by Owen (Rept. Brit. Ass. 1839, p. 82) from the Rhaetic bone bed, Aust Cliff. The vertebræ were distinguished, among other characters, by the rugosity of the non-articular surfaces of the centra. Actually the species is distinct, but this character of rugosity is more widespread than Owen suspected. After studying the characters of the pectoral arch of a specimen of *P. rugosus* in the British Museum, Professor Seeley created the genus *Eretmosaurus* with *P. rugosus* as the type species. The validity of Seeley's genus is doubtful, as the pectoral girdle is much obscured and where not obscured is unlike Seeley's figures.

Genus *Thaumatosaurus.*

*Thaumatosaurus megacephalus* (Stutchbury). To this genus should be referred the type skeleton of *Plesiosaurus megacephalus* described by Stutchbury in 1846 (Q.J.G.S. vol. ii. p. 412, pl. 18). The specimen came from the Lower Lias of Street, Somerset.

*Incertæ sedis.* A worn and incomplete cervical vertebral centrum from the Lower Lias of Weston was described by Owen in the Report of the British Association for 1839 (p. 77) under the name of *P. subtrigonus.* There is no doubt of the specific distinctness of the specimen, but its affinities are uncertain.

Likewise of unknown affinities is the cervical vertebra from the Rhaetic bone bed at Aust Cliff described by Owen as *P. Costatus* (*op. cit.* p. 80). Probably this species should be referred to a new genus which would also include the undescribed skeleton labelled *Pleisosaurus brachycephalus.* This latter specimen is from the Lower Lias of Bitton, Gloucestershire, and though completely disarticulated has been mounted in plaster by someone who must have been familiar with its original appearance. On careful examination practically all the skeletal features of importance can be made out. The specimen is not *P. brachycephalus* but a species closely allied to *P. rostratus.* Finally, there is in the collection a small cervical vertebra mounted upside down, and partly obscured, in plaster. No history is attached to the specimen, but it bears the name *Plesiosaurus depressus* given to it by Owen. This name has not been published and is not even a manuscript name. As the specimen, in its present condition at least, cannot be regarded as specifically determinable the name must be regarded as invalid.

The Bristol Collection is therefore very representative. *Plesiosaurus costatus* and *Eretmosaurus rugosus* occur in the Rhaetic ; the latter is also common in the Lower Lias and in addition has Upper Lias affinities : thus the most important periods of Plesiosaurian history are represented. The Upper Jurassic form, *Peloneustes,* was probably derived from a longirostrine plesiosaur allied to *P. rostratus.*

### Mr. M. A. C. Hinton.—*Extinct Cave Fauna of the Bristol District.*

Afternoon.

Excursion to Wookey Hole and other caves.

Excursion to the Bristol Zoological Gardens.

## Tuesday, September 9.

### Dr. F. B. Turck.—*The Cell and its Fluid in the Process of Growth and animal Metabolism.*

Various mechanical, physical and chemical agents were employed in animals to release the 'wound hormones.' The degree of injury made by these agents was kept below the lethal or shock-producing effect and, by careful adjustment in the experiments, also kept below the morbid effect.

It was learned that when shock and death was avoided and the morbid stage was not reached, that a stimulating effect resulted with acceleration of growth and metabolism. This acceleration of growth and metabolism was established by spacing the mild forms of mechanical, physical and chemical injuries at regular time intervals.

The tissue fluid released by these various injuries was secured and injected into fresh animals, about once or twice a week, according to the degree of concentration. This resulted in the stimulation of growth and metabolism similar to that in the animals which had been injured in the milder form.

The heated extract, up to 140°C, of this tissue fluid, was as active as the original tissue fluid substance. On burning off the protein, but retaining some of the organic residue with the mineral content, the injected mixture was active in producing the stimulating effect in growth and metabolism. Breeding experiments showed the favourable effect in the offspring and demonstrated the transmission from parent to the embryo.

Tables of conductivity and pH and spectrographs are included.

Dr. B. P. Uvarov.—*Cyclic Polymorphism in Locusts, and the Periodicity of Locust Invasions.*

While the technical methods of controlling locusts are well developed and very effective, a successful organisation of anti-locust campaigns meets with many difficulties. The main difficulty lies in the fact that locusts are not a permanent pest, but may be absent from a country one year, and appear in enormous swarms in the next. For instance, the Desert locust, a species alluded to in the Bible, invaded the whole of North and East Africa, Persia, India, Iraq, Palestine and Turkey in 1914–1916, and then nothing was heard about it for over ten years. Since 1926–1927 a new outbreak started, and, by 1929–1930 Africa, from Tanganyika to the Mediterranean, and S.W. Asia as far north as Transcaucasia and Turkestan, were overrun by devastating swarms.

Such sudden outbreaks involving whole countries find some of them not fully prepared to meet the danger, and during the recent invasion truly gigantic efforts were required to save the crops. An effective organisation of an anti-locust campaign cannot be improvised at a moment's notice, while it is clearly impossible to keep the organisation in readiness during long intervals between invasions.

The key to the solution of the locust problem is, therefore, in finding out the laws governing periodic outbreaks of locusts. Recent work in this direction in Russia, South Africa, Sudan and elsewhere, proved that the periodicity of locust outbreaks is intimately connected with the fact that all known species of locusts occur in two forms, or *phases*. These forms differ from each other in a number of structural and colour characters, but more particularly in the habits. During the intervals between outbreaks locusts are represented by the solitary phase, which is a harmless grass-hopper without definite social habits. When the outbreak begins, the solitary phase is transformed into the gregarious one; the individuals of this phase form dense swarms and undertake long migrations. Experimental work on phases has shown that the solitary phase can be turned into the gregarious one, if the locusts are kept in a crowded condition; reversedly, one can obtain the solitary phase by breeding gregarious individuals under isolated conditions.

Periodic outbreaks of locusts, thus, depend on the cyclic transformation of these insects from one phase into another, and back again. The actual factors causing and favouring the transformation are still insufficiently known, but it is clear that the problem of the successful control of locusts cannot be solved until these factors are thoroughly investigated.

A special Locust Committee has been appointed recently by the Government to consider the locust problem, and it was decided to organise exhaustive investigations into the question of periodic outbreaks and their causes. It is hoped that the actual work will begin shortly, and this concentrated scientific attack on locusts should produce results of great practical value in the shape of some means of foreseeing and preventing locust invasions, which would mean an enormous saving in crops, human energy and money for a large number of countries affected by the plague.

Dr. Nellie B. Eales.—*The Mandible of Fœtal Elephants.*

A study of three fœtal specimens of the Elephant, of which two are African and one Indian, reveals changes in the development of the mandible of a very definite character. Modifications in the growth of the simple generalised fœtal jaw are partly due to the gradual assumption of the highly specialised features of the Proboscidean mandible, and this is to be expected. But the three fœtal specimens exhibit also a condition unique amongst modern mammals, in that the jaw bends ventralwards anterior to the first deciduous premolar (DP 2), thus turning away from the upper jaw. Now this is the type of jaw which, according to Palæontologists, occurred in the Elephant's ancestors, those long-jawed beasts like *Tetrabelodon*. The fœtal Elephant, therefore, retains this peculiar ancestral character and affords a clear confirmation of the work of Palæontologists on the lineage of the Proboscidea.

A comparison between the mandibles of the three fœtuses and of young and old post-natal stages shows that the down-turned pre-alveolar region of the jaw enlarges up to about the middle of the gestation period, since the oldest fœtus is about this age and has the longest type of jaw. Between this stage and the full-term fœtus (about 22 months) we have neither specimens nor records, so that we do not know how the metamorphosis occurs. The shortening of the pre-alveolar region and the downward rotation of the symphysis in post-natal stages are, however, very striking and, from a comparison of the specimens available, it is evident that there is a retrogressive change in this portion of the mandible, while the alveolar portion of the body and the whole of the ramus are becoming progressively specialised and enlarged.

### Dr. P. D. F. Murray.—*Factors in the Early Development of the Vertebrate Skeleton.*

The mechanism of early skeletal development, with special reference to the avian limb, is discussed in the light of two main series of experiments. In one series fragments of limb buds of chick embryos which had been incubated for three, four and five days were grafted upon the chorio-allantois of eight-day chicks and allowed to live as grafts for from seven to nine days. In the second series the cartilaginous femora were removed from six and seven day embryos, were cleaned of all soft parts except the perichondrium, and were grafted in the same way.

The main conclusions reached in the discussion are :

(1) The development of the gross form of the cartilaginous skeleton is an affair of factors intrinsic in the individual skeletal segments themselves, i.e. the gross skeletal form self-differentiates. This is true of both shafts and epiphyses. Halves of limb buds of four and five-day chicks, grafted on to the chorio-allantois, have developed femoral shafts of practically normal form with normal curvatures, in the absence of either pelvis or of tibio-fibula or of both. Cases have occurred in which femora have developed with nearly normal heads in the absence of the pelvis and of nearly normal condyles in the absence of the tibia and fibula.

(2) Mechanical factors extrinsic to the skeletal segments, such as pressures and tensions resulting from the growth of contiguous segments, from muscle pull, &c., are not of primary importance as active factors in the development of skeletal form, but are of great secondary importance in providing conditions suitable for the action of the intrinsic factors and in bringing into existence the detailed perfection of form required of the functioning skeleton.

(3) The gross form of the bony skeleton, in the case of replacing bones, is predetermined by that of the cartilaginous skeleton. Certain facts suggest that an important condition affecting early osteogenesis is a loosening of the perichondrium around the middle of the cartilaginous shaft through the growth of the two epiphyses.

### Mr. G. L. Purser.—*A Reconsideration of certain Embryonic Stages.*

## Afternoon.

### Miss P. M. Jenkin and Dr. E. B. Worthington.—*A Symposium on the Ecology of African Lakes.*

Dr. E. B. Worthington.—Until the last few years biological work on the African lakes has consisted largely of the collection and description of species within the different lakes. The ecological side was hardly touched till the Government fishing surveys of Lakes Victoria, Albert and Kioga in 1927–28. There are two methods of working out the ecology in such lakes. One is to start at the top of the chain with the large predaceous fishes and reptiles. By catching these in large numbers and examining their stomachs and gonads, their feeding and breeding habits are ascertained. The same is done with their food, and so the 'food-chains' in the lake are built up link by link down to the phytoplankton and other vegetation. The other method is to start at the bottom of the chain, to find out as much as possible about the physical and chemical conditions, and to link up these with the plant life and so with the animal life. Of course, both of these methods as well as quantitative collecting of the plankton, nekton and benthos are necessary for a thorough ecological survey.

The method of building up food-chains is well illustrated by the conditions in lake Albert. At the top of the chain is the great Albert Perch (*Lates albertianus*) which is itself eaten only by human beings and crocodiles. It feeds on the Tigerfish (*Hydrocyon*) and other fishes. The tigerfish eats small *Haplochromis* which eat shell-fish which eat minute algæ and detritus. Other food-chains lead to the phytoplankton and large aquatic plants ; and aquatic tortoises, birds and mammals enter into the ecology also. In Victoria and Kioga the food-chains are rather simpler owing to the absence of large predaceous fish. Further ecological work will be done on Lakes Rudolf, Baringo and Edward in 1930–31, and the object of the paper is to invite criticisms and suggestions as to methods of research rather than to recount results from past work.

Miss P. M. Jenkin.—By examining the physical and chemical conditions in lakes the ecological problem discussed by Mr. Worthington can be attacked from the other end: and by measuring those conditions in conjunction with the distribution of the smaller organisms, at different times and places, another chapter may be added to the story told by the food-chains. In all environments there is a constant interchange of chemical substances between the organisms therein and their environment, but when this latter is watery, the estimation of the interchange can be most readily made.

Many of the simpler chemical substances to be found in solution in fresh water together form the food supply of the phytoplankton, and their absence may act as limiting factors for growth. Such substances are present in quantities directly dependent on the nature and amount of all the living and decaying organic matter in the water. It should be possible to make accounts for phosphates or carbon dioxide, for instance, balance.

But there are other factors influencing the organisms in a lake, by altering the conditions under which they live. Of these the two most important are :—

1. *Climatic* factors changing the physical conditions and having important effects, both direct and indirect, upon fauna as well as flora. In extreme cases these, too, may act as limiting factors.

2. *Geological and Topographical* factors affecting the chemical conditions in the Lakes by the supply and withdrawal of water and salts in solution. The Rift Valley Lakes in Kenya, for instance, have no outlet, and the consequent concentration of soda, dissolved out from surrounding alkaline lavas, varies so widely (L. Naivasha, 0·004 N. alkali reserve, to L. Nakuru, 0·29 N.) as to be the apparent cause of sharply marked differences in fauna and flora in the series of five lakes examined. If this apparent correlation be true the problem arises as to which of the links in the many food-chains may be directly influenced by such a factor, and which only indirectly.

The work on the Rift Valley Lakes is unfinished and is only referred to in the hope of raising suggestions and criticism on methods of further investigation. It seems to indicate the possibility of finding, in Africa, series of lakes varying quantitatively in one or two fundamental factors only, and offering a line of investigation into the relation of the environment to the lowest and most important members of the food-chains.

## SECTION E.—GEOGRAPHY.

### Thursday, September 4.

Mr. W. W. Jervis.—*General Introductory Survey of the Bristol Region.*

Mrs. D. Portway Dobson.—*The Bristol District in the Prehistoric Period.*

The simplest way to explain the occupation of the Bristol district during the prehistoric period is by means of distribution maps. These shew that in lower Palæolithic times the district was extensively occupied, for animal remains are found in the caves and fissures of the limestone rocks, and also in the Pleistocene gravels of the river valleys. Human occupation in the pre-Mousterian times is only proved, so far, for some of the gravels.

In upper Palæolithic times the distribution is in some respects the same, as certain caves contain the bones of animals belonging to the fauna of both early and late Pleistocene times. The newer loess, however, also shows traces of animal life belonging to upper Palæolithic times, as do certain cave sites such as Avelines Hole, which have yielded no bones of the older fauna. Artifacts probably of Mousterian times have been found, and a great quantity of flint implements and other traces of Aurignacian Man have been recovered from the Mendip caves.

Of Neolithic times there is at present scant trace, save in two caves, but of the Megalithic period, or the time of transition from the stone to the true Bronze age, there is abundant material, illustrated both by megalithic structures and beaker pottery. Generally speaking, this comes from the high or moderately high ground, with a few notable exceptions.

With the full Bronze age circumstances seem to have changed, for though the majority of existing barrows are above the 100 ft. contour line yet there are numerous traces of the occupation of the flat lands by people in this period, and the quick deposition of peat and the intensive cultivation of the fertile alluvial lands have probably obliterated many more barrows and covered up both implements and, possibly, living sites of the more or less indigenous people of the middle Bronze times and of the invaders who came into the country during the first half of the last millenium B.C.

In the early Iron Age the caves were again occupied, and so were the swamps south of the Mendip Hills where the lake villages were constructed. Judging by the immense number of hill-top camps and the remaining Celtic field systems most of the settlements of the early Iron Age were also on the hills. Thus the nature of the country seems materially to have influenced the settlements of prehistoric man in this district, and while the limestone caves gave harbourage to the palæolithic hunters, the Cotswold Hills were also congenial to the long-barrow builders, while the low-lying country supplied the needs of at any rate some of the folk of the succeeding centuries.

### Col. E. W. Lennard.—*Some Intimate Bristol Connections with the Overseas Empire.*

This paper collated under geographical headings the links, some well known and others more obscure, between the City and Port of Bristol and Britain's past and present overseas Empire. Much of Bristol history is also Empire history and the city has been not unjustly dubbed 'The Cradle of the Empire.' Before John Cabot's first voyage in 1497, many Bristol attempts to seek Atlantic territories had been made. Bristol readily found for the famous Cabot voyages of 1497 and 1498 ships, money and men. Following these voyages there was established at Bristol 'The Company Adventurers to the New Found Lands,' the pioneer corporation of the British Empire.

From the fact that John Cabot's pension after his return to Bristol was paid by one Richard Ameryk, senior collector of Customs, and later Sheriff of Bristol, an interesting theory with regard to the first naming of the new continent was discussed. From the time of discovery onward touch was maintained by Bristol with the new continent, and in 1603, Captain Martin Pring of the city explored much new coast line and discovered Plymouth Harbour at which the Pilgrim Fathers landed seventeen years later. Sir Ferdinando Gorges, the 'father of English colonisation in North America' resided at and died in Bristol. He was proprietor of the entire Province of Maine, New England. There were other Bristol settlements from which both counties and towns named after the city have sprung up. That between 1654 and 1685 10,000 emigrants shipped or were shipped from Bristol is revealed by recently discovered MS. books at the city Council House. In discharge of a debt of £16,000 to Admiral Sir William Penn, born and buried in Bristol, his son William Penn received the grant of Pennsylvania which by the King's order was named after the Admiral, his illustrious father. The first settlements in this new province were made from Bristol. Fox, Whitefield, Wesley and Burke were all intimately associated with both Bristol and the American Colonies. The growth of Bristol trade with America was traced and the fact recalled that with the western port lies the honour of establishing the first passenger and mail steamship communication with the American continent.

From its discovery by Cabot, Newfoundland was vitally connected with Bristol over a long period of history, and the first Governor of the Colony, John Guy, sailed from Bristol with emigrants of both sexes in May, 1610. A further Bristol settlement was established in 1618. The importance of the Newfoundland fishing trade right up to the eighteenth century was discussed.

British claim to the territory of the modern Dominion of Canada is primarily based upon John Cabot's landfall in 1497. In 1631 a Bristol expedition explored much of Hudson's Bay and its members were the first Englishmen to winter upon Canadian territory. James Bay is named after their commander, Captain Thomas James, who took possession of that region in the name of the Merchant Adventurers of Bristol.

From 1526 onwards, Bristol's trading and other connections with the West Indian Colonies have been considerable, and for long she was the principal English port for these. Bristol merchants largely developed the sugar plantations and frequently owned them. Bristol's methods of supplying these plantations with both white and black labour were discussed. Prominent Bristolians figuring in the history of the West Indies are Admiral Penn who captured Jamaica, Captain Woodes-Rogers (the third Englishman to sail a ship round the world) who freed the Bahamas from their buccaneers and was appointed Royal Governor, and Henry Swinburne, the traveller who from Marie Antoinette received a grant of all the uncultivated lands in St. Vincent. Particulars of Bristol's once vast West Indian trade were detailed.

The first recorded Bristol trade connections with Africa were in 1552 and, following the abolition of the Royal Company's monopolies, an enormous commerce grew up with the West Coast Colonies. Immense shipments of slaves were made by Bristol ships to Virginia and the West Indies. The decline and ending of this traffic in 'black ivory' was noted, and later and more reputable trade connections detailed. T. E. Bowditch, born in Bristol, was the first Englishman to penetrate to the interior of Africa, and his writings in 1819 led to the British Government taking over the Royal African Company's territories.

In the early building of the Indian Empire by the East India Company, the important parts played by many Bristolians upon sea and land were examined, as was also Bristol's contribution of great soldiers and administrators in later years. Past and present trading links were detailed.

In conclusion, the late Earl of Meath's comment that Bristol's contribution to the Empire is perhaps greater than that of any other city was quoted, and modern Bristol's pride in her Empire heritage stressed. Bristol's present-day Docks and Empire-wide industries were mentioned and, as evidence that something of the pioneering spirit of the fifteenth century remains, Bristol's fame in air-craft construction and her early establishment of one of the great municipal airports of the Kingdom quoted.

AFTERNOON.

Excursion in and around Bristol.

## Friday, September 5.

**Presidential Address** by Prof. P. M. ROXBY, on *The Scope and Aims of Human Geography.* (See p. 92.)

Mr. PEIRSON FRANK.—*The South-West Lancashire Regional Town Planning Report.*

Reference is made to some of the contents of this report, including a review of the physical and geographical characteristics of the region; the existing conditions and tendencies, future urban development ('ribbon' or grouped); zoning proposals; communications by road, rail, canal and air; proposed open spaces; future mining; preservation of natural amenities; architectural amenities; public utility services; coast erosion; land drainage. The composition and work of the Advisory Committee and of the proposed Statutory Joint Committee are alluded to and a number of maps and diagrams are shown.

Prof. PATRICK ABERCROMBIE.—*Satellite Towns.*

Discussion, opened by Dr. VAUGHAN CORNISH.

AFTERNOON.

Mr. S. J. JONES.—*The Historical Geography of Bristol.*

The defensive value of the tongue of sandstone overlooking the marshes of the Frome-Avon confluence above the Avon Gorge was probably realised in the period following the withdrawal of the Roman legions, but there is no definite evidence of fortifications on the landward side prior to the Norman castle and walls. With increasing security, the trade potentialities of the site in relation to the unique system of waterways leading to the Severn estuary and thence to Ireland and the open sea were duly appreciated. Traditions of early trade linger in the aisles of St. Mary le Port and St. Nicholas despite many architectural changes. With the name of Canynge one associates the expansion of trade into more remote parts of Europe and the completion of St. Mary Redcliffe, in which the virility of the merchant class found full expression. The impetus to trade and industry which followed the discovery and colonisation of the New World stimulated a territorial expansion rendered possible by the dissolution of the monasteries. Despite the later restrictions of monopolists, development was steady under the ægis of the Merchant Venturers, who not only exploited the New World, but also gained an increasing hold on European markets after the weakening of the Hanseatic League owing to internal dissensions largely of a religious character. The New World connections were ultimately the basis of the tobacco and chocolate industries. Although Bristol has never recovered from the long delays which mark the history of its dock improvements, natural advantages have nevertheless prevented a serious decline, and it remains an important port for West Indian and American products. The establishment of an air-port at Whitchurch is the latest step in the history of the development of the city's communications.

Dr. V. STEFANSSON.—*Bristol and the Ancient Colonisation of Greenland.*

## Saturday, September 6.

Excursion to The Mendip Region, visiting Wells, Glastonbury, Wookey Hole, Cheddar Gorge and Burrington Coombe.

## Monday, September 8.

Mr. A. G. OGILVIE.—Report of Committee on *Human Geography of Tropical Africa.*

Mr. S. J. K. BAKER.—*The Population Map of Uganda: A Geographical Interpretation.*

Uganda consists fundamentally of a long-denuded peneplain in which the Archaean foundation is partly covered by Palæozoic 'Argillites.' Earth movements occurred in late Oligocene times and produced two low but gigantic domes. These trend from north to south, and along their main axes the Western and Eastern Rift Valleys were respectively formed. The outpouring of lava now represented by the Mfumbiro and Toro volcanics in the west and the Elgon Series in the east may be associated with the rifting movements. The uplifted peneplain of Uganda lies at an average elevation of 4,000 feet in the shallow depression between the two domes. To the south the waters of Lake Victoria have gathered while the marshes of Lake Kioga occupy the heart of Uganda.

The country adjacent to Lake Victoria is well watered, the mean annual temperature is high and the range is small. The flat-topped hills of Buganda support a rich savana vegetation; the intervening valleys are usually choked with papyrus swamps. Away from Lake Victoria, with decreasing rainfall and an increase in the range of temperature, a drier type of savana prevails. However, on the slopes of Mount Elgon and in the Western Highlands the rainfall increases with altitude. In the Western Highlands a rich cover of mountain grass has an extensive distribution.

The salient features in the distribution of population may be discussed in the light of this physical background :—

i. In the Kingdom of Buganda there is a strong concentration of population in the vicinity of Lake Victoria, a fact which has its counterpart in the centralisation of administrative and educational services around Kampala. This is 'par excellence' the area of banana culture, though cotton has recently achieved importance as a commercial crop. After a slight break, related to the distribution of tropical rain forest to the west of the Victoria Nile, there is another area of concentration in the southern part of the Eastern Province. The population here provides the labour for the growth of cotton, and by its rapid development this region is inevitably challenging the supremacy of Kampala. The distribution of population round Mount Elgon is interesting, for the amazingly populous southern slopes may be contrasted with the sparsely peopled northern slopes.

There are some unpopulated areas within this broad zone which stretches from Buganda eastwards and these are explained by the conditions of health. The Sese Islands, for example, were depopulated by the sleeping sickness epidemic of 1904–6, although people have been reintroduced since 1919. The contrasts in the shore line distribution round Lake Victoria result from the ravages of sleeping sickness.

ii. An outer zone stretching roughly from S.W. to N.E. shows a sparse distribution. The eastern part of Ankole, infertile of soil and lacking in rainfall may be included within this zone. The whole of north-eastern Uganda, especially the Karamoja district, contains scanty pastures sparsely peopled by semi-nomadic tribes.

iii. Population increases upon approach to the Western Highlands. Kigezi and Western Ankole have a dense distribution comparable with that of Ruanda and Urundi across the boundary. Toro and Bunyoro are less thickly peopled though they each show a concentration round their respective administrative centres at Fort Portal and Hoima. In the West Nile district beyond the arid trough of the Nile there dwells a considerable population.

This relative distribution represents an adjustment to environmental conditions, but it must not be considered as final; control of disease and the development of communications may modify its detail. It is certain, too, that Uganda could support a much larger population, though the modern economic development of the country bears witness to the ' human effectiveness ' of the present distribution.

## Mr. W. Fogg.—*Morocco: some Aspects of the Sebou Basin.*

The Sebou is the largest river in Barbary, its basin having an area approximately one and a half times that of the Thames. The N. part of its basin is the S. and S.W. slope of the Riffan Arc, with a surface of impermeable rock, the run-off from which is the principal cause of the winter and spring floods in the lower basin. Overthrust phenomena are of much interest in this section.

The S. part of the basin is the N. and N.W. part of the Middle Atlas, with massive tabular and folded limestones at the surface, the vauclusian springs of which maintain the large flow of the river in its lower basin, during the summer drought.

The lower basin is a large clay plain of very low altitude, on which the winter flood waters spread and form extensive marshes, behind the high levees of the Sebou.

The seasonal advance and recession of these marshes has given rise to a curious seasonal migration of the natives, and some interesting land reclamation schemes for French colonisation.

In the Middle Atlas an extensive and well-defined Alpine nomadism is practised by the native Berber tribes, but on the Riff slopes sedentarism is the rule, both these forms of adjustment showing close relationship to the physical conditions.

The souk, or native market, is a very important geographical feature, in that it very largely explains the great lack of native towns, which is one of the most striking features in the geography of Morocco.

## Mr. V. S. Swaminathan.—*The Villages and Village Life in the Tamil Country.*

The paper deals with a stretch of country, forming a well-defined natural region, roughly 54,000 square miles in area, with nearly 22,000,000 inhabitants; coinciding approximately with the ancient Pandya, Chola and Pallava kingdoms (forming the East Coast Central and East Coast South divisions of the Madras Presidency, after the census returns); having a culture and civilisation of its own, and cut off from the north by virtue of its geographical position, though this was not complete enough to prevent the peaceful penetration of the region by the Indo-Aryan emissaries, which began many centuries before the Christian era. Tamil is the language of the overwhelming majority of the population.

Early tradition, backed by historical data, points to the existence of wealthy cities on the one hand, and of highly developed village institutions involving real local self-government, and administered on an elaborately organised system on the other. The importance of rural activities and of village life in the Tamil Nad is great, since it lacks mineral wealth, and possesses no great industries comparable in magnitude with the textile manufactures of Bombay and Calcutta.

The influence of climate and relief features in determining land-utilisation, and hence the distribution of population and settlements (villages), are dealt with in some detail. A few typical villages from different districts scattered over the area are touched upon, followed by a summary of the salient features of the village life in the Tamil Nad.

### Afternoon.

Dr. C. E. P. Brooks.—*Climatic Changes in Historic Times.*

It appears probable that there have been during historic times certain periods when the climate of large areas differed appreciably from that of the present century. The conditions are discussed during a number of critical periods, as far as the available evidence permits :—

ca. 2200 B.C. Dry in Europe and western Asia. In western and central Europe the rainfall was in places only about half the present amount.
800–400 B.C. Wet and stormy, especially in central Europe.
0–200 A.D. Approaching present conditions.
500–800 A.D. Probably rather dry, especially in central Asia.
1200–1400 A.D. Wet and stormy in north-western Europe.
1700–1750 A.D. Dry in western Europe.

Prof. A. E. Douglass.—*Past Changes in Climate in Relation to Settlements in the New World.* (In Section A Room.)

The annual rings of trees provide a means of studying certain characters of past climates. In the south-western parts of the United States showing an annual rainfall of 15 to 25 inches, the rings of the *Pinus ponderosa* give a very effective record of rainfall variations from year to year, increased growth accompanying increased rainfall. Long series of such ring values have been studied and variations have been found related to the eleven-year sunspot cycle.

Since, in the region referred to, the climate is fairly constant over a large area, annual characters in rings may be traced over an extended forest district and thus exact dates may be carried from tree to tree. For example, we can pass from the older central part of a living tree to the outer part of an old building beam in a village one hundred miles away, and then from the central part of the latter beam to the outer part of, perhaps, a log from a distant prehistoric ruin. Thus, a chronology of rings and rainfall has been carried back to A.D. 700. But this exact dating of the rings gives also the actual years of cutting the logs provided the outermost rings are still present. Thus, in return for providing material for building a climatic history the archæologists have received the building dates of some forty prehistoric ruins. The oldest and the largest of the ruins so far dated, is Pueblo Bonito (New Mexico) whose construction period extended from 919 to 1127 A.D. The method can be successfully applied in many parts of the world, but not necessarily in all.

## Tuesday, September 9.

**Discussion** (Sections C, E, H) on *The Relations between past Pluvial and Glacial Periods.* (See programme of Section H.)

### Afternoon.

Excursion to the Avon Gorge, Blaise Castle and the Aust, for the Cliff section.

## Wednesday, September 10.

Prof. Ll. Rodwell Jones.—*Physical Factors concerned in the characteristic Functioning of the Port of London during the Period* 1800 *to the Present Day.*

This paper deals with the tidal and channel conditions of the Thames estuary, as these affected shipping throughout the nineteenth century.

To the natural excellence of the estuary was due the fact that little artificia improvement was carried out till near the end of the century, while the very heavy programme of dredging which has given 30′ L.W.S.T. up to Purfleet, 27′ to the middle series of docks, and 20′ to the Surrey dock, has all been carried out in the last twenty years under the energetic control of the P.L.A. The very considerable tidal range gives, of course, much greater depth than these for most of each tide.

The points of critical limiting depth in the early and middle century were shown by examples from early charts. The position of the legal quays, the cartage distance of wharfs from the city, the convenient distance at which a lighter could work (always on the tide) between vessel and wharf; all these were factors in the crowding of the Pools in the earlier part of the century.

The physical conditions favoured loading from and into lighters, and so important became the river wharfinger and lighterage interests, that a 'free lighter' clause was introduced into the whole series of Thames Dock Acts; and in the docks to-day much of the loading and unloading is done by lighters.

With the coming of big steamers in the 80's and 90's, shoals below Gravesend in the Lower Hope and in the Sea Reach, began to give trouble. These have been successfully dealt with, and the channel is largely self-maintaining.

Diagrams were shown illustrating the changes in the outer estuary, below the limit of dredging, for the period from 1812 to the present day. Some of the swatchways appeared to be compensatory in character, the decay of one accompanying the forming of another.

Brief allusion was made to the major new works which were keeping the port of London abreast of the times, viz., the enlargement of the entrance of the older docks, the splendid George V. dock, and the quite new entrance lock, dry dock and landing stage at Tilbury.

### Dr. S. W. Wooldridge and Mr. D. J. Smetham.—*The Geographical Features of the Boulder Clay Margin in Essex and Hertfordshire.*

The limit of the Boulder Clay and its associated gravels in both Essex and Hertfordshire coincides with a marked change in every aspect of the geography of the country. The Boulder Clay area shows a general unity in its features of morphology, drainage, soil, vegetation and surface utilisation. In Essex these features are sharply contrasted with those of the London Clay country with its broad low-lying stretches of drift-free pasture land diversified by scattered remnants of a dissected high level plateau retaining a capping of sand and gravel. The contrast is clearly reflected in the historical geography and agricultural development of the area.

An equally striking contrast which, however, involves somewhat different factors occurs in Hertfordshire where the Boulder Clay country gives place abruptly to the Chalk plateau. Particular attention will be drawn to the well-marked geographical characteristics of the Vale of St. Albans where the glacial deposits project westward in a clearly defined strip between the Eocene escarpment and the base of the Chiltern dip-slope and where their influence upon settlement and agriculture is of especial interest.

An important and unrecorded feature of the area as a whole is the widespread occurrence of loams essentially identical with true loess which have exercised considerable control on the agriculture and which may be regarded as marking the continuation of the loess belt of continental Europe into southern Britain.

### Mr. H. J. Wood.—*Agricultural Distributions in Scotland.* (With exhibition of maps.)

With a view to compiling an agricultural atlas of Scotland a series of maps, on a scale of eight miles to the inch, have been constructed to show agricultural distributions on a dot basis. Two base maps were necessary, one of parishes, and the other to show the extent of the uncultivated area, *i.e.* moorland, loch and forest. The latter was based on the inch Ordnance Survey maps of Scotland, black outline edition. These maps together served as a basis for plotting the agricultural statistics generously furnished by the Scottish Department of Agriculture and relating to 1927, a normal year.

By way of introduction four sets of graphs were drawn to show the main historical features of agriculture north of the Border, and to provide a setting for the year 1927.

The two sets showing acreages under various crops and the numbers of animals, respectively, deal with the period 1866-1929. Those concerned with produce, of cereals in the one case and crops other than cereals in the other, deal with the period 1884-1928. In addition a map was constructed to show the monthly rainfall régime of certain Scottish stations.

The ten maps showing distribution of the major crops, and the four showing animal distributions, portray the main features of Scottish agriculture and provide the broad setting. Only detailed agricultural research in the field can solve the many interesting problems that they raise.

Miss C. P. SNODGRASS.—*Some Aspects of the Agricultural Geography of the Lothians and Berwickshire.*

In order to show the influence of the physical and economic features of this region (Midlothian, East Lothian and Berwickshire) on the agricultural distributions, maps of the topography, geology, rainfall and population density are exhibited for comparison with a number of cartograms showing the distribution of the principal crops and some of the principal classes of livestock.

A brief statement is given regarding the influence of such environmental factors as altitude, rainfall (amount and seasonal distribution), soil type, and distance from a large consuming centre upon the agricultural distributions, and the main types of farming which have been developed in the region as a result of these distributions are indicated.

These main types are :—

(*a*) Farming for the production of cash crops in Lower Mid- and East Lothian.

(*b*) Dairy farming for the production of fresh milk in Western Midlothian. (Dairying for the production of fresh milk also overlaps cash crop farming in lower Midlothian and the N.W. corner of East Lothian.)

(*c*) Semi-arable sheep farming in the higher parts of the farmland in East Lothian and S.E. Midlothian and on almost all the farmland in Berwickshire.

(*d*) Hill sheep farming on the moorlands.

# SECTION F.
# ECONOMIC SCIENCE AND STATISTICS.

## Thursday, September 4.

Mr. G. PONSONBY.—*The Incidence of the Cost of Road Maintenance and Construction.*

With the important exception of the period of about two centuries, during which time a certain proportion of the roads of Great Britain were maintained and constructed under a system of controlled private enterprise—by the Turnpike Trusts—the amount of resources devoted to this purpose in almost all countries and at all times has been largely determined by governing authorities. And in particular since the beginning of the present century, it has been generally accepted that such authorities are at the same time the most appropriate and effective bodies to be vested with the responsibility of this work.

The result of this governmental control over so large and important a branch of national economy has been, that both the quantity and direction of economic resources which communities have collectively applied to the maintenance and construction of roads, have been determined by motives other than purely economic. And whereas over wide fields of economic activity the amount of economic resources applied to particular ends has been determined with the object in view of obtaining a maximum net gain, expenditure upon roads has been determined by the deliberations of rulers and administrators, with whom political rather than purely economic considerations have been uppermost. But their policies are not without their economic consequences. It is with the latter that this paper is mainly concerned.

We can well imagine a state in which there was some kind of control whereby resources were devoted to road construction and maintenance in exact accordance with economic requirements; in which the users of the roads were a small and well defined group, legitimately bearing the whole burden of road expenditure, raising money from amongst themselves or borrowing at current rates of interest, and spending in close relation to the gains to be derived from such expenditure.

In the absence, however, of those strictly financial considerations which control those responsible for a balance sheet, whether in public or private undertakings, there is the possibility that resources will not be devoted in such a way as will reap a maximum economic gain. So that there is a positive economic loss, measured by the difference between that maximum net gain which would have accrued had resources been applied with a view to obtaining that maximum net gain, and the net gain accruing as a result of applying resources in compliance with other motives.

I suggest that the ways in which a positive economic loss as defined above may come about, may be classified under three main headings.

First, in cases where the total amount devoted towards the provision of road transport is either far beyond, or indeed considerably less, than that which would have been applied had genuine economic demand been properly reflected. Cases, that is to say, on the one hand, in which it would have been to the interests of road users to have been relieved of payments towards road maintenance and construction, in spite of a consequential lowering of the standard of the roads; or, on the other hand, cases in which it would have been to the road users' benefit to contribute larger sums towards the roads, since the benefit derived from the better roads which would be the result of additional expenditure would more than outweigh the burden of the extra payment incurred.

Over-expenditure as above defined is likely to occur when, say, military, administrative or other political considerations, such as the relief of unemployment, lead to the provision of a road system which, however beneficial, does not bring benefits commensurate with money expended. Whereas inadequate expenditure is likely to occur as a result of a lack of organised representation of road users, or a lack of administrative machinery to carry out the necessary works.

The second set of circumstances in which a positive economic loss may result occurs when the burden of maintaining and constructing roads is thrown indiscriminately both upon those who make use of the road and those who make no use of it whatever, or equally upon those who make considerable use and those who make only a slight use of the roads in question. (In the first case we assumed road users to be a clearly defined group, among whom all road costs were equitably distributed.)

In this case, by throwing the burdens of road construction and maintenance in a manner unrelated either to benefits derived or costs incurred, the machinery of Government has become an engine of wealth distribution, and certain road transport operations are undertaken, which, but for the fact that part of their 'all-in' costs (including road costs) have already been paid for by others, would not have been undertaken at all. And those to whom little or no benefit accrues, but who nevertheless are called upon to bear a portion of the real costs of road transport, suffer from taxation the character of which is definitely onerous.

A third circumstance of Governmental control occurs, when, although there is a perfectly reasonable and adequate *total* expenditure upon roads at any one time, and the total burden apparently falls equitably upon road users and road users alone, yet the allocation of the sums collected is not spent in such a way as to bring about maximum economic advantage to the whole body of road users. One form of traffic is favoured against another; one geographical area is starved whilst another is over-provided in the matter of roads.

The consequences of this third form of deviation from strictly economic consideration, are similar in character to those of the second.

In the light of these possible circumstances, it is the purpose of this paper, first to analyse the sources from which sums spent upon road construction and maintenance in Great Britain since the beginning of the century have been derived; and, secondly, to examine the manner in which those sums have been expended.

**Discussion** on *The Value and Limitation of Costing in Industry and Agriculture*.—Mr. A. Cathles, Dr. J. A. Venn, Prof. J. H. Jones.

## Friday, September 5.

Sir John Mann, K.B.E.—*Some Neglected Aspects of the Housing Problem.*

The paper does not attempt to describe Slum Conditions or deal with statistics of overcrowding and its results, nor does it deal with post-war efforts to improve conditions by legislative and voluntary means. It submits for discussion notes on some aspects of the Housing Problem—which the writer (from many years' practical touch with the question) thinks have received far from adequate attention.

*Management.*—Mathew Arnold contended that ‘ conduct is three-fourths of life ’ and certainly the failure of many landlords of small houses to do their duty as well as the behaviour of many tenants in the use of their houses, is responsible for three-fourths of the difficulties in decently housing working people. Conduct counts for more than construction; good habits are as important as good habitations; wise management will do more for housing than costly capital expenditure. The acceptance of rent for a small dwelling involves a continuing duty on the landlord to see that his property is occupied without danger to the health and comfort of his tenants and their neighbours. This continuing duty of a landlord who sells Shelter distinguishes his function from that of the sellers of Food or Clothing who have no responsibility for the use made of the goods sold.

As small houses deteriorate, their ownership tends to drift into the hands of ignorant, and at times unscrupulous, landlords. Generally the worse the property, the worse the landlord. The landlord has immense and most valuable power over his tenants—a power which has not been sufficiently exercised for good and is too often completely neglected. But rent restrictions at present paralyse the landlord's powers. These restrictions will be removed, probably gradually: as they are removed the control of tenants' use of their houses will again become possible and should be enforced by the Health Authorities, both directly and indirectly through the landlords, who will no longer have an excuse for not overtaking arrears of repairs.

Landlords and tenants alike require to be educated, and it may be added, also many Local Authorities. This educative process must take place through an aroused public opinion, bringing pressure to bear upon the Authorities to enforce their powers, and also by the encouragement of firm, wise and sympathetic methods of management, including those originated many years ago by Miss Octavia Hill. Some details will be given of these methods and of their striking success and growing importance.

*Simpler Standards.*—Good management will ensure that the best use is made of existing houses, but by itself it cannot relieve overcrowding which can only be overcome by providing additional houses.

The majority of the slum dwellers, variously estimated at from 70 per cent. to 90 per cent., are decent, hard-working people, doing their best to be clean and orderly, but too often with little chance of success. They respond wonderfully to improved conditions. Unfortunately only a relatively trifling proportion of those who can afford the rents plus expenses of working at a distance from their homes, succeed in becoming tenants of new subsidised houses with their admirable amenities. The very large number of those decent people who do not so succeed and are fated to remain in the slums, along with the much smaller number who are less deserving or whose conduct is erratic, are the crux of the housing problem. The number of these unfortunates is so great that the nation cannot afford to pay subsidies to re-house them all, even if they were all suitable for immediate transfer straight from the squalid slums.

It is suggested that supreme attempts must be made to meet such cases partly by more active reconditioning on the one hand, and on the other hand by a distinctly new departure in housing policy—the provision of new types of houses of simpler standard, to come ‘ midway ’ between the unregenerate slums and the well-equipped modern houses—to act as it were as stepping-stones from the slums.

The present standard for new houses, most admirable in itself, is beyond what the nation can afford at present, and even when reduced by subsidy is still beyond the reach of the low-paid workers. Building is a ‘ sheltered ’ industry and its present scale of costs involves rents beyond the reach of those in ‘ unsheltered ’ occupations. The building subsidies operate to keep down the general wage scale.

It is recognised that the reduction in standard which is advocated will be unpopular but it will hasten building and its drawbacks should be minimised, if not fully compensated, by improved methods of management by owners and better supervision by Local Authorities.

*Reconditioning.*—Much may be done by reconditioning and by 'making down,' converting large houses into several small ones, but mainly by compelling the owners of existing slums to put them in order where possible, and thereafter seeing that they are kept in good order. Good second-hand houses are better than neglected slums, just as a decent second-hand suit of clothes cleverly patched perhaps, is better than rags for those who cannot afford new clothes. The purchase of existing slums for reconditioning requires very special care ; it is being increasingly attempted, mainly by Public Utility Societies, but they are only able to touch a tiny corner of the field. The financial results are very varied and at present insufficient to encourage private enterprise to buy and recondition on any large scale. As the capital value of slum property falls (it is at present thought to be artificially inflated) its acquisition for improvement and renovation should become profitable to both private enterprise and semi-philanthropic bodies. So long as subsidies are granted for building new houses, so long can claims be justified also for financial assistance for approved schemes of reconstruction.

*Midway Houses.*—In the writer's opinion subsidies are not justified for new houses of the present high standard and should be confined to encouraging experiments in various types of simpler dwellings. They should be elementary in fittings, but strong. They must be controlled and managed firmly and sympathetically. Their success will depend largely upon their management. Just as classes of tenants vary, so the types of houses should vary. A very special type is urgently required for the struggling poor and even for the lowest class of tenants, the submerged, who are weak and characterless, and rejected by all self-respecting landlords. If ever a subsidy is justified it should be for the provision of housing for this class—if on no other grounds than the savings in cost of Police and Public Health Administration.

After careful inquiry, a Municipal Housing Commission in Glasgow before the war advised that a trial be made in building new types of simple houses, whose mere existence would justify clearance schemes, remove any excuse for misplaced leniency in administration and provide for those dispossessed or rejected from other dwellings. This recommendation was adopted by the Corporation of Glasgow, but partly through the intervention of the war, little action has yet been taken. Experiments, however, have been made elsewhere and prove that the idea is by no means Utopian. Outstanding examples are to be found in Holland—at the Hague and Amsterdam. These are extraordinarily interesting and instructive and will be briefly described.

Sir Josiah Stamp, G.B.E.—*Report of the Committee upon Inheritance.*

## Monday, September 8.

Prof. P. Sargant Florence.—*A Statistical Contribution to the Theory of Women's Wages.*

There is evidence that the low wages obtained by women compared to men in similar industrial occupations does not reflect a corresponding inferiority in their efficiency and reliability, as measured by various objective tests. And though the heaviest types of work are closed to women, there are plenty of occupations still pursued by men for which women are not physically unfitted. Under these circumstances one would expect employers, in so far as they are 'economic men,' to be continually substituting women's labour for men's, thus getting 'better value for money.' In fact, however, statistics do not show such substitution on any large scale.

How is this apparent paradox to be explained ? Is women's efficiency estimated as low, compared to men's, as their comparative wages indicate ? Are employers not economic men ? Do the conventions or such institutions as Trade Unions block purely economic action ? All these theories have been put forward, but the most important factor has not hitherto received much attention : the restricted supply of available women.

Except among the very poorest 'strata,' or where (as in the textile industry) skill has been acquired, or where homework is still carried on, women leave industry on marriage and do not readily re-enter the labour market. The supply of women available for employment consists therefore chiefly of spinsters (and, possibly, widows) of working age, i.e. 14 to 65. But the population Census and other statistical enquiries show that in typical manufacturing centres the great majority of unmarried women of working age are already occupied. And from the minority possibly available as substitutes for men must be subtracted invalids, schoolgirls of working age, inmates of institutions, the idle rich, widows with young children, women 'keeping house' for widowers or other relatives, girls helping their mothers when the family is large, etc.

No doubt some of those less available women might be induced into industry *at a price*, but the 'economic' employers may be unwilling to raise the whole level of women's wages merely to obtain those marginal cases. The marginal supply price of the number of women employed *at present* is low, probably because that number consists of women who merely have to keep themselves or whose bargaining position is weak. But if inroads are to be made on the less available supplies of women, the wages offered would have to rise steeply.

Since the family tie makes women particularly immobile, cheap supplies cannot readily be obtained from oversupplied districts, such as mining areas, nor can an employer take on women in his own district one by one whenever he can get them cheap. Women do not customarily work side by side with men on the same job but are substituted, if at all, in *quanta ;* a battery of machines or a whole department is transferred *en bloc*. In part, the substitution of women for men that is theoretically to be expected fails to occur, because the wage the employer would have to pay to be sure of ample and continuous *quanta* of women's labour would be very much higher than current wages, and might entirely cancel the 'better value for money' that the current women's wage gives.

**Presidential Address** by Prof. T. E. Gregory, on *Rationalisation and Technological Unemployment.* (See p. 105.)

Major L. Urwick.—*Pure Theory of Organisation with special reference to Business.*

## Tuesday, September 9.

Mr. D. Caradog Jones.—*The Social Survey of Merseyside.*

The nature and purpose of a Social Survey. How it differs from a Regional or Industrial Survey. The justification for such enquiries. In character they should be scientific and co-operative. The ideal and the actual. Use of existing data combined with original investigation.

Definition and general description of the area of investigation. Its industrial and social development might be sketched as a background to the picture. The growth of population could be traced, analysing separately the figures of birth, death and migration. The population as at the last Census could be related to existing industries. Special consideration should be paid to locally important industries and occupations ; they could only be treated in any detail after investigation. A want of balance would thus be revealed between the supply of and the demand for labour. The economic consequences of a surplus of labour would then lead naturally to a discussion of the minimum standard of living.

Social reform in the narrow sense aims at improving the lot of those whose standard of life falls below the average level. The fundamental social problems are associated with lack of health, lack of wealth, lack of occupation, lack of education—and, affecting all these, lack of organisation. At the same time it is essential to be able to define the average in order to determine what is below the average. This provides a key to the selection of suitable subjects for special investigation. The normal is studied but special attention is directed to the poor rather than to the wealthy, to the unemployed rather than to the employed, to those who live in crowded and cramped conditions rather than to those who are comfortably housed.

Some of the measurements to be attempted. The design of the main enquiry card. The random sample principle in selecting households to be investigated. The qualifications of the investigator. Examination and coding of the completed cards. Analysis of the data and preparation of tables. Publication of results.

This main enquiry to be supplemented by others. The collection of household budgets. Advantages and disadvantages of different methods. Difficulties to be overcome.

A more complete survey is possible in investigations where the total number of cases to be examined is reasonably small. Sub-normal types in illustration. Failure in life may be due to causes outside the personal control of the individual. On the other hand it may be due to innate incapacity or defect. Some of the problems to which the latter type of human failure gives rise. Outline of enquiry initiated. A central register for all cases investigated. How confidence is preserved as to the identity of the individuals concerned.

Other enquiries in progress, or contemplated if resources permit, relate to the industrial position and prospects of the area, to such questions as racial migration which is of some importance in Merseyside, to the welfare of infants and adolescents, to education and the use of leisure, and to organised religion. The difficulty of weaving all these different investigations together into a single coherent and logical scheme. A possible plan outlined.

## Dr. H. A. Mess.—*The Social Survey of Tyneside.*

The Social Survey of Tyneside was privately inaugurated in 1925 and occupied three years. It covered that portion of the North East Coast industrial region which lies along the tidal waters of the river Tyne. It was a study of the area as it has changed over a period of about a century, but with more detail for the latter part of the period. The method adopted was mainly assembly and interpretation of existing data, supplemented to some extent by direct observation. Social conditions in the Tyneside towns were compared statistically with those of the average English town of that category, e.g. county boroughs were compared with the average county borough of England and Wales.

Several salient features of the social life of the area were thus brought to light. Wherever possible social phenomena were mapped out in time and in space. Thus the high degree of overcrowding, found in practically all the Tyneside towns, was found to date back several centuries, and to be characteristic of an area extending from the Cheviots almost down to the Tees. A number of explanations commonly offered were thus shown to be untenable. It seemed probable that the low standards of a disturbed Border area, with subsequent early and rapid industrial development, were mainly responsible for it.

Attention was called to the high infantile mortality in several parts of the area, and to the very high tuberculosis mortality prevalent in almost every part of it.

Attention was also called to the unusually high proportion of large classes in the elementary schools of the majority of Tyneside towns.

The relation between population and industry was examined. Birth rates are above the average for the country, though dropping here as elsewhere. Until about 1881 the industries of the area attracted many immigrants, but about that date the balance of migration turned outwards. The present population is mainly dependent upon the depressed heavy industries, and there is exceptional unemployment. Partial revival of the staple industries, the rise of new industries, migration, and a falling birth rate will gradually bring about a new equilibrium between population and industry.

Modern rapid transport has had two marked effects on what were formerly a number of self-contained towns. It has interwoven them socially and industrially. It has also segregated the classes. The smaller industrial towns along the Tyne, almost entirely working class in character, have insufficient financial resources for the duties of local government; they are also lacking in social leadership. Their boundaries no longer correspond to social realities, and a reduction in the number of separate administrative areas is very desirable.

Attention is called to the special features of this survey. (*a*) It is the first social survey of a group of neighbouring towns. (*b*) It illustrates the possibilities, and also the limitations, of the method of assembly and interpretation of existing data. (*c*) The historical method throws much light on causation of social phenomena.

The practical results of the survey have been a quickened social intelligence and social conscience, some improvements in local administration, and several philanthropic experiments.

## Prof. F. W. Ogilvie.—*Margins.*

## Wednesday, September 10.

Mr. W. Hamilton Whyte.—*The Standard of Living and the post-War Trade Depression.*

Amongst the important economic features of post-war Europe are a diminishing rate of increase in population along with an increase in production. This has been accompanied by a fall in the cost of living, thus providing a greater margin for expenditure on non-necessary goods. An examination of existing data suggests that Great Britain has not been an exception to this general tendency. The proportion of children to wage-earners has fallen, the volume of production has expanded, while the level of prices has decreased. So far as the employed population is concerned the standard of living has not fallen. Great Britain has suffered more than most European nations through unemployment, but the reduction in the standard of the unemployed has been offset by an increase in the resources of relief and various forms of socialised income. Hence it appears as though the country has been able to maintain her standard in spite of an abnormal percentage of unemployed.

Does this analysis throw any light upon the problem of unemployment ? There are at least three probable causes of unemployment : (*a*) Slowing down of production. Such does not seem to have applied until recently. (*b*) Rationalisation by which production is maintained by a reduced supply of labour. This may ultimately check the decline in unemployment in the staple industries but it is not likely to lead to any big increase in the demand for labour. Even if it leads to a fall in prices through diminished cost, the demand for staple products is not very elastic. (*c*) Disparity between wholesale and retail prices. At present this disparity tends to widen. Certain factors encourage production in face of falling prices, such as reduction in costs and methods of control. Other factors tend to prevent retail prices falling in the same ratio as wholesale, such as increased expenditure on consumable goods at the expense of saving, an increase in the volume of socialised income and the relative stationariness of wages which form a big item in distributive costs. This disparity tends to expand the distributive trades disproportionately to other trades and thus lower our competitive power in international markets. A fall in retail prices should increase employment in the production of goods for which the demand is elastic, stimulate new industries by releasing new purchasing power and assist export trade by improving our competitive power. At the same time this new demand would be more variable since it could only arise after primary wants have been met. It would necessitate greater elasticity in organising production and a more rapid displacement of plant. It also implies greater mobility of labour and a rising standard of organising ability. It is along such lines that unemployment is likely to be permanently reduced.

This examination suggests that in Great Britain the high rate of unemployment does not necessarily imply a corresponding fall in the standard of living. The nation seems to have been able to maintain its standard and support an abnormal unemployed population over and above. But this does not minimise the gravity of the problem. In so far as unemployment is due to increasing expenditure at the expense of savings it means a decline in the nation's competitive power. If it indicates failure to reorganise industry it means a decline in enterprise. The maintenance of a large section of able-bodied workers in idleness, even if the nation can afford it, must in time lead to deterioration.

# SECTION G.—ENGINEERING.

## Thursday, September 4.

Internal Combustion Engines :—

Mr. T. F. Hurley and Mr. R. Cook.—*The Influence of Turbulence upon highest useful Compression Ratio in Petrol Engines.*

A series of tests were made on an E.5 Ricardo, sleeve valve, variable compression engine to determine the influence of turbulence on H.U.C.R. (Highest Useful Compression Ratio). The method employed was to observe the types of air movement obtained by fitting various directional vanes to the inlet ports of the engine and to correlate these observations with the corresponding H.U.C.R. of a standard petrol.

The outstanding feature of the air movement observations, which were made under 'motoring' conditions, was the difficulty experienced in producing indiscriminate movement as opposed to rotational movement. It was found that there was a strong tendency for the air to swirl about a vertical axis and even when a predominating indiscriminate motion was produced it was accompanied by varying degrees of swirl. It was also found that with rotational movement the pressure was least and the linear velocity greatest near the centre, which seems to indicate that the motion approximates to that of a free vortex. The presence of inward radial flow near the surfaces of the cylinder head and the piston was also demonstrated.

Turbulence was found to have a marked influence on H.U.C.R. Thus, the H.U.C.R. of an ordinary petrol varied from 5·7:1 to 7·9:1 according to the type of vanes employed, the former value being obtained with indiscriminate turbulence and the latter with a purely rotational movement. It would appear that, in this connection, the initial direction of the entering air is the determining factor rather than the initial speed.

Swirling motion was invariably accompanied by very harsh running, particularly at the higher compression ratios, whereas with indiscriminate turbulence the running of the engine was exceptionally smooth.

A possible explanation of the difference in the results obtained with swirling and indiscriminate turbulence is put forward and a suggestion is made that these experiments might form the basis of a design for a new type of combustion chamber.

## Mr. C. F. Abell, O.B.E.—*Some recent Progress in Air-cooled Aero-Engine Development.*

The chief factors controlling the development of aircraft engines are:—(1) The weight per horse-power; (2) frontal area and head resistance; (3) effect of high altitude conditions on engine performance.

These points are discussed and the progress of the air-cooled engine traced from 1919 when the aero engine was well advanced owing to the demands of the war, to the present time.

Several different types of air-cooled aircraft engines are considered and their chief points noted, with illustrations, as follows:—(1) Radial type; (2) in line type; (3) 'Vee' type; (4) hexagon type; (5) 'H' type.

While manufacturers of air-cooled motors have been engaged on the work of development in their own particular spheres, research organisations, both in this and other countries, have carried out many investigations into aerodynamic questions affecting the drag of air-cooled engines.

The special forms of cowling to which two such developments have led are illustrated and explained. In both cases considerable reduction in drag is obtained with consequent increase in efficiency.

In connection with engine development, some interesting detail points of design actually in production with modern air-cooled engines are reviewed and illustrated. These include:—

(1) The development of a split single-throw crankshaft to take the place of the normal solid crankshaft.

(2) The manufacturing problems in connection with crankcases, and the factors which led to the abolition of castings in favour of drop forgings.

(3) Developments in cylinder design from the solid steel cylinder with integral combustion head, to a later cylinder employing a loose head, formed by a light alloy forging.

(4) Increase in engine performances at high altitudes and methods adopted to secure these.

The paper is illustrated with a number of slides, showing details of the components discussed.

## Dr. S. J. Davies and Mr. E. Giffen.—*The Present Position of the High-speed Heavy-Oil Engine.*

The progress of the heavy-oil engine towards higher speeds of revolution in the last five years has been such that it is to-day in direct competition with the petrol engine for land transport, aircraft, motor boats and similar purposes.

The advantages of heavy-oil engines over petrol engines include: improved fuel consumption, especially at low loads, as determined by weight, and still more by volume; use of a cheaper fuel—under present market conditions; relative absence of fire risk; greatly reduced fouling of the lubricating oil. Against these are sometimes urged the disadvantages of higher maximum pressures and lower mean effective pressures in the cylinder.

Development has proceeded along three main lines which are determined by the methods in which the fuel is brought into contact with the air supplied for combustion. These are (*a*) direct injection of the fuel into a simple combustion chamber; (*b*) injection of the fuel into an ante-chamber in which combustion begins, the consequent rise of pressure there causing the fuel to be forced into the main combustion chamber, where combustion is completed; (*c*) compression of the air into a cavity in the combustion chamber, either in the piston or cylinder head, injection of the fuel being arranged to take place into the air, escaping from the cavity at high velocity at the beginning of the expansion stroke.

These three main methods are discussed in detail and comparisons are made between them from the points of view of performance, mechanism of combustion, and the necessary accessories.

Other suggested types of engines using heavy oils are also described.

In conclusion, a general comparison of existing engines is made.

Discussion.

AFTERNOON.

Visit to Portishead Power Station. Tea by invitation of Bristol Corporation Electricity Department.

Visit to Avonmouth Docks.

## Friday, September 5.

**Presidential Address** by Sir ERNEST W. MOIR, Bt., on *The Interdependence of Science and Engineering, with some examples.* (See p. 119.)

**Discussion** (Sections G, I) on *Air Pressure Variations encountered in Engineering Works and their Physiological Effects.* (Sir ERNEST MOIR, Bt., Prof. Sir LEONARD HILL, F.R.S., Capt. G. C. C. DAMANT, C.B.E., Mr. R. H. DAVIS).

Sir ERNEST MOIR, Bt.—The subject of the discussion is of very great importance in the furthering of engineering and allied enterprises. There is a need for the full light of science to be brought to bear on the use of air, reduced in volume by mechanical means and so increased in pressure, in connection with the construction of works of utility.

The effects of compressed air on man working either in a diving dress or at more or less air-tight shafts, tunnels, foundations, caissons, etc., have occupied the thoughts of physicists, engineers and medical scientific research workers for many years.

The object of this discussion is to get as much useful knowledge as possible from those having experience in the handling of such pressures, the control of staffs and workers who labour under its influence, and the prevention of injury to them. Furthermore, to gain knowledge of the best way to use even higher densities of air with safety and thus permit operations that in the past would have meant permanent injury or death to those who attempted to use it.

In the presidential address, references are made to some of his experiences, and the introduction of recompression in a medical air lock, and further light will be thrown on the subject by those taking part in the discussion, especially by Sir Leonard Hill who has already made a great study of the problem, by Captain Damant whose achievement in recovering gold from the *Laurentic* is well known, and by Mr. R. H. Davis who, with his firm, has developed a special diving apparatus for making descents greater than have ever before been made.

Prof. Sir Leonard Hill, F.R.S.—By the use of a submersible decompression chamber, designed by Mr. R. H. Davis, combined with the breathing of oxygen during decompression, the limit of depth for diving has been greatly extended. It has been shown that it is safe to breathe oxygen at an absolute pressure of three atmospheres for half an hour. Baboons, goats, and a donkey have been compressed to 300 feet of water pressure, and in some cases to 350 and 400 feet. They behave normally while under such pressures and can be decompressed safely with the help of oxygen in from half an hour to forty-five minutes. Divers have been exposed safely to 300 feet of water pressure, and proved as capable of doing work at that pressure as at less pressures. It is safe to decompress rapidly from 300 or 400 feet to 66 feet of water pressure because bubbles of nitrogen are prevented from forming by the latter pressure. At 66 feet and downwards during decompression oxygen is breathed; by filling the lungs with oxygen the diffusion of nitrogen from the blood is hastened. The decompression is made in stages, there being a pause at 30, 15, 10 and 5 lb. of pressure. Any sign of ' bends ' coming on at a lower pressure stage is removed by returning to the one higher and breathing oxygen for a longer period. Trials are being made on animals of more prolonged exposures, using partly deoxygenated air for the higher pressures so as to avoid any danger of oxygen poisoning. The depth possible for submarine work in compressed air no doubt can be greatly extended by the use of oxygen breathing apparatus in the decompression air lock.

The experiments are being carried out with pressure chambers set up by Mr. R. H. Davis at Messrs. Siebe, Gorman's works, and with his co-operation. Deep-sea diving-tests are being carried out by the Admiralty Diving Committee and will be published in due course.

The katathermometer has proved of considerable value as a measure of ventilation and as a simple anenometer. Calibrated from 100 ° to 95° F., it enables one to measure the rate of cooling on *its surface* at approximately skin temperature. A new form has been introduced calibrated from 130° to 125° F., for use in hot climates and workshops. A nomogram has been prepared from the results of extensive observations in wind tunnels at various known temperatures and wind velocities, and by means of this the velocity of air movement can be at once arrived at after making readings of the instrument. The instrument, and nomogram, which answers both for the old and new form, can be had from Messrs. Hicks, 8 Hatton Garden, E.C. Messrs. Angus and Soper have carried out the preparation of the nomogram.

Capt. G. C. C. Damant, C.B.E., R.N.—Theory of Haldane's method of decompression and the experiments which led to the framing of the Admiralty tables for controlling the ascent of divers so as to protect them from compressed air illness. Twenty years' experience of this method in practical diving work in deep water—the lessons learned and their possible application to caisson work.

### Afternoon.

Visit to Bristol Aeroplane Company, Filton.

Visit to Messrs. Stothert & Pitt's Works, Bath.

## Saturday, September 6.

Excursion to G.W.R. Locomotive Works, Swindon.

## Monday, September 8.

The Trend of Airship Construction :—

Lieut.-Col. V. C. Richmond, O.B.E.—*The Development of Rigid Airship Construction.*

(1) *The Influence of Operational Conditions on Design.*

(2) *Shape and Aerodynamic Characteristics.*—The influence of fineness ratio, and block coefficient on drag. The effect of turbulence on a parasitic resistance, etc.

(3) *Structural Arrangement.*—The effect of increase in size and alteration in shape in modifying the conventional Zeppelin arrangement. Alternative methods of distributing weights and lifts. The problems arising from the provision of adequate passenger accommodation.

(4) *Structural Detail.*—The requirements of rapid erection and easy maintenance and repair. Developments in girder and joint construction—materials employed, weights and factors of safety.

(5) *Propulsion.*—Airship engine requirements and the use of alternative fuels. The arrangements of engines and propellers.

(6) *Fabric Work.*—Gasbag and outer cover requirements. The development of the metal cover.

(7) *Passenger Accommodation.*—The various possible alternative arrangements, the scale of luxury and the weight involved.

(8) *Handling and Mooring.*—The mechanisation of the handling of airships and its influence on their design.

Mr. B. N. Wallis.—*The Design and Construction of H.M.A. R100.*

Herr Direktor W. E. Doerr.—*The Airship 'Graf Zeppelin.'*

Discussion.

Afternoon.

A series of cinema films of the Airships R100, R101 and 'Graf Zeppelin,' shown in the Great Hall of the Merchant Venturers' Technical College.

## Tuesday, September 9.

The Economical Production of Power :—

Mr. G. A. Orrok.—*The Use of High Steam Pressures and Temperatures in the Generation of Power.*

A brief historical note indicates the line of development towards increased steam pressures and temperatures in power generation. Both pressures and temperatures in general use increased only slowly until fifteen–twenty years ago, but since then the rate of development has rapidly increased.

The increase in operating efficiency due to the use of higher pressures and temperatures is considered in detail. Certain assumptions are made as to the losses which may be expected in a central station, and curves are drawn to show the maximum economy (in B.T.U.'s per K.W.H.) to be expected under any given conditions of pressure and temperature. Consideration is then given to the effect of variation of load factor. An empirical formula is suggested as an indication of the maximum possible station economy, taking into account variation in pressure, temperature, and load factor. This formula is used to obtain a family of curves, showing, for a given initial steam temperature, the maximum economy for any combination of pressure and load factor. Comparisons are made between the curves thus obtained and the actual operating results from a large number of central stations.

In conclusion, an estimate is made of the increase in economy to be expected from : (1) increased load factor, (2) increased steam pressure, (3) increased steam temperature, and (4) improved conditions of operation.

An appendix gives the main particulars of the high-pressure power stations of America.

Sir Henry Fowler, K.B.E.—*The Question of Fuel Consumption in Locomotive Practice.*

Difficulties peculiar to steam locomotives. Effect of varying work. Steps taken to deal with this. Methods adopted in design and operation to conserve fuel and to ensure economy in running.

Mr. A. LENNOX STANTON and Mr. THEODORE STEVENS.—*Obscured Fundamentals in the Development of Integrated Electricity Supply.*

The paper presents the case of an *integrated* electricity supply and refers to its potential possibilities for creating employment. It points out that at present this integrated electricity supply does not exist and includes twelve charts, together with supplementary data, showing the trend and scope presented by the changing conditions of electricity supply in Great Britain. These show :—

1. Growth of authorised sources of electrical energy during a five-year period 1922–23/1927–28.
2. Changes in the number of authorised distribution systems.
3. The trend of changes in predominant sizes of generator units.
4. Changes taking place in voltages of generation.
5. The advances made in steam practice.
6. The average cost per ton of the fuels used.
7. The units sold to consumers under power, traction, public lighting and domestic services ; the revenue pertaining thereto in percentage terms ; the average price per unit sold ; the average cost of fuel per unit generated and the average total cost per unit generated.
8. The average revenue from lighting and domestic supplies per unit sold for individual undertakings.
9. The average revenue from power consumers per unit sold for individual undertakings.
10. The trend and growth of staff and workmen employed.
11. Relevant official data of the *anticipated* position of electricity supply under the Grid Schemes a few years hence.

The authors draw attention to limitations laboured under in every phase of the industry and show data proving that between countries there is no true basis of comparison which ignore quantitative and other qualifying factors of the greatest importance.

In conclusion, the authors put forward the development of an *integrated* electricity supply as a pre-eminent and essential avenue, leading to the solution of many pressing industrial and social problems in the national life and, touching upon proposals to utilise the tidal energy of the Bristol Channel for the generation of electricity, draw special attention to the necessity for a searching examination being made into all its financial and contractual liabilities.

### AFTERNOON.

Visit to Petters' Oil Engine Works, and Westland Aircraft Works, Yeovil.

Visit to Messrs. R. A. Lister & Co., Dursley.

## Wednesday, September 10.

THE DESIGN OF STEEL STRUCTURES :—

Mr. J. S. WILSON.—*Structural Steel Design and Regulations.*

Regulations relating to the design of structures have developed from two sources: partly from simple rules and guiding proportions which have been adopted with the object of simplifying the processes of design and to render details of design more consistent, and partly to make it possible to produce designs which may be strictly comparable as to strength, workmanship and durability.

Although structural design in all materials has much in common, the paper relates more particularly to steel construction.

Rules and regulations which have been used or have been proposed, have had for their object one or other of the following :—

(1) To facilitate design, keep details consistent and eliminate personal eccentricity (or genius ? ) of the designer.

(2) To simplify manufacture and cheapen production.

(3) To enhance durability by increasing the stiffness, thickness or riveting of parts or members above that required by considerations of strength only.

(4) To codify methods of calculation of loads and stresses so that engineers working independently may get strictly comparable results.

(5) To give structures a strength capable of withstanding a standardised system of loading while the stresses in their members do not exceed specified maximum intensities.

Examples of each type of rule are given.

The adoption of well-considered rules of the first four types aid good design and results in what is sometimes called good practice. Selections of rules based on extensive experience have been put together by various engineers in the form of *Designing Specifications.* Examples of rules and specifications are discussed.

The idea that a multiplicity of rules and regulations can take the place of experience or prevent bad or weak design on the part of the inexperienced is deprecated.

In relation to loads and the corresponding limiting stresses, designing specifications are of two fundamentally different types. In one type the maximum intensity of stress allowed in a member is adjusted to suit the position of the member and the severity of the loading conditions in that member. In the other the stress is limited to a nominal maximum for all members, but the loads carried by different members are adjusted according to an empirical formula which increases the loading where the conditions are more severe.

Regulations relating to stresses have an important influence on weights and costs of structures, and for structural work sent abroad the country adopting conservative practice and low limiting stresses, though producing comparatively robust and durable structures, may be seriously handicapped in international competition.

## Prof. C. Batho.—*Theory and Experiment in Structural Design.*

A field of investigation of importance to the structural engineer is the development, by analysis and experiment, of exact methods for the calculation of the stresses and deformations in a structure, and the determination of the conditions which govern its breakdown under excessive loading. The results must often be complicated, and efforts should be made to reduce them to forms applicable in practice.

The problems involved fall into two groups: (1) consideration of the structure as an elastic framework, (2) the study of the distribution of stresses and deformations in the members and of the manner in which the connections transmit the forces and affect the strength and stability of the structure.

Statically indeterminate structures are often necessary or desirable. The difficulty of calculation of redundant structures is often exaggerated owing to neglect of the systematic treatment, by means of deformation diagrams and influence lines, developed mainly in Germany. The calculation of frameworks without diagonals, such as building frames, is possible by various methods, but often very lengthy. Recent investigations indicate that reliable approximate methods are possible. Begg's experimental method is of value in these investigations.

Problems of the second group are largely experimental and include (*a*) intensive laboratory explorations of the strain and deformations in structural elements, connections and built-up members, (*b*) laboratory tests to destruction, (*c*) experiments on actual structures. Examples of these are discussed in the paper. It appears to be desirable to investigate the simpler elements of construction before attempting to analyse the results of tests on complete structures. Tests to destruction should be directed towards the observation of the conditions of initial breakdown. Experiments on the conditions of elastic breakdown of materials under non-uniformly distributed loads are desirable.

Investigations such as are outlined have led, and should still lead, to increased economy by making possible more rational methods in design.

## Discussion.

## Report of Committee on *Electrical Terms and Definitions* (Prof. G. W. O. Howe).

Report of Committee on *Earth Pressures.* (See p. 264.)

Report of Committee on *Stresses in Overstrained Materials.* (See p. 264.)

## SECTION H.—ANTHROPOLOGY.

### Thursday, September 4.

**Presidential Address** by Dr. H. S. Harrison, on *Evolution in Material Culture.* (See p. 137.)

**Discussion** on *A Proposed National Folk Museum* (Sir Henry A. Miers, F.R.S., Dr. R. E. Mortimer Wheeler, Prof. J. L. Myres, Miss Barnard).

Sir Henry A. Miers, F.R.S.—A distinction is made between (1) Open Air Museums, in which are re-erected old furnished dwellings, &c.; (2) Folk Museums, which contain collections of materials illustrating bygone life; (3) Period Museum, generally an old house, filled with contemporary exhibits; (4) Period House or Period Cottage, which is simply an old house without museum exhibits; and (5) Period Room, either original or a facsimile, reproducing the conditions of life and forming a separate exhibit in an ordinary museum.

It is pointed out that this country has nothing corresponding to (1) and (2), though examples may be found of all the other types, and there are in addition many Folk Museum exhibits gathered together in various museums as local 'bygones,' or contained in ethnographical collections.

There is, therefore, a vast amount of material from which a Folk Museum might be equipped, and there are many dwellings and other old structures in different parts of the country which are in danger of destruction and might be saved if an Open Air Museum were instituted.

A joint Committee of the Royal Anthropological Institute with representatives of the British Association and other bodies has reported unanimously in favour of utilising the Botanic Gardens in Regent's Park as a National Open Air Museum, with the adjacent St. John's Lodge as an associated Folk Museum.

A list is given of the most prominent Open Air Museums in Sweden, Norway, Denmark, Finland, Holland and Roumania; and of Folk Museums in Sweden, Esthonia, and Switzerland; together with a large number of Period and Memorial Houses in Sweden; sufficient to indicate how widespread is the desire in these countries to preserve such national and historic relics in contrast to the almost complete apathy which has prevailed in the British Isles.

The author believes that if a national Open Air and Folk Museum can be established in London, even on a comparatively small scale, efforts will be made in many other districts to preserve houses and materials which are rapidly disappearing and are in danger of total extinction.

#### Afternoon.

Mrs. D. Portway Dobson.—*General Survey of pre-Roman Sites in the Bristol District.*

Fauna of the early palæolithic period has been found in the Bristol district in the Durdham Down fissure, in Wookey Hole and other limestone caves. Implements of this period have recently been discovered in the gravels of the Severn and Avon rivers. Upper Palæolithic times are also well represented in the caves of Mendip, notably Gough's Cave at Cheddar and Aveline's Hole in Burrington Coombe. These sites have yielded both implements and fauna of the Aurignacian period, while the gravels at Clevedon are rich in bones of horse and other animals belonging to the close of the same period. The district is well provided with remains of the megalithic age, including the fine chambered tumulus of Stoney Littleton and many more long barrows, dolmens at Druid Stoke, Broadfield Down and other places, and the stone circles at Stanton Drew. Bronze Age barrows abound on the high ground, and the well-known Wick tumulus which has yielded beaker pottery, lies on the flat land near the Severn channel to the north-west of Bridgwater. Pottery from the barrows may be seen in the museums, and bronze implements are numerous.

The Early Iron Age is represented by the Hallstatt or early La Tène settlement on Little Solisbury Hill, near Bath, by the later La Tène villages of Glastonbury and Meare, together with the cave sites of Wookey Hole and Read's Cavern, and the contemporary camps of Worlebury, Dolebury and Stoke Leigh.

### Dr. R. E. Mortimer Wheeler.—*A pre-historic, Roman, and post-Roman Site in Gloucestershire; the Excavations at Lydney.*

### Mr. R. W. M. Wright.—*Celtic and Saxon Bath.*

History and legend are inextricably intermixed in the origin of Bath, and the pages of mediæval romance ascribe Bladud as the eponymous hero and Celtic founder of the 'Waters of the Sun.' The treasure of Bath's medicinal springs was known throughout the Bronze and Iron Ages, and successive waves of Celtic colonisation brought Aquae Calidae into parallel importance with the lake villages of Glastonbury and Meare. Extensive territorial settlements existed on the surrounding hills, and well-defined trackways across the Avon valley led to the ancient site of the Hot Springs. These springs were presided over by a divine tribal patroness, Sul Minerva, accompanied by the practised rites of heliolatry. Without doubt the Avon valley was first occupied by the Romans in A.D. 54–68, and for a period of three hundred years the city of Aquæ Sulis gradually developed as a spa. Evidence from extant monuments reveals several assaults on the town by Goidelic tribal hordes, and early in the fifth century the Romano-British inhabitants were scattered to the surrounding hills after a raid on the district by the Scots. A century later Gildas presents in quasi-legendary form the battle of Mons Badonious about A.D. 516, when the series of sporadic Saxon invasions were checked. In A.D. 577, however, the City of the Waters (Akemancester) fell to the sword and firebrand of the Saxon, its destruction being lamented in a ninth-century poem 'The Ruin' in the *Codex Exoniensis.* Bath soon rose from the vale of desolation with a canton population, and early in the seventh century Christianity became firmly established in this district. St. Gregory, Maeldruib, St. Aldhelm and others visited this Saxon city, and in A.D. 676 Osric founded the first monastery on the lower slopes of Lansdown. Offa in the following century established a Collegium on the site of the present Abbey, which became the most celebrated monastery in the province of Wiccia. The Saxon burgh with its Abbey was adopted into the province of Wessex by Alfred the Great, its transportation being signalised by a meeting of the Witan within its newly-built walls in A.D. 901. Here Edward the Elder established a Mint, which continued until the reign of Henry I. The golden age of Saxon Bath was reached in A.D. 973, when Edgar was crowned in the small Saxon Abbey in proximity to the ever-rising hot springs.

### Mrs. E. Clifford.—*Report on the Barnwood Discoveries.*

About thirty years ago a gravel pit was opened in a 40-acre field 2 miles N. of Gloucester on the Roman highway known as Irmin Street, which leads from Gloucester (Glevum) to Cirencester (Corinium) and on to Silchester and London. In the lower levels of these Jurassic débris gravels occur numerous teeth, tusks and bones of *Rhinoceros tichorinus* and *Elephas primigenius.* About ten years ago an implement of late Acheulian or early Mousterian date was found. The brick earth has yielded a fine Mousterian point and other implements of upper Palæolithic date. In these levels Tardenoisian implements have also been found. On the surface Neoliths have turned up in large quantities. A beaker burial was found in 1928, a La Tène II burial in 1927, and near by two groups of cremation burials of pre-Roman date.

In 1918 a cutting was made from the road, and it was quickly seen that a Roman cemetery had been found, enclosed by trenches. The portion excavated (modern buildings prevented complete investigation) contained over 100 skeletons and between 60 and 70 cremation graves, many of which were accompanied by cooking pots and water bottles; brooches, a mirror, scale armour, beads, &c., occurred in small quantities. So far as can be ascertained from the pottery, &c., the cemetery ceased to be used after 200 A.D.

## Friday, September 5.

Miss D. A. E. Garrod.—*Excavations in the Caves of the Wady el-Mughara.*

Mr. G. Horsfield.—*First Excavations at Petra.*

(1) Occupation by pre-historic man, Edomites, Nabatæans, Romans, Byzantine Christians, Arabs and Crusaders.

(2) Discovery of first Edomite megalithic monument.

(3) Identification of the earliest Nomad stronghold with El Habis, probably the 'Sela' of 2 Kings 14. 7. Identification of the Nabatæan citadel, the 'rock' of Diodones Siculus, with El Biyara.

(4) Identification of the Nabatæans with the tribe of Nabatu, found on the lower Tigris in the eighth century B.C., and at Teima, half-way across the western desert route, in the sixth century B.C., later dispersing the Edomites.

(5) Discovery of the Nabatæan walls of the city of the fourth century B.C.

(6) Dating indicated by pottery. Athenian black-glazed ware of the fourth century B.C. Rhodian wine jar handles from 300 B.C. and a fine painted egg-shell ware of oriental character, peculiar to Petra, and probably influenced by Achæmenid Persia. Several pencils for writing cuneiform were found.

(7) Differentiation for the first time between tombs and houses. Large residential quarters identified with streets, communicating stairs, houses and cisterns.

(8) Discovery of a burial in quicklime unique in antiquity.

(9) Elucidation of 90 per cent. of the 100 cult places, previously planned by German expeditions, as houses, quarries and water-catchment areas.

(10) Identification of the Great High Place, Zibb Atuf, as the central sanctuary of Dushara, the Nabatæan sun-god, described by Suidas.

(11) Discovery of an ancient Dushara sanctuary at El Barid.

(12) Place-name survey made to clear up confusion of previous topographical name-lists.

(13) Exploration of copper smelting station at Fenan and discovery of a new one at Sabra, a southern station below Petra.

Sir Flinders Petrie, F.R.S., and Mr. Eann Macdonald.—*Neolithic and Palæolithic Work in the Beersheba Basin.*

The valley of the Wady Ghuzzeh abounds in rolled palæoliths, which in the region of Beth-pelet have been collected by the thousand. The main types are those of Europe—heavy picks for root grubbing and sharp ovates for plant cutting—tools for the winter and summer foods. Some new types appear, such as the skew-handled borer. The surface has also many neolithic settlements, with well-formed hoes, some slightly polished, and the largest scale of flaking known. Pottery is associated in these settlements, like the earliest known in Egypt. The same form of flint hoes continued to be made down to the Israelite period, 1100 B.C. (I hope that the settlements will be described by Mr. Macdonald, who excavated them.)

Mr. R. F. Parry.—*Cheddar Excavations.*

Account of the excavations at the entrance of the Great Cave, Cheddar, during the winter months of 1927–28–29, with a description of the flint and bone implements of Magdalenian Age, including a 'baton de commandement.' This and part of a broken specimen found in the same cave in 1903 are the only specimens found in England. Account of the excavations at Soldier's Hole, near the above, with description of the flint and other implements of Magdalenian and Solutrian Age.

Dr. H. Taylor.—*Recent Work of the Spelæological Society.*

Excavations were undertaken in southern Ireland by invitation of the Royal Irish Academy. For the first time in that country human remains were found in association with a Pleistocene fauna; they were sealed down by a layer of stalagmite, above which debris of the Neolithic and Bronze Ages had accumulated.

A barrow at Tyning's Farm, Mendip, has been excavated, and work on a second is nearing completion. In all eight vessels have been found, with many associated objects; the primary burials were of the Middle Bronze Age, the secondary of the transition from Late Bronze to Hallstatt.

Of eight caves in this country undertaken by the Society, six have been excavated fully and two are in their last stages. They have yielded an almost complete sequence of fauna and culture from true Upper Aurignacian to the present day.

Survey work in the field has been continued. Some of the results will be presented in another paper. The river terraces of the Bristol Avon have been shown to contain Acheullean and Mousterian implements.

## Afternoon.

### Sir Flinders Petrie, F.R.S.—*Excavations at Beth-pelet, Palestine.*

For three seasons excavations have been made by the British School of Egyptian Archæology at Beth-pelet, now Tell Fara, near the Egyptian border, 18 miles from Gaza. It is the most important frontier fortress south of Palestine, as it was strongly fortified by the Hyksos, dominated the only pasture and free water near the frontier, was again fortified by Egyptians in the XIX and XXII dynasties, and lastly by Vespasian. Historically, it has yielded the only continuous view of the Hyksos period, XV and XVI dynasties, 2375–1590 B.C. It gives the mode of fortification, abundance of scarabs for dating, bronze daggers of Crete and Cyprus, steeldagger before 1300 B.C., and a great quantity of pottery. The series of Philistine tombs is dated by scarabs to 1300–1050 B.C. The city was the home of the Jewish royal guards of Pelethites, Shishak was the greatest builder in Palestine, which he entirely occupied. The scarabs result in placing the Hyksos XV and XVI dynasties immediately after the XII, and parallel with the XIII, XIV and XVII in Egypt. This is compatible with the Egyptian records and the Cretan archæology. The supposed date from the Kahun papyrus was therefore probably reckoned on the seasonal calendar.

### Mr. H. St. George Gray.—*Exploration of Somerset Earthworks.*

Like the neighbouring counties of Wilts and Dorset, Somerset is rich in earthworks, but very few of them have yet received the attention of the archæological excavator. Ham (or Hamdon) Hill, 5 miles W. of Yeovil and near the Fosse Way, has not been by any means neglected, and Worlebury Camp, Weston-super-Mare, was considerably dug over in the middle of last century. Trial excavations have also been made on a more or less extensive scale (during the time of my residence at Taunton) as follows: Castle Neroche, 7 miles S.S.E. of Taunton (1903), Small Down Camp, near Evercreech (1904), Norton Fitzwarren Camp, 2½ miles W. of Taunton (1908), Old Burrow Camp, Exmoor—just over the Somerset border (1912), Cadbury Castle, South Cadbury (1913) and Cadbury Camp, Tickenham (1922). Owing to its small size it was found possible to examine Kingsdown Camp, between Radstock and Frome, during 1927–29 in some detail, and the work proved very productive; it is partly of pre-Roman and partly of Roman construction. Ham Hill, with its immense size (210 acres, ramparts about 3 miles), has for years been of absorbing interest, and the greater part of one room is devoted to the finds from this camp in the Somerset County Museum. Other great camps in the county of which little is known, except superficially, are Dolebury, Maesbury and Banwell on Mendip, Douseborough on Quantock, and Elworthy Barrows (probably a Neolithic camp) on Brendon. With boundary earthworks, such as Wansdyke, we do not propose to deal.

### Mr. C. W. Phillips.—*The Circle, Avenue and other Earthworks on Walton Down, near Clevedon.*

On the summit of Walton Down, overlooking the Bristol Channel on the north and the Gordano Valley on the south, there is a large circular work with an avenue going from it north-eastwards to another earthwork roughly semi-circular in plan. All these works are far too weak ever to have had any defensive value, and they seldom exceed 1 ft. 6 ins. in height. Actually they are constructed of stone, but this has become overlaid with earth and turf. The circle, which has an original entrance on the south-west side nearly opposite the exit point of the avenue, is not a true circle, but might be more accurately described as a polygon; the sides are very numerous. The avenue presents an unusual feature in that it suddenly increases its width about half-way between the circle and the semi-circular work. At this point there has clearly been some sort of gate, and here also a slight stone causeway begins to run down the middle of the avenue, but is soon lost when it passes through the semi-circular work. The circle has a slight ditch round it on the outside, but inside there is nothing to be seen save that there is a slight rise towards the centre. No excavation has actually been done among these works. They are in a remarkable state of preservation. The whole complex looks very like some kind of sacred spot. A few hundred yards to the west of the circle are a number of hut circles which have never been excavated. A certain amount of gravel flint occurs naturally at this spot. One hundred yards to the east of the semi-circular work are faint traces of a rough enclosure containing many hut circles. A number of these was excavated in the middle nineteenth century, and it seems from the finds that they were occupied by Neolithic folk, or at the latest by men of the early Bronze Age. Still farther to the east along the ridge are numerous traces of ancient cultivation. The ground is covered with a dense copse, but the path going through the copse has continually to mount over the remains of old dry stone walling. Finally, immediately to the south of the avenue, is a very regular three-sided work set on the edge of the descent into the Gordano Valley. It seems unfinished, for the fourth side is entirely absent. Further, it seems to bear no relation to the circle and avenue, and has a ditch on its outside which would make it quite defensible had it got a fourth side. Arrangements were made in the summer of 1929 to get the whole site photographed, but the aeroplane company instructed to do the work has not carried it out to date.

Sir Arthur Keith, F.R.S.—*Memorial Lecture: What Dr. John Beddoe did for Modern Anthropology.* (Chairman: Sir Evan D. Jones.)

## Saturday, September 6.

Excursion to Stanton Drew, Cheddar, Meare, Glastonbury, Wellow.

## Monday, September 8.

Mr. L. S. B. Leakey.—*The Kikuyu System of Land Tenure.*

Mr. A. L. Armstrong.—*The Antiquity of Man in South Africa as demonstrated at the Victoria Falls, Rhodesia.*

Miss M. A. Murray.—*Excavations in Minorca.*

Sir Richard A. S. Paget, Bt.—*Influence of Mouth Gesture on the Development of the Alphabet.*

Since Emanuel de Rougé's discovery of the relationship between the classical alphabet and Egyptian hieroglyphs, many theories have been advanced. Most of these, e.g. that of Dr. Alan H. Gardiner (1916) accept hieroglyphic influence.

Assuming a pictographic origin, the method of formation is generally agreed to be 'acrophonic,' i.e. each alphabetic sign is a picture which stands for the first letter of its name.

But the number of actual pictures for names beginning in any given letter was large—whether in Egyptian or Sumerian—what then was the principle of selection?

The present hypothesis suggests that, just as in the origin of speech the vocal organs tended unconsciously to imitate the pantomimic gestures of the body and especially of the hands, so in the origin of alphabetic writing the hand of the scribe tended unconsciously to imitate the form and movements of the organs of articulation.

Hence in Sumerian, according to Langdon, the same symbols were used for P, B, and perhaps also W. W was represented by the same symbols as M, and V and F (if they existed at all) were recorded as B and P respectively—i.e. various sounds made by essentially the same lip gesture were represented by the same symbols.

In a great many Sumerian words there appears to be a tendency for the script to conform to the mouth movements of the word which it represents. It is as though Sumerian writing was in process of evolving, unconsciously, into a natural alphabetical or syllabic script in which vertical tongue movements were represented by vertical lines—lip projections by parallel horizontals, etc.

If the various alphabets of the world are examined it is seen that the same mouth symbolism does run through nearly all. Thus, the symbols for M are almost invariably those of a closed mouth or of two closed lips. Similarly, those for P and B, U and W, represent two lips or a closed mouth opening; symbols for L show an elevated tongue, etc.

In the English alphabet (essentially the same as the Latin) nearly all the letters have a close relation to the form of mouth which produces the sound for which the letter stands. Thus:

A (earlier ∢) represents an open mouth.
B represents two lips in profile, facing right.
C (from <) is a mouth closed at the back as for sounding K or G.
D (from △) is an elevated tongue (as also in T and N).
E is a mouth facing right, with a tongue at mid-height.
F is the underlip incurved beneath the upper, or two lips projecting.
I is an elevated tongue not touching the palate. (Hence the (modern) dot over i.)
L is the same, but actually touching the palate.
M two lips pointing up.
N (from N) tongue touching palate as in △.
O front view of rounded mouth.
P (from Γ, Π) possibly represents two lips closed.
R (from P or R) a tongue bent back at the tip.
S (from ϟ) tongue reaching forward and upward.
T compare I, plus contact with palate.
U, V, two lips pointing up.
W, VV, two lips, pointing down, compare M.
Z, compare ϟ.

The mouth gesture theory of alphabets is not new. Thus, it was put forward in 1667—with respect to the Hebrew alphabet—by F. M. von Helmont. (Reprinted 1916 by Prof. W. Vietor, Marburg.)

In 'How the Alphabet was Made' (*Just So Stories*, 1902), Mr. Rudyard Kipling has suggested similar origins for the letters A, O, U, M, G and K.

Madame Cantova, a school teacher of Aigle, Switzerland, has also noted that our written language is a representation of articulation (*L'Educateur*, Lausanne 27 April 1929, pp. 147–8). She infers that good articulation promotes good handwriting.

The gesture principle (if valid) may be useful in suggesting phonetic values for symbols in unknown scripts.

### Afternoon.

## Mr. E. Estyn Evans.—*An Industry of the Late Bronze Age in Western Europe.*

An investigation of the metal types found in some late Bronze Age hoards of Brittany, Normandy and Picardy showed that they were identical with certain characteristic bronzes known in S.E. Britain and generally considered to be intrusive there. In particular, a variety of flange-hilted sword (with *carp's tongue* point), hitherto little noticed, marks off this industry very sharply. Its distribution is equally distinctive. The sword is common in the Iberian Peninsula, where, as farther north, it is definitely exotic. It does not occur in Italy or Central Europe, but is found in the Jura and Switzerland, and was evolved in the pile dwellings, from an Hungarian prototype, during the period of agricultural and industrial prosperity coinciding with optimum climatic conditions north of the Alps (1200–900 B.C.). That this superior weapon, of specialized pattern and known origin, should invariably be associated, in Western Europe, with other bronzes of fixed forms and similar origins, argues for a movement of peoples rather than for casual commercial contacts; and it is contended that the ground fabric of the late Bronze Age cultures of Western Europe was laid by a scattering of peoples from the west Alpine province shortly before 1000 B.C. This movement, the first of a long series reaching Britain in the last millennium B.C., was distinct from those of the early Iron Age. In accordance with the views of Kraft and Bosch-Gimpera, the first Celtic tongue probably reached these Islands at this time.

Mr. E. G. Bowen.—*The Racial Geography of Europe at the Dawn of the Age of Metal.*

An attempt has been made to gather together from various sources the nucleus of a card catalogue giving details of skeletal remains dating from the age of the dawn of metal in Europe. Maps of Europe have been constructed showing the distribution of Cephalic Indices of skulls of this period. Hyperdolichocephalic skulls (C.I. 72 and under) are found to be a very marked feature of the western fingers of the loess lands in Europe, while broader-headed groups are more typical of the mountain country and the coastal entries. In addition to the hyperdolichocephalic skulls on the loess patches, there are broad-headed elements, presumably of ' Beaker ' type, found there as well. From a study of the various human cultures associated with these skeletal remains it is clear that we are beginning to deal with an European scheme of life based on the development of the trade routes and not, as at earlier times, with local or regional life. The dawn of the age of metal is a very important period in the formation of modern Europe—the population begins to assume its present form and the racial types that we distinguish at present are found already in this early population. The greatest change seems to have been that the hyperdolichocephalic folk who formed such an important element in the population of Europe at the dawn of the age of metal seem to have vanished almost entirely, except, perhaps, in some remote corners such as central Wales, or north-eastern Portugal, where they can still be distinguished in the present-day population.

Mr. W. A. Heurtley.—*A Neolithic and Early Bronze Age Site in Western Macedonia.*

The site lies on the south bank of the Haliakmon, at the point where the modern road from Macedonia to Thessaly and South Greece crosses the river by the iron bridge built by the Turks in 1912. The object of the expedition was to elucidate certain problems of the Thessalian and Macedonian Neolithic Age, and to obtain, if possible, precise stratigraphic information about certain black-polished and painted pottery which makes its appearance in Thessaly at the end of the First Neolithic Period, and which has usually been attributed to invaders from Central Europe. The evidence supplied by the excavation goes to show that this attribution is justified. There are three phases in the history of the site. During the first phase, it was occupied by people who used pottery identical with that of the First Thessalian Period, and who were presumably Thessalians. (These people remained until the site was finally deserted in the Early Bronze Age.) The second phase was ushered in by an extensive conflagration, with which the appearance of the new black-polished pottery, of a new class of painted pottery, of crusted ware, and of incised spirals coincides. The simultaneous appearance of these novelties, and their strongly Danubian character, places it beyond reasonable doubt that the desired evidence for the earliest incursion of Northerners into Greece has been obtained. The most interesting find was perhaps a complete skeleton buried in a crouched position in a round hole sunk through the debris of one of the burnt houses. Above it lay a thin layer of ashes and several broken vases of the new kind, some blackened by fire. It thus seems probable that the burial is that of one of the invaders. The skeleton has been cut out with its surrounding earth and transported to the Museum at Salonica, where it awaits examination by an anthropologist. Close upon the heels of these Northern invaders came Early Bronze Age people from Macedonia, bringing with them their characteristic pottery. Their arrival and settlement constitute the third phase in the history of the site.

Mr. L. A. Cammiade.—*Pluvial Periods in Palæolithic India.*

Recent finds near Madras show a series of four consecutive cultures of palæolithic type. A coup-de-poing culture of Chellean and of Acheulean type was followed by a culture characterised by broad flakes struck from tortoise cores. The flake culture came into contact with a culture of upper palæolithic type using burins, backed blades and end scrapers, and was superseded. The upper palæolithic culture gradually passed into a microlithic culture of Banda type. The sequence of the four cultures is established stratigraphically. Their antiquity is determined by the geological formations with which they are severally associated. The geological formations associated with these cultures point to a succession of periods of heavy and scanty rainfall. There seem to have been three periods of each, beginning with a long, damp period that preceded the advent of the makers of coups-de-poing. The changes in the rainfall were due to changes in the quantity of moisture brought by the north-east monsoon. The moisture brought by this current would be affected by changes in the glacial conditions in Asia north of the Himalayas. It may also have been affected by conditions obtaining in East Africa at the time. The antiquity of the palæolithic period of India can be more precisely determined by a study of these climatic changes than by any other means.

Mr. S. J. Jones.—*The Domestication of the Horse.*

The concentration of animals in oases and river valleys during periods of prolonged drought no doubt afforded excellent opportunities for domestication, and the increasing number of horse bones in the upper layers at Anau may denote such conditions. That the value of the horse as a draught animal and for speed was early realised on the steppes of Western Asia is a legitimate assumption. In Mesopotamia, the ass and the ox precede the horse, the introduction of which is associated with invasions of peoples from the north. Contemporary nomadic raids may possibly be traceable in India and China, whilst towards the middle of the first half of the second millennium B.C. the Hyksos brought Egypt under the sway of the steppe. The destruction of the Tripolje culture may indicate similar but probably earlier movements towards western Europe, the loess belt assuming great importance in this respect. The circle of horse skulls in the tumulus of Mané Lud, Locmariaquer, may suggest an over-lap of steppe and maritime influences rather than direct relations with the eastern Mediterranean, as has previously been suggested. Art *motifs*, as in the case of the famous Maikop Vase, sometimes supplement direct evidence, whilst the Centaur myths of Greece may provide further correlations between archæology and ancient lore.

Miss G. Caton-Thompson.—*Excavations at Zimbabwe and other Ruins in Southern Rhodesia.* (Chairman: Dr. Randall MacIver.)

## Tuesday, September 9.

**Discussion** (Sections C, E, H) on *The Relation between past Pluvial and Glacial Periods.* (Chairman: Prof. H. J. Fleure.)

The Chairman, in introducing the discussion, said that the time seemed appropriate for a general review of the situation, as, while field observers in different regions were noting more and more minor departures from the general scheme of glacial and interglacial periods worked out by Penck and Brückner for the Alps, yet there was every indication of increasing agreement with the main outlines of the Penck-Brückner scheme over wide areas, if it be allowed that the Riss and Würm glaciations were to a certain extent episodes of one major glaciation, and that the Günz glaciation was not more than a preliminary stage of the Mindel. It was being generally realised that, after glacial conditions had established themselves in such an area as Central Europe, the procession of Atlantic cyclones would be headed off and forced to find its way eastward farther south, for example along the Mediterranean, as was the case even during the cold spell of 1929. Also, at such a time, there might be heavy precipitation and consequent very low-lying glaciation, probably outlasting the glaciation in Central Europe, along the oceanic fringe facing the westerly winds.

There is need to know what climatologists can say as to the probable position of the belt of cyclones and westerlies during the periods of maximum glaciation in Europe.

In the Pleistocene, the western Sahara, at least, was at times greener than now. We need to know whether this condition affected most of the Sahara, and what the contemporary conditions would be in Arabia which, in addition to presenting a high westward edge to catch precipitation, is so placed that, if cyclones went eastward along the Mediterranean, they would probably penetrate across North Arabia to the Persian Gulf.

Gregory long ago suggested that European glacial maxima corresponded with equatorial pluvial maxima, and Leakey has brought forward evidence for the same conclusion. Is this sanctioned by the climatologists?

If the belt of cyclones and westerlies included the Mediterranean and North Africa while Europe was covered with ice and the equatorial regions had a great deal of rain, was it legitimate to suppose that the belt of desert was obliterated, or did it lie farther south than at present, as is suggested by the observations of Falconer in Nigeria ?

Dainelli has worked out phases of glaciation on the northern border of India corresponding to the three later Penck-Brückner phases for Europe. Is it right to make the hypothesis that heavy glaciation on the Central Asiatic and North Indian highlands would weaken the summer monsoon effect and so give parts of India and China dry periods during ice maxima, coupled with heavy flooding of the Indo-Gangetic trough ?

On the archæological side we have the suggestion that both the Chellean and the Aurignacian cultures originated in N. Africa or S.W. Asia and spread thence to Western Europe. We need to know whether the spreads of these, and possibly of other cultures, can be linked up in any way with climatic changes which seem to furnish possibilities of judging contemporaneity.

These questions are linked up with those of race. Beyond the region of Europe-Africa-Southwest Asia, the sequence of early cultures seems to be everywhere less full than within those areas. In particular, one finds a mixed upper Palæolithic culture including Tardenoisian elements in many areas. Where these areas present *culs-de-sac* there usually survive types which are hyperdolichocephalic with ridged skulls, often strong brows and cheek-bones, and some prognathism, types which recall certain types found both in the Aurignacian layers and, in places, up to modern times within the Euro-African area mentioned. Are we, in face of the traces of migrations of early date, connected probably with climatic changes ?

See table facing this page.

Mr. L. S. B. Leakey gave the following account of The *Pluvial Sequence in East Central Africa and the Question of Glacial Pluvial Correlation.*

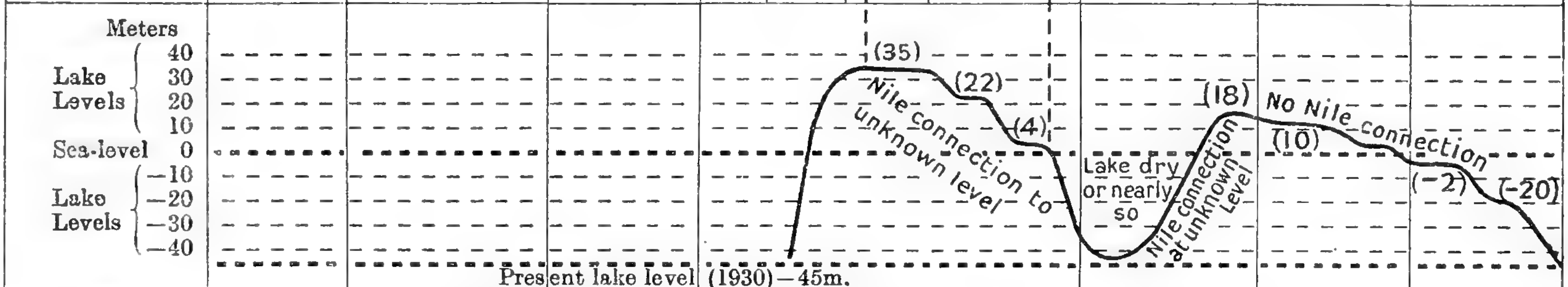
The following table reads from below upwards :—

| *Name of Period.* | *Culture.* | *Notes.* |
|---|---|---|
| Nakurian . . | Late Kenya Wilton Nakuru Culture. | Second small post-Pluvial wet phase. |
| | Period of aridity. | |
| Makalian . . | Early Kenya Wilton Elmenteitan. | First small wet period or post-Pluvial wet phase. |
| | Period of aridity and drying up of lakes. | |
| Decline of Gamblian II. . . | K. very late Aurignacian. K. Stillbay. | Decline of Pluvial. |
| Gamblian II . . | Late Kenya Aurignacian. Late Kenya Mousterian. | A long Pluvial period with a marked pause in middle. |
| Dry pause | | |
| Gamblian I . . | Early Kenya Aurignacian Early Kenya Mousterian. | |
| Period of intense earth movements. Rift Valley faulting and volcanic eruptions. Volcanoes such as Longonot, Menengai, &c., formed. | | |
| | Dry Period. | |
| Kamasian ? (Subdivisions not worked out) | Kenya Acheulean<br>Kenya late Chellean | A very long Pluvial period with possible subdivisions not yet worked out. |
| ? | ? | Cultures at present only known in the top of the series of deposits. |
| ? | ? | |

The result of the work which we have carried out in East Africa from 1926 to 1929 is that we have established the following sequence of events during the comparatively recent geological period of the Pleistocene and recent times, and we have also been

Miss G. Caton-Thompson and Miss E. W. Gardner then presented the following *Table of Events in the Faiyum.*

| Geological Periods. | Pliocene. | Plio-Pleistocene. | Pleistocene. | | | | | Transition. | Recent. | |
|---|---|---|---|---|---|---|---|---|---|---|
| Archæological Sequence | — | — | Palæolithic. | | | | | — | Neolithic | Historic IV Dynasty 2800 B.C. |
| | | | Lower | Middle | | | Upper | | | |
| | | | | — | — | Late Mousterian | Egyptian Mousterian. | | | |




| | | | | | | |
|---|---|---|---|---|---|---|
| Climate | Rain in Pliocene probably followed by several wet and dry periods. | Dry | Rain | Dry | Rain | Less than in Palæolithic followed by increasing desiccation with wetter periods at pauses. |

No attempt is made to indicate the relative lengths of the periods. The beginning of the Pleistocene is taken arbitrarily as beginning with the Chellean. Only the human periods found in the Faiyum are inserted. One difficulty in dealing with the climatic record is the complication introduced by variation of lake level, as the result of Nile connection. Since, however, the Nile continued to raise its bed *after* 18*m.*, while the second lake in the Faiyum was cut off and undoubtedly fell after that level, the pauses represented on the right hand side of the curve must be due to wetter periods when rainfall equalled evaporation. With regard to the correlation with European glaciations, it seems to us essential that the meteorologist should give us guidance on the following points :—(1) To what extent must Northern Europe be glaciated to produce a sufficient shift of climatic zones to influence North Africa ? (2) Would variations in the local glaciation of the Alps, by themselves, produce a climatic effect in North Africa ?

Our evidence points to a Mousterian culture lasting on far down the period of first desiccation. We suggest (G.J. 1928, pp. 38–39) that this is likely to overlap into the Upper Palæolithic.

able to show that certain definite stone-age culture phases belong to the different periods. Where the cultures are represented by series of tool types indistinguishable (save in material) from well-known European stone-age cultures we use the European terminology but prefix the word 'Kenya.' Where an East African culture is similar to, but not identical with, one of the European ones, we use new names unless we can find parallels in South Africa, in which case we use the South African name with the prefix 'Kenya.' In using culture names already used elsewhere we specify that we do not intend to suggest any contemporaneity of the European and East African cultures, the former may be earlier or later.

It is obviously unwise to use similar stone-age cultures in areas so far from each other as Europe and East Africa as evidence of contemporaneity of the periods in which they occur and, therefore, all thought of correlation on the basis of the cultures is out of the question. Rather we must seek some other data upon which we can attempt to determine whether any given culture is earlier or later in East Africa than it was in Europe.

At first one turns naturally to fauna, bearing in mind the value which has always been placed on fauna in Europe as a means of dating any deposit.

But here again one is immediately foiled because fauna, like man, is not the same all over the world at any one time. Fauna may well be an index of temperature and climatic conditions, but is no good for long distance correlation.

Turning from fauna we seek a solution to our problem in climatic correlation. Meteorologists to-day tell us that such great climatic changes as the advances and retreats of the ice-sheets in the Northern and Southern Hemispheres during the Pleistocene, cannot but have been accompanied by very marked climatic changes in other parts of the world which were not glaciated. Dr. C. S. Simpson and Dr. C. E. P. Brooks have done a great deal of work on this subject and, on theoretical grounds, have shown that the glacial phases should have been accompanied by periods of increased rainfall and the interglacial periods by periods of less precipitation. Their views may be summarised briefly as follows. The changes in climate which resulted in the glacial periods were due to fluctuations of solar radiation. The glacial maxima were in pairs Günz and Mindel, Riss and Würm separated by a long Mindel-Riss interglacial. Each pair of glacial maxima would have been accompanied by one major Pluvial period in non-glaciated parts of the world.

Secondary causes, such as the shifting of storm belts, &c., probably also played a part so that each of the main Pluvial periods, which correspond to two glacial maxima, may have sub-divisions. The minor climatic fluctuations, such as the Achen retreat, the Bühl advance, &c., were probably not due to the primary cause of change of solar radiation, but secondary causes such as polar shift, storm belts, &c. The bigger of the minor oscillations would affect the climate of the other parts of the world to a less extent than the true glacials, but probably enough to be recorded in lake basins without outlet. The first major Pluvial is thus correlated with the Günz and Mindel glaciations of the Alps, and the second major Pluvial with the Riss and Würm glaciations, the long interval being the equivalent of the Mindel-Riss interglacial. In many parts this Mindel-Riss interglacial was marked by earth movements on a large scale.

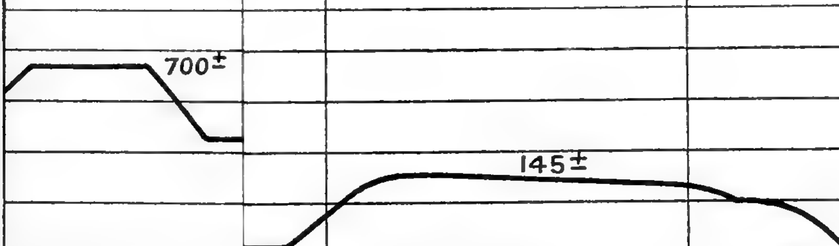
Blanckenhorn gives an identical sequence for the Jordan valley :—

(Minor Pluvial fluctuations.)
Aridity.
Second Diluvial, two terraces with a dry period between.
Period of aridity, Rift Valley faulting and earth movements.
First Diluvial.

He also suggests that the First Diluvial=Günz and Mindel and the Second, Riss and Würm. On grounds of climatic correlation, therefore, it would seem that we have the following table for the major climatic fluctuations :—

| *Europe.* | *East Africa.* | *Jordan Valley.* |
|---|---|---|
| Günz and Mindel | Kamasian | Older Diluvial in Jordan Valley. |
| Long Mindel-Riss Interglacial | Rift Faulting, &c. Dry | Rift Faulting, &c. Dry. |
| Riss and Würm | Gamblian II and Gamblian I | Younger Diluvial, two peaks. |
| Achen retreat | Dry period | Dry period. |

| | | | |
|---|---|---|---|
| GAMBLIAN PLUVI<br>Much less volcanic a<br>Period of less rainfall. | O A<br>ND<br>y low<br>ably | NAKURAN Post Pluvial Wet Phase.<br>Renewed wet conditions. | GRADUAL CHANGE TO PRESENT DAY. |
| Gradual development of both these cultures. | y for<br>areas | *a.* Nakuru Culture.<br>*b.* Upper Kenya Wilton.<br>*c.* Njoran.<br>} Roughly contemporary, exact order unknown. | Iron Age ? |




| | | | |
|---|---|---|---|
| RISS WÜRM interglacial. | THE<br>e Cli-<br>n. | Sub-Atlantic (wet). | A.D. |
| Mousterian group I to Late Mouste | | Neolithic Bronze. | Iron Age. |

le shown, for exampl

| Climatic Changes in East Africa | KAMASIAN PLUVIAL (Many Eruptions.) (Very long wet period: sub-divisions not yet worked out. Much longer (probably) than the Gamblian Pluvial.) | | | | | DRY PERIOD. Marked by Volcanic eruptions. Rift Valley Faulting Earth Movements, &c. | |
|---|---|---|---|---|---|---|---|
| Culture Sequence in Kenya during the Pluvials, &c. | ? | ? | Early Kenya Chellean | Late Kenya Chellean | Kenya Acheulean | Nanyukian? | |
| Levels of Lake Nakuru relative to present-day levels. *N.B.—No scale of time* | NAKURU BASIN NOT YET FORMED (AS SUCH). The area was part of the much larger basin of the 'Kamasian Sea.' | | | | | GRADUAL FORMATION of NAKURU and other Lake Basins in Area previously occupied by 'Kamasian Sea.' | |
| Suggested European Equivalent Climatic changes | Günz | Günz Mindel Interglacial | | Mindel. | | Mindel-Riss Interglacial. | |
| South-west European Cultures. | Pre-Chellean | Chellean. | | ? | Chellean. | Late Chellean | Acheulean. |

| Climatic Changes in East Africa | GAMBLIAN PLUVIAL (Much less volcanic activity.) Gamblian I | Period of less rainfall | Gamblian II | DECLINE INTO A DRY PERIOD. (Gradual drying up of lakes, ultimately absolute desiccation.) | MAKALIAN POST PLUVIAL WET PHASE. (Renewed wet conditions.) | DECLINE INTO A DRY PERIOD. (Lakes sink very low indeed probably dry up). | NAKURAN Post Pluvial Wet Phase. Renewed wet conditions. | GRADUAL CHANGE TO PRESENT DAY. |
|---|---|---|---|---|---|---|---|---|
| Culture Sequence in Kenya during the Pluvials, &c. | Lower Kenya Aurignacian; Lower Kenya Mousterian (Contemporary) | Gradual development of both these cultures | Upper Kenya Aurignacian; Upper Kenya Mousterian (Contemporary) | Advanced Kenya Aurignacian, Kenya Stillbay. ?? Too dry for man in areas worked. | Elmenteitan (Pottery common). Lower Kenya Wilton | ?? Too dry for man in areas as worked. | *a* Nakuru Culture, *b* Upper Kenya Wilton, *c* Njoran. (Roughly contemporary, exact order unknown) | Iron Age? |
| Levels of Lake Nakuru relative to present-day levels. |  | | | | | | | |
| Suggested European Equivalent Climatic changes | Riss. | Riss Würm interglacial | Würm. | Achen Retreat | Bühl and subsequent small fluctuations | Retreat to the Period of the Climatic optimum | Sub-Atlantic (wet) | A.D. |
| South-west European Cultures. | ? | Mousterian group from Levalloisian I to Late Mousterian | | Aurignacian. Solutrean | Magdalenian | Mesolithic. | Neolithic Bronze. | Iron Age. |



* (*Note.*—No time or scale shown, for example Gamblian I and II each longer than Makalian.)

I have already outlined the East African sequence (worked out before Dr. Simpson's and Dr. Brook's theories were known to us) and it is obvious that it fits in very well.

The later climatic fluctuations must also be considered.

According to Brooks, certain events stand out as of more or less world-wide occurrence and not as local fluctuations. They are :—

1. The Bühl advance, after the Achen retreat.

2. The warm, dry period of the climatic optimum circa 5000 to 3000 B.C., and the Sub-Atlantic wet period of circa 850 B.C.

In Kenya we have reason to believe that the Nakuran or second post-Pluvial wet phase was not earlier than 3000 B.C., owing to the presence of beads in deposits of the Nakuran phase which are almost certainly not earlier than that and are probably later. On many grounds there is reason to correlate the maximum of Nakuran wet phase with that at 850 B.C. In that case the climatic optimum would equal the dry period between the Makalian and Nakuran at about 5000 to 3000 B.C., followed by a gradual amelioration of climate which culminated in the maximum of the Nakuran wet phase about 850 B.C.

The Bühl would then equal the maximum of the Makalian wet phase, and the gradual decline from the maximum of the Makalian to the period of desiccation would equal the fluctuations during the period between the Bühl and the Climatic Optimum.

For the moment, accepting the suggested Glacial Pluvial correlation, we may compare our evidence with that of Europe (on the continent). This comparison is set out in the accompanying table (see folder).

Mr. Simmons expressed his regret that Mr. Wayland's contribution to the discussion had been delayed in transit. He thought that the evidence from Uganda would, on the whole, favour the idea of an earlier and a later pluvial period in the Pleistocene.

[A summary of Mr. Wayland's contribution will be found at the close of this report.]

Mr. Leslie Armstrong, in discussing *Early Man and the Correlation of Glacial Deposits and Pluvial Periods*, said that the deposits excavated in the Pin Hole Cave, Creswell, have yielded clear evidence that a cold, wet period marked the termination of the Upper Palæolithic age in northern England. (? a minor glaciation.) The associated culture is a developed Aurignacian, which appears to be the British equivalent of the upper Magdalenian of France, and contemporary with it in time. Beneath this level are zones of upper Aurignacian and proto-Solutrean cultures with faunal evidence indicative of moderately warm climatic conditions and an interglacial phase. At the base of these is an upper Mousterian cultural level resting upon the lower cave-earth and associated with a cold fauna. The lower cave-earth contains definite evidence of glacial conditions having prevailed during two periods, separated by a comparatively short, warm, interglacial phase ; also evidence of two periods of prolonged submergence due to the ponding back of water in the ravine to a maximum height of 30 feet above the present stream level. If this evidence represents one glacial period as a whole, viz., two advances of the ice-sheet and an interval of retreat (which is the view I favour), then the fauna represented in the inter-glacial phase points to warm climatic conditions of considerable duration as lion and giant-deer are fairly abundant, and horse and bison are common. The human artifacts in this interglacial zone are of middle Mousterian culture. I equate this glacial period, as a whole, with the third major glaciation of England. (? Würm.) Beneath the glacial beds is a zone of lower Mousterian culture, the oldest in the Creswell succession, associated with a moderately warm fauna. (? Riss-Würm interglacial.) For the remainder of the lower Palæolithic series I adopt the East Anglian evidence and equate them as indicated upon the accompanying chart.

It is generally conceded that in N.W. Europe we have evidence of four glaciations of major intensity, each separated by a comparatively short interglacial phase and a warm climate, and the respective pairs of glacials separated by a long interglacial phase. Simpson and Brooks, on meteorological evidence, have demonstrated that each pair of glacial periods with the intervening warm interglacial phase, should coincide with a pluvial period in unglaciated regions. On this hypothesis there should be in equatorial regions two major pluvials to correspond with the four major glacials of N.W. Europe, and minor pluvials, or wet phases to correspond with the later glacial episodes. This is precisely what Leakey has established on geological evidence in Kenya. In Uganda, Wayland has similarly established two major pluvials and the same can be demonstrated in Rhodesia and British Bechuanaland, where the associated cultural evidence I obtained in 1929 supports and confirms Leakey's evidence in Kenya.

The African pluvials are preceded and followed by periods of intense desiccation which no doubt compelled great human migrations. If the meteorological hypothesis of the relation of glacials to pluvials is accepted, it follows, from the cultural evidence set out in the chart, that the African representatives of these cultures ante-date those of Europe. The desiccation of the unglaciated regions, which set in when the ice retreated from the glaciated areas, provided the impetus which swept the waves of migration northwards and, with them, the Aurignacian culture into Europe.

Prof. J. W. Gregory, F.R.S., gave the following account of *The Correlation of Pluvial Periods and the German Drifts.*

On the discovery in 1893 of the glaciers of Mount Kenya and of their former extension more than 5,000 ft. below their present level, I inferred from the widespread range of the subalpine flora and fauna of Equatorial Africa, on Kilima Njaro, Kenya, Ruwenzori, in Abyssinia and the Cameroons, and from the presence of the drowned canyon off the Congo, that the climate had formerly been colder and wetter owing to a high pressure area and a greater elevation of the area. The date of the greater glaciation of Mt. Kenya was concluded as 'approximately contemporaneous with that of Europe,' and that the glaciation of Europe 'forced the cyclone-track along the Mediterranean farther south, and thereby gave Equatorial Africa a moister and colder climate.' The glaciation was naturally accompanied by an increased and more widespread rainfall, of which direct evidence was given by various lake deposits in the equatorial part of the Rift Valley.

The Kenyan glaciation and the deposits of Lake Suess and other extinct lakes indicated pluvial periods approximately contemporaneous with the glaciation of N.W. Europe; and as these lakes were regarded as having occurred at different dates they indicate more than one wet period.

The correlation of the variations in the extent of the glaciers and in the amount of the rainfall in Equatorial Africa with those in N.W. Europe raises questions of interest to anthropology, geology and meteorology. That major variations in the European climate would have affected that of Northern Africa is obvious; and in 1914 I adopted the view that the pluvial period of Palestine and Cyrenaica was due to the southward diversion of the low pressure systems by the European ice-sheets; and those countries may serve as the link for the correlation of the climatic variations of Central Europe and Central Africa.

The possibility of the precise correlation of variations in rainfall in E. Africa and the British Isles depends on whether the changes in the European glaciation were synchronous. Some direct evidence and the extreme uncertainty in the correlation of European glaciations, interglacial periods, and dry periods marked by beds of loess, suggest that the glaciations waxed and waned independently at different centres.

Prof. Boswell points out that even in the British Isles different areas were glaciated at different times and that the suggested correlations of the British and the Alpine glaciations are doubtful. If the increase in the glaciers of Britain and the Alps had been synchronous, the corresponding variations in N. Germany should have been also simultaneous. But the efforts to correlate the German and Alpine glaciations have so far been unsuccessful. Dr. C. Gagel states: 'I hold it impossible, with our present knowledge, to extend the Penck-Brückner classification to N. Germany'; and his conclusion is still valid. Obermaier remarks the different conclusions based on the N. Germany and Alpine deposits. Wohlstedt correlated the Baltic End-moraines with the Würm, but declined to make any more detailed correlation with the Alpine succession.

The difficulty in the correlation of glacial deposits even in a country of exceptionally uniform character is shown by the uncertainty in the classification of the major divisions of the N. German drifts. It is still uncertain how many times that country was glaciated. Thus, E. Geinitz, the champion of 'mono-Glazialismus,' believes in one glaciation with ten stages of retreat. Beyer (1927) advocates two glaciations; K. Beurlen (1927), three glaciations; W. Wolff, three or four; Van Werveke, adopts three for Magdeburg district, but says there was an older one elsewhere and that there

| Glacials (England). | | | Pluvials (Rhodesia). | | |
|---|---|---|---|---|---|
| **Horizon.** | **Climate.** | **Culture.** | **Horizon.** | **Climate.** | **Culture.** |
| ? Bühl | Local Glaciers Cold and Wet generally | Azilio-Tardenoisian (Creswell, &c.) | Makalian of Kenya | Wet Phase | Early Wilton of Rhodesia<br>Developed Aurignacian of Rhodesia |
| Inter-Glacial (? Post Würm) | Cold<br>Warm . . .<br>Cold | Developed Aurignacian (Creswell)<br>Aurignacian ,,<br>Proto-Solutrean ,,<br>Upper Mousterian ,, | Inter-Pluvial | Desiccation | Developed Aurignacian of Rhodesia<br>?<br>Upper Rhodesian Aurignacian |
| 4th Glacial (? Würm) | Glacial<br>Warm . . .<br>Glacial | Middle Mousterian (Creswell) | Gamblian I & II of Kenya | 2nd Major Pluvial | Upper Rhodesian Aurignacian<br>Middle ,, ,,<br>Lower ,, ,,<br>Rhodesian Mousterian } contemporary<br>Lower Rhodesian Mousterian |
| Inter-Glacial (? Riss-Würm) | Warm | Lower Mousterian (Creswell)<br>Levalloisian (E. Anglia, &c.)<br>Late Acheulean ,, | | | S. African Levalloisian (? Derived) |
| 3rd Glacial (? Riss) | Glacial | | | | Developed S. African Acheulean<br>S. African Acheulean |
| Inter-Glacial (? Mindel-Riss) | ? Warm | Levalloisian (E. Anglia, &c.)<br>Acheulean Lower ,,<br>Developed Chellean ,, | Inter-Pluvial | Severe Desiccation | S. African Acheulean (at end of period)<br>?<br>Lower S. African Acheulean (early in period) |
| 2nd Glacial (? Mindel) | Glacial | | Kamasian of Kenya | 1st Major Pluvial | Lower S. African Acheulean<br>,, ,, Levalloisian |
| Inter-Glacial (? Günz-Mindel) | ? Warm | Chellean | | | ,, ,, Developed Chellean |
| 1st Glacial (? Günz) | Glacial | | | | ,, ,, Chellean (? Derived) |
| Pre-Glacial | ? Warm | Early Chellean<br>Pre-Chellean | Pre-Kamasian | ? | Early Chellean of Rhodesia<br>Hope Fountain Chellean do. |

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were certainly four; Range (1926) adopts four; K. Keilhack (1926) holds for five, with three of them represented in N. Germany.

The classification of the German Loess is also uncertain. According to Soergel, loess occurs on three main horizons; K. Keilhack attributes its deposition to five epochs—those of the Günz, Mindel, Riss and two in the Würm. As to the age and correlation of some of the beds of loess there is marked disagreement.

The local character of the glacial successions renders this uncertainty in correlation intelligible. Thus, instead of the ice having advanced all across north Germany with a continuous front, it apparently invaded different areas at different times. Thus, Keulen (1927) refers to the 'West to East wandering of the glaciated districts' and holds that the second glaciation did not extend over N.E. Prussia.

A further test of the correlation of the English and N. German glaciations is afforded by the Scandinavian erratics. If they were carried by land ice 600 miles from the area of the rhomb-porphyry in the Oslo Fiord to Yorkshire and Norfolk, the Scandinavian ice should at the same time have overflowed into N. Germany. According to Prof. Boswell the Scandinavian boulders in England are pre-Chellean or according to an alternative correlation, pre-Acheulean; so that we should expect the maximum development of the Scandinavian ice to have been in the Lower Palæolithic; but in Germany the invasion of the plains of Hanover by ice which deposited such vast numbers of northern boulders that the cobbled roads are a museum of Swedish and Baltic rocks—was in the time of the Fläming moraine. It is assigned to the Warthe stage of the last glaciation and is referred to the upper Mousterian.

In reference to the arguments based on the Scandinavian erratics, it should be remembered that those found in England came from low ground bends, the Oslo Fiord, and not from the high mountains of S.W. Scandinavia; and as the conspicuous rocks which the ice from those mountains would have crossed have not been found in England, the transport of those rocks by glaciers appears doubtful. The fact that those boulders come from the Baltic coast, and those in Holland include fossiliferous rocks from the bed of the eastern Baltic, makes the dispersal across the North Sea easier to explain by floating ice, and the blocking of the eastward flow of the British ice, for which the Scandinavian ice was involved, may be accounted for by earth movements in the bed of the North Sea. That extensive earth movements happened in Germany during the glacial period is now widely accepted; and the correlation of the German glacial deposits is being rendered the more complex by the claim that some of the main features in the glaciated areas are due to glacial and post-glacial earth movements (e.g. Soergel, 1923; Kraus, 1925; Beurlen, 1927).

As regards the correlation of the North German drifts and the Palæolithic stages there is also considerable difference of opinion. Wohlstedt classifies the upper part of the German glacial series as follows:—

| | | |
|---|---|---|
| Weichsel (Vistula) Glaciation | Its recession—Magdalenian<br>Its maximum—Solutrian | =Würm and Baltic Terminal Moraines |
| Weichsel—Warthe Interglacial | Aurignacian | |
| Warthe Glaciation (with Fläming moraine) | Late Mousterian | (Inferentially=Riss) |
| Warthe-Saale Interglacial | Early Mousterian | (Inferentially Mindel) |
| Saale Glaciation | | |

According to Boswell the general view is that the Mousterian was in the Riss-Würm Interglacial epoch—the view, amongst others, of Soergel and Penck. But according to Beyer the Mousterian was coincident with the Riss glaciation. According to Wiegers (1920) the Riss glaciation was late Acheulean, and the Mousterian lasted to the Würm. Wohlstedt ranks the Mousterian as coincident with the Fläming Moraine and the maximum of the Warthe glaciation, which from his classification would be equivalent to the Riss; the earlier Mousterian (Micoquian) he correlates with the Saale-Warthe Interglacial. According to his views the Saale glaciation would be the Mindel; whereas according to Van Werveke it is the Riss.

Hence the classification of the North German drifts is uncertain as regards number of glaciations, the position of the associated beds of loess, and correlation with the British and Alpine glaciations. Detailed correlation with more distant areas must be still more difficult.

Well established coincidences in weather and rainfall in remote localities show that they are affected by world-wide meteorological variations. Hence there is no improbability in the synchronism of a glacial period in one area with a glacial development in another. The uncertainty in the correlation of glacial deposits in adjacent areas, even in parts of a country, such as the North German Plain, where the geographical uniformity would appear most favourable to concurrent glacial developments, indicates the difficulty in the correlation of meteorological processes in more distant and different areas.

Dr. K. S. Sandford said that he felt very deeply the difficulty of correlating from one area to another, and that he feared he had not been able to find in Egypt the evidence of an arid period between two Pleistocene pluvial periods. He was therefore unable to agree with Misses Gardner and Caton-Thompson on this point.

Prof. W. J. Sollas, F.R.S., said that he felt somewhat disturbed by the voices urging caution. He hoped that investigators would have the insight to discriminate between major and minor episodes in the history of the Pleistocene Ice Age, and would have the courage to go on making hypotheses which were, at any rate, guides to further investigation.

Prof. Sölch (Heidelberg) said that while there were many divergent opinions in Central Europe, as in England, he thought that most workers accepted the Penck-Brückner sequence with the Günz episode either omitted or reduced in importance, and the Mindel-Riss interglacial period strongly emphasised. Most workers in the Alps now accepted the view that the Hötting breccia belonged to the Mindel-Riss interglacial; and indicated relatively warm conditions. The opinion was very generally held now in Central Europe that the latter part of the Pleistocene Ice Age had witnessed an uplift of some 500 m.; the climatic consequences should be borne in mind.

Prof. P. G. H. Boswell was unable to attend, but sent the following notes on *Early Man and the Correlation of Glacial Deposits :—*

The numerous attempts which have been made to correlate the successions of glacial deposits in the areas of the Alps and North-west Europe have been founded on one or both of two conceptions: (1) that the maximum glaciation (as defined by either the area covered by ice or the intensity of cold) is synchronous in the Alpine area and north-western Europe, and (2) that human industries are approximately contemporaneous, and not merely homotaxial, in the two areas, and are similarly related to the glacial and interglacial phases in each of the areas.

In the first place it would be well if we had some direction from our meteorological friends as to the probability or otherwise of maximum glaciation being synchronous in the Alpine and N.W. European areas. From the geological standpoint the 'maximum glaciation' of N.W. Europe is not the simple conception it used to be. Work carried out during the last few decades has served to show that in the successive glacial episodes which made up what we understand by the Great Ice Age, the centres of growth of the ice-sheets migrated farther southwards in western Europe as they approached the Atlantic. In the simplest case what we may term the maximum glaciation of eastern England antedated the maximum glaciation of western England and Ireland. Since, also, the more easterly part of each ice-sheet advanced less towards the south than the westerly, it is difficult to assign a date to the maximum glaciation of western Europe as a whole, and still more difficult to correlate it with the maximum extent of the Alpine ice, even supposing that this maximum was synchronous in the eastern and western Alpine areas.

Closely connected with the ice-load and its melting is the problem of river and sea-terraces. While correlation based on differences of level may be possible in such an area as the Mediterranean, it does not appear likely to yield results on the Atlantic shores, having regard to the differential movement known to have taken place and the sporadic nature of the records we possess.

The mammalian faunas afford help in broad correlation, but the species have too long a time-range for linking up small rock-divisions. Hence it would seem that we may expect most from the human industries themselves. If we use these industries for detailed correlation we must regard them as contemporaneous, notwithstanding the time occupied in the migration of the peoples responsible for them, or in the diffusion of technique.

Let us therefore examine the possibilities of correlation of British and Continental deposits on this basis. It will help if we set out the British stratigraphical succession in a table, the first column giving the order of superposition actually observed (except where stated below) in the field.

(Only broad terms in the industries and stratigraphy are introduced.)

| *Horizon.* | *Climate.* | *Industry.* |
|---|---|---|
| Newer Drift (Boulder Clay, &c.) | Glacial | Magdalenian. |
| Brickearths, Valley deposits, &c. | ?Warm | Aurignacian. |
| Upper Chalky Boulder Clay, Contorted Brickearths, &c. | Glacial | Mousterian. |
| Sands, Gravels and Brickearths. | ?Warm | Acheulean and developed Chellean. |
| Chalky-Jurassic and Chalky-Neocomian Boulder Clay (=Lower Chalky B.C.) | Glacial | ? |
| Sands and Gravels | ?Warm Molluscan fauna very like present day, but possibly derived. | ? |
| Scandinavian Drift of Durham, Yorks, Norfolk, &c. (Norwich Brickearth). | Glacial | ? |
| Cromer Forest Bed | Warm in the main, Fauna in part derived. | Early Chellean. |
| Red, Norwich, Weybourne, &c., Crags. | Earlier crag deposits. Warm, later cold. | Pre-Chellean. |

The serious problem presented by the above table is that, if we accept the Cromer Forest Bed implements as Early Chellean, the Chellean industry must straddle a powerful glacial episode, and presumably be of long duration. When I pointed out this difficulty to the Abbé Breuil in numerous discussions last summer he was very loth to admit any such straddling, and preferred to be non-committal regarding the dating of the Cromer Forest Bed implements. We agreed that an *impasse* had been reached and that we must await further evidence. Now the belief seems to be general that the Mousterian on the Continent belongs dominantly to the Riss-Würm interglacial episode (the Levallois industries I and II being, according to the Abbé Breuil, late Mindel-Riss, and the Levallois III being Riss-Würm) and that the Acheulean and developed Chellean belong to the Mindel-Riss interglacial episode. Granting this to be the case, I leave my archæological friends, if they insist on Alpine correlations, to correlate the Günz (or first East-Alpine) glaciation with either of the two earlier glaciations as they please. As compared with early Alpine glaciations there is apparently one cold period too many in the British area. The case at present is worse than that of the mythical Irishman's waistcoat, which had one button too many at the top and one buttonhole too many at the bottom, for here we have one button too many in the middle.

At this stage a note is perhaps desirable on the Scandinavian Drift, lest it should be thought that this deposit is due to a minor fluctuation of an ice-sheet. The Scandinavian Drift is found in Durham, Yorkshire and East Anglia, being termed the 'Norwich Brickearth' in the last-named region. The Cromer Till and Contorted Drift, which were also formerly included in this group of deposits, are at the moment regarded by different geologists as due to one of three different ice-advances (those of the Scandinavian Drift, the Great (Chalky) Boulder Clay and the Newer Drift); they may therefore be disregarded for the time being.

The Norwich Brickearth is underlain near Lowestoft by a 'warm' Forest Bed which has always been accepted, from the evidence of its fauna and flora, as equivalent to the Cromer Forest Bed.

The peculiar lithology and characteristic boulders of the Scandinavian Drift differentiate it from the later boulder clays. It has the appearance of a much more ancient deposit, for it is frequently decalcified and weathered; moreover, it is eroded until its surface has become hummocky or so that the deposit now exists only as outliers. The mature, open valley-systems of Norfolk and Suffolk are cut through a widespread sheet of it, but the Chalky Boulder Clay of the succeeding glaciation wraps over from the plateau on to the valley flanks and floors. The valley-systems may thus be regarded as interglacial, for the Scandinavian ice-advance was followed so far as East Anglia was concerned, by an important period of ice-recession, possibly the most important experienced by the British area as regards length of time and amelioration of climate. This recession was succeeded by a re-advance of ice which had passed over very different rocks.

At the moment I see only two alternative ways of avoiding the *impasse* referred to above: (1) to regard the Cromer Forest Bed implements as older than Early Chellean, notwithstanding the fact that the Cromer Forest Bed fauna contains abundantly mammalian forms which accompany the Chellean industry *passim*, or (2) to assume a certain degree of contemporaneity in ice-retreat and advance as outlined in the following paragraph.

The Scandinavian ice-sheet is known to have hemmed in the British ice-sheet on the coasts of Durham and Yorkshire; the ice-sheets were, therefore, contemporaneous so far as that area was concerned. At the same time, the Scandinavian ice (which had possibly reached Norfolk before it reached Yorkshire) had begun to retreat from Norfolk and Suffolk, allowing the valley-systems to be carved out in the interval before the British ice had time to advance from Yorkshire over East Anglia. Thus, the Scandinavian Drift and the (Lower) Chalky Boulder Clay would be pene-contemporaneous and the list of glaciations in the above table would be reduced by one. The difficulty of the considerable time-interval required for valley-erosion and the weathering of the Norwich brickearth *within* a glacial episode would still remain.

It is doubtful whether geologists would accept the second alternative. If, then, there is doubt about the Chellean age of the Cromer Forest Bed implements, and if the true Chellean industry should be found in the deposits which intervene between the Scandinavian Drift and the Lower Chalky Boulder Clay, the difficulty of the additional glacial episode would disappear, and the Scandinavian Drift would mark the first glacial episode in the British area. As the later East Anglian 'Crags' cannot be regarded as deposits of arctic character, and yield no evidence in themselves for correlation with a Günz glaciation, such a solution would probably commend itself to geologists.

Mr. J. D. Solomon considered that it is doubtful whether the Alpine sequence of glaciations as developed by Penck and Brückner is applicable to East Anglia; and, indeed, the contortion of the glacial deposits and the paucity of good exposures make it difficult to work out any sequence at all in the deposits.

There are, however, certain pieces of evidence which yield good indications of the relations between the glaciations and the successive Palæolithic cultures of this area. These may be briefly enumerated as follows:—

(i) At Hoxne, in Suffolk, an Acheulean Culture is found in deposits lying between a lower, chalky-Kimmeridgic boulder clay and an upper Chalky boulder clay.

(ii) At Whitlingham, near Norwich, a mixture of unrolled or slightly rolled Acheulean implements together with some rolled and glacially striated Chellean implements is found in what appears to be a river-terrace gravel, which is overlain by a small thickness of what appears to be a boulder clay.

(iii) In the latest gravels of the Cromer Morainic ridge, implements of late Acheulean date have been found; and the striking glacial topography of the Cromer district cannot be dissociated from the formation of the ridge and of those gravels.

(iv) Throughout the South-easterly part of Norfolk a tripartite glacial succession is known, viz.:—

(3) Chalky Kimmeridgic boulder clay, fresh and little weathered.

(2) Mid-glacial sands and gravels (Marine).

(1) Norwich Brickearth, with Scandinavian boulders, much weathered. This evidence bespeaks at least a tripartite glaciation in South Norfolk and North Suffolk, as the Norwich Brickearth plainly represents an older glaciation than either of the Hoxne boulder clays between which the Acheulean industry is found.

But considerable difficulty arises when an attempt is made to trace this threefold division northwards into the Cromer district; for there is here no obvious representative of the Norwich Brickearth, the Cromer Till, relatively unweathered, resting directly on the upper members of the Forest Bed series. However, in the cliff-section between Sheringham and Mundesley there are many patches of weathered boulder clay incorporated in the contorted drift, as well as patches of sand with marine fossils; and it would seem that these may quite possibly represent the Brickearth and Mid-Glacial sands of further South. When the cliff-section is traced East of Mundesley, the glacial deposits thin out considerably, and two layers of fresh boulder clay separated by a deposit of laminated clays overlie directly a series of deposits attributed to the Forest Bed series.

It is just possible that this identification is mistaken, and that instead of the Forest Bed we are really dealing with a series of interglacial sands, clays and peats *below* which the equivalent of the Norwich Brickearth may occur. It may be mentioned in passing that a Chellean implement, apparently deriving from a peaty bed, was found on the beach at Eccles. This is considered by Mr. Reid Moir as evidence for the presence of a Chellean horizon in the Forest Bed; but as, in spite of the large quantity of artifacts obtained from that horizon, no quite undoubted Chellean coup-de-poing has yet been found, the writer regards the attribution of the Chellean industry to that horizon as non-proven.

Further, since the Forest Bed contains no Scandinavian pebbles and is, therefore, earlier than any glaciation in this area, the assumption of a Chellean Culture in the Forest Bed would imply the occurrence of two glaciations between the Chellean (*sic*) of Cromer and the Acheulean of Hoxne, a result which seems improbable. Thus, in the writer's view, the sequence of events was as follows :—

(i) Deposition of Crag and Forest Bed series, the basement bed of which contains a series of pre-Chellean industries and also some glacially striated pebbles, which may well be derived from the Alpine area.

(ii) Advent of North Sea ice, and consequent deposition of Norwich Brickearth.

(iii) Retreat of ice, followed by a period of weathering, with probable deposition of some peaty beds in the low-lying ground which marked the course of the old estuary in which the Forest Bed was laid down; occupation by Chellean man. Followed by an incursion of the sea resulting in the deposition of the mid-glacial sands and gravels.

(iv) Return of ice, principally from the West, and consequent deposition of Chalky-Kimmeridgic boulder clay of Suffolk. It is possible that the belt of 'Cannon-shot' gravel and somewhat dissected glacial topography just north of Norwich may belong to this epoch.

(v) Retreat of ice; period of valley cutting and some terrace formation; occupation by Acheulean man.

(vi) Return of ice from the North; this glaciation was not as intense as the preceding one and produced only a small thickness of boulder clay at Hoxne and Whitlingham; the Cromer Ridge marks either a slight later re-advance or else a lengthy halt during the retreat period of this ice-sheet.

(vii) Retreat of ice, with possible reincursion in the low-lying area of the Wash, where upper Palæolithic implements are recorded from a boulder clay by Mr. Reid Moir.

A fairly coherent correlation may be made between this series and the Alpine sequence starting from the known fact that the fauna of the Crag and Forest Bed are generally correlated with that of the Günz-Mindel interglacial. We have thus :—

| | | | |
|---|---|---|---|
| Stage | (i) | Günz-Mindel interglacial— | Pre-Chellean. |
| ,, | (ii) | Mindel glaciation | |
| ,, | (iii) | Mindel-Riss interglacial— | Chellean. |
| ,, | (iv) | Riss glaciation | |
| ,, | (v) | Riss-Würm interglacial— | Acheulean—(?) Early Mousterian. |
| ,, | (vi) | Würm glaciation | |
| ,, | (vii) | Post-Würm warm period— with (?) Bühl stadium. | Aurignacian, Solutrian. |

Mr. H. J. E. Peake sent the following note in answer to Prof. Boswell's :—

I have read with great interest the memorandum prepared by Prof. Boswell on the correlation of the successions of glacial deposits in the Alps and North-west Europe. As a geologist he is sceptical of the possibility of solving the problem by geological means, and turns to archæological evidence as supplying more reliable data for the purpose. As an archæologist I have similar doubts as to the efficacy of my own subject, though I am inclined to believe that the possibilities of the geological approach have been under-rated.

I would submit that the true succession of types of the Lower and Middle Palæolithic phases, with which alone we are concerned, appears to-day to be by no means as certain as it did ten years ago. Broadly speaking, we have evidence of successive stages of two industries, a core industry and a flake industry. In the core industry we term those stages Proto-Chelles, Early Chelles, Evolved Chelles and St.

Acheul, distinguishing many more intermediate stages. In the flake industry we have the types known as Levallois and Le Moustier and perhaps others.

Collections from many sites exhibit most if not all the stages of the core industry, all apparently unrolled, and the deposit can only be dated by the most developed types found, since the others appear to be rough or possibly unfinished tools; much the same may be said for the flake industry. Moreover, the latter industry was present long before the former went out of use, though it may, and probably did, continue later, at any rate in North-west Europe. Thus, the simple succession, Early Chelles, Chelles, Evolved Chelles, St. Acheul and Le Moustier no longer holds good.

Further than this, implements of St. Acheul type have been found in plateau gravels, at any rate as high as 150 feet above river level, and I believe higher. The circumstances under which these have been found suggest that they may have been dropped on the surface of these gravels long after the latter were deposited, and subsequently covered over by rearrangement of the surface. We cannot, I think, be absolutely sure that the same process has not occurred in lower gravels, like the Boyn Hill terrace at Maidenhead, 80 feet above river level. The same criticism applied, though perhaps in a lesser degree, to the various types of the flake industry.

It is true that there may be difficulties in using for our purpose sea-terraces, not so much because of differential movements, which have been hardly sufficient to disturb our conclusions, at any rate south of the Humber, but owing to the scarcity of the deposits. River-terraces are, however, plentiful, especially in the Thames basin and elsewhere in the South and East of England.

Speaking as an amateur, with a slight experience only of the Thames basin, I would suggest that the problem would be advanced if the whole of this basin, and others in neighbouring areas to the North and South of it, were examined with the detail and accuracy employed by Overy and Sandford in dealing with the Upper Thames. We need, I would suggest, a number of accurate sections across the valleys of the Thames and its tributaries, showing in the case of each terrace and plateau gravel the height of the base and the highest level reached, given in terms of height above the present summer level of the river at that spot. If such sections, at no great intervals, were made for the Thames basin, the Hampshire basin and the basin of the Wash, these could probably be correlated with accuracy. The East Anglian gravels are in close connection with the boulder clays, as are some of the Thames gravels in Hertfordshire and Essex. It would thus be possible to correlate the terraces with the different boulder clays, and thus with the northern ice-sheets.

Again, it should be possible to equate our terraces, not only with those of the Somme, but with those of the Rhine, and by means of the latter river with the Alpine glaciations. It is even possible that the desired correlation could be obtained from evidence in the Rhine basin alone, since its source is in the midst of the Alpine region, while on its passage through Holland it cuts across a moraine of the northern ice-sheet. By some such means, provided great accuracy were used, we should obtain a more reliable result than can ever be achieved by the study of archæological evidence.

Miss Garrod gave as her contribution to the discussion a study of the evidence that led her to identify a relatively late Pleistocene pluvial period in Palestine.

Dr. G. C. Simpson, C.B.E., F.R.S., suggested that it is probable that there are large variations of solar radiation which, as a first approximation, we may consider to be periodic. During the periods of maximum radiation the climate of the world as a whole is warm and wet, while during the periods of minimum radiation it is cold and dry. The effect of these changes varies from place to place. On the borders of a region where ice forms in the winter, the effect will be as follows. With the solar radiation at its minimum the temperature will be low, but owing to the absence of cloud and precipitation there will be little or no accumulation of snow, the winters will be cold and the summers warm. The conditions will give rise to steppes.

As the radiation increases there is more snow-fall and ice-sheets develop. As the radiation increases still further, the mean temperature rises. More rain falls but less snow, and the melting is increased. Finally, when the temperature and rainfall are at their maximum all ice may disappear—probably right to the poles. A complete oscillation of solar radiation will lead to one warm and wet epoch, one cold and dry epoch and two advances and retreats of the ice.

There appear to have been two such oscillations of solar radiation during the Pleistocene period. In North-west Europe the Pleistocene period came in with low radiation and a cold dry epoch (the Weybourne Crags) then the ice advanced on account of increased precipitation (Günz Glaciation) only to retreat as the radiation and temperature increased, bringing in a warm wet epoch (the Cromer Forest Bed). The maximum of solar radiation was passed and the temperature and precipitation decreased. On account of the decreasing temperature ice formed and advanced (Mindel Glaciation), but as the precipitation decreased the ice retreated and there was a cold, dry interglacial epoch (Mindel-Riss Interglacial). In this epoch the minimum of solar radiation was passed and the ice advanced again (Riss Glaciation) and retreated as the temperature rose, leading to the second warm interglacial epoch (the Riss-Würm Interglacial). The sequence continued with another glacial epoch (the Würm Glaciation) followed by another cold interglacial epoch (this gave rise to the steppe conditions in North-west Europe associated with the Magdalenian Culture). The present conditions in Europe were introduced by a drift of the Pole; but the world as a whole is still in a cold, dry interglacial epoch.

According to this sequence there should have been during the Pleistocene period two pluvial periods of world-wide extent correlated with the Günz-Mindel and the Riss-Würm interglacial epochs respectively; and three arid periods, the first before the Günz Glaciation, the second during the Mindel-Riss interglacial epoch and the third at the present time.

Dr. C. E. P. Brooks said that, in considering the various bases of correlation of Quaternary events in different parts of the world, stratigraphy, palæontology, archæology—we must not forget that the succession of climatic changes may also have a high correlation value. A large ice-sheet, because of its high reflective power for solar radiation and high emission of terrestrial radiation, is a powerful cooling agent, and winds blowing outwards from the surface extend the cold over a large area. The existence of an ice-sheet covering Scandinavia and the Baltic would, therefore, cause "sympathetic" glaciation of the Alps. Where ice-sheets ended in the sea, the latter was cooled by the melting ice, and this must have extended the area of cooling still more widely, and finally, the presence of extensive areas of ice must have profoundly modified meteorological conditions in all parts of the world. Glacial phenomena were developed in a large number of places in all continents from the Equator to the Poles. Such a widespread distribution implies a general cause rather than a series of independent local causes. Hence, whatever the cause actually was, and it is not unlikely that two or more causes, geographical and cosmic, operated together, it is *a priori* highly probable that the main glacial stages were contemporaneous in all parts of the world. Support, if any is needed, is provided by the researches of de Geer and his collaborators, who have shown that the final retreat of the glaciers was probably contemporaneous in Europe, Iceland, North America, the Argentine and the Himalayas.

Deposits indicating the presence of more water are as widespread as those indicating more ice. Thus, in regions which were not glaciated we can distinguish a Pluvial period, which is definitely of Pleistocene age and to that extent roughly synchronous with the Glacial period. Unfortunately, pluvial and glacial phenomena are typically developed in different regions, but in western U.S.A. the greatest extension of the Sierra Nevada glacier ended in old Lake Mono when the latter also attained its maximum development. For the present, however, the correlation of the various stages of pluviation and glaciation must rest mainly either on archæological evidence or on meteorological considerations.

The amount of water actually in the atmosphere at any time is relatively very small, hence the total annual rainfall over the globe must be equal to the total annual evaporation.

Whatever the initial causes, there is little doubt that the great fall of temperature in high and higher-middle latitudes was due largely to the ice itself, on land and sea, and that in tropical regions temperature during the glacial periods was little, if at all, lower than at present. There was a great increase in the temperature difference between low and high latitudes and this must have led to greater wind movement, both horizontally and, because of the increased storminess, vertically. This must have increased the evaporation and consequently also the rainfall. Over the great ice-sheets themselves, however, conditions must have been largely anticyclonic and the total water precipitated less than at present, still further increasing the rainfall over the non-glaciated regions. The most marked changes would be expected on the equatorial flanks of the ice-sheets, as on the southern side of the Alps, and within the equatorial belt of low pressure which receives the trade winds. Hence, there are several meteorological reasons for correlating pluvial with glacial periods ; we may go further and say that the glaciation of high mountains within the tropics was probably due more to increased precipitation than to lower temperature.

The three interglacials represented in Penck and Brückner's Alpine scheme do not seem all to have had equal importance. It is generally supposed that the second, the Mindel-Riss, was several times longer than the first and third, and Dr. Simpson believes that its character was entirely different. At least it is reasonable to assume that where fewer than four glaciations were developed the reduction was in some cases caused by the merging of either the Günz and Mindel, or the Riss and Würm, or both. Investigation of 'pluviated' regions generally shows only two pluvial periods of Pleistocene age, and I think that the best correlation that can be made on meteorological grounds at present is to equate the first pluvial with the Günz and Mindel, the second with the Riss and Würm.

Prof. G. B. Barbour, speaking about northern China, said that the apparent Pleistocene sequence there was two periods of aridity with accumulation of loess separated by a period of relatively moist conditions. Prof. Barbour thought the loess periods could be correlated broadly with the main glacial periods.

Mr. L. S. Cammiade said that the N.E. monsoon of the Coromandel coast and Eastern Ghats could not at any time have been much heavier than now. There seems to have been one long major pluvial period in the Pleistocene along the east coast of India with formation of laterite, which demands an alternation of moist and dry seasons, both warm. The decay of laterite began at a very remote period archæologically and apparently it was only then that man appeared.

Prof. A. E. Douglass gave an illustrated account of the method of investigating tree-rings as a key to past climatic changes. At present we cannot date rings beyond a fairly recent period, but opportunities should be taken for following possible extensions of this method into remoter periods.

The Chairman in summing up said that there would be general agreement with both Prof. Gregory and Prof. Sollas that one could go seriously astray in trying to make correlations too detailed, while one could also miss the major truths by timidity in refusing hypotheses of correlation altogether because of difficulties about details which were likely to be regional peculiarities.

It was interesting to note the widespread tendency to identify an earlier and a later ice age separated by a long interglacial period, and themselves subdivided into episodes. Prof. Sölch's statement that continental opinion was generally in favour of identifying the Hötting breccia as belonging to the Mindel-Riss interglacial was important because, if this opinion maintains itself, it suggests the need for a large modification of the clever and interesting theory of Simpson. Prof. Sölch's reminder of the trend of continental opinion towards the acceptance of the idea of an uplift of about 500 m. in one of the later phases of the Pleistocene Ice Age was also useful, as the importance of orographical changes had been rather overlooked in some recent discussions. If orographical changes of this magnitude did occur, it was probable that some estimates of time involved in glacial phases would need serious revision.

The general agreement of Miss Gardner, Mr. Leakey and Mr. Armstrong concerning two major pluvial periods in Africa with a relatively dry period in between was interesting, even if Dr. Sandford could not agree, as it illustrated the same trend of opinion as that found among European workers. It also reinforced the *a priori* correlations of glacials and pluvials.

The evidence from China and India was also most interesting, as it suggested a scheme dovetailing with that of two major glacial periods and one interglacial.

There were endless difficulties still to be met, but the discussion showed a widespread movement towards a broad agreement, and the exchange of views would, it was hoped, help field observers to notice relevant points in the course of their work.

Mr. E. J. Wayland's communication on *Pleistocene Pluvial Periods in Uganda*, referred to above, is summarised as follows :—

The word pluviation must be given either absolute or relative significance, and the same applies to the term pluvial period. In order usefully to limit the application of the latter term, it is here given geological connotation and defined as *a period of* *geological significance during which rainfall was in general considerably heavier than in earlier times, and than it is to-day, over an area sufficiently large to be of some account in world events.*

Evidence of relatively high precipitation in the past is not difficult to obtain. What is required, however, is a body of geological fact that can only be interpreted as arising from an increase of rainfall, and, after long sustainment, a subsequent decrease to that of the present day.

In the case of high lakes, the high level could be engendered by a decrease in evaporation, but it does not seem probable that rainfall would remain unaltered if any considerable change in evaporation took place. Fortunately, a long-sustained high lake is likely to leave behind it some evidence of its origin (*e.g.* in Uganda, damming of erosion valleys by lava flows, tectonic tilt, depression of ground); but one which owes its existence to heavy rainfall will display not only evidence of high levels in the form of wave-cut cliffs and beaches, etc., but also of high level built-up gravel terraces of deltaic origin at the mouths of affluent rivers.

Pluviation pure and simple is incompetent to produce river-terraces in a valley already at grade—an essential for terrace-production is an alteration of base-level. The problem is, what is the cause of this alteration ? The answer involves sorting out tectonic and climatic factors, so far as they are represented.

The Uganda evidence in relation to pluvial periods may be classified under five heads : (*a*) Heavy rainfall ; (*b*) Aridity ; (*c*) Change from drier to wetter conditions ; (*d*) Change from wetter to drier conditions ; (*e*) Chronological evidence. Only the last-named can be touched upon here.

*Chronological Evidence.*—1. The great African pene-plain has been more than once disturbed. Such disturbances occurred in late Pliocene or Early Pleistocene days, and again, with very marked effects, in post-middle or late-middle Pleistocene times.

2. In the history of climatic changes, climatic and tectonic effects are intermixed, and the problem has been to disentangle them. The first climatic event with which we are concerned was a long period of precipitation, in all probability in late Pliocene and/or early Pleistocene.

3. The next climatic event is a dry period in the middle Pleistocene.

4. Then followed a period of heavy precipitation in middle Pleistocene, or post-middle. Extremely important earth-movements occurred during this period.

5. A very marked oscillation to arid conditions followed, and was succeeded by a relatively wet climate, apparently not of long duration, and by no means as severe as the two preceding wet periods.

6. Associated with the wet periods are definite Stone Age cultures.

The geological evidence leaves no room for doubt that the first two wet periods were of long duration, and there can be hardly any doubt that they were true pluvials. So far as dating goes these appear to correspond each with a pair of recognised glacial periods, that is to say, Pluvial I was more or less contemporaneous with the Günz and Mindel glaciations, whilst Pluvial II was more or less contemporaneous with the Riss and Würm glaciations. The two pluvials were separated by a marked dry interpluvial period.

Recent investigations by Dr. G. C. Simpson into the probable cause of the great Ice Age resulted in a theory which demands precisely those climatic conditions that appear to have obtained in eastern central Africa during the Pleistocene ; and all available evidence favours the idea that these two pluvial periods were true pluvials in that they were contemporaneously manifested over wide areas of the earth's surface.

Geological investigations, at any rate, must go much further before we can be justified in adopting, with complete confidence, any such correlation as that which I have accepted ; meanwhile the suggestion provides a working hypothesis.

### Afternoon.

Excursion to Bath.

### Evening.

Prof. G. B. Barbour.—*The Geological Background of Peking Man (Sinanthropus).*

## Wednesday, September 10.

Mr. L. S. B. Leakey.—*Types of Pottery associated with various Stone Age Cultures in Kenya.*

Miss Tildesley.—*The Standardisation of Anthropometric Measurements.*

Mr. E. W. P. Chinnery.—*Natives and Government Mandated Territory in New Guinea.*

The native population of that part of New Guinea formerly owned by Germany and now being administrated by Australia under mandate from the League of Nations is believed to exceed half a million. Over 117,000 of these people have been visited by Government officials, and new tribes are coming under control every year.

For the purpose of administration the territory is divided into eight districts, each in charge of a district officer and staff, which includes European officers of armed constabulary and a number of native constables recruited in various parts of the territory.

The natives of New Guinea are known as Papuasians and are divided into two main linguistic groups—those who speak Austronesian languages and show Micronesian, Polynesian and Melanesian influences, and those who speak languages known as Papuan.

Generally the Papuan-speaking peoples occupy the interior, but here and there their settlements occur also on the coast. For the most part, however, the coastal regions and the small islands forming part of the territory are occupied by Austronesian-speaking peoples, though in places like the Morobe district, Melanesian thrusts extend inland for some distance, especially on the Markham river, where they may be found 100 miles inland. Micronesian influences are seen amongst the people of the smaller islands lying nearest to the Equator, while Polynesian influences are found on the Feads, Mortlock and Tasman Islands, north and north-east of Bougainville.

The natives of New Guinea present great differences in type, language and culture, and offer a maze of problems to the anthropologist.

In order the better to help these people along the road to progress, the Government of the Territory has established a system of training the cadets of its district administration staff in anthropology. These young men, chosen in the first place for educational and general capabilities, spend two years on the out-stations under the supervision of experienced district officers, and then, having proved themselves likely to be successful officials, are attached for a year to the School of Anthropology, Sydney University, where they receive training in anthropology, the elements of tropical hygiene, surveying, etc., and then return to the Territory to take up positions as patrol officers, where they will be in contact with the natives, but still for some years under supervision until they merit promotion to the rank of Assistant District Officer.

The New Guinea Administration is making every effort to develop its native races without unnecessary damage to the very elaborate institutions which form part of native life, thus fulfilling the trust imposed on it by the League of Nations 'to advance the moral and material welfare of the native population.'

Dr. M. Vassitz.—*Excavations on the Neolithic Site at Vinča.*

Mr. A. J. Goodwin.—*The Royal Regimental System of arming the Ama Mpondo.*

Presented and taken as read :—

(i) Henry M. Ami.—*Five years' excavations at Combe-Capelle, Dordogne, in the Moustierian, by the Canadian School of Prehistory.*

The paper contains an account of the work done and the results obtained by the Canadian School of Prehistory from the site placed at its disposal by the French Government in 1925. The discoveries of implements and fossil organic remains in the *five* beds or strata superimposed one upon the other are noticed, which beds rest unconformably at a series of Pleistocene or Quaternary deposits upon the eroded surfaces of the Upper Cretaceous chalky limestone strata forming the crescent-shaped hill of Combe Capelle, near Monferrand-du-Périgord, Dordogne.

The finds include materials illustrating different phases of the Moustierian of the district, and are correlated with others from Le Mointier, La Ferrassie, Les Eyzres, La Quina, La Micoque and other classic sites of that part of France. Combe-Capellians were great inventors of useful domestic tools, and show much skill and dexterity in making them.

(ii) Dr. Hans Reck.—*The Geology and Palæontology of the Oldoway Man.*

(iii) Mr. A. T. Culwick.—*Paintings and Rock-shelters in the Singida District, Tanganyika Territory.*

*Note.*—There was accidentally omitted from the Report, 1929, the title of a paper read in Johannesburg before Section H on August 2, 1929, viz. :—

Prof. G. H. Stanley.—*Primitive Metallurgy in South Africa : some products of their significance.*

This paper contained, *inter alia*, the results of examination of samples or specimens collected at Zimbabwe by Miss Caton-Thompson, and is published in full in the *Journal* of the South African Association.

## SECTION I.—PHYSIOLOGY.

### Thursday, September 4.

**Presidential Address** by Prof. H. S. Raper, C.B.E., F.R.S., on *The Synthetic Activities of the Cell* (see p. 160). Followed by Discussion.

Prof. A. F. Stanley Kent.—*Some Unpublished Work on the Heart.*

In 1892 the author first announced the existence of a muscular connection between the auricles and ventricles of the Mammalian Heart and stated that the connection was *multiple*—that in addition to what is generally now known as the A–V bundle, a connection existed on the *right* side of the heart.

The existence of this additional connection, which may take one of two forms, can be demonstrated by careful histological study, while its functional activity may be shown experimentally. In addition, there are certain clinical observations which support this opinion.

Prof. G. A. Buckmaster.—*A Renewed Study of the Blood-platelets.*

Dr. F. W. Edridge-Green, C.B.E.—*Pseudo-Isochromatism and the Detection of Colour-Blindness.*

Colours are called pseudo-isochromatic when they are distinguished easily by normal-sighted persons but are regarded as identical by certain classes of the colour-blind.

The pseudo-isochromatic tests in common use are those of Stilling, Ishihara and my Card Test. In order to ascertain the relative merits of these three tests, thirty-eight consecutive, referred or appeal, cases were examined at the Board of Trade with the following results. It should be noted that none of these cases was examined by me alone, that in all at least three persons and often more were present at the examinations. In every case Captain Ellery or Captain Dowdy and the Recorder were present. Each candidate was first examined by the pseudo-isochromatic tests, and the fact whether he was dangerously colour-blind or not was ascertained subsequently.

Ten of the thirty-eight cases were passed and twenty-eight rejected. Of the twenty-eight who were failed sixteen failed with all three pseudo-isochromatic tests, but twelve of those rejected passed Ishihara completely and eleven passed Stilling. Of those who were passed seven showed defects with my Card Test, and of those who failed all were rejected by my Card Test, in most cases failing very badly, as for instance, not reading card eight, a yellow-green C on an orange ground. The explanation of the difference is that a test for a dichromic will not necessarily detect a trichromic who has three colour sensations, red, green, and violet but no yellow sensation, and who is obviously dangerous, as shown by the examination with the lantern and spectral tests. In my Card Test, also, the pattern is the same on every card, so that the examinee has to judge by colour alone and cannot follow the design of the artist.

An important point to note is that a test which is difficult to the normal-sighted is not necessarily difficult to the colour-blind. This is shown by the number of colour-blind persons who can read Stilling's plates numbers nine and ten, the reading of which is supposed to indicate very good colour perception. In fact, a test may be constructed which can be read by the colour-blind but not by the normal-sighted.

### AFTERNOON.

Prof. R. J. S. McDowall.—*The Function of Carbon Dioxide.*

Prof. C. Lovatt Evans, F.R.S., and Mrs. M. Grace Eggleton.—*The Removal of Lactic Acid after Exercise in the Mammal.*

Miss E. M. Killick.—*The Adaptation of Small Animals to Carbon Monoxide.*

Canaries and mice were exposed daily to atmospheres containing carbon monoxide; the concentration of CO varied in different experiments from 0·12 per cent. to 0.26 per cent., and the exposure given varied in duration from 10 to 30 minutes. In all cases the animals used developed an adaptation to the gas, becoming less affected with successive exposures.

In the case of the canaries, the rate of absorption appeared unchanged during adaptation, but there were indications that the red cell count in the blood increased; in the case of the mice, the results of two experiments suggested that with the appearance of adaptation, the absorption of carbon monoxide was retarded.

## Friday, September 5.

Dr. J. H. Shaxby.—*The Weber-Fechner Law.*

*Summary:*—An attempt to formulate the general relation between Sensation and Intensity of Stimulus on a statistical basis. It is shown that a chance distribution of stimulus quanta among a finite number of sensory units in a receptor leads to the observed forms of the relations between $\frac{I}{\Delta I}$ and S or log I.

That log I and not I is the abscissa in the latter case is due to the mathematical form of a frequency statistic, and not to any special physical or physiological peculiarity of sense-organs.

Prof. R. Ruggles Gates.—*The Blood Groups and their Inheritance.*

The blood groups are probably best represented by the formulæ $O_{\alpha\beta}$, $A_{\beta}$, $B_{\beta}$, ABo. They are inherited as fixed Mendelian units. There have been three views of their inheritance; (1) Two independent factors (Hirzfeld), (2) Three allelomorphic conditions (Bernstein, Furuhata), (3) Two linked factors with 11 per cent. of crossing-over (K. H. Bauer). The last view explains the cases which are exceptional. The agglutinogens A and B probably arose as dominant mutations from O.

Among isolated peoples, the American Indians are mostly O (a few A), the Eskimo, Lapps and Australian natives being mostly O and A. Isolation will account for these conditions if B originated as a mutation much later than A, since the highest proportions of B occur in South and East Asia. If mutations from O to A and B are still occurring, they are probably of less frequency than 1 per cent. Such mutations, in addition to wandering of races, may be necessary to account for the present condition. Chimpanzees are mostly A (a few O), platyrrhine monkeys and lemurs contain B. Probably the human blood groups developed independently in man, through parallel mutations.

**Discussion** (Sections G, I) on *Air Pressure Variations encountered in Engineering Works, and their Physiological Effects.* (See Section G.)

## Monday, September 8

**Discussion** (Sections I, J) on the question, *In what Sense can we speak of Primary Colours?* (Opened by Dr. J. Drever. Prof. H. E. Roaf, Dr. J. H. Shaxby, Dr. F. W. Edridge-Green, C.B.E.)

Dr. J. Drever.—Though the employment of the term 'primary' with respect to colours may be regarded at the present time as characteristic mainly of the physicist's approach to the investigation of the phenomena of colour, there are really four distinct points of view from which it is apparently possible to speak of 'primary' colours—those of the artist, the physicist, the physiologist, and the psychologist respectively. The artist's primaries are red, yellow, and blue, the physicist's red, green, and blue, the physiologist's red, green, yellow, and blue, and the psychologist's also red, green, yellow, and blue, though a different red and green from those of the physiologist.

Artist and physicist apply what is essentially the same logical principle of classification. The artist finds that all the known bright colours can be derived from a mixture of red, yellow, and blue pigments, while the physicist claims that all the colours of the spectrum and of nature can be produced by an admixture of red, green, and blue lights. The psychologist and the physiologist in turn challenge the right of the physicist to dogmatize concerning colour at all, which, they point out, is a physiological, and ultimately a psychological, not a physical, phenomenon. Moreover, the psychologist claims that yellow is as much a *primary* colour as red, green, or blue, and the physiologist's investigation in perimetry would seem to confirm this claim. The phenomena of perimetry, indeed, as well as the phenomena which appear with increase of luminosity of the spectral series up to the limit at which the individual colours disappear, suggest that red and green are relatively secondary to blue and yellow, the latter being perhaps the true physiological primaries.

Attempts have been made to determine more precisely—that is by wave-length—the primary colours, and the general results may be summarised as follows:

| | Physical Primaries (König) | Psychological (Edinburgh) | By Perimetry (Baird) |
|---|---|---|---|
| Red . . | Outside Spectrum and complementary to 494$\mu\mu$ | 649$\mu\mu$ | Outside Spectrum and complementary to 491$\mu\mu$ |
| Yellow . | — | 562$\mu\mu$ | 569$\mu\mu$ |
| Green . | 505$\mu\mu$ | 510$\mu\mu$ | 491$\mu\mu$ |
| Blue . | 470$\mu\mu$ | 472$\mu\mu$ | 461$\mu\mu$ |

The agreements no less than the discrepancies are significant. The red of both physicist and physiologist is outside the spectrum and appears bluish. The green of the physiologist is its complementary and also appears bluish. Physicist and psychologist agree as regards green, and physiologist and psychologist as regards yellow, while all three are more or less in agreement as regards blue. The discrepancies, however, are such that it would appear desirable to discontinue the practice of speaking of 'primary colours,' at any rate without specification of the sense in which the term 'primary' is employed.

Prof. H. E. Roaf.—Colour being a sensation, a primary colour is one that appears to be a single entity.

The sensation of colour can be produced by stimulation by radiations of certain wave-lengths; the physiological problem is to determine what receptors are necessary in order that the whole range of the visible spectrum and the extra spectral colours can be recognised.

There is experimental evidence that three separate kinds of receptors are necessary, whilst histologically two different kinds, namely rods and cones, are recognised. In many birds, however, there are three sets of cones which must be stimulated by different parts of the visible spectrum. Experimental evidence is in favour of colour vision in man being of the same nature as the histological evidence suggests that it would be in birds.

Some of the evidence can be classified under the following headings :—hue discrimination, red-green confusion, adaptation, visual acuity, halation and blue blindness.

Mr. A. W. Macdonald and Mr. A. Schlapp.—*Quantitative Aspects of the Action of Drugs.*

Mr. J. B. Young.—*The Pupillary Mechanism of the Teleost Fish, Uranoscopus.*

At the Naples Zoological Station I recently had occasion to study the pupil of this bottom-living fish. It was found that the mechanism is exactly the opposite of that found in mammals in that stimulation of the sympathetic chain causes constriction of the oculomotor nerve dilatation of the pupil. These results were confirmed by operations on the living animal. It was found that the pharmacological responses of the sphincter iridis muscle were the same as those of the mammalian muscle. We have, therefore, in this fish, a remarkable case of a muscle which has the pharmacological characteristics usually associated with a parasympathetic innervation and yet receives its motor nerves from the sympathetic.

Afternoon.

Visit to Bath.

## Tuesday, September 9.

Dr. Watson Williams.—*Chronic Toxaemia as a Cause of Degeneration of Mind and Conduct.*

In attacks of acute infection accompanied by obvious illness, high fever and delirium, we naturally attribute the patient's altered mentality to germs or toxins reaching the brain through the blood and therefore do not hold him responsible for his acts and conduct until with convalescence he is restored to his right mind.

It has become recognised of late years that in patients who survive attacks of Encephalitis Lethargica there may develop changes of conduct which may attain definite criminal manifestations. The author pointed out that similar results may follow long-standing focal sepsis without clearly apparent physical illness.

The importance of this in medico-legal work need not be stressed.

Prof. J. A. Nixon.—*The Factors concerned in Diabetic Coma.*

Although the discovery of insulin has marvellously advanced the treatment of diabetes, the explanation of the symptom-complex of diabetic coma seems further away than ever. It is, indeed, doubtful whether any of the phenomena which attend diabetic coma are responsible either for its production or its fatal ending. Though, as Josli says, air-hunger is ' the most significant clinical finding,' diabetic coma may occur even without air-hunger. Every one of the signs which at one time or another have been considered to play a part in the production of coma seems to have been proved blameless.

Two of the factors concerned are the fat and the sugar intake and the third is insulin, furnished from within or from without. There is little doubt that diabetic coma can always be averted if the fat intake of diabetic patients is reduced and at the same time a generous allowance of carbohydrate food be given along with adequate insulin.

Demonstrations :—

(*a*) Dr. Carey Coombs.—*Conducting Tissue in the Heart.*

(*b*) Dr. F. W. Edridge-Green, C.B.E.—*The Theory of Vision.*

(*c*) Miss M. O. P. Wiltshire.—*Reactions to Adrenaline.*

(*d*) Mr. F. W. Wakefield.—

(1) *A New Method for studying Permeability Changes.*

(2) *A Simple Device for the Detection of dangerous amounts of Carbon Dioxide in the Air.*

Afternoon.

Visit to Long Aston Research Station.

## SECTION J.—PSYCHOLOGY.

### Thursday, September 4.

The Psychology of Adolescence :—

Prof. Olive Wheeler.—*Variations in the Emotional Development of Normal Adolescents.*

The three chief emotional developments characteristic of adolescence are associated with the three major adjustments involved in 'growing up.' They are :—

I. *Emotional Developments during Adolescence.*

(1) The increased feeling for self, tending to the development of psychological independence and the finding of a vocation.

(2) The appearance or intensification of sexual emotions, tending to the development of a hetero-sexual attitude and the finding of a mate.

(3) The development of social, æsthetic and religious emotions, tending towards the formulating of a working philosophy of life.

II. *Variations* occur among 'normal' adolescents.

(*a*) in time and rate of emotional developments ;

(*b*) in general emotionality ;

(*c*) in emphasis on the chief kinds of emotional developments (indicated in I above);

(*d*) through conflicts between these developments. Differences between the sexes in these respects..

III. *Environmental Influences* affecting variations.

(*a*) the family circle ;

(*b*) the school or educational institution attended (whether co-educational or not, &c.);

(*c*) modern industrial conditions (particularly modern difficulties in finding suitable employment) ;

(*d*) the religious organisation (if any) with which the adolescent is associated.

Dr. R. G. Gordon.—*The Basis of Social Adjustment.*

The problems of adolescence are largely problems of adjustment to society, and adjustment depends on the formation of a sentiment of the social self. A sentiment is a high-level development only possible in the presence of a brain which functions adequately. The social self, the object of the sentiment, is essentially the extraverted ego as opposed to the introverted ego. All or any of the instincts, emotional dispositions and derived emotions may be organised in relation to the extraverted ego, but there are three principal instincts involved. The herd instinct is not so definite a factor in human behaviour as is sometimes supposed, as it has become very much modified. The desire for physical contact may have been transferred to the mating instinct and sexual play, and for the rest it seems to facilitate the ill-defined instinct of suggestion, primitive sympathy and imitation. On this largely depends the education of the individual, and it enables him to build up his relationship to other selves. The sex instinct gives the necessary drive towards extraversion without which the social self cannot be developed. We cannot consider the social self as a simple addition of these three instincts, for they must be integrated together and with other emotional dispositions as well as certain physical factors, and out of this integration there emerges something new and unique.

The study of the mental defective gives us a slow motion picture of the development of the social self, slow enough in some instances to obtain a static cross section. The mental defective is not lacking in any of the instinctive bases of the social self, yet he is conspicuously lacking in integration, discrimination and control, and so in the power of social adjustment. These are functions of the cerebral cortex and the ament lacks an efficient cortex. Hughlings Jackson's work showed how the cortex is concerned with control, Sherrington's showed the rôle of the cortex in integration, and Head's illustrated the function of discrimination. These functions are concerned with adaptation and this can only be developed if the cortex comes to full development. This may be hindered in cases of primary amentia ; amentia from deprivation and distortion of interests.

Recent work by the author and Dr. Thomas has shown the continued development of the mentally defective and the late adjustment to social conditions made by backward children. Amentia should, therefore, be regarded not only as a deficiency of intelligence, but another group should be recognised who show deficiency in social adjustment. These latter may be divided into those who compensate by an excessive egocentricity, such individuals often being epileptic, and those who are generally inefficient who may be described as psychasthenic. This does not, of course, imply that all psychoneurotics are defectives, but many who are regarded as psychoneurotic are really suffering from a degree of amentia.

## Miss A. H. McAllister.—*Adolescent Modes of Thinking.*

An investigation of 253 original stories for children, written by women student teachers, average age 18 years 2 months, reveals evidences of certain common trends of Thought. (*a*) Sixty-two per cent. *fairy tales*, i.e. stories in which the fantasies of day-dreaming are employed. Twenty-four per cent. submitted *nature tales*, and only 14 per cent. contributed *home tales*, i.e. simple every-day incidents which might occur in ordinary life. (*b*) An examination of the fairy tales gives the following data regarding the adolescents' trend of thought.

(1) Sixty per cent. of the stories make mention of ' mother ' only, as if hers is the only adult influence at work in the home.

Twenty per cent. only make reference to ' father,' and that only in conjunction with mother.

The same tendency to exclude father is evident also in the *nature tales*, which mention mother and offspring in their descriptions of bird and animal life without any reference to the other parent. In the few *home tales*, father appears only to inflict punishment for wrong doing.

The adolescent seems to exclude father from conscious thought of home. This may be due either to a sudden adolescent realisation of the importance in the home of mother, whose offices are largely ' taken for granted ' during childhood, or it may be due to the gradually developing sex-consciousness intensifying the ' mother ' impulses in the girl herself.

(2) In 48 per cent. of the fairy tales the human actors live ' In a little white or thatched cottage on the edge of a wood or forest.'

The wood or forest is a place of high adventuring in which difficulties are resolved by the intervention of ' fairy ' power, so that the goal is attained romantically but surely. The goal is always clearly set before the adventurous mortal and is self-appointed.

This reveals quite obviously the adolescent tendency to form clear-cut ideals ; but it is to be noted that the adolescent has little thought for what difficulties may really mean and always sees them as being cleared from the path of achievement—not by self-help, but by other forces obligingly prepared to make each path smooth.

(3) The adolescents' fairyland is a Utopia, a Heaven in which all desires are fulfilled, all beauties enjoyed. (*a*) It is a place of exquisite beauty glowing with sunshine and perfumed with the scents of iridescent flowers. (*b*) It is a busy world : all have tasks to do and must do them well. (*c*) It is a secret world, open only to a chosen few who have found the secret doorway by their own seeking.

In their descriptions of this ideal world the adolescents reveal a surprising conception of what community life ought to be, but it is significant that these conceptions are worked out under the conditions of a Utopia. Not one of the home tales submitted describes a happy, wholesome picture of normal home life.

One hundred and forty-two original stories by women teachers, average age thirty, were brought in for comparison. Their treatment of fairy tales and nature tales emphasised one or two additional trends of the adolescents' thinking.

(1) While both sets of writers seek in a fairyland compensations for the ills of present life, their method of attaining it is different. The adolescent causes the mortal completely to leave his present world and escape to fairyland by magic aid, changing his form and being completely different; the thirty-year-old brings fairyland to the mortal as he is, in his own surroundings: there is neither change of form nor circumstance.

The adolescent seeks compensation for disappointment and unhappiness by ranging in imagination a world quite other than this, in which dreams come true. The more mature have learned to find 'Heaven about them' in the ordinary ways of life.

(2) Many of the adolescents and thirty-year-old writers seek to cure a child of some vice by the experience in fairyland, but the adolescent makes the experience a punishment. The older writers make it a demonstration of the beauty and desirability of virtue. The adolescent is negative in his thinking about right and wrong, tending to condemn and eschew evil rather than to approve and seek after virtue.

## Miss M. Phillips.—*The Adolescence of the Young Wage Earner.*

The differences between the adolescent in industry and normal adolescence are to be found rather in the development of the sentiments than in intellectual development. Though there may exist a rough correlation between social and economic status on the one hand and intelligence on the other, yet the range of intelligence to be found among repetition workers is always large, and where any of the following three factors are present—periods of industrial depression; the good conditions offered by certain firms; the existence of areas whose elementary schools are as yet imperfectly combed by any scholarship system—this range may be almost complete.

Where low intelligence exists, its main effect is, together with the unstimulating mental environment in which many repetition workers live, to form sentiments whose ideational, emotional and impulsive material is scanty, crude and ill-organised. Where, on the other hand, such factors in the mental environment as home, school and work are stimulating and formative, a development of the sentiments may take place which is analogous to the rapid development under good physical conditions of a hitherto undersized, because undernourished body.

The most serious defect of the young wage earner's environment usually consists in the unstimulating and unsatisfying work which he is often offered. This commonly results in an attitude of unambitious, unadventurous resignation, or in withdrawal into fantasy, or both. Even during work hours the adolescent's mind may be centred on his leisure time, past or present, or may be employed by dreaming. The same withdrawal from the real world may take place in the sphere of personal relationships; boys and girls may refuse to undertake the adventure which getting to know each other involves.

Failing any transformation of the industrial environment, the way out lies in the enrichment of leisure time by varied and stimulating intellectual, emotional and æsthetic material. For girls, especially, the first essential is experience of vivid, attractive, unfamiliar personalities. Welfare schemes, continuation schools, clubs of various kinds may all undertake with success this task of transforming personality through the transformation of leisure.

### Afternoon.

## Mr. R. J. Bartlett.—*Some Effects of Low Frequency Vibration on Body and Mind.*

Unpublished work on the effect of metronome ticks on breathing has shown that subjectively-imposed rhythms are related to the breathing frequency and that, in particular, as the beat of the metronome is changed in length, the breathing period lengthens or shortens until a critical length of beat is reached and then, after a period of uncertainty, accompanied by feelings of anxiety, irritation or annoyance, a new subjective rhythm sets in.

The experiments now described were performed to put to the test the possibility that similar effects would result from the low frequency vibration set up by a running motor.

An old electric motor was loaded eccentrically and run at speeds from 12 to 20 revolutions a second, and the pneumograph records from subjects sitting in a chair subjected to the resulting vibrations were taken.

The records reveal marked changes in the amplitude, frequency and form of the respiration curve and show that, at certain critical speeds, which the subject finds 'very unpleasant,' breathing is at first very irregular but tends, with continuation of the stimulation, to settle down into rapid, shallow breathing that in the opinion of the subject is in unison with the vibration frequency of stimulation, and that changes to deep, gasping breathing when the stimulation is stopped.

The subjective reports contain reference to :—

(*a*) Vibration of the body or its parts, changing in position and intensity with changes of speed.

(*b*) States of tension that, subjectively, make it possible to control or endure the unpleasant bodily vibrations and accompanying emotional experiences.

(*c*) Cutaneous sensations such as 'pins and needles,' 'tickling,' and 'cold.'

(*d*) General physio-psychological effects as headache, dullness, dizziness, tiredness and sleepiness.

(*e*) Pulse and respiration effects as 'throb of temples,' 'throb of pulse,' 'the pulse and breathing both being in unison with the mentally counted rhythm.'

(*f*) Revival of unpleasant travel experiences, including marked fear of catastrophe and travel sickness. Cold, sleepiness and fear are outstanding effects of certain revolution frequencies and in all probability are attributable, in part at least, to a partial deprivation of oxygen caused by the hurried, shallow breathing induced by the critical speeds. The danger of such effects on motor drivers and air pilots needs no emphasis.

### Mr. Eric Farmer.—*A Consideration of the Frequency Distribution of Certain Tests.*

The examination of the frequency distribution of certain intelligence and sensori motor tests showed that those that were scored by the amount of a task done in a given time had Gaussian (*i.e.* saddle-backed) frequency distributions, whereas those that were scored by the time it took to do a given task had skew distributions with the mode nearer the good tail than the mean is. Since it is better for the purposes of correlation if the test scores are normally distributed, it is suggested that where possible the use of time-scored tests should be avoided.

Certain visual tests had J-shaped frequency distributions. Certain physiological tests such as pulse rate, on the other hand, had normal Gaussian distributions. It is suggested, therefore, that the J-shapedness of the visual frequency distributions may be due to the fact that they measure the capacity of a sensory end organ, and not because they are in the main physiological in character.

### Mr. H. E. O. James.—*Interference.*

## Friday, September 5.

### Mrs. S. Isaacs.—*The Relation between Thought and Phantasy in Young Children.*

I. Watching the free play of a group of intelligent young children, three main types of spontaneous activity can be made out (which, however, are by no means sharply separated, but readily slip over one into another); (*a*) the practice of bodily skills of all sorts; (*b*) make-believe dramatic games; and (*c*) direct curiosity as to the *why* and the *wherefore* of physical objects, and of animals and plants, for their own sake. This last is the main field in which relational thought develops.

II. The relation between make-believe and active thought again seems to be three-fold: (1) circumstantial, (2) conative, and (3) cognitive.

(1) Imaginative play creates practical *situations* which often lead on to genuine discovery, to verbal judgment and reasoning and to interchange of argument.

(2) There is a profound *conative nexus* between thought and phantasy. The psycho-analysis of young children has shown the importance of symbol-formation in

the earliest development of the ego. The *first* value to the child of the physical (as against the social) world is as a canvas upon which to project his personal wishes and anxieties; his first form of interest in it is one of dramatic representation. And dramatic make-believe play helps to lessen inner tension arising from the earliest anxieties, and so to free the child's interest for real events and real objects. Imaginative play builds a bridge by which the child can pass from the symbolic values of things to active inquiry into their objective characters.

It may well be that the most significant difference between children of different ages is to be found in the direction of their *interests.*

(3) The third relation falls strictly within the field of *cognition* itself. The child's ability to evoke the past selectively and appropriately in imaginative play seems to be closely connected with the growth of his power to evoke the future in constructive hypothesis, and to develop the consequences of 'ifs.' In his make-believe he takes the first steps towards that emancipation of meanings from the *here* and *now* which makes hypothesis possible.

III. The *mechanisms of the child's phantasy* ('ego-centric' and 'syncretistic thought') must be distinguished from his true *judgments.*

Make-believe is neither an hallucination nor a phobia. The child's 'magic' and 'omnipotence' are not fixed as organised systems of belief; they are strictly an affair of feeling and phantasy. Relational thought and true belief slowly emerge from this matrix of feeling and phantasy under the cumulative pressure of experience.

**Presidential Address** by Prof. C. W. Valentine on *The Foundations of Child Psychology, and their Bearing on some Problems of General Psychology.* (See p. 176.)

## Afternoon.

### Mr. R. W. Pickford.—*Some Effects of Style and Rhythm in Reading.*

Subjective observations have been collected in order to study problems of interest both in general psychology and in the special branch of reading. Two series of extracts selected from general literature, twelve in the first and nine in the second, were read, for the most part aloud, by eighteen subjects, of both sexes, who gave introspective reports after each reading. One of the extracts is quoted in illustration, and three of the series of introspections upon it are given too, for the same purpose.

In a note on the psychological standpoint adopted it is pointed out that the mind is conceived as a system of activities or processes, and the object of psychology as that of tracing their functional relationships. The terms 'conflict' and 'solution' are used to refer to specially differing modes of functional organisation: conflict occurs when activities are not in harmonious integration; solution when they are.

Style is regarded as psychologically distinct from rhythm, being more closely connected with meaning—in the sense 'cognitive synthesis'—than it, and having relative functional independence. A confused style may give rise to emotional or unemotional conflicts; contempt and vituperation are interesting defence-systems which may be linked with the former class. Discord between style and idea expressed is also a source of conflict; which, here again, may have either emotional or intellectual qualities in dominant degree.

Solution may be brought about by style in an unwitting manner, that is, one which defies the subject's powers of analysis: clarity favours solution, and it may be recognised as such, or function through pleasant feeling-tone. In the highest order of styles there is an organic harmony with the matter expressed: when this exists the complete apprehension of a piece involves the threefold synthesis of the subject's preconceptions, the language-form—that is, the style and rhythm, and the ideas expressed by the author in his writing. Various incomplete degrees of such a synthesis have been observed.

In this paper metre is treated as a special case of rhythm; and their relationship is not further analysed. Rhythm may give rise to conflict owing to the failure of the systems involved in the subjective organisation of the rhythm itself to combine with the cognitive processes: on the other hand, the disturbance due to the rhythm may be independent of the cognitive synthesis. Several modes of these conflicts are discussed in brief.

The solvent effects of rhythm are sometimes connected with pleasurable feeling-tone, which is closely linked with æsthetic experience : but a sense of movement is sometimes reported, which is a coarser function. Indications are given of the manner in which rhythm may work towards solution while the style is causing conflict at the same time. The final point discussed is the capacity of rhythm to select and emphasise parts of the subject-matter of a piece, through the meaning shaping itself in the rise and fall of the sentences : in some cases the subject is unaware that this is happening ; in others he can give a shrewd analysis.

I should like to thank the Medical Research Council for their help ; Mr. F. C. Bartlett for his advice and criticism ; and Dr. R. H. Thouless for the opportunity to work in his laboratory at Glasgow.

Miss C. A. Simmins.—*The Mental Processes involved in learning a Foreign Language.*

Considered psychologically, the numerous devices used in teaching a foreign language fall into three main groups :

(1) Those which aim at giving the pupils *new sense perceptions.*
(2) Those which aim at *practice.*
(3) Those which aim at *production* on the part of the pupils.

As soon as a pupil is required to *produce,* that is, to *understand* and to *use* the new language as a means of conveying thought, he will make *mistakes in grammar.* It is with the problem of grammatical accuracy that this paper is concerned.

Grammar is organised knowledge about the way in which relations are indicated in a language. The mistake in grammar is the failure to indicate a relation correctly.

With the aim of determining which of four contrasted methods is the most effective in teaching pupils to indicate relations correctly in a foreign language an experiment was planned at University College, London, under the direction of Prof. Spearman.

A course of lessons in German was given to the upper classes of four elementary schools. The first five lessons were precisely the same for all four groups. After this, the grammar introduced was taught to each group by a different method.

*Group A.*—A thorough-going Direct Method. Grammatical difficulties were not explained.

*Group B.*—A modified Direct Method. Grammatical difficulties were carefully explained.

With both A and B care was taken to avoid opportunities for error, so that the wrong impressions held by some to be so dangerous in learning a new language, were not made.

*Group C.*—The same method as Group B, except that here the girls were allowed to attempt to reproduce sentences exemplifying the grammar learned. *Errors were made and corrected.*

*Group D.*—As group C, except that questions were asked requiring the *application* of the grammar *in a new setting.* Again errors were made and corrected.

The groups were tested in grammatical accuracy by questions given orally in German, and answered by the pupils in written German. Comparable groups were selected and the scores compared.

Results.

(1) *Grammatically accurate reproduction of sentences learned.* The scores made by Groups C and D are consistently and significantly higher than those made by Groups A and B. The success of Groups C and D may be explained in the following way :— A question is a direct incentive to individual effort. A call is made upon conative reserves ; there is in consequence a sharper focussing upon the problem. While consciousness is still sharply focussed, the mistake made is corrected. The precise relation of the relevant word to the whole sentence has become something of real significance to the individual. Unless he understands clearly, he will make mistakes, and to make a mistake is damaging to self-esteem. The success of the method used with C and D demonstrates the effect of Conation upon Cognition and of the individual's response to incentive upon the clearness of his awareness.

(2) *Application in new sentences.* (*Transfer of Training.*) Here the advantage is consistently with Group D who had been given some opportunity during the lessons of applying the grammar in new settings. I suggest that in this way an attitude especially favourable to transfer was developed—a 'look-before-you-leap' attitude ;

a tendency to direct the mental energy at once to particular aspects of a new situation; a keener and swifter sensitiveness to the presence of a certain kind of relation, and to the *way of indicating it in the foreign tongue.*

Group D was, then, the most effective method. It is suggested, in conclusion, that a modification of the technique of modern language teaching on a basis of psychological investigations might bring about a considerable saving of time and energy in the learning of languages.

### Mr. H. Binns.—*Some Experiments with Wool-textile Trade Advertisements.*

### Dr. C. S. Myers, C.B.E., F.R.S.—*The Place of Industrial Psychology in a University City.*

In every important industrial University centre we should before long see established a Lecturer in Industrial Psychology, whose duty it would be to give theoretical and practical instruction in the subject to certain students of economics, education, commerce, and engineering, and to others who will later take up a business career, to carry out research and to train future investigators in industrial psychology, to promote better vocational guidance and to engage in it, to act as a consultant to local industrial and commercial firms, and to help an influential local Committee (which could well be constituted as the Committee of a Branch of the National Institute of Industrial Psychology) in getting together a large group of local people interested in the subject and in encouraging the directors of local concerns to employ in them the aims and methods of Industrial Psychology.

## Monday, September 8.

### **Discussion** (Sections I, J) on the question, *In what Sense can we speak of Primary Colours?* (See Section I.)

### Dr. R. H. Thouless.—*The Influence of the Physical Object on Perception, and its Bearing on the Laws of Perspective.*

If an experimental subject is made to look at an inclined circle and to indicate the shape that it appears to him either by drawing or by matching it with one of a series of ellipses of different ratios of short to long axis, he draws or chooses an ellipse, not of the shape which the laws of perspective would lead us to expect, but one in which the above ratio is considerably greater. In other words, he perceives the inclined circle not in the shape of the image on his retina but in a shape which is intermediate between that and the actual (circular) shape of the object perceived. Similarly, if two discs of different actual sizes are arranged at different distances from the subject until he sees them as equal, the actually larger disc must be at such a distance that its retinal image is considerably smaller. Again, the perceived relative sizes of the discs is intermediate between the relative sizes of their retinal images and their relative actual sizes. A similar observation with respect to brightnesses was reported by Hering, who used the term 'Gedächtniss-farben.'

The shape, size or brightness perceived by the subject may be called the 'phenomenal' character, in contrast with the corresponding 'stimulus' or 'perspective' character which is the shape, size or brightness impressed on the receiving end organ. The results reported may be stated in general terms as a tendency of the phenomenal character to differ from the stimulus character and to be intermediate between this and the real, physical character of the object. This peculiarity of perception may be called 'phenomenal regression.' Hering's term 'memory-colours' is inappropriate, since experiment shows that the regression depends not on memory of the real character of the object, but on the presence at the time of perception of sensory cues giving indications of the real character.

The presence of phenomenal regression has obvious implications for the teaching of drawing. In teaching children to draw in accordance with the laws of perspective we are not teaching them to draw things as they 'really' see them. On the contrary, we are teaching them to depart from the really seen phenomenal figure in favour of the perspective figure.

Dr. R. H. Thouless.—*Dr. Houstoun's Substitute for Weber's Law.*

It has long been known that Weber's Law does not adequately represent the facts of variation of differential threshold ($\Delta I$) with stimulus (I). If $\Delta I/I$ is plotted against I, an asymetrical curve is obtained with the two ends pointing steeply upwards. If Weber's Law were true, this should be a straight line parallel with the base. Dr. Houstoun has found, however, that for light sensation, if $\Delta I/I$ is plotted against log. I, a curve is given which is approximately the normal curve of error. The approximation is, in fact, remarkably close. If this proves to be true of all stimuli, Weber's Law that $\Delta I/I$ is a constant must be replaced by the law

$$I/\Delta I = k \cdot e^{-\frac{(\log . I - \log . I^1)^2}{2\sigma^2}}$$

in which $I^1$ is the value of I at which the curve attains its maximum and $\sigma$ is the standard deviation.

Dr. Houstoun also proposes to replace Fechner's 'fundamental formula' ($dS = k.dI/I$) by the formula

$$\frac{dS}{d(\log I)} = ke^{-\frac{(\log I - \log I^1)^2}{2\sigma^2}}$$

which by integration gives

$$S = k\int_{-\infty}^{x} e^{-\frac{(\log I - \log I^1)^2}{2\sigma^2}} d(\log I)$$

in place of Fechner's Law $S = K(\log . I/I_0)$.

Afternoon.

Mr. G. C. Grindley.—*Psychological Factors in Peripheral Vision.*

Mr. C. A. Mace.—*The Psycho-physics of Desire.*

Despite the prominence of the concept of 'purpose' in contemporary Psychology, little has as yet been achieved in the way of controlling human activity by scientific means even where this activity is most obviously purposive in nature. This divorce between general theory and applied science is due in the main to our ignorance concerning the way in which general 'good intentions' become specific to particular operations, and concerning the precise influence of conditions which *primâ facie* are relevant to this process of *specification.* Here, then, is an enticing but neglected field for experimental investigation. The chief problems are brought into view by investigation of the incentive conditions operative in ordinary industrial tasks, and by the experimental determination of the influence of similar conditions in the performance of tasks commonly employed in laboratory tests of psycho-physical traits.

An 'incentive condition' may be defined as any factor which modifies the content or direction, the intensity, the quality, the duration or the date of occurrence of a specific intention controlling the performance of a task, and it is a reasonable postulate that such characteristics of an intention will be reflected in, and therefore measurable by reference to, corresponding characteristics of the performance expressive of the intention.

Preliminary experiments suggest that principles such as the following are involved :

1. That under certain commonly realised conditions the *prescription of a standard* operates as an incentive condition modifying each of the characteristic phases of an intention, but that different types of standards influence the various phases in different ways. Thus, the prescription of a fixed standard will intensify effort at one time and for a certain period, whereas a 'sliding scale standard' will intensify effort at another time and for a different period. In the special investigations here referred to, the fixed standard proved on the whole the more potent incentive.

2. That the higher the prescribed standard the more potent it is as an incentive condition so long as the task continues to be performed, but the higher standards modify the 'quality' of the intention in ways that weaken the tendency of the intention to recur.

3. That the increase of facility due to transitory incentive conditions is permanent, and not limited to the period for which such conditions are directly operative.

4. That the *initial* effect of a reward is to foster favourable characteristics in an intention, whilst the *initial* effects of a penalty are unfavourable.

5. That certain forms of supervision operate favourably independently of fear of the supervising agent, and that influence of this kind is carried over to unsupervised periods of work.

The investigations here referred to suggest certain modifications in accepted interpretations of 'work curves' based upon industrial statistics. It is suggested, furthermore, that in general a practicable policy for the control of human volitional activity lies not so much in the endeavour to induce radically new motives in human pursuits, but rather in the piecemeal modification of specific characteristics of the desires that are actually operative and empirically manifested in social and industrial life.

Mr. J. M. Blackburn.—*Analytic Tests in Relation to Rifle-Shooting Efficiency.*

## Tuesday, September 9.

Dr. W. J. Pinard.—*Perseveration and the Introvert.*

Dr. H. Banister.—*The Psychology of the Tuberculous Patient.*

Dr. P. C. P. Cloake.—*Conditioned Reflexes: their Interest to the Psychologist.*

This paper describes briefly some of the experimental work of Pavlov and his pupils on excitatory and inhibitory conditioned reflexes, and their value in elucidating the nervous processes which occur in the cerebral cortex.

The various ways of inducing inhibition, by extinction, by delaying the presentation of the unconditioned stimulus, by differentiation of a specialised stimulus, and by combination of the conditioned stimulus with an indifferent stimulus without reinforcement of the combination, are referred to.

Experiments indicating the identity of generalised inhibition in the cortex and sleep are described, and the neurological mechanisms of catalepsy and somnambulism shown to be less extensive states of cortical inhibition.

Finally, the experimental production of functional nervous breakdown in the dog is described and attributed to the intense interaction of excitatory and inhibitory processes in the cortex.

This is considered to support the psychological theory attributing functional nervous diseases to intrapsychic conflicts.

Pavlov's methods of restoring dogs so affected to their normal state are shown to have similarities to empirical methods of treating human neuroses.

Dr. A. Wohlgemuth.—*Psychological Analogues of the Conditioned Reflex.*

### Afternoon.

Visit to Stoke Park Colony, with Section L. Demonstrations in Mental Deficiency and its Recognition, by Prof. R. J. A. Berry.

# SECTION K.—BOTANY.

## Thursday, September 4.

**Presidential Address** by Dr. A. W. Hill, F.R.S., on *Recent Developments and Present-day Problems in Taxonomic and Economic Botany.* (See p. 191.)

Mr. E. M. Marsden Jones and Dr. W. B. Turrill.—*Species Studies in Plants.*

*Research.* Concentrated work has been in progress for about six years on British species of *Silene*, *Centaurea*, *Saxifraga*, *Ranunculus*, *Geum*, *Anthyllis*, and *Primula*.

*Results.* The polymorphism of *Silene vulgaris* Garcke and *S. maritima* L. is shown to be very great and the combinations of characters very numerous owing to intraspecific crossing. A new scheme for the analysis of wild populations is proposed and examples are given. The relationships of the two species are discussed. Controlled intraspecific and interspecific hybridisation has proved the genetical basis of much of the variation found in nature.

Considerable light has been thrown on the puzzling taxonomy of *Centaurea* by intensive genetical and field studies. So-called species have been shown, both by analysis and by synthesis, to be either hybrids or of hybrid origin, with *C. jacea* L. as one parent and *C. nigra* L. (*C. obscura* Jord.) or *C. nemoralis* Jord. as the other. Natural hybrid swarms have been analysed in localities as far apart as Berkshire and Devonshire. *C. scabiosa* L. does not cross with other British species in the author's experience, but both this and other investigated species show a considerable range of interspecific variation.

By crossing *Saxifraga rosacea* Moench with *S. granulata* L., each with 16 gametic chromosomes, a non-segregating tetraploid was obtained with $n=32$. This has now bred true to the third generation of over 500 plants.

Sex and colour variations have been studied in *Ranunculus acris* L. and *R. bulbosus* L. From the breeding and cytological investigations on sex there has emerged the conception of a 'time-factor' involving the interval between the initiation of the reduction division in anthers and in ovules.

The hybrid *G. intermedium* Willd. has been shown to segregate on selfing. On back-crossing with either parent (*G. rivale* L. or *G. urbanum* L.) the progeny are nearer the parent used. Back-crosses between *G. intermedium* and *G. rivale* can easily be mistaken for true *G. rivale.* A wild hybrid swarm is described.

In *Ranunculus ficaria* L. and *Anthyllis vulneraria* L. research on intraspecific variation has made some progress and in *Primula vulgaris* Huds. the investigation of certain naturally occurring mutations has been completed.

*Conclusions.* 1. A conception of 'species' must include a consideration of isolation—geographical (as in *Centaurea scabiosa* and *C. collina*), ecological (as in *Silene maritima* and *S. vulgaris*), or genetical (as in *C. nigra* or *C. nemoralis* and *C. scabiosa*).

2. Hybridisation is one of the ways in which taxonomic units are produced (as in *Saxifraga potternensis*).

3. Some accepted taxonomic units are not stable but are heterozygous phenotypes which segregate on selfing, back-crossing, or further crossing, giving rise to complex hybrid swarms (as in *Centaurea* plants of the *nigra-jacea* group).

4. Species, as commonly accepted, are much more variable, much more complex, and much more heterozygous in intraspecific characters than is generally realised.

5. The species question is dynamic rather than static, and is to be investigated by methods of experiment with living material combined with field-studies and herbarium and library research.

Prof. D. H. Campbell.—*The Preservation of the Red Wood Forests in the Western States.*

Afternoon.

Visit to Mr. Hiatt Baker's garden at Almondsbury.

## Friday, September 5.

**Discussion** (Sections K, M) on *Mineral Elements in Plant Nutrition.* (Opened by Dr. T. Wallace. Prof. W. J. V. Osterhout; Dr. Gregory; Dr. Winifred Brenchley; Mr. G. K. Fraser; Sir John Russell, F.R.S.).

Dr. Winifred Brenchley.—The mineral constituents of the plant may be classed as major and minor according to the quantity present. The *major* elements are chiefly recognised nutrients whose importance varies according to the stage of development of the plant. In barley phosphorus is most essential during the early weeks, and may be entirely withheld later on without detriment to the yield, though the phosphorus uptake is affected. Its absence during the young stages is critical and results in serious or total reduction of ear formation as well as reduction of yield. The length of the critical periods during which the element is essential varies according to the time of sowing, seasonal conditions and the variety. Experiments in hand (June, 1930) suggest a somewhat similar course of events for nitrogen, though possibly this element is of more general importance throughout a longer period of development.

The significance of most of the *minor* constituents is hardly known as yet. Many elements are very toxic if they are present in relatively small amounts, but some appear to be of definite value in minute traces. Boron, for instance, is essential for the normal growth of such plants as broad beans, runner beans, and clovers, being intimately associated with the development of meristematic tissue and the production of flowers. Further evidence is now available which indicates that the need for boron may be varietal, as well as specific. Perfect development of plant and grain has constantly been obtained with Spratt-Archer and Goldthorpe barley in the absence of boron, but Plumage Archer failed this year at the time of flowering, irrespective of the date of sowing. Ear emergence was imperfect and very few grains developed, indicating faulty pollination; this was coupled with the sudden production of large numbers of small tillers at the time the grain should have been filling out.

In the presence of boron better development of ears and grain was obtained.

The variation in varietal response to minor nutrients may account for the apparent discrepancies in the results of different workers with such elements as boron, manganese, iodine, etc. The possibility also exists that other elements, hitherto unrecognised as plant nutrients, may prove to be essential in minute quantities for the development of various plants.

Minor plant constituents are also important from another aspect as their absence may predispose the plants to certain diseases or may be the specific cause of diseases, e.g. the Grey Speck disease of oats, which Samuel and Piper have shown to be due to deficient manganese absorption on certain soils. There are indications that environmental conditions may affect the value of certain minor elements as nutrients, but little definite information is yet available on this point.

Mr. A. Malins Smith.—*The Composition of Upland Bog Waters and its Relation to Algal Vegetation.*

Afternoon.

Excursion to Brockley Combe.

## Saturday, September 6.

Excursion to Forest of Dean.

## Sunday, September 7.

Excursion to Somerset Peat Moors by way of Burrington Combe and Cheddar.

## Monday, September 8.

Prof. W. Goodspeed.—*Cytogenetic Evidence as to Species, Origins and Relationships in the Genus Nicotiana.*

Mr. M. Thomas.—*Fermentations in the Cells of Higher Plants in the Presence of Oxygen.*

Although alcoholic fermentation (= zymasis), i.e. the accumulation of ethyl alcohol and, sometimes, acetaldehyde, occurs only rarely in the cells of higher plants in air, it has been observed in the absence of oxygen, in low concentrations of oxygen, and in certain relations of carbon dioxide and oxygen, hydrogen cyanide and oxygen, and sulphuretted hydrogen and oxygen.

These facts support current theories on the sequence of changes in cell respiration, and the recently won knowledge of the enzymes and of the cytochrome exchange system at work during respiration.

When results obtained by a single experimental method fail by themselves to convince, as is the case in most problems of metabolism, the multiplication of methods becomes an urgent need. Although interference with the usual production—removal sequences, by varying the external environmental conditions—is not a new mode of investigation, its scope has been considerably extended in recent years. Its use in work on sugar respiration in plants points to the importance of acetaldehyde as a link in the chain.

Many of the experiments have been carried out on apples, and some of them have already clarified ideas on certain physiological diseases of storage of apples and pears.

Prof. D. Thoday and Mr. N. Woodhead.—*The Growth and Metabolism of Kleinia articulata.*

In the sunflower stem the increase in vascular tissue, and especially of xylem, is organised sectorially in correlation with the development of the leaves with which it is in direct communication, and from an early stage the parenchymatous tissues accommodate themselves to the unequal growth of the different parts, and later are variously stretched (Thoday, Annals of Botany, **36**, 1922, 489-510). This relative passivity of the parenchyma, in contrast to the ordinary conception of the part it plays in growth expansion, led to a study of a succulent composite, *Kleinia articulata*. Here the bundles are all small and widely separated. Only in stems bearing branches is interfascicular cambium developed, and even then vascular tissue is only differentiated in a few very small groups. Growth in diameter occurs in two stages, an early stage during the first expansion of the stem, and towards the end of the growing period; in the latter the pith becomes fissured. Nitrogen starvation favours swelling at both these stages. Shading results in very slender stems, the structure of which suggests interpretation as young unswollen stems; and the slender inflorescence axis is very similar. In the rhizome, on the other hand, secondary growth is pronounced and a periderm is formed—in the slenderest a continuous zone of woody tissue is formed, in the thicker the zone is discontinuous.

A continuous zone of wood is also found at the narrow constriction between an aerial branch and its parent stem; here all the tissues are liquefied except the local periderm and small phloem groups.

Nitrogen starvation in sand culture resulted in abundant rhizome formation, but did not affect the time of flowering.

Mr. H. Evans.—*Buffering and Acidity in Kleinia articulata.*

The titratable acid-content is low as compared with the values given by Hempel for *Kleinia cuneifolia*, but of the same order as in certain other succulents with a relatively low acid-content.

Preliminary observations on the effect of light and darkness showed that the plants were rather variable in their reaction, the acidity often diminishing in darkness.

The stem is almost invariably richer in acid than the leaves, and generally the acidity increases from the tip to the base of a stem. The outer tissues are invariably richer in acid than the pith.

The difficulty in obtaining a well-defined end-point in the titration of the extracts with alkali led to some observations on the buffer system. The original intention was to gain more information about the best conditions for titration by construction of titration-curves. The pH values were obtained by the use of Gillespie's drop-ratio method. It was found that there was an extensive buffer system at the region of pH corresponding to the end-point. Analysis of the sap showed that the acid present was malic acid, and extracts, especially of old stems, gave a well-defined inorganic phosphate reaction. A precipitate is formed on titration of the juice with alkali, and this was found to be partly composed of calcium phosphate. Titration-curves of mixtures of calcium phosphate and malic acid were very similar to the titration-curves of the extract of old stems. Young stems showed a different type of curve, approximating more closely to the titration-curves of mixtures of aluminium malate and malic acid. Calcium phosphate plays an increasingly important part in the buffer system, with increasing age of the stem from which the extract is made, since both calcium and phosphate accumulate with age.

The buffer conditions in the extracted sap are not identical with the conditions which obtain in the intact tissues, for soluble calcium and soluble phosphates are more or less sharply segregated from each other in the tissues.

### Miss L. E. Hawker.—*A Quantitative Study of the Geotropism of certain Seedlings, with special reference to the Nature and Development of their Statolith Apparatus.*

Seedlings of about 80 different species selected from the Dicotyledons, Monocotyledons and Gymnosperms have been examined in order to give a general review of the morphology of the statolith apparatus from its first appearance in the seedling to its establishment in the young plant.

An attempt has been made to determine quantitatively the amount of statenchyma present at different stages in the development of seedlings of each of the three groups.

The presentation times for gravity have been measured for different periods in the development of the seedlings in about 15 representative species, and the lowest of these in each case, i.e. the maximum sensitivity to gravity, shows an interesting correlation with the maximum amount of statolith starch present, which is in accordance with the statolith theory.

A suggested explanation is offered of the phenomenon of physiological zygomorphy in the light of the statolith theory.

## Afternoon.

### Prof. R. R. Gates.—*Haploid Plants.*

A haploid *Oenothera rubricalyx* appeared in a culture of *Oe.rubricalyx* × *eriensis* in 1929, the remaining plants being true hybrids of a non-viable type. The haploid egg cell of *rubricalyx* was probably induced to develop parthenogenetically by the presence of pollen tubes belonging to a distantly related species. The haploid plant was a slender dwarf with narrow leaves, and totally sterile. Its cells and nuclei are smaller than the type. Other recent cases of haploid flowering plants are discussed. They have occurred in Datura, Nicotiana, Solanum, Triticum, Matthiola, Oenothera and Crepis, and may apparently result from the entrance of foreign pollen tubes or from the stimulus of low temperature acting on the egg, or occasionally they may occur 'spontaneously.' Cases of haploid merogony are also recorded in Nicotiana and other genera.

The occurrence of haploid sporophytes having the same general morphology as the diploid shows that one set of chromosomes is sufficient to produce the sporophyte. This appears to be contrary to the former antithetic theory of the sporophyte as a post-sexual phase intercalated between two gametophytes.

### Prof. J. Walton.—*A Fossil Hollow Tree of Lower Carboniferous Age and its Contents.*

In 1865 Wunsch recorded the occurrence of petrified Lepidodendroid trees in beds of volcanic ash on the north-eastern shore of Arran in the Firth of Clyde. Carruthers, Binney, and Williamson have examined and described various plant fossils from these beds. A re-examination of some of the large blocks of these trees at present in the Manchester Museum, by means of *peel-sections*, shows that before fossilisation had occurred the trees had partly decayed, and all that remained was a hollow shell of bark which had become filled with a varied collection of plant fragments from outside.

In two specimens, one at Manchester, the other at Cambridge, there appear to be four or five steles in each hollow trunk. It seems probable that they represent fragments of the single stele which broke into fragments when the soft surrounding tissues decayed, and fell down side by side into the base of the stump. Some of these fragments are penetrated by *Stigmarian rootlets*. Several Stigmarian axes, as well as the roots of other kinds of plants, indicate clearly that there were plants growing at a higher level which sent their roots down into the hollow trunk. Among the more frequent plant remains in the stumps are petioles of a *Lyginorachis* closely allied to *Lyginorachis papilio* Kidston, *Protocalamites, Lepidocarpon,* and several types of Lepidodendroid stems.

Dr. E. H. Moss.—*The Parkland of Alberta.*

The various floral provinces of central and southern Alberta are related to climatic factors and soil types. The Parkland is a broad transition or tension belt occurring between the prairie on the south and the wooded areas to the north and west. It consists, in its typical form, of alternating patches of prairie and aspen vegetation, the prairie areas occurring on dry knolls and south-facing slopes, the poplar groves being confined to depressions and north-facing slopes. Consideration is given to the dominant grasses of the prairie association and to the chief constituents of the aspen association. Of about 100 species of vascular species recorded from aspen groves in the Parkland, only some thirty-five are regarded as constants. Amongst the various factors that have influenced the invasion of trees into the prairie, burning is held to be of greatest importance. With the settlement of large parts of the country in recent years, fires have been of less frequent occurrence, and consequently there has been a marked advance of poplar trees into the northern part of the prairie. Consideration is given to the rôle of *Symphoricarpos* in initiating the advance of the aspen association upon the prairie and in occupying grassland areas partially denuded by ground squirrels and badgers.

Mr. J. Stirling.—*Study of the Morphology of Heterostyly.*

Dr. Macgregor Skene.—Semi-popular Lecture: *Dormancy and Germination.*

## Tuesday, September 9.

Prof. O. V. Darbishire.—*Observations on the Protothallus of the Lichen Pertusaria communis (L.) D.C.*

*Pertusaria communis* is one of our commonest bark-loving crustaceous lichens. The marginal portion or protothallus consists of the primary growing tissues, from which the mature tissues of the metathallus gradually develop. The fungal hyphæ of the protothallus spread well in advance of the gonidial layer. The author describes how new algæ cells are taken in at the protothallus, gradually absorbed by the tissues of the latter and finally incorporated in the gonidial layers which form the metathallus. In the metathallus itself the gonidial layer spreads by vegetative division among the separate gonidia. The views of Frank and Nienburg concerning the way in which the algæ of the gonidial layer spread in the case of allied species, by means of 'travelling algæ' and 'pushing hyphæ' respectively, are discussed and criticised.

Mrs. N. L. Alcock.—*A Phytophthora on Strawberries causing a Root-rot.*

A disease of strawberries has been prevalent and severe in certain parts of Scotland. The root system is affected with a rot that ultimately leads to the death of the plant. A Phytophthora has been found intimately and continuously associated with this disease, and that this is the cause of the trouble is considered probable.

The Phytophthora has sexual organs, both amphigynous and paragynous. The disease has been found to be contracted from the soil, and the oospores probably hibernate and germinate there, but further experiments are in progress to determine this and other points.

Dr. W. R. Ivimey Cook.—*Cystochytrium radicale, a new Species of the Protista in the Roots of Veronica Beccabunga.*

The organism enters the roots as a small uniflagellate zoospore which bores its way into the cells and there becomes spherical. It invests itself in a thick cyst which appears to be composed of chitin. The nucleus divides, and the organism grows into an elongated body still surrounded by the cyst. The cytoplasm becomes vacuolated and the greater part of the centre is filled by a vacuole. The nuclei are arranged around the periphery embedded in cytoplasm; several hundred may be present. The external shape of the fungus varies very much, sometimes being irregularly curved or half-moon shaped; occasionally it gives the appearance of being branched.

Nuclear division is mitotic, all the nuclei within the same individual dividing simultaneously. The nucleus of the host cell is found associated with the parasite, and generally appears unhealthy. When mature the whole of the soma divides up into a large number of uninucleated zoospores which escape by the bursting of the cyst wall. They are able to penetrate the wall of the host cell, and then either make their way out of the root or enter another cell, where they pass through the same cycle.

No apparent pathological effect is produced in the host, and there are no external signs to indicate that a plant is attacked by the fungus.

The parasite is considered to belong to the Chytridiales, and is probably allied to *Catenaria* or *Hyphochytrium*.

### Prof. Dame HELEN GWYNNE-VAUGHAN, G.B.E., and Mrs. H. S. WILLIAMSON.—*A Reinvestigation of the Life-history of Pyronema confluens.*

*Pyronema confluens* fruits readily in single spore culture, the fertile hyphæ branch dichotomously, antheridia pushing in among the paired oogonia. The young trichogyne is cut off by a wall; this opens for a brief period, allowing the male nuclei to enter the oogonium, and is then closed. As first observed by Harper, the sexual nuclei fuse in pairs in the oogonium. The young ascogenous hyphæ lack septa; below the asci, after divisions have occurred, many of the cells contain two nuclei. The divisions in the sheath and paraphyses show six chromosomes, those in the ascogenous hyphæ twelve. As recorded by Claussen, the first divisions in the ascus are meiotic, showing twelve gemini. In the third telophase six chromosomes only are seen.

### Dr. B. BARNES.—*On Variations in Fungi induced by heating the Spores.*

Spores of *Eurotium herbariorum*, *Botrytis cinerea* and *Thamnidium elegans*, taken from strains known to be constant under ordinary conditions of culture, have yielded variant forms after exposure for a short time to high temperatures. The more extreme variants appear to be permanent; less marked variants have tended to revert to the normal form.

The behaviour of the permanent variants suggests that they are less vigorous than the normal strains. Some are of weak growth, some stale with great readiness, and some form scanty crops of spores, or fail to develop all the stages shown by control cultures.

Comparable results have been obtained by other workers by the application of heat to higher plants and to insects.

### Miss H. HESLOP HARRISON.—*The Cytology of the Genus Euphorbia.*

The root-tips of seventeen species of Euphorbia have been examined. These species may be arranged in two series according to their chromosome number. In the first are four species, three having a diploid chromosome number of 20 and one of 40. In the second are thirteen species, three having 12 somatic chromosomes, six having 18, one having 30, two having 36 and one having 42. Those species with 18, 30, 36 and 42 chromosomes possibly form a true polyploid series based on three.

In two species, *E. terracina* and *E. Welwitschii* tetraploid tissue has been found. In the latter case this has arisen by the fusion of the two nuclei in binucleate cells.

Chromosome shape has been found to vary considerably from tissue to tissue. This is very clearly shown in *E. verrucosa* and *E. terracina*, where the plerome chromosomes are so contracted as to become almost unrecognisable.

Distinct evidences of chromosome fragmentation have been found in two species—*E. capitata* and *E. caput-Medusæ*.

In several species the chromosomes on the metaphase plate are arranged in pairs, the members of each pair being obviously corresponding maternal and paternal chromosomes. In many cases distinct chromatin connections are seen between them.

AFTERNOON.

Excursion to Potterne.

Excursion to Portishead Marshes.

# DEPARTMENT OF FORESTRY (K*).

## Thursday, September 4.

Remarks by Sir JOHN STIRLING-MAXWELL, Bt.

Mr. D. W. YOUNG.—*Cultivation of Hardwoods.*

Mr. A. HOWARD.—*Our British Grown Hardwood Trees and Timbers.*

Prof. D. H. CAMPBELL.—*The Preservation of the Red Wood Forests in the Western States.*

AFTERNOON.

Mr. J. MACDONALD.—*The Measurement of Standing Trees.*

The necessity for a satisfactory method of measuring standing trees has become clamant in sample plot work in Great Britain, as it is no longer possible in many of the plots to fell sample trees for the purposes of measurement. Apart from scientific work, a good method of measuring standing trees is necessary for ordinary commercial work. The following methods are possible :—

A. *By ocular estimation.*

This is the method most widely used in practice, and in the hands of expert practitioners is capable of considerable accuracy.

B. *By direct measurement.*

This involves climbing the tree and measuring the actual heights and girths. It is possible in only a small number of cases, and need not be seriously considered.

C. *By indirect measurement.*

i. By measurement with dendrometer.

This method can be quite accurate, but is inconvenient in practice, *e.g.* in dark woods, in windy weather, &c. Not generally suitable.

ii. By volume tables. iii. By form factor tables.

These methods, the former commonly used in the United States and the latter in Germany and other continental countries, may be used to estimate the volume of an individual standing tree, but are properly applicable only to whole woods. There is thus the possibility of serious error in applying the tables to individual trees.

The commonest and most accurate type of volume table gives volumes for trees of different heights and diameters. They assume that all trees with the same diameter (or girth) at breast height and the same height have the same volume. This is not so, as the third factor on which volume depends, namely taper or form, is neglected, and it is this omission that makes the tables inapplicable to individual trees.

iv. *By the form-point method and form-class volume tables.*

Recent work on mensuration has been concerned largely with the idea of form and form-class, and in most of the work trees with the same rates of taper have been grouped together into 'form-classes.' Trees in the same form-class, with the same rate of taper, the same breast-height diameter (or girth) and the same total height are certain to have very similar volumes ; for this reason volume tables based on form-class can be applied with some confidence to individual trees. The one difficulty with standing trees is in determining the form-class. In Sweden Tor Jonson discovered empirically a relation between the form-class and the 'form-point,' which is situated at the centre of gravity of the crown. This form-point can be determined with practice quite easily, and when it is known the form-class also is known.

This method appears to give satisfactory results in the open woods of Scandinavia, but it can rarely be applied in our dense plantations. Some other method of obtaining the form-class is therefore necessary.

v. *By partial measurement and the use of stem curve.*

In the method now being tested it is proposed to measure actual girths on the stem as far up as possible, and for the remainder of the tree to obtain the necessary girths by interpolation on the appropriate form-class curve. The measurements on the lower part of the stem are generally sufficient to give the form-class when plotted as percentages of the breast height girth, and the remainder of the curve may be fairly safely taken as representing the shape of the stem above the last possible point of measurement. The greater part of the volume is in the lowest length of the stem, and this is generally measured accurately.

Dr. R. N. Chrystal and Mr. E. R. Skinner.—*The Biology of Xylonomus brachylabris (Hymenoptera, Ichneumonidæ), a Parasite of the Larch Longicorn Beetle, Tetropium gabrieli, Weise, and some Studies of the Willow Woodwasp, Xiphydria prolongata Geoffr. (Hymenoptera, Siricidæ) and its Parasite Thalessa curvipes Grav. (Hymenoptera, Ichneumonidæ).*

The larch longicorn beetle (*Tetropium gabrieli*, Weise) in Britain, its relation to the larch woods and distribution throughout the country. The parasitic insect enemies of this beetle have never been studied in this country, and it is only very recently that a continental worker, Schimitchek, has published a paper upon them. The present paper gives the results of some biological studies on *Xylonomus brachylabris* carried out at the Imperial Forestry Institute, Oxford, and deals with the pairing and egg-laying habits of the parasite, the attack on the host larva, the cocoon and emergence of the adult. The occurrence of another species of *Xylonomus*, *X. irrigator*, as a parasite of *T. gabrieli*, is also described.

The studies of the Willow woodwasp, *Xiphydria prolongata*, Geoff., and its parasite, *Thalessa curvipes*, Grav., were commenced about two years ago while some work on the Sirex woodwasps and their parasites was in progress. Up to date, the best account extant of the biology of the Willow woodwasps is that by Leisewitz in 1907. Since then nothing further of any note has appeared dealing with their biology. This paper contains a general account of their biology in Britain and some of the observations, especially those referring to oviposition and the egg-stage, are believed to be new. The discovery in 1929 of the *Thalessa* parasite was the starting point of a further series of observations on its life-history and habits, this parasite being especially interesting in view of the fact that it is closely allied to *R. persuasoria* L., the large Ichneumonid parasite of the Sirex woodwasps.

## Friday, September 5.

**Discussion** on *Mineral Elements in Plant Nutrition* (see sectional programme preceding).

Afternoon.

Excursion to Long Ashton Research Station.

## Saturday, September 6.

Excursion to Forest of Dean.

## Monday, September 8.

Mr. W. H. Guillebaud.—*Experimental Studies on the Artificial Regeneration of Oak.*

An account of experimental investigations dealing with the nursery practice, direct sowing and planting of oak. The work was started in 1927, and the paper is in the main a report of progress. Over seventy series of field plots have been established in eleven centres in the south of England. In each experiment the treatments are replicated and the plots scattered.

The chief difficulty experienced in attempting to establish oak by artificial means has been the slow initial growth of the oak plants and their consequent prolonged struggle with the heavy weed growth characteristic of the soils on which oak is usually grown. This slow initial growth may be due to the combination of many factors, of which the following are being investigated :—

1. *Frost.*
2. *Oak Mildew.*
3. *Effect of root competition of weed growth.*
4. *Lack of available mineral food salts in the soil.*
5. *Insufficient competition between the oak plants.*
6. *Race and type of plant.*

In addition to the above studies work is in progress in connection with :—

Nursery practice of raising oak, control of oak mildew, manuring of seedbeds, grading, &c.

Direct sowing, including methods of soil preparation, date of sowing, density of sowing and depth of sowing.

Degree of weeding necessary in different herbage types, and

Methods of mixing larch with oak.

The provisional results obtained to date are discussed and lines for future work indicated.

## Mr. W. L. Taylor.—*The Afforestable Lands of Great Britain.*

For the purpose of definition, the term afforestable land has in this paper been made to include those types of land which can suitably be devoted to the production of timber, having regard to natural conditions and to the particular fact that, in so populous a country, the forester cannot hope to obtain the permanent use of lands capable of responding profitably to husbandry and of producing the relatively more valuable food crops. The limitations imposed by natural conditions are, in the main, those of meteorology, topography, geology and the prevailing vegetation, and the inter-relations of the natural factors, one with another, render the character of lands available for afforestation so varied as to make it impossible to deal with more than principal types in any discussion of moderate length.

The afforestable land of Britain lies chiefly in the west and north, overlying the harder and less readily weathering rocks. The present utilisation is generally as poor mountain pasture, grouse moor or deer forest. The most productive type is the bracken or dry, grass-covered slopes, of moderate elevation and exposure, giving indication of a deep soil, good aeration and good natural drainage. Other types are the heather moors, with or without grasses and rushes ; wet, rushy ground and 'white moors' clothed for the most part with *Molinia cœrulea* and other grasses. *Scirpus* and cotton grass moors are wet and difficult types. In all cases soil quality-class is dependent upon the degree of elevation and exposure ; the quantity of moisture ; the presence or absence of podsol conditions and pan and the porosity and composition of the soil itself. Peat deposits, varying in character and depth, occur throughout the parts of the country which are also the areas of high rainfall, and present many problems, especially where the locality is one of indeterminate drainage or, as is more often the case, is associated with an unfavourable vegetation and impermeable subsoil. Faith is, however, becoming stronger where the *flora* includes *Molinia* and rushes, for plantations of the spruces and certain others of the conifers can then, with the aid of drainage, successfully be established on the deeper peats. In the mountainous districts soils are frequently peaty, wet and sour, and, together with the morainic deposits, sometimes concreted and always leached, in the depressions, form perhaps the most difficult lands to assess and afforest.

Elsewhere in Britain are to be found the considerable areas of scrub and devastated woodlands ; the sandy heaths of East Anglia and the south of England ; the poorer sands of the Trias in the counties of Nottingham, Stafford and Cheshire ; the chalk downs of Hampshire, Wiltshire and Dorset, and the very similar soils of the Chiltern Hills and their outliers and, lastly, the littoral dunes, examples of which are being afforested on the Moray and Carmarthen coasts.

## Dr. J. Burtt Davy.—*A Preliminary Report on a recent Investigation of the Forest Floras of Northern Rhodesia, Nyasaland, Pemba and Zanzibar.*

After participating in the 1929 meeting of the British Association at Cape Town and Johannesburg, the writer availed himself of the opportunity to carry out a long-projected investigation of the forest flora of Northern Rhodesia and Nyasaland, the mangrove swamps of East Africa and the woody plants of the islands of Zanzibar and Pemba.

The tour covered a period of twenty weeks from the date of leaving England, and involved considerable travelling interspersed with the collecting of specimens, during the whole period from the date of arrival at Cape Town on July 19. Leaving the British Association party at Victoria Falls, a visit was paid to the Siburu Forest in the Zambesi basin, some 70 miles west of Livingstone, to see the Rhodesian teak, *Baikiœa plurijuga*, in its natural habitat on the Kalahari-sand formation. From Livingstone the 'Great North Road' to Lusaka was followed, thence to the east to Fort Jameson on the Nyasaland border over the 'Great East Road,' which had been opened for traffic only the previous year and was in a bad condition as regards bridges and ponts. From Fort Jameson, South and Central Nyasaland was explored by traverses in different directions, taking in a number of the high mountain peaks in order to investigate the remnants of rain-forest still to be found there. The Northern Rhodesian tour occupied three weeks, and the whole of September was devoted to travelling and collecting in Nyasaland. Owing to the limited time available and the infrequency of the sailings of the lake steamers, it was found impossible to visit the extreme north-western end of the Protectorate, for which purpose the Association generously made a grant in aid. In consequence, the grant has not been claimed.

Two weeks were spent in Zanzibar and Pemba Islands, where the principal points of botanical interest were visited and valuable collections were made, thanks to the admirable arrangements made by H. E. the British Resident and various officials.

On my homeward voyage more collections were made at various ports of call, no less than forty numbers being found at Aden, owing to the recent rains.

Two thousand eight hundred numbers were collected in all, of which very few have been worked out as yet, but these include several undescribed species. Among the latter is a new species of *Pterocarpus*, *P. stevensonii*, from the Siburu teak forest, which furnishes a useful timber ; this is named after Mr. D. Stevenson, the officer in charge of the Forestry Service of Northern Rhodesia, who gave the information as to its economic value. The tree was not in flower at the time of our visit, but Mr. Stevenson has obtained flowers and fruits. Particular attention was given to the Brachystegias which furnish the dominant species over a large extent of the savannah-forest country ; as a result, the number of species known to occur in Nyasaland has been increased from ten to twenty-four species and a new variety.

The vegetation of Northern Rhodesia, as far as this and a previous journey have enabled me to ascertain, falls within the type known as the deciduous savannah-forest, though varying greatly in density of growth and usually with open canopy. Remnants of fringing forest occur along the larger perennial streams. The difference in density and size of the component trees appears to be correlated with the amount of available moisture due to the difference in amount of precipitation and in the depth and character of the soil and of the soil drainage. The whole of the 700 miles crossed in Northern Rhodesia is an undulating plateau of about 4,000 feet elevation, here and there intersected by large tributaries of the Zambesi, the Kafue, Lunsemfwa and Luangwa. There are no very marked differences in altitude after reaching the summit above the Zambesi valley ; consequently the vegetation and flora are more uniform in character than in regions with a more diversified topography.

It had been supposed that the frontier between Northern Rhodesia and Nyasaland might have provided a phyto-geographical boundary. This is not the case in the vicinity of Fort Jameson, and one meets the same type of vegetation when traversing the West Nyasa Province of Nyasaland, with no marked differences in component species. On the eastern slopes of the escarpment overlooking the Lake, however, are to be found remnants of true rain-forest, and a similar type of vegetation is found also on the higher peaks met with in the southern part of the Protectorate. At the lower levels a more definitely tropical type of savannah-forest occurs. Useful information has been obtained on the geographical distribution and ecology of the components of the flora, which are discussed in the paper.

### Afternoon.

Excursion to Tortworth.

## Tuesday, September 9.

Mr. W. R. Day.—*The Relation of Frost Damage to Larch Canker.*

The general view with regard to the development of larch canker is that the fungus *Dasyscypha calycina* (Schum) Fuckel is the cause of the canker formation, and that the cambium is killed by the fungus during the winter. The evidence for the latter point is that no new wood is ever formed at the time the cambium dies. In this paper it is endeavoured to demonstrate (*a*) that the cambium not infrequently dies in the spring, and (*b*) that the cause of death is frost, in many cases at least. Cankers of typical form may be caused by frost, and anatomical evidence is brought forward to demonstrate this. The larch canker fungus is always present in these cankers, where it lives apparently as a saprophyte. This work is preliminary to further study, and no claim is made here that *D. calycina* is not in many cases the cause of canker development.

Dr. M. C. Rayner.—*Observations on the Behaviour of Armillaria mellea in pure Culture with certain Conifers.*

There are a number of puzzling features in the reactions shown by Conifers to attack by *Armillaria mellea.*

In this paper an account is given of observations on pure cultures of seedlings of two conifers, *Corsican Pine* and *Douglas Fir*, exposed to infection by actively growing rhizomorphs of *Armillaria mellea.* The facts described will be illustrated by lantern slides of photographs at various stages of growth.

This common tree-attacking fungus has features of special interest to students of mycorrhiza and the evolution of the mycorrhizal habit. It belongs to a group of fungi that include most of the known or suspected mycorrhiza-formers of trees; moreover, in the case of at least one herbaceous host species, it is recorded as forming an association of balanced parasitism sometimes described as *symbiosis.*

Certain conclusions deduced from the experiments are offered as a basis for discussion.

Mr. K. St. G. Cartwright and Mr. W. P. K. Findlay.—*The Diagnosis of Decay in Timber.*

After a brief survey of the rough methods used by foresters, timbermen and others for determining the presence of fungi causing decay in timber, this paper describes methods which have been developed in order to recognise the presence of a fungus in the very early stages of an attack and the means whereby the identity of the causal fungus may be established. Many different species of fungi cause much the same general effect on any one species of timber, and frequently the microscopic examination of the decayed wood gives but little further information. It has been found that most of the wood-destroying fungi when grown in pure culture on artificial media show differences whereby they may be recognised, especially if a standard culture obtained from a named sporophore is available for comparison. Cultures of all the more important wood-destroying fungi have been collected by the authors, working for the Forest Products Research Laboratory, Princes Risborough, and these are now being classified and described.

## Wednesday, September 10.

Mr. R. A. G. Knight.—*The Moisture Content of Wood in Relation to Hygrometric Conditions.*

Timber in general use assumes a moisture content which is dependent on surrounding hygrometric conditions, and will vary as the state of the atmosphere changes.

After a preliminary discussion, moisture content—temperature humidity equilibrium curves, determined by experiment at the Forest Products Research Laboratory, are presented.

The practical importance of the work is indicated by an outline description of an experiment to ascertain the most suitable moisture contents of woods for furniture manufacture, and by an account of the observed effects of climatic conditions on timber in the process of air seasoning.

Mr. S. H. Clarke.—*The Tertiary Wall of Wood Fibres.*

Existing accounts of the gelatinous tertiary wall show that while it may be present in the fibres of root, stem and branch of practically any dicotyledonous tree species, it is not necessarily present in every annual ring, nor in all parts of the same ring in any one tree. It has been suggested that the tertiary wall represents a form of arrested growth. The findings of a study of the chief species of home-grown elm are in agreement with these accounts. Apparently, while contributing to the density of the wood, the tertiary wall does not produce a proportional increase in the mechanical strength. It is often stated that woods with gelatinous fibre walls shrink less during drying than woods from which they are absent. A comparison of samples of elm whose fibres had varying proportions of tertiary walls lent no support to this view.

Mr. J. Bryan.—*Antiseptic Treatment of some Home-grown Conifers.*

The paper gives a summary of the theories of impregnation of wood with preservatives under pressure and also discusses the variables affecting impregnation. The adaptability of different species of home-grown conifers to antiseptic treatment by different methods is also discussed.

## SECTION L.—EDUCATIONAL SCIENCE.

### Thursday, September 4.

The Pre-School Child :—

(*a*) Miss Margaret Drummond.—*Education.*

The term pre-school is intelligible only with reference to countries which have instituted compulsory education. The age at which children must begin to attend school is in our own country five, in New Zealand seven, in Egypt six, in China six, in Japan six (except for invalids and the children of paupers), in Afghanistan six, in Mexico six, in the United States six, seven, or eight—nine in the case of one State.

The idea of the lower age limit for compulsory education is that the child in its very early years is not ready for education. In the narrow sense of the word this is probably true ; in the wider sense it is obviously false.

It is, however, true that a special type of education has to be provided.

The considerations which determine that such education should be provided may be (*a*) social or economic, (*b*) educational, (*c*) scientific. These three sets of considerations may act independently, but they tend to co-operate.

The three great pioneers of pre-school education—Comenius, Pestalozzi, Froebel—were actuated mainly by social or economic and educational interest. Scientific interest, as an incentive for the establishment of pre-school education, is a very modern development. Dr. Montessori represents in a remarkable way all three classes of interest. At present the United States and Canada show the scientific interest in its purest form.

Nursery Schools.—The term nursery school was invented by the Consultative Committee to the Board of Education, which in 1907 was asked to consider what public provision should be made for the education of children under five years of age. At that time about a third of the children of England between the ages of three and five were enrolled in the public elementary schools. The Committee considered that in favourable conditions the best place for such young children was the home ; when conditions were unfavourable they advocated a special type of institution to which they gave the name nursery school.

The recommendations of the Committee combined with other factors, social and economic, to bring about the exclusion of the young children from the schools, so that in 1916, when Sir George Newman began his campaign in their favour, only about an eighth of the total were in regular attendance in the schools.

The Education Act of 1918 empowered Local Education Authorities to establish nursery schools where necessary for the mental or physical welfare of children under five.

So slow was progress, however, that in 1922 the Nursery School Association was founded to further the interests of these children.

Private experiment, meanwhile, influenced largely by the Kindergarten movement, on the one hand, and, on the other, by Robert Owen's great social work at New Lanark, had done much to formulate aims and ideals and to create an educational method. In recent years the work of Maria Montessori and of Margaret MacMillan has revitalised the movement and stirred public imagination in its favour. The discoveries of the psychologists showing the enormous importance of the experiences of the very early years in the development of personality traits added scientific weight to the forces already active. Political parties made the nursery school part of their educational programme. Local Authorities, urged by public interest and support, began to stir in the matter, and to-day the time seems ripe for a change in the foundations of our educational practice which will have a far-reaching influence throughout our whole social structure.

(*b*) Dr. J. A. Hadfield.—*Mental Health.*

Psychopathologists agree that abnormalities such as delinquencies, and psychoneuroses (obsessions, hysteria and sex perversions) have their roots in the first years of life. Illustration.—By discovering their origins, possibility not only of curing them, but finding means of prevention.

Mental health depends on full and harmonious development of innate tendencies. Necessity of (*a*) opportunity for expression, (*b*) ideal by which to control them, and towards which to guide them.

Relation of freedom and discipline from point of view of procuring mental health. Basic causes of abnormalities.

(*c*) Dr. W. E. Blatz.—*Motivation of Child Behaviour.*

(*d*) Discussion.

**Presidential Address** by the Rt. Hon. Lord Eustace Percy, P.C., on *A Policy of Higher Education.* (See p. 219.)

Afternoon.

Visit to the New Hospital School for Cripples, Winford.

## Friday, September 5.

The Curricula of Central Modern and Senior Schools :—

(*a*) Mr. W. A. Brockington, C.B.E.—*A General Survey.*

The paper deals generally with the historical development of post-primary education on the basis of the age-break at 11+, and the reasons for adopting a new type of organisation which offers an alternative form of secondary education with a minimum leaving age of 15.

(*b*) Mr. J. A. White.—*The Selective Central School.*

With only twenty years' experience behind it, and without the prestige of the ordinary secondary school on the one hand, and the strong vocational attraction of the technical and trade schools on the other, the central school has rapidly increased in popularity and has succeeded in winning public support from both employers and parents. Its success has indicated the need for more varied and more flexible types of education of a secondary character than were in existence at the time of their establishment, and, in urban areas with a large school population, the necessity for providing as economically as possible far greater facilities, at any rate for the children of special aptitudes, than could be supplied in the elementary school. Indirectly, this gave promise of benefiting also the schools from which the pupils were selected, for it relieved large classes and, by reducing the range of abilities left in the school, limited the problem of the teachers.

In the selective central school the average of intelligence will usually be at least as high as that in the ordinary secondary school. The curriculum, therefore, will, in its fundamental subjects, especially in the first two years of the course, be very similar to the usual secondary school curriculum with perhaps a little less emphasis on the text-book work. It is in the third year when the central school problems begin to take definite shape and call for intelligent and discriminating solution. From that time onwards the work of groups of pupils and sometimes even of individuals will be determined not so much by stereotyped theories of what is, or what is not, a good general education, but rather by a combination of forces, viz. the observed abilities of the child combined with (1) the opportunities which the area affords for their profitable development, and (2) the wishes and the possibilities which parents and home circumstances respectively display.

### (c) Mr. H. T. Morgan.—*The Non-selective School and its Curriculum.*

The results of selection by examination; type of pupil, definitely non-intellectual. Physical education and its importance for adolescents. Games and outdoor occupations. The value of arts and crafts. Balance necessary between speech or language on the one hand, and handicrafts on the other. Importance of practical work. Courses of study. The great need of scientific determination. The time factor: need for consolidation of subjects. How far can curriculum be individualised? How much knowledge is really important for every child? What choice can be given and how can it be made? Training and drill in 'studying.' The discovery of aptitudes or the creation of appetites. The conversion of pupils into students. Great importance of the cultivation of thinking. Social training most important of all but not primarily a matter of curriculum. Individual development through individual study; social consciousness through group activities. Breadth and elasticity in the curriculum essential. Need for freedom and experiment and the danger of standardisation. The menace of examination and certification.

### (d) Miss V. E. Carr Gordon.—*Factors governing the Scope of the Curricula in the Modern Girls' School.*

The trend of the curricula in any school is governed by at least four important factors. It depends, first of all, on the ideals and beliefs of the person who frames it, secondly on the type of child to be catered for and the locality in which the school is situated, thirdly on the building and equipment available, and, fourthly, on the number and personnel of the staff. The writer of this paper believes that the first aim of the educator should be to promote enthusiasm and zeal for work. The child must *desire* to achieve. She can then be taught to employ adequately the mechanism of thinking and so discriminate between relative values. These two processes are often simultaneous, but the first is none the less the foundation of the second.

In order to summarise briefly the next three points above, it may be useful to describe a modern non-selective girls' school. The locality is a mining town in Northumberland where children grow up with peculiar limitations of experience. The building has been designed to meet the requirements of a liberal primary education, and includes two domestic science rooms, a handwork room and a needlework room. There are thirteen staff specialising in their own subjects. Pupils enter the school at the age of eleven and are divided into three grades according to ability, the aim of the curricula being to give scope for the development of the potentialities of every type of child.

The majority of the pupils respond most readily to instruction in practical subjects, and the school might be said to have a domestic science bias, but the teaching is in no way vocational. The natural instincts of girlhood gravitate towards the domestic arts; the special need of the locality is for good home-making, and many of the pupils become servants when they leave school. Nevertheless, the first aim of the workroom is to *educate* the child *through* the craft.

Each modern school must develop according to its individual conditions, and the more experimental it is the greater will be its success. Formal and traditional methods should not hamper and restrict the functioning of the time-table. The woman of to-day must be infinitely adaptable. Not only has she to be supremely competent in the home, but she has also to take an increasingly responsible part in the work of the State. She must therefore learn to attack her duties with enthusiasm, and her judgment must be clear and unprejudiced. If the school of the future can develop the characters and minds of the pupils so that they acquire a feeling of conscious control over their destinies, so that they *use* life and are not merely used *by* it, if it teaches them to appreciate to the full opportunities for self-development and social service, to discriminate between the significant and the insignificant, it will become the most potent factor in the creation of a vigorous national life.

(*e*) Discussion (Mr. A. H. RUSSELL, Dr. M. BOEHMKE).

Report of Committee on *The Production and Distribution of Educational and Documentary Films.* (Sir RICHARD GREGORY.)

Report of Committee on *Educational Training of Boys and Girls in Secondary Schools for Overseas Life.* (Mr. C. E. BROWNE.)

Report of Committee on *The Teaching of General Science in Schools with special reference to the Teaching of Biology.* (Sir PERCY NUNN.)

AFTERNOON.

Visit to Local Schools.

## Saturday, September 6.

Excursion to Dauntsey School, West Lavington and Devizes Castle and Museum.

## Monday, September 8.

Report of Committee on *Formal Training.* (Dr. C. W. KIMMINS.)

DISCIPLINARY VALUES IN EDUCATION :—

(*a*) Sir PERCY NUNN.—*The Conception of Mental Discipline.*

(*b*) Miss H. M. WODEHOUSE.—*The Discernment of Disciplinary Values apart from Experiment.*

Our final answers to questions, in the science of education as in other sciences, must rest partly on careful experiment and partly on the collection of statistics on a very large scale. But these 'objective' methods are beset with difficulties. Can we supplement them ? e.g. as regards disciplinary value, can we obtain suggestions and some amount of evidence in any less cumbrous way, that will be useful as far as it goes ? The object of this paper is to defend a 'subjective' method, of individual observation and reflection on experience, chiefly the person's own experience in a not too distant past.

This method is obviously fallible, yet it may serve as check upon the principal enemy : that custom of dogmatism which is far more fallible than any observation and reflection can be. 'The study of this school subject has such and such effects.' 'Did it have these effects on you ? If so, give details. If not, why not ?' The answers are often valuable in themselves, and also prepare usefully for objective investigation.

Such recollection, and the careful imagination which it guides, are needed for examining notable dogmas, such as Bacon's on the training of attention by mathematics. Strong points of the method are that it brings out (1) the vast differences in the effect of a subject when it is handled by different teachers ; (2) the crucial differences made (*a*) by the pupil's success or failure in the study, (*b*) by the presence or absence of his goodwill.

The subjective method clearly can be used to good purpose only where the disciplinary effect has taken place within consciousness. This does not imply full consciousness of details, nor does it necessarily imply such experience as can be rendered into words. But it does imply some effect on *attitude* or effect on *idea*. If something other than these takes place outside consciousness—if the growth of some capacity is stimulated or some 'faculty' directly strengthened—this method cannot report upon it. Whether such other effects do take place, and what objective methods can be devised to detect or disprove them, are most important questions outside the scope of this paper.

(*c*) Prof. F. A. Cavanagh.—*Some Further Practical Considerations.*

(*d*) Discussion (opened by Prof. C. W. Valentine, Mr. A. E. Lynam).

## Tuesday, September 9.

### English and Foreign Ideas on Method of Education in relation to Industry and Commerce :—

(*a*) Mr. W. Henderson Pringle.—*Higher Commercial Education—Some Developments and Obstacles.*

The present position of English higher commercial education outside of the universities. Co-ordinated effort required. Difficulties in the way—particularly the chaotic condition of local administrative areas. A suggested solution. The governing bodies of the larger colleges.

(*b*) Sir Francis W. Goodenough, C.B.E.—*A Business Man's View of Education for Commerce.*

The importance of science in commerce and industry—the need of scientific methods of market research and marketing—modern conditions demand employment of skilled personnel not only in production but in management and marketing—amateur minds and methods incapable of contending with trained foreign competitors—facts pointed out 20 years ago by Sir Robert Blair before the British Association—great advance since in technical education (for Production): very little progress in commercial education (for Marketing)—Business men not asking for early specialisation, but for suitable general education as prelude to special education and training—Ideals in business—Commerce a worthy profession—Importance of character and ability as well as knowledge—Every profession needs specialised as well as fundamental education—The training required for Commerce—The need for close co-operation between business men and educationists—Hopeful outlook.

(*c*) Dr. W. A. Richardson.—*The Technical College and Continued Education in Relation to Industry.*

In mediæval times the master craftsmen provided not only the practical training and theoretical knowledge needed by the apprentices, but strove also to give an education in citizenship. Whilst industry still gives and must give the greater part of the practical training necessary to the man in industry, the university, technical college and continuation school in their different forms provide the technical knowledge. In all countries the education given depends upon the type of industrial work. Members of the administrative, designing and scientific staff are in this country drawn partly from the universities, but abroad exclusively from highly specialised technical colleges, where full-time studies extending over a number of years are taken. In these the contact with industry is provided mainly by professors and lecturers who, by consultation or otherwise, have some share in industrial life. In England, however, men to fill the higher posts are also largely drawn from workers who have been ambitious enough to train in evening classes of technical colleges, or have been able to profit by special arrangements in industry which allow selected apprentices to obtain education in working hours. Such arrangements are unknown abroad. Consequently, the English technical colleges differ from such institutions elsewhere. They provide large numbers and a great variety of part-time courses for those already engaged in industrial pursuits, whilst those able to follow full-time courses gravitate rather to the universities than to the technical colleges. The career of youth in England is thus much less predetermined than elsewhere; the education less stereotyped and more adaptable to local needs, and gives opportunity to the able and ambitious in spite perhaps of early educational disabilities to rise to the top. The contact with industry is, moreover, direct and complete. Manufacturers and industrialists take direct interest by acting on governing and advisory boards in technical education; members of the teaching staff, owing to part-time arrangements, are men from industry; and the student, himself an apprentice, brings something of the factory with him. In England there is no compulsory continuation system as abroad, where every worker has to attend evening classes of general or vocational character. In England, therefore, the bulk of factory workers receive little or no further education, technical or otherwise, but abroad it is more and more the tendency to set up compulsory continuation courses covering all engaged in industry. So far as adult education is concerned, with perhaps the exception of Sweden and Denmark, more facilities for workers are now provided in England than elsewhere, not only through voluntary organisations, but by the direct efforts and co-ordinating influence of the delegacies for extra-mural education now operating in all universities.

### (*d*) Miss E. Webb Samuel.—*Industry and the Young Person.*

The 'young person' defined. The young person in different social strata. Great variety of educational opportunity according to social background. Comparatively poor educational opportunity of the vast majority. The young person at the end of his school career. His attitude toward 'going to work.' The attitude of his parents toward the same problem. Extent to which the attitude in both cases is influenced by family and social conditions. Results of an investigation into same in schools in both urban and rural areas.

What industry expects from its 'young person' recruits. The function of the schools in 'delivering the goods,' *i.e.* in producing the right kind of young person. This function as non-vocational training. The success of the school can only be complete within definite limits because of the age at which the 'young person' leaves it. It does not send the finished product nor the skilled worker into industry but the young person of infinite potentialities. The duty of industry towards the young person because it benefits by youthful and therefore inevitably unskilled labour of immature people. How far this duty has been realised in industry.

The young person is not the finished product as far as life is concerned any more than in relation to industry. The duty of the State towards the young person in the provision of further education. Different attitude of Britain and America towards the employment of juvenile labour and its effect on educational systems of both countries. The attitude of the young person in both countries to educational facilities. Part-time education of the young person in both countries. Probable effect of development of mechanical power on the employment of juvenile labour, and the resulting opportunities for development of educational facilities for the young person in industry.

### (*e*) Mr. A. Abbott, C.B.E.—*Education for Industry and Commerce.*

Any comparison of English and foreign systems of technical education must take account of the respective stages of development of—

(*a*) the industrial and commercial organisation of each country, and

(*b*) its general system of education.

The great industrial and commercial development of England took place long before she had a comprehensive and highly organised system of secondary education; indeed, she has only just completed the establishment of this system. Germany and other Continental countries did not begin to develop their industries on a large scale until after their systems of secondary education had been well established for many years.

Each English technical or commercial college receives, therefore, two streams of students—a broad stream, whose full-time schooling has finished at the age of 14, and a narrower stream, whose education has been prolonged to the age of 16 or 18; it is, in fact, an undifferentiated institution. Similarly, there are two streams of recruits to the executive and responsible posts in English industry and commerce, since men of both types of preliminary education find their way to the very highest positions.

In Continental countries it is more frequent for technical and commercial schools to be differentiated on the basis of the educational antecedents of their students. Thus, the technical and commercial high schools of the Continent receive only students from secondary schools who are prepared to undertake advanced work; students from elementary schools usually go into schools of lower type which often prepare them for entry into specified occupations rather than for wide careers.

It may be that the recent development of secondary education in this country will have the effect of causing our technical and commercial schools to follow the example of the Continent, and that differentiation will become more common. We have already gone some distance in this direction by establishing junior technical and trade schools for both boys and girls. In any case, it is likely to have a profound effect on the staffing of industry and commerce, since the more responsible posts will inevitably be filled to a greater extent by persons who have had greater educational advantages; in other words, the vertical mobility of labour in England will be diminished.

During the last twenty-five years the number of pupils in grant-aided secondary schools in England and Wales has been increased fourfold. During the same period industry and commerce in England have also undergone great changes; but the modifications in the structure of education and in the organisation and conduct of industry and commerce went on side by side for many years without any definite contact between representatives of each of them. Co-operation between industry and education is now, however, becoming more frequent and more systematic. It may be (*a*) local, (*b*) regional, or (*c*) national. The regional form of co-operation is new, and exists as yet only in the geographical county of Yorkshire.

During the last two years inquiries into (*a*) education for engineering, and (*b*) education for 'Salesmanship' (*i.e.* for marketing goods and services at home and overseas) have been carried on by officers of the Board of Education acting under the guidance of suitable committees. It is hoped that the results of these inquiries will lead to greater co-operation.

Looking at education for industry and commerce as a whole, it is clear that real progress has been made during the last few years. The most significant change is in the public attitude towards the student. While a few years ago the student was regarded as an individualist making laudable attempts to improve his qualifications, he is coming to be regarded as a man who is playing his part in a great corporate attempt to restore and increase the national welfare.

(*f*) Discussion (Mr. J. L. Holland).

B.B.C. Exhibit and Demonstration.

Visit to Stoke Park Colony (see Section J).

## SECTION M.—AGRICULTURE.

### Thursday, September 4.

**Discussion** on *The Influence of Fertilisers on the Yield and Composition of Plants.*

(*a*) Sir John Russell, F.R.S.

The primary effect of a fertilizer on the plant is to increase the uptake of the particular nutrient thus supplied and consequently increase its amount in the plant tissues. The rule is general and applies to any soluble salt added to the medium in which a plant is growing. Up to a certain point the uptake is proportional to the amount supplied; beyond that it falls off.

The additional nutrient may cause an increase in growth but not a strictly proportional increase. For small increments of the nutrient the growth may be less than proportional and for larger increments more than proportional; for still larger increments it is again less than proportional and beyond a certain point ceases altogether. The curve is sigmoid, but it may be nearly logarithmic.

In consequence, the first increment of added fertiliser to a crop insufficiently supplied with the particular nutrient may cause an increase in the amount of the nutrient ion in the plant without correspondingly increasing the amount of growth. The percentage of the ion in the dry matter of the plant therefore increases. Further increments of the fertiliser may cause an increase in growth proportional to the added fertiliser so that there is no change in the percentage of the nutrient ion in the dry matter. Still further increments cause little or no further growth: the effect on composition depends on whether uptake of the nutrient ion still continues. Uptake of nitrogen and of potassium goes further than that of phosphorus. Instances are given showing how dressings of phosphatic fertiliser to soils deficient in phosphate increase the phosphorus content of the crop, but when larger quantities of phosphate are present in the soil dressings of phosphatic fertiliser cause no further increase. Dressings of nitrogenous fertiliser, however, continue to increase the nitrogen content of the crop.

The ions entering the plant are not necessarily linked with any particular ion and in consequence the additional uptake of any one ion increases its amount relative to the others so reducing their percentage amounts in the dry matter or ash of the plant. Thus, addition of nitrogenous or potassic fertilisers lowers the percentage of phosphorus in the crop. These variations in composition are greatly damped down in the maturation process: the grain reflects them only feebly: its composition is determined mainly by temperature and water supply at the various stages of growth, in other words—the weather.

Excess of any particular nutrient ion in the plant causes characteristic disturbances in its structure and behaviour. These are specially clearly marked for nitrogen; apparently the plant functions normally only within a certain range of values for the ratio $\frac{C}{N}$. These disturbances are most easily counteracted by increasing the supply of potassium, which increases the efficiency of the leaf as a producer of carbohydrate and therefore restores the $\frac{C}{N}$ ratio to more normal values; hence, in practice, the $\frac{K}{N}$ ratio is useful to the expert adviser.

Deficiencies of the various nutrients also cause well marked disturbances, the recognition of which is useful to the agricultural expert as a means of diagnosing serious but not slight soil deficiencies. It is not always possible to separate these deficiency effects from the effects of the relative excess of some other nutrient.

### (b) Dr. L. R. Bishop.—*Barley.*

On English arable soils the only fertiliser causing important increases in yield is nitrogen. One hundred-weight of sulphate of ammonia per acre gives an increased yield of approximately six bushels of grain. It does this without marked effects on the composition and 'quality,' since the extra nitrogen absorbed from the manure is approximately balanced by the resulting extra carbohydrate. Potash and phosphate fertilisers have no appreciable effects on barley composition on English arable soils and only on certain deficient soils do these manures increase the yield noticeably. Heavy doses or late top dressings of nitrogenous fertilisers raise the nitrogen percentage of the grain since they do not produce a proportional increase in carbohydrate production.

The important nitrogenous constituents have been studied in detail and it is concluded that the amount of each of the separate proteins in the mature grain is regularly and solely related to the total nitrogen content of the grain in a way which is characteristic of each variety. The protein amounts are thus uninfluenced by the manuring or by soil or season. The amount of glutelin is directly proportional to the total nitrogen content. With increasing total nitrogen the hordein increases more rapidly and the group of salt-soluble constituents increases correspondingly less rapidly. Owing to these regularities the total nitrogen content is a good index, within each variety, of the effects of the amounts of the separate proteins on the so-called 'quality' of the grain.

A knowledge of yield and nitrogenous composition has an important practical bearing since there is a wide range in prices between high-class malting barley and low-grade barley used for stock feeding. A range which is greater than that between different grades of any other cereal. The 'quality' is therefore a valuable and important characteristic, and conditions affecting 'quality' are worthy of study. The reason why particular attention has been paid to the nitrogenous composition is that it has been noted in the past that high quality in barley is chiefly associated with low nitrogen content, while in the future there may be an even closer connection between value and nitrogen content; for it has been shown that the latter can be used to predict from the barley two quantities of importance in the resulting malt—'extract' and 'permanently soluble nitrogen.'

### (c) Dr. E. M. Crowther.—*Influence of Fertilisers on the Yield and Composition of Potatoes.*

Although the yields of potatoes are more influenced by factors, such as season, soil type, potato variety, and disease than by the artificial fertilisers used, progress in recent years in the study of potato manuring has already greatly modified the practical use of fertilisers for this crop. The potato is primarily a dung crop. Formerly, with liberal supplies of dung the addition of phosphates probably sufficed, but under modern conditions there is much evidence that phosphatic responses occur only in very deficient soils or in highly farmed land where nitrogen and potash supplies are liberal. The importance of an adequate balance between nitrogen and potash has been established. Although high grade potassium salts are especially favoured, the adverse effects of high chloride and sodium contents are shown only in occasional years.

The influence of fertilisers on potato quality is still obscure, in spite of the dogmatism of the text books and the confident assertions of salesmen. As potatoes are not important industrial raw materials in this country, there has been little attempt to standardise potato quality even in arbitrary terms. Further, the English taste differs completely from the Continental ones. At present, potatoes are graded almost entirely by variety and type of soil. A preliminary fertiliser trial with quality tests and chemical analyses on the tubers was carried out on a large scale in 1929 by Rothamsted and the Research Laboratories of Messrs. Lyons. Here, too, the difference between two experimental centres (Rothamsted on heavy loam soil and Woburn on sandy loam soil) proved greater than those due to fertilisers at either centre. There was evidence that the nitrogenous manures increased the nitrogen content and decreased the values assigned to consistency and flavour. Potash manures reduced the dry matter and nitrogen contents and increased the values assigned to consistency and flavour.

(Appreciable effects on flavour were obtained only at Woburn.) Effects due to the form of potash used were slight; potassium chloride gave the best consistency; both dry matter and nitrogen in the tubers decreased with increasing amounts of chloride. The variations between replicated plots are so great that field experiments must be very carefully designed before reliable results may be expected.

### (d) Dr. Winifred E. Brenchley.—*Meadow Hay.*

Fertilisers exercise an indirect, as well as direct, action on yield on grass and arable land by influencing the growth of plants other than the desired crop, so varying the degree of competition to which the crop is subjected.

The competition of weeds or undesirable plants is a much more important factor n determining yield than is generally recognised.

On arable land the type of manuring that is favourable to good crop production also favours the growth of weeds. If seasonal conditions encourage the germination and growth of weed seeds before the crop makes headway the competition causes serious reduction of crop yield, which is frequently more marked with heavy than with light manuring. The potential weed flora may be very abundant, quantitative experiments at Rothamsted showing that the numbers of viable weed seeds buried in the soil may run into hundreds of millions per acre with certain types of fertilisers.

On grass land the effect of artificial fertilisers is variously influenced by liming. On heavy land, long-continued treatment with sulphate of ammonia and minerals tends to produce acid soil conditions, and the production of a herbage composed of the less desirable grasses and giving relatively low yield. Liming under these conditions increases yield greatly, and changes the balance of composition in favour of more desirable species. On the other hand, on the same soil liming has much less or no beneficial effect with one-sided manures, such as superphosphate alone, or where the soil is tending towards an alkaline reaction due to the use of nitrate of soda and minerals.

### (*e*) Mr. A. R. Clapham.—*Sugar Beet.*

Sugar beet is almost always grown on land which has received liberal dressings of farmyard manure, and under these conditions the further response to artificial fertilisers is small—much smaller than the effect of season and variety. Additional nitrogen usually increases the yield of tops, and may increase that of roots also, but to a smaller extent. Nitrate of soda is, generally speaking, more effective than sulphate of ammonia. The increases in yield of tops are almost always associated with a depressed percentage of sugar in the roots, and this is more pronounced when the additional nitrogen is given as a top dressing than when it is applied with the seed.

The response to additional potassic fertilisers varies a good deal with soil and season, and there is often an increased yield of roots and an increase in the percentage of sugar. The increase is greater with the low-grade potash salts than with either potassium chloride or potassium sulphate. This result appears to be due to the sodium content of the low-grade salts, for sodium chloride is often as effective as an equivalent weight of potassium chloride.

Phosphatic fertilisers may increase yield of roots, but rarely affect the percentage of sugar to an appreciable extent.

Perhaps the most interesting of these results are the nature of the responses to additional nitrogen on the one hand and to additional potash or soda on the other. It would appear that nitrogen acts by increasing the size and number of the leaves. A considerable fraction of the products of photosynthesis is expended in the formation of new skeletal material above ground, and is never translocated to the roots. Potash, on the other hand, increases the efficiency of the leaf as an assimilating organ, but has a much smaller effect than nitrogen on the total leaf area. A large fraction of the assimilated materials is therefore transferred to the root, and both the yield of roots and the percentage of sugar in the roots are increased. As might be expected, the effect of nitrogen in depressing the sugar percentage depends a great deal on the time of application. If given with the seed it may result in an increased yield without an appreciable lowering of the sugar percentage; but if applied late there is a considerable delay in maturity, as well as a decreased sugar percentage, and there may be no compensating increase in yield.

## Afternoon.

### Mr. T. Wallace.—*Soil Surveys.*

### Mr. T. Swarbrick.—*Stock and Scion Relationship.*

### Mr. F. Tutin.—*Investigations on Tar Distillate Washes.*

### Mr. A. W. Ling.—*Sugar Beet Investigations.*

### Mr. C. V. Dawe.—*The Work in Agricultural Economics at Bristol University.*

The work was begun in the late autumn of 1924. It was necessary to collect farm accounts in order to arrive at the factors relating to farm finance. This led to making a decision between detailed cost accounts and simple financial accounts, and because of the limits of staff at the University, the latter type of account was decided upon. Both kinds of accounts have their advantages and disadvantages for advisory work.

The method employed is to invite farmers to submit their simple accounts for analysis and report. The farms have been grouped into soil and farming types, and comparisons between individual farms are made within each of these groups. One aim is to widen the farmer's outlook and to show him what others are doing. Under this system the farm is treated as a unit, no attempt being made to allocate costs, income or profits between various departments of the farm. Until lately the analyses and reports have been made upon an empirical basis, but now attempts are being made to use the statistical method upon the data collected during the past five years. One object is to see if there is any kind of correlation between the factors, capital, production, and cost. If so, then it may be possible to classify farms in a different way from which they have been grouped in the past. This statistical work is still in its experimental stages and, although it promises to give valuable results, it is too early to be definite about anything at present.

Considerable work has been done in ascertaining the cost of growing sugar beet in Hereford and Worcester.

Mr. F. Hirst.—*Problems in connection with the Establishment of the Canning Industry.*

## Friday, September 5.

**Discussion** (Sections K, M) on *Mineral Elements in Plant Nutrition.* (See Section K.)

Dr. J. F. Tocher.—*The Adulteration of Milk with Water.*

The detection of the adulteration of milk by the addition of water is difficult because apart from breed and selection, (1) milk is composed very largely of water, (2) the proportions of the solid constituents vary with every individual cow, and (3) the number of cows whose milk is bulked varies in commercial milk. It is very desirable for the protection of the public that a criterion should be obtained which would enable the public analyst to state definitely, within certain limits, the probability that a particular sample of milk has been adulterated by the addition of water. In the opinion of the writer no such criterion has yet been found. Certain workers depend on the supposed constancy of the freezing point of milk in genuine milk. Others on (1) the supposed constancy of the refractive index, (2) the real or supposed shortage in ash, (3) the supposed minimum of solids-not-fat, and (4) the supposed minimum and low variability of total solids. In all these cases the computors have not taken into account the natural variations in the proportions of the constituents of genuine milk and the fact that the composition of milk must bear a relation to the number of cows whose milk is bulked.

The supposed constancy of the freezing point has been specially stressed by prominent workers in America and in the Dominions, but most American and Dominions data are from selected cows and not from a general population of both selected and unselected cows whose milk constitutes the supply for this country. It is natural, therefore, that the bulked milk from selected cows should show greater constancy in the values of the depression of the freezing point and in other characters than the values found from commercial milk in this country.

In a paper shortly to be published, the writer shows that the proportions of the various constituents of milk in samples *low* in solids-not-fat are quite different from the proportions of these same constituents in samples *high* in solids-not-fat. He further shows the nature of the relationships which exist between the proportions of the various constituents and finds non-linearity to prevail.

The writer's data consist of results of analyses of over 500 samples of milk from individual cows for all the constituents of milk.

As a first approximation to a criterion the following equation was obtained :—

$$\tilde{x}_0 = -18{\cdot}41 + 5{\cdot}52x_0 - {\cdot}28x_0^2 + {\cdot}55(x_1 - 4{\cdot}64) - {\cdot}88(x_2 - 2{\cdot}42) + {\cdot}40(x_3 - {\cdot}74) - 1{\cdot}12(x_4 - {\cdot}84)$$

where $\tilde{x}_0$ = predicted solids-not-fat ; $x_0$ = solids-not-fat ; $x_1$ = lactose ; $x_2$ = casein ; $x_3$ = albumin and globulin ; and $x_4$ = ash.

When the number of cows whose milk has been bulked is known, the difference $(x_0 - \tilde{x}_0)$ can be compared with $\sum_{(x_0 - \tilde{x}_0)}$. Relative differences greater than those found from genuine samples show that the sample has been watered. A better approximation may be expected from an equation of the second degree. Fifteen constants have been obtained from the data in an equation which can be used by all analysts in order to measure the probability that a sample of milk has been watered. Before a worker can use this equation, he must determine the percentages of the following constituents in the sample, viz. casein, albumin, globulin, lactose, ash, solids-not-fat and butter fat.

AFTERNOON.

Sir FREDERICK KEEBLE, C.B.E., F.R.S.—*Agricultural Problems in South Africa.*

## Saturday, September 6.

Excursion to the farms of Mr. E. R. DEBENHAM, at Briantspuddle, Dorset, proceeding via Castle Cary, Sparkford, Sherborne and Dorchester, and returning to Bristol via Blandford, Shaftesbury, Warminster and Bates.

## Monday, September 8.

**Presidential Address** by Dr. P. J. DU TOIT, on *Veterinary Science and Agriculture.* (See p. 229.)

Followed by Discussion. (Dr. N. H. ANDREWS, Major WALTER ELLIOT, M.P., Sir ROBERT GREIG, Dr. J. B. ORR.)

## Tuesday, September 9.

**Discussion** on GRASS LAND IMPROVEMENT :—

(*a*) Prof. R. G. STAPLETON.—*Indigenous Plants in relation to Pasture Improvement.*

(*a*) The use of the terms ' indigenous ' and ' wild.'

(*b*) The characteristics of so-called ' indigenous ' grasses.

(*c*) The dangers of present-day methods of harvesting and of the popularity of 'indigenous ' seed.

(*d*) The difference between so-called ' indigenous ' and pedigree-bred strains.

(*e*) Some results obtained from the use of ' indigenous ' and of pedigree-bred strains.

(*f*) What may be expected from the use of pedigree strains bred for permanent pasture purposes.

(*b*) Dr. J. B. ORR.—*Balance between Nutritional Level of Grazing Animals and Pasture.*

Rate of growth and rate of production of the grazing animal is limited by the amount and composition of the pasture. Any food constituent of the pasture may act as a limiting factor.

Under natural conditions herbivorous animals have been evolved with a rate of growth and production suited to their grazings.

When the modern improved breeds, which have been developed on rich pastures supplemented by indoor feeding, are put on to low-grade natural pastures without supplementary feeding, the rate of growth is usually greater than can be supported by the natural pastures, with the result that the improved breeds must be graded down to the level of the pasture by the elimination through sterility or disease of the rapidly growing and rapidly producing individuals.

In improving the quality of cattle in pastoral areas it is necessary that the grading up of the stock should be accompanied by the grading up of the pastures.

Modern research in pastures is attempting not only to increase the quantity of the herbage, but also to improve the quality by discovering and removing the limiting factors.

### (c) Mr. W. Godden.—*Effect of Fertilisers and Rainfall on the Composition of Pasture Grass.*

An account is given of certain fertiliser trials conducted at the Rowett Institute. These are plot trials on two different types of soil to determine the effect of nitrogenous fertilisers with and without potash, phosphates, and lime on the yield and composition of grass (*a*) cut at frequent intervals during the growing season to resemble grazing, (*b*) cut as hay and aftermath.

The result is communicated of the analyses of pasture samples collected at various centres in Kenya Colony after a period of drought and after heavy rainfall immediately following the period of drought.

### (d) Prof. J. A. Hanley.—*The Management of Grass Land.*

### (e) Mr. H. J. Page.—*The Economics and Research Aspects of Intensive Grass Land Management.*

## Commissioner D. C. Lamb.—*The 'Human Aspect' in relation to Agriculture.*

Contributing causes to economic, social, political and financial troubles are the worship of false economic gods and neglect of the 'humanities.' The economics should be made to 'square' with the 'humanities.' For example, it is morally wrong to maintain able-bodied men and women in idleness, and what is morally wrong cannot be economically right.

What have the worship of false economic gods and the 'humanities' to do with agriculture ? Everything, since agriculture is the foundation on which our whole social superstructure rests.

Forty years ago the founder of the Salvation Army launched a social service programme set forth in 'Darkest England and the Way Out.' That programme had two main ideas : (1) a social reform movement embodying 'back to the land' to offset the persistent urbanisation of the community, and (2) reform of social and spiritual wrecks, misfits and suchlike, where it was considered that contact with nature—work on the land—would be a helpful factor.

More than two years ago the Industrial Transference Board drew attention to the long waiting lists of unsatisfied applicants for small holdings, many with previous agricultural experience, who desired to settle in Britain. The Board also said : 'Many miners' families have had actual experience in agriculture, while many more are only one generation removed from the land. They are inured to hard work, and frequently live in the midst of or on the edge of purely agricultural country.' The possibilities of small holdings are not by any means fully utilised in Britain, although there may be doubts of success in view of the competition of Continental small-holders. But the imperial asset of the vacant lands in the Dominions overseas points to emigration as a way out, and the Industrial Transference Board frankly looks to a large emigration movement as a solution of part of our unemployed problem. After some considerable experience in dealing with social problems, the founder of the Army said : 'Of all the remedies propounded for the immediate and permanent relief of distress arising out of unemployment emigration still holds the field.' The same is true to-day.

Shrewd observers of social and economic conditions here and students of overseas requirements are convinced that a considerable swarming of people from the Homeland to the Dominions is long overdue. In the swarming of bees is seen a principle applicable to the human race. So far as the British people are concerned they can be hived within the confines of the Empire. If bees are not kindly and wisely handled at swarming time they may easily be lost to the owner, and someone else will profit by their settlement. A considerable economic loss is one result, for bees, like human beings, are wonderful wealth producers.

If there is to be a big movement of people back to the land in Britain or in the King's overseas Dominions then skill in agriculture and consideration of the 'humanities' is all-important for success. Failure of crops, disappointing markets, droughts, floods, forest fires and those things which skill in agriculture seem unable to ward off, can only be met by human and divine sympathy.

In the Army's 'back to the land' movement its efforts fall into two main categories: (1) Anglo-Saxon, and (2) Indian. The Anglo-Saxon effort has resulted in the transplantation of 200,000 men, women and children to the King's overseas Dominions; less than 1 per cent. have failed. The Army has also some experience in Group Settlements. Its Indian efforts have been twofold in close co-operation with the Indian Governments: (a) settlement and reformation of Indian criminal tribes in several provinces, and (b) settlement of Indian Salvationists at Shantanaggar in the Punjab. The Indians were drawn from overcrowded districts and settled on irrigated land. The Army assumed certain financial responsibilities over a period of thirty years and undertook the guidance and control of settlers.

The uplifting influences of the contact with nature are easily discerned, whether the 'patient' be a social wreck or an economic victim—for example, the influence of pedigree stock at Hadleigh Colony on the morale of the colonists. When it was decided to have pedigree stock on the Colony and that stock arrived, the moral effect upon the men was at once perceptible. Good horses meant good harness and decent carts, good grooming, cleaning and tidying the stables.

One of the Army's contributions to a solution of post-war conditions in Britain has been the training in farm-work and transplantation overseas of boys. More than 5,000 boys have undergone training since 1923, and 90 per cent. of them are known to be still on farms two years after landing overseas.

In its experience the Army has demonstrated the healing influence of the contact with nature by the success of all its schemes of farm-training and settlement. The result has been the regeneration of tens of thousands of human beings and, in the broad sense, an economic gain. Politicians and others are agreed that adventure overseas offers many advantages both to the individual migrant and to the country that receives him.

The time has probably arrived, not only to encourage migration and settlement on the land, but also to reconsider the whole question of migration from the point of view of the principles of rationalisation and the 'humanities' and agriculture. An Empire Development Board of business men furnished with adequate financial appropriations to encourage development is needed. But when all this is done, if we forget our 'humanities' rationalisation, economic dogmas and agriculture are bound to suffer.

Dr. H. W. Miles.—*Recent Research in the Potato Root Eelworm and its relation to Potato Sickness.*

EVENING DISCOURSE.

# WIRELESS ECHOES

BY

PROF. E. V. APPLETON, F.R.S.

One of the most striking features of the history of wireless communication is the way in which practice has so often been ahead of theory. It is true that, at the beginning, the theoretical predictions of Clerk Maxwell concerning the existence of electric waves had been published thirteen years before they were practically realised by Hertz. But such a striking precedence of theory before practice has not often occurred since. Indeed, for the last thirty years things have usually been just the opposite. Things deemed impossible or nearly so by the theorist have always been happening, and we have been able to accomplish more than we could understand.

This evening I want us to consider one of these fields of wireless investigation in which unsolved problems abound. I refer to the subject of the propagation of short waves. To justify our theoretical ignorance to some extent, I ought to say that we can see at any rate why unsolved problems should exist. We know nowadays that when wireless waves are emitted by a sending station they travel upwards into regions through which we cannot follow them; and it is only at the starting and finishing points of their journey that we can make any observations on them.

We may first consider a special case. Suppose we wish to send wireless signals from one point on the earth's surface to another, say fifty miles away. How do the waves travel from one place to the other? At first we might naturally be inclined to say, 'Why, there is no difficulty about it! We can take a wireless receiver to all intermediate points between the two stations and still hear the signals, so obviously the waves have travelled straight along the ground from one station to the other.' That would be quite true, but it would be only part of the truth. It can be shown that waves also reach the receiving station by much more circuitous routes. A little consideration will show us that this may be possible. The earth, I need hardly remind you, is not flat but round, so that there are two direct paths from one place to another. The question may be asked, 'Why don't some of the waves come round the earth the other way?' And the answer is that they do; but, of course, since the journey is much longer they take a longer time to do so.

I have mentioned this particular example because it brings us to one of the most fundamental points we have to consider to-night. If it is true that a wireless station receives signals other than those which have travelled straight over the shortest distance, how are we to find out where such wandering waves have been? If we wished to estimate how long a journey a traveller had made on a trip by train, we could use the time taken on the journey and an estimate of the speed of travel. We might also take into account the appearance of the traveller, fresh or otherwise, at the end of the journey. Now such information is exactly that which we are able to obtain from our wireless experiments. We are able to observe the times at which these vagrant waves start out and arrive, and thus find the time they have taken on their journey; we also know their speed through the air, and from their strength or weakness at the end of the journey we can make deductions as to the kind of time they have had on the way. It sometimes happens that the waves which have made a particularly long or difficult journey are so distorted on arrival as to be almost unrecognisable, so that in justice to our railways we must admit that the analogy with the railway traveller breaks down somewhat. One of the most frequent forms of distortion is that in which waves sent out travelling in a normal erect fashion are found to arrive travelling in a horizontal recumbent position, their bodies, as it were, having been twisted through a right angle. Again we must gratefully admit that our railway analogy has broken down.

I mentioned that we know the velocity with which waves travel, namely 300,000 kilometres per second, or 186,000 miles per second—which is, of course, the velocity of light. Actually, as we shall see, sometimes the speed of the waves, along some parts of the journey, is a little less than this, but we may take this as our value for the time being. Moreover, as in the first part of our discussion, we shall be dealing with times of transit that are quite small, it is useful for us to remember that the velocity of the waves is such that in one-millisecond, or one thousandth of a second, the waves have travelled 300 kilometres or 186 miles.

The first experiments on the timing of the journeys of wireless waves were carried out almost simultaneously in this country and in America, though the methods of timing in the two cases were quite different. The English experiments were carried out using an 80-mile base. That is to say, there were 80 miles between sending and receiving stations. In this case it was found that at night, but not usually in the daytime, for every signal sent out from the sending station two or more were observed at the receiving station, the second coming just over 1/3 millisecond after the first, and the third about one millisecond after the first, indicating paths about 100 km. and 300 km. greater than that traversed by the first signal.

It was natural for it to be assumed that the first signal had travelled straight along the ground from sender to receiving station, that is, a distance of 130 km., while the second had travelled 100 km. further, and the third 300 km. further.

Now the unexpected result that long waves were able to travel round a spherical earth had led Kennelly and Heaviside to suggest that there is in the upper atmosphere a layer of electricity which is sufficiently conducting to be a reflector of wireless waves. It was therefore natural to assume that the echo-signals which were observed were due to waves reflected from the layer. This is made clearer in Fig. 1. The

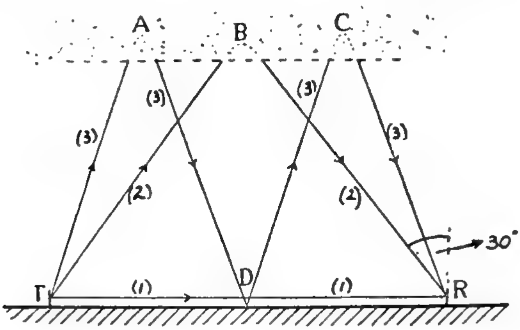



FIG. 1.

first signal received is the one which travels by the path (1) along the ground, the second is that which has been reflected once at this layer and marked (2), and the third is one which has been doubly reflected and marked (3). Since the path length of the first is known, that of the second and third can be calculated, and it is found that they agree with a layer height of about 100 km. This would indicate that the angle between the direction of the downcoming waves constituting the first echo and the vertical at the receiver was about 30°. In another series of experiments in which this angle could be measured by an entirely different method it was found to have an average value of about 30°, thus confirming the results of the first series.

The British method of timing echo-signals, though very accurate, is a little complicated, so I will not burden you with details of it. In the American experiments a more direct method of timing was adopted. The two stations, sending and receiving, were closer together, being situated only six miles apart. Very short signal impulses were sent out by the sending station and recorded at the receiving station by means of a high-speed galvanometer. Evidence of echo-signals were obtained, and from the speed of travel of the photographic film used the echo-time could be estimated.

In the earlier series of the American experiments the echo-delay was of the order of 1½ milliseconds, indicating a path-length difference of the order of 450 km. Now as the stations were close together, we must assume that the waves causing the echo had gone almost straight up and down, so that the height of the reflecting surface was about 220 km. Now there is a big difference between 100 km. and 220 km., though it must be remembered that in English experiments 400-metre waves were used, and in the American experiments 75-metre waves were used. The question, therefore, arose as to whether there was a real difference between the height of the reflecting region in England and America, or whether the discrepancy was due to the different distances and different wave-lengths used. The question of different distances was ruled out when the English experiments were repeated at short distances comparable with that used in America, and echo-times of 2 milliseconds were again obtained, indicating again a height of 100 km. (*i.e.* total path 200 km.). This particular difference gained added interest when it was remembered that, of the two men who had suggested the existence of this layer as a way out of theoretical difficulties, Heaviside was an Englishman and Kennelly an American Professor. Had nature arranged a sufficient difference between the heights of the region in question in this country and America for both their claims to be separately recognised ?

The question was settled when experiments at short distances were carried out here in England using a number of different wave-lengths. In these experiments it was found that the long waves are reflected at lower heights than the short waves. Also it was found that there was a discontinuity in the curve in which echo-time was exhibited as a function of wave-length. Our theory as to the mechanism by which the echo-waves are reflected is not quite clear, due largely to the fact that we are uncertain as to the exact influence of the earth's magnetic field, but we are fairly certain that, the higher the frequency, the more electricity is required to reflect the waves. It thus looks as if there are two regions which reflect waves. All frequencies below the critical value are reflected by the lower region, and all frequencies above it are reflected by the upper region.

You will by now have seen how the discrepancy between the American and English results is to be explained. Using long waves and low frequencies, we had been getting echoes from the lower region, while in the American experiments the shorter waves had been penetrating the lower region and reaching the upper one.

The critical frequency or wave-length, which varies with the time of the day, is an interesting one, for echoes from both regions are often obtained simultaneously with it. Recognition of this led us to be able to suggest an alternative explanation of some of the American records. The American observers had on one occasion found examples of three echoes, such as are shown in the upper half of Fig. 2.

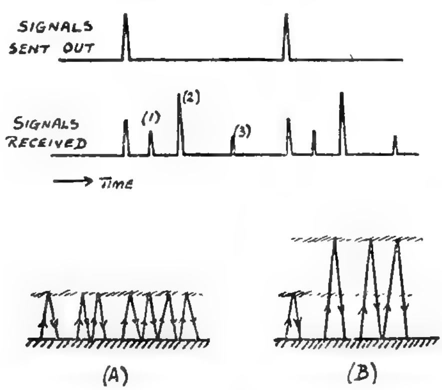



FIG. 2.

Now the echo-times appear to be in the ratio 1, 2 and 4, and they were therefore interpreted as being due to multiple reflection from a single region. Now this interpretation presented three difficulties, as follows :—

(1) The echo-times were not exactly 1 : 2 : 4, but about 0.9 : 2 : 4.

(2) Why was there no third order reflection ?

(3) Why was the second echo stronger than the first, although it had made two reflections at the same region ?

These difficulties were found to disappear when the two-region hypothesis was applied to them, and simultaneous reflection at heights of 100 km. and 220 km. was assumed. The alternative methods of explaining these results are shown diagrammatically in the lower half of Fig. 2 (A and B).

I ought, of course, to mention that these echoes, due to waves which are reflected from the upper atmosphere, are the causes of the signal variations which we experience when listening to distant stations at night. But the individual echoes in that case cannot be recognised by the ear. We require some other form of record in which the effects of direct signal and echo can be separated. As many of you are aware, the most recent application of electrical communication, namely, television, has reached the stage which is sufficiently advanced for broadcasting experiments to begin, and now there are seven half-hour periods of experimental transmission each week conducted by the Baird Television Company through the B.B.C. stations.

Some months ago it seemed to me of interest to inquire what kind of effect such wireless echoes as we have been considering would have in a broadcast of television. The question to be answered was: would the echoes be sufficiently distinct from the main signal to cause ghost images?

To examine this point we must first consider how a television image is made up. Let us consider first the transmitter. Here we find that a spot of light, which may be called the television 'eye,' is made to traverse or look over the whole of the image in $\frac{1}{12\frac{1}{2}}$ of a second. The 'eye' is made to do this by allowing light to pass through the holes of a disc which is revolving uniformly. There are thirty holes in the disc, which is made to rotate anti-clockwise. Moreover, each hole is succeeded by one which is a little nearer to the centre of the disc than itself. Thus, the picture is scanned from right to left and from bottom to top. Now if there are thirty holes and if the speed of rotation is 12.5 per second, it requires only simple arithmetic to show that the spot of light runs up a strip in $\frac{1000}{30 \times 12.5} = 2.7$ milliseconds.

Now at the receiving end there is a similar disc which by an ingenious device, due to Mr. Baird, is made to revolve exactly in step with the disc at the transmitter. Now when the television 'eye' finds the particular part of the strip it is traversing light or dark, an electrical impulse is sent out by wireless informing the receiver, as it were, whether such a particular point should be light or dark in the reproduced image.

Now at night, when echoes are prevalent, such a light or dark message can be received both along the ground and after reflection by the upper atmosphere. We thus might expect two light or dark messages to be received, the echo image from the Heaviside layer coming after the main one a fraction of a millisecond later. Since the spot is travelling always up the picture we should expect such echo-signals to cause images situated *above* the main one.

Some time ago, through the medium of the *Wireless World*, I was able to ask amateurs receiving television to look out for such echo-signals. And, appropriately enough, the first set of observations I received were made by an amateur in Bristol, Mr. W. B. Weber, who has been very successful in receiving the Baird transmissions. He sent me a most interesting communication saying—

1. That he had received echo images by night and not by day.
2. That the echo image was always above the main one.
3. That, so far as he could measure, the shift upwards was almost about $\frac{1}{7}$ of a whole strip.

From these interesting observations it is possible to calculate the echo-time which I make out to be about $\frac{1}{3}$ millisecond, corresponding to a path difference of 108 km.

As the distance between Brookman's Park and Bristol is 100 to 103 miles, the result gives a height of 108 km. for the reflecting region, which is in satisfactory agreement with the value obtained by other methods for this particular wave-length.

So far we have been dealing with what may be called short-range echoes. But echoes are often experienced in long-distance transmission, in which case transmission straight along the ground no longer takes place. For example, let us consider the case of transmission across the Atlantic. In this case, the first signal to arrive is that due to waves which have been once reflected by the layer. But wave-tracks making two or more journeys to the layer are also possible. The separation of these signals, which is a matter of milliseconds, cannot, of course, be done by the ear, but they can often be recognised in the transmission of still pictures, because they give rise to multiple images. The Marconi Company, who have developed a very efficient system of facsimile transmission, have experienced such multiple images, and Mr. T. L. Eckersley, of the research staff of that company, has been able to deduce some very interesting results from a study of these records.

One very interesting fact is brought out by Mr. Eckersley's work on this subject, and that is that the shorter the wave-length used the fewer the echoes. This is satisfactorily explained by assuming that the shorter the wave-length the more likely it is that a ray will penetrate the layer, so that large angle deviation no longer occurs.

I mentioned in an early part of this discourse that it is nowadays quite a common thing for signals to be obtained both ways round the earth. This result was first recognised by a German radio engineer, Herr Quäck, who published in 1926 what we might call documentary evidence of signals received in Germany from America, both by the Atlantic and Pacific paths. In the following year Quäck supplemented his first announcement by another just as startling, to the effect that he had succeeded in recording on an oscillograph signals which had travelled once, twice, thrice and even four times round the earth. These multiple signals were found to follow one another at intervals of approximately one-seventh of a second, which is the time required for electric waves to travel a distance equal to the circumference of the earth.

In 1926 it was, of course, known that short waves appeared able to cover enormous distance with but little loss of strength, but I do not think that anyone would have predicted from the facts then known that a short wave signal would be reduced only to about one half of its original strength in travelling round the earth.

Although the first round-the-earth echoes were obtained using the American transmission received in Germany, it was soon found that the same type of effect could be obtained using local sending stations. A special study was made of the signals sent out by Nauen at a place Geltow, situated about 50 kilometres away. These experiments yielded results such as are shown in Fig. 3, where we see examples of echo signals spaced one-seventh of a second apart.

These observations are specially noteworthy in that they yielded an accurate value of the time required for a wireless signal to travel round the earth. This value was found to be 0.1385 second. Now if we assume that the short-wave signal travels along the ground we can calculate what the time should be, since we know the circumference of the earth and the velocity of light. This calculated time is found to be 0.1338 second. Now the difference is clearly outside the limit of experimental error, and indicates that the waves do not travel along the surface of the earth. We picture the waves as travelling to and fro between the ground and the layer, thus taking a 3 per cent. longer path than that which hugs the surface of the earth.

Now although the phenomenon of multiple signals is of great interest to us from a theoretical point of view, we must not forget that to the practical man they are spurious signals which are liable to mutilate the Morse signals which are to be received. Whenever the amplitude of the echo signal exceeds about 30 per cent. of the main signal it is found that trouble in practical operation is experienced.

In his investigation of round-the-earth echoes Quack came across another type of echo, the origin of which is not quite clear. This was an echo which was observed to be associated with the direct main signal and came at intervals of about one-hundredth of a second after it. These echoes were, therefore, satellites of the main signal and, therefore, quite distinct from the round-the-earth echo. Sometimes as many as seven of these echoes were observed immediately after the main signal. This interval of one-hundredth of a second indicates that the waves had been reflected at some point 1,500 km. away, and this led Quäck to suggest that these were the ordinary type of Heaviside layer echoes, the waves having travelled vertically up and down and the height of that layer being 1,500 kilometres. This hardly seems to me to be possible, for the kind of antenna used in Quäck's experiments appears to be incapable of sending energy vertically upwards. Here we might note that in America observers have also observed these medium retardation echoes, but their times of retardation show a very wide range of values.

Mr. Eckersley, of the Marconi Company, has brought forward evidence which strongly suggests that some of these echoes are due to scattered radiation from the point on the ionised layer where the short-wave beam meets it. In this case we are to look upon the beam as being somewhat like a searchlight, an echo being received from the luminous patch where the searchlight meets the layer. This certainly gives echo times which fit in with some of the observed values, but there are many others which are as yet unaccounted for.

One very interesting practical point arises when these satellite echoes round the main signal are received. It is usually found that at the same time the round-the-earth signal is present and is free from such subsidiary echoes. Now the satellite echoes tend to distort and blur the main signal and sometimes make it quite unrecognisable. We therefore meet with instances in which the signals received

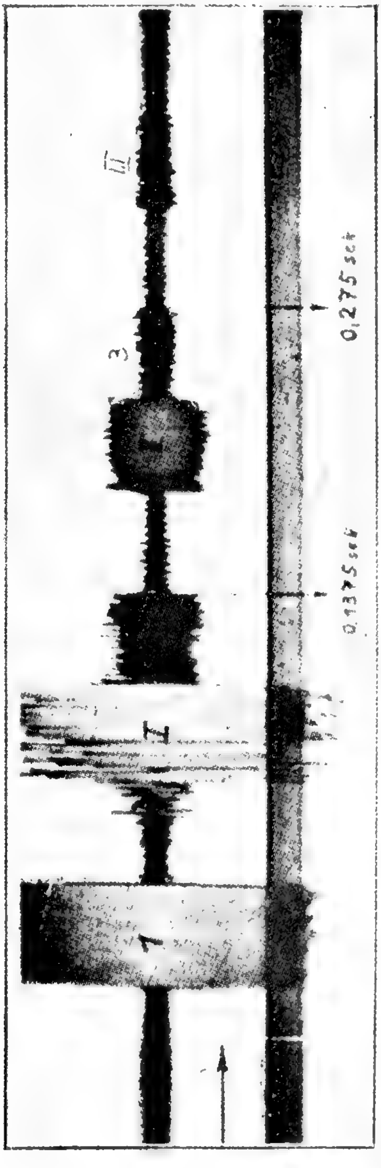



FIG. 3.

[*Illustrating Evening Discourse on Wireless Echoes.*

[*To face p.* 430.

directly from a station a few kilometres away are quite unreadable, while the signals from the same station which have made a journey round the earth are received with perfect clarity.

So far, in this discourse, I have dealt with echo-phenomena for which there are now fairly widely-accepted explanations. I now turn to a group of phenomena the origin of which must still be regarded as being something of a mystery. I refer to the remarkable echoes which can sometimes be heard, not a millisecond, but several seconds after the cessation of the original signal and which were discovered three years ago by a Norwegian wireless amateur observer. Here I would like to observe that no subject seems to have been more fortunate in the assistance rendered to it by amateur experiments than that of wireless transmission. It is agreed by those best qualified to form an opinion that it was the amateurs who discovered the extraordinary suitability of very short waves for long-distance propagation. Amateur observers have rendered extremely valuable service on special occasions when observations were needed, for example, during the 1927 eclipse in this country. As we have seen to-night, it was a Bristol wireless amateur who first obtained satisfactory data about television echoes, while it was a Norwegian amateur, Engineer Hals, who first observed the echoes of long delay.

Engineer Hals, who lives at Bygdö, near Oslo, in Norway, had for some time been making observations on wireless signals during auroral displays and sending in periodic reports to Prof. Störmer, the well-known authority on aurora, who lives in the same locality. One day in December 1927, Prof. Störmer met Engineer Hals accidentally and they began to discuss wireless reception, and in the course of this conversation Hals stated that he had been listening to the Dutch Short Wave Station at Eindhoven working on about 30 metres, and had heard echoes three seconds after the original signal, and also ventured the suggestion that the echoes were due to waves which had been to the moon and back.

Prof. Störmer, recognising that if the existence of these echoes could be established, a remarkable discovery had been made, asked Engineer Hals to make a written report of his observations. When this was received it was found to read, somewhat naively, as follows :—

'I hereby have the honour to advise you that at the end of the summer, 1927, I repeatedly heard signals from the Dutch short-wave transmitter PCJJ (Eindhoven). At the same time as I heard the telegraph signals I also heard echoes. I heard the usual echo, which goes round the earth with an interval of one-seventh of a second, as well as a weaker echo about three seconds after the principal signal had gone. When the principal signal was especially strong, I assume that the amplitude for the last echo, three seconds after, lay between one-tenth and one-twentieth of the principal signal in strength. I will only herewith confirm that I really heard this echo.'

In the meantime, Prof. Störmer had, through the courtesy of the Norwegian telegraphic department, arranged for the Eindhoven station to send special test signals to see if oscillographic records would provide what might be termed documentary evidence of the existence of the echoes. Unfortunately, however, although audible echoes were observed, it was not possible to recognise them on the photographic film because of the many other atmospheric disturbances.

Another series of observations was arranged in the autumn of 1928, the same transmitter being used and attention being concentrated on aural observations to see if the same echo could be identified by several observers. In these observations Dr. van der Pol, of Eindhoven, also joined and was able to confirm the existence of echoes heard as long as 30 seconds after the original signal. Further confirmatory evidence of the reality of the phenomenon was obtained by Mr. R. A. L. Borrow and myself, who heard echoes on February 19, 1929, at King's College, London.

But almost as startling as the discovery of the echoes themselves was the theory immediately put forward by Prof. Störmer to account for their long delay. The question to be answered was : Where had the wireless waves been during the relatively long period of 25 to 30 seconds ? The moon could be ruled out as a reflector, for a journey there and back for wireless waves is a matter of about two seconds. Could there conceivably be a reflector at ten or twenty times the moon's distance ? Now Prof. Störmer had, as far back as 1903, been led, in his study of aurora, to consider the existence of streams of electrified particles shot out from the sun and impinging on the earth's atmosphere. Such streams of electrons themselves constitute a current and as such would be deflected by the earth's magnetic field. The calculation of the trajectories of these electrified particles was a mathematical problem of great difficulty: but it was solved by Prof. Störmer, who showed that there were certain portions of space, e.g., round the Equator into which the particles could not enter. For example, for a typical case, the forbidden spaces are as shown in Fig. 4.

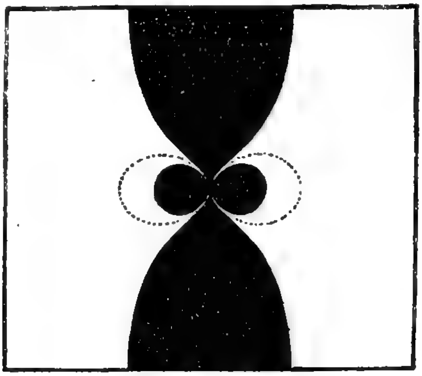


Fig. 4.

Here the forbidden spaces are shown in black and the spaces the electrons can enter shown in white. The earth is supposed to be a very small body situated in the centre of the diagram. You will notice that a toroidal space bounded by electrified particles is supposed to surround the earth, and Prof. Störmer's theory of the origin of echoes of long delay is that the waves constituting the echo have been reflected at the inner surface of this toroid.

You will perhaps see the matter more clearly in Fig. 5, which shows a quadrant

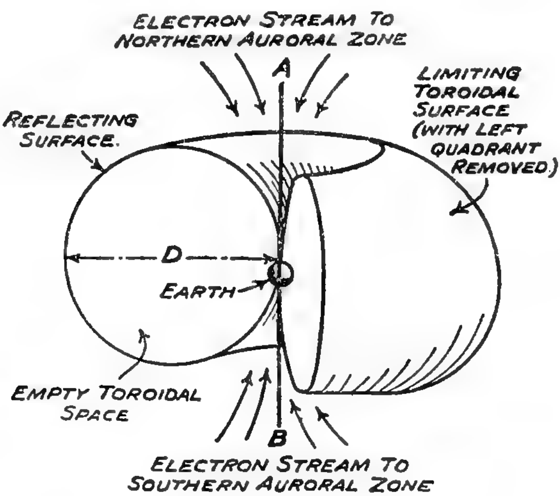



Fig. 5.

of the toroid removed. The maximum distance D depends on the speed of the particles. Thus, for ordinary cathode rays this distance will be about two million kilometres, which would give an echo line of about four seconds.

It will be seen that Prof. Störmer's theory demands that the waves causing the echo have actually penetrated the Kennelly-Heaviside Layer, and this assumption has been called into question by various writers so that Störmer's theory is not accepted by everyone.

But it must be admitted that the theory has led to some remarkable predictions. Prof. Störmer states that the months of October and February should be the most favourable ones for the occurrence of echoes, and this has actually been found to be the case. Also the theory, as will readily be seen from the slides, suggests that conditions should be more favourable for the production of echoes at the Equator than in temperate latitudes—and, again, the prediction has been fulfilled. However, the results obtained in experiments at the Equator have, unfortunately, at the same time brought evidence which is a little disturbing and difficult to reconcile with the theory as first put forward. Because of this we must consider it a little more in detail.

In May of last year, a French astronomical expedition went out to Paulo Condore in Indo-China to make observations on the total eclipse of the sun—and a party of French wireless engineers accompanied the expedition to make observations on the influence of the eclipse on wireless transmission generally, and on short-wave echoes in particular, if such could be heard at the site chosen.

A short-wave transmitter, operating on a wave length of 25 metres, and situated on board ship, was employed for the special tests for echoes. The transmissions consisted of musically-modulated continuous waves, the actual signals being two dots, sent each half minute. Echoes were heard frequently during the half-minute intervals, the time-lag ranging from five to twenty-five seconds. Throughout the four-hour period beginning at noon, local time, echoes were observed to follow practically every signal, and, at times, the strength of the echo was approximately one-third that of the original received signal. Störmer's prediction of the suitability of equatorial sites for observations on echoes was, therefore, amply borne out.

On the day of the solar eclipse, however, it was found that echoes disappeared between 13 hr. 51 min. and 13 hr. 54 min. 29 sec., while the eclipse total began at 13 hr. 53 min. 45 sec., and finished at 13 hr. 58 min. 25 sec. There is, therefore, no doubt whatsoever that the two effects were related. The eclipse of the sun was unfavourable for the production of echoes of long delay.

A particularly interesting effect was observed during one of the tests after the eclipse. When signals were being sent out every half minute, the operator forgot to send one particular signal. This was noted by the receiving operator, but he actually heard an echo due to the signal sent out 40 seconds earlier !

Thus, we have now the additional evidence that the effects of sunlight seem a necessary accompaniment to the production of echoes. Now we know that the effect of sunlight is to increase the ionisation in the Kennelly-Heaviside Layer so that the difficulty of its penetration should be correspondingly greater. Such evidence, therefore, seems to be against Störmer's theory. But if we try to suggest an alternative explanation we get into almost equal difficulties. We know that in a densely ionised medium the waves are slowed up so that they no longer travel with the velocity of light. Is it possible for the waves to have travelled actually in the layer for 20 seconds not having gone really far from the earth's surface ? So far as we can see, such slow motion would mean very marked absorption of the waves, and yet we have to account for the intensity of the echo being sometimes as much as one-third that of the original signal. The matter is, indeed, as puzzling as it is fascinating. No decisive answer to the problem can yet be given. We must, if we can, wait patiently for those Octobers and Februarys when conditions appear to be most favourable for the occurrence of echoes, and accumulate more data. We now have highly developed methods of finding the direction in which wireless waves arrive at a station, and I hope that when these can be applied in the reception of Hals-Störmer echoes a good deal of suggestive information will be obtained.

I think it is, perhaps, most fitting to conclude our survey of short-wave wireless phenomena at this point, leaving the problem of these echoes of long delay with you as an inviting subject for your own speculation. A tussle with such a problem will, at any rate, illustrate for you the healthy state of the subject of wireless at the present day.

We are told that the mythical nymph, Echo, because of her incessant chattering, was deprived of speech to the extent that she could only repeat exactly the question put to her. She could neither answer nor give information in any way.

I think, however, that we can claim for some of our wireless echoes a little more than that. They repeat their original, it is true, but they sometimes do it with sufficient lack of exactitude and at such significant intervals as to give away some of Nature's secrets. Remembering, therefore, the objects of our Association, I hope that you will agree that they have formed a fitting subject for our consideration this evening.

EVENING DISCOURSE.

# THE NITROGEN INDUSTRY AND OUR FOOD SUPPLY.

BY

Dr. R. E. SLADE.

Thirty-two years ago Sir William Crookes, in his presidential address to this Association, reviewed the problem of feeding the ever-increasing population of the world, and he devoted particular attention to the supply of wheat, which is the staple food of the white races. He concluded that :—

(1) Only about one hundred million acres of uncultivated land in the world were suitable for growing wheat.

(2) If the average yield of wheat from the land remained stationary at 12·7 bushels per acre, all the available wheat lands would be required by 1931, and after this there would be a world shortage of wheat.

(3) Some means of fixing atmospheric nitrogen must be found by the chemist so that the yield of wheat over the whole world could steadily rise from 12·7 bushels per acre to provide for the bread-eaters.

The conclusion that only one hundred million acres of uncultivated land were fit for growing wheat was based on a very careful estimate, and I do not think that any better estimate could have been made at the time.

Actually the world has now an area producing wheat much in excess of that predicted by Crookes.

The excess area suitable for wheat is due to :—

(*a*) Improvement in agricultural methods making it possible to grow wheat in dry areas.

(*b*) The discovery and use of breeds of wheat which will ripen in a shorter summer, thus pushing the wheat-growing area further north.

(*c*) The invention of improved agricultural machinery—especially the petrol tractor and the reaper, binder and thresher, which have made it possible for large areas to be brought under cultivation with the minimum of man power, and therefore with the minimum of population on the land.

Dr. Slade then dealt with the effect of the development of the nitrogen industry on agriculture.

For many centuries he said nitrogen was used as a fertiliser in the form of farmyard manure, and certain rotations of crops, which kept up the nitrogen in the soil, had been popular; but it was not until 1840, when Liebig first pointed out the true function of nitrogen, potash and phosphorus, that fertilising became an art based on science. Liebig's work became widely known in a very short time. If you read the novels of Surtees you may remember that, when Mr. Jorrocks gave up hunting and devoted himself to farming, we find him toasting nitrate of soda, and in his election address to the electors of Hillingdon he declares himself an enthusiastic supporter of 'guano, nitrate o' sober or any of them artificial compounds.' This was written in 1844.

A little later Lawes and Gilbert started their experiments at Rothamsted, which definitely proved the part taken by nitrogen in agriculture.

From 1840 the use of nitrogen in the form of Chili nitrate steadily increased, and about 1880 sulphate of ammonia became available from bye-product coke-ovens, and by 1903 the world consumption of these two fertilisers had increased to 1,975,000 tons, equivalent to 351,000 tons of nitrogen.

In 1905 calcium cyanamide was manufactured on a commercial scale for use as a fertiliser, and in the same year the arc process for combining the oxygen and nitrogen of the air to form nitric acid was launched unsuccessfully in Canada. Two years later, in 1907, the arc process was established in Norway, and calcium nitrate was put on the market as a fertiliser. Thus Crookes' dream of the commercial production of nitrogenous fertilisers from the nitrogen of the air was realised in less than ten years. But it was the Haber-Bosch process for the fixation of nitrogen which was to supersede all others, and make it possible for us to produce all the nitrogen fertilisers we require now, and as far in the future as we can see.

In 1906 to 1909 Prof. Haber investigated the chemical equilibrium between nitrogen, hydrogen and ammonia when heated under a pressure over a catalyst. Then Dr. Bosch, of the Badisch Analin u. Soda Fabrik, successfully manufactured ammonia on a large scale by this process in 1913 to 1914.

The fixation of nitrogen in the form of ammonia is so much cheaper than by any other process that this process has very largely replaced the cyanamide and arc processes, and it has shaken the Chili nitrate industry to its foundation, so that the Chilean Government and the nitrate manufacturers have had to revise their methods and their processes.

[Dr. Slade then showed diagrammatically how the nitrogen industry had already achieved considerable importance in 1898, and it was not until 1920 that the synthetic nitrogen production became greater than the Chilean nitrate production.]

### Nitrogen Fertilisers.

After this short summary of the position of the nitrogen industry to-day, let us consider what becomes of this nitrogen.

The world's consumption in 1928 of nitrogen in the form of ammonium sulphate from synthetic ammonia and bye-product coke-ovens, cyanamide, nitrate of lime, nitro-chalk and ammonia liquor was 1,442,000 tons, and in the form of Chili nitrate 401,000 tons, making a total of 1,843,000 tons, of which 185,000 are used in industry and 1,658,000 tons are consumed as fertilisers. If fixed nitrogen is worth £50 a ton—which is about its price in sulphate of ammonia to-day—then the value of the nitrogen used in fertilising was £83,000,000.

Now in an acre of typical English arable soil we have in the top nine inches a quantity of humus containing about 2,500 lbs. of nitrogen, and at certain times of the year changes take place in the soil making some of this nitrogen into nitrates, in which form it is available for absorption by the plant. The result of this is that in the spring about 1 per cent. of the nitrogen in the humus is present in the form of nitrate; thus we have present about 25 lbs. of nitrogen available for the plant. As this available nitrogen is used up by the plant it is partly replaced by more nitrate being formed from the humus, but during the time of greatest growth there is a considerable depletion of available nitrogen in the soil. Owing to the continuous breaking down of the humus the nitrogen absorbed by the plant is often more than 25 lbs., besides what is washed away by rain. The supply of available nitrogen may be increased by the addition of nitrates or ammonium salts, for the latter are rapidly oxidised to nitrates in the soil.

If we spread 1 cwt. of sulphate of ammonia over an acre of ground this adds 23 lbs. of nitrogen to this area, or, if we consider the top nine inches of the soil over this area of an acre, we add 1 lb. of nitrogen to each 120,000 lbs. of soil. Such a small amount that we might be doubtful whether it would be sufficient to make any appreciable difference to plants grown on this area. But we have seen that this quantity is of the same order as the quantity of available nitrogen already in the soil.

I cannot demonstrate to you in the course of a lecture the effect of nitrogen on the growth of an actual plant, but I can show you a cinematograph film which has been used for propaganda purposes in Germany. This film was produced by taking a photograph of the plants every twenty minutes—day and night—by means of a clock. Thus one second corresponds to about five hours' growth and one minute to twelve days.

We will now enquire into the magnitude of the increased yields of crops which can be obtained by the use of nitrogen fertilisers. The figures given in Table I are average increased yields of various crops obtained on good soil for the addition of each pound of nitrogen in a fertiliser. They are the averages over many years and many different soils, so that they are the increased yields that may be expected for the addition of each pound of nitrogen—if there is not a deficiency of potash or phosphoric acid in the soil.

TABLE I.

| 1. Crop. | 2. Increase for 1 lb. Nitrogen. | 3. Nitrogen in crop. | 4. Nitrogen Efficiency. |
|---|---|---|---|
| | lbs. | per cent. | per cent. |
| Wheat (grain) . . . . . | 11·4 | 1·8 | 20·4 |
| Barley (grain) . . . . . | 14·2 | 1·3 | 18·5 |
| Oats (grain) . . . . . | 12·4 | 1·6 | 19·7 |
| Potatoes (tubers) . . . . . | 94·0 | 0·3 | 28·2 |
| Swedes (roots) . . . . . | 94·0 | 0·2 | 18·8 |
| Mangolds (roots) . . . . . | 150·0 | 0·2 | 30·0 |
| Hay . . . . . . . | 42·3 | 1·45 | 61·5 |

The figures in column 2 are calculated from values in the Annual Report of the Rothamsted Experimental Station for 1929. The percentage of nitrogen in the crops was not determined—the figures given in column 3 are taken from published data of food analyses. The percentage of nitrogen in crops fertilised with artificial nitrogenous fertilisers are usually higher than in unfertilised crops, so that these figures are probably somewhat low. The nitrogen efficiency of the fertilisers in column 4 is the percentage of the nitrogen in the fertilisers which appears in whole or part of the crop described in column 2.

In Table II the increased yields are recalculated so as to show the increased crop obtained from 1 cwt. of sulphate of ammonia in common units.

TABLE II.

| Crop. | Increase for 1 cwt. of sulphate of ammonia. |
|---|---|
| Wheat . . . . . . . | 4·5 bushels or 2·41 cwts. |
| Barley . . . . . . . | 6·5 ,, or 3·02 ozs. |
| Oats . . . . . . . | 7·0 ,, or 2·62 cwts. |
| Potatoes . . . . . . | 20·0 cwts. |
| Swedes . . . . . . . | 20·0 ,, |
| Mangolds . . . . . . | 32·0 ,, |
| Hay . . . . . . . | 9·0 ,, |

If we feed grass to a cow giving 2 gallons of milk a day we find that 1 lb. of nitrogen causes sufficient extra grass to grow to keep the cow alive and to give 2 gallons of milk per day. Since 2 gallons of milk contain 0·8 lbs. of proteins, or 0·128 lbs. of nitrogen, we have 12·8 per cent. of the nitrogen of the fertiliser appearing in the milk, or the efficiency with which the fertiliser is used via grass to make milk is 12·8 per cent.

The efficiency of meat production is lower, 1 lb. of nitrogen fertiliser only producing 0·05 lbs. of nitrogen in beef, or an efficiency of 5 per cent. on the fertiliser.

Summing up these nitrogen efficiencies we have :—

| | | |
|---|---|---|
| Fertiliser to grain . . . . . . | about 20 per cent. | |
| ,, Potatoes . . . . . . | ,, 30 ,, | |
| ,, grass or hay . . . . . | ,, 55 ,, | |
| ,, milk . . . . . . | ,, 12·7 ,, | |
| ,, beef . . . . . . | ,, 5 ,, | or lower. |

It is not surprising that grass shows a higher efficiency than other crops because the roots cover the ground more completely. I think that the efficiencies on the whole are very high. Compare them with the energy efficiency of a high-class locomotive on the railways, which is not more than 8 per cent.

These efficiencies are the best estimates I can get of the average results obtained in this country. In good years when the rainfall and temperature are right the yields are much higher—in years of drought the extra yield obtained from nitrogen fertilisers is less. I have records of experiments where the nitrogen efficiency fertiliser to grass was as high as 80 per cent., and others in a year of drought when this efficiency was only 20 per cent. Though in this case a growth of grass in the winter after the experiment had been completed showed that the fertiliser had not been used during the summer.

### Food Production.

The food of a man in this country is approximately—

| | |
|---|---|
| Meat, Fish . . . . | 15·5 per cent. |
| Cereals . . . . . . | 18·5 ,, |
| Milk, cheese, &c. . . . | 24·5 ,, |
| Potatoes and roots . . . | 25·8 ,, |
| Sugar, Fruit, &c. . . . | 15·5 ,, |

and the nitrogen efficiency in growing these foods from fertilisers is probably about 17 per cent.

The amount of protein consumed per head is 86·5 grams per day ('Our Food Supply Before and After the War,' by A. W. Flux. Presidential Address to the Royal Statistical Society, June 1930). This contains 14 grams of nitrogen, so that if this food were grown by using fertilisers at an efficiency of 17 per cent. we should require to use 82 grams of nitrogen in fertilisers to produce the food for one person for one day. Or the fertiliser required to feed one person for a year must contain $365 \times 80$ grams or 29 kilos of nitrogen. One ton of nitrogen in the form of sulphate of ammonia or nitrate of soda will therefore produce enough food for thirty-four people for one year.

Since the total amount of nitrogen consumed in fertilisers during 1928 was 1,658,000 tons, the amount of extra food produced from this fertiliser would contain enough nitrogen in the form of proteins to support 56,000,000 people. And there would be sufficient carbohydrates and fat associated with this protein to form a complete diet.

Sir Daniel Hall in his presidential address to the Agricultural Section at Oxford showed that 2 to 2½ acres of land were required under cultivation to feed one person. Let us compare this with 1/34 tons of fixed nitrogen. If we assume that the total capital required to build a nitrogen factory is £70 to £100 per ton year of nitrogen, this is inclusive of everything, then for a maximum of £3 invested we can support one person. If would be impossible to bring 2 to 2½ acres of land under cultivation at so low a capital cost.

I do not think that land can usually be settled and cultivated at a less capital cost than £10 per acre, including roads and railways, houses and agricultural machinery, so that to bring 2½ acres under cultivation would need £25 capital as compared with £3 necessary to produce the fertiliser to produce the same amount of food. I would particularly like to draw attention to this calculation in some countries where governments are always ready to consider and finance schemes to build railways and roads to open up new country or to build irrigation schemes, although the capital to be invested for a given amount of food-producing capacity is often enormous. Let these countries rather devote attention to making full use of the land already cultivated near their consuming centres or on roads and railways, and when these are producing at their full capacity it will be time to open up new districts.

### The Stability of the Nitrogen Fixation Industry.

When we obtained increased yields of crops with artificial nitrogen fertilisers we are usually making use of the stored-up energy of the sun in past ages. We will therefore consider whether we are using our store of energy extravagantly in this way—in other words, is the game worth the candle ?

In fixing 1 ton of nitrogen and making it into fertilisers we use for all purposes about 5½ tons of coal, so that to provide the fertiliser to feed one person for a yeaı we require 3¼ cwts. of coal.

The population of the world (excluding China and Turkey) is now about 1,940 millions, and we have seen that 56 millions, or 2·8 per cent., are now being fed with food grown by nitrogen fertilisers.

Of the nitrogen fertilisers consumed in the year 1928 about 1,000,000 tons of nitrogen was produced by synthesis, needing 515,000,000 tons of coal. This quantity of coal is almost negligible when compared with 1,500,000,000 tons mined every year. The rest of the nitrogen was produced by bye-product coke-ovens or as nitrate of soda from Chili.

The population of the world increased by 10,000,000 each year from 1913 to 1928. If we had to feed this increase of population by increased nitrogen fertilisation we should have to build each year a works about one and a half times as big as Billingham. This works would fix 300,000 tons of nitrogen per year, and would cost upwards of £30,000,000. In order to run the works we should require 1.6 million tons of coal per year. If we built a works of this size every year for a hundred years we should then be consuming 160,000,000 tons of coal a year for nitrogen fixation, or only 10 per cent. of the coal which is being used in the world to-day. At least two-thirds of the coal consumed in the fixation of nitrogen is used for power production, so we could reduce the coal required to one-third the value mentioned if other sources of power were available.

There are still large areas of the world suitable for cultivation, and many of us saw such areas last year in South Africa and Central Africa. It is therefore improbable that all the food requirements for the growing population of the world will have to be supplied exclusively from nitrogenous fertilisers for some time to come.

Even if some generations hence such a time does come I feel assured we can look with confidence to our agriculturists, our chemists and our engineers being capable of finding a solution to the problem of supplying food for the growing population. Perhaps before long the present tendency of the birth rate to decrease with a higher standard of living will have spread to all nationalities and peoples, and the problem of over-population will never occur.

### The Distribution of Nitrogen Fertiliser.

Let us now investigate the use which the world makes of the nitrogenous fertilisers which are now available. I have already mentioned that of the 1,843,000 tons of nitrogen consumed in 1928, 1,658,000 tons or 90 per cent. was used in agriculture.

In Table III are shown the quantities of nitrogen consumed in the different countries during the year 1928.

Table III.

| — | Nitrogen consumed. | |
|---|---|---|
| | Metric tons, 1928. | lbs./acre arable. |
| Germany | 615,200 | 22·3 |
| France | 166,900 | 6·7 |
| Belgium | 63,600 | 45·9 |
| Czecho Slovakia | 33,800 | 5·0 |
| Denmark | 29,100 | 9·8 |
| Holland | 73,400 | 70·6 |
| Italy | 68,300 | 4·7 |
| Poland | 54,600 | 2·7 |
| Spain | 67,300 | 3·8 |
| United Kingdom | 61,600 | 10·4 |
| Total Europe | 1,134,800 | |
| U.S.A. | 383,600 | 2·4 |
| Japan | 113,300 | 16·8 |
| Egypt | 35,900 | 9·2 |
| Other Countries | 175,600 | |
| Total (World) | 1,834,200 | |

I wish that I could present to you a yearly flow sheet of the world's nitrogen cycle. How much atmospheric nitrogen is combined by electric discharges? How much by bacteria? How much by our synthetic ammonia processes? How much humus changes to give nitrate? How much nitrate is washed away, and how much goes into the crop? What happens to that going into the crop, and how much of it forms humus? What happens to the dissolved nitrogen going down the rivers into the sea? And how much comes back to land in the form of fish? Again how much nitrogen is liberated again from combination?

Is there a dynamic equilibrium in this nitrogen cycle, or are we drifting in one direction? Are we gaining nitrogen in the air or are we losing it?

We cannot get answers to these questions. There is no doubt that in the past nitrogen was stored up in coal, and in Chili nitrate and this is being liberated now—it is a mere drop in the ocean of nitrogen, but we do not know whether nitrogen is being stored up anywhere at the present time.

The vegetable foodstuffs consumed by man in the U.K. are estimated to contain 50,000 tons of nitrogen. If these are grown with a nitrogen efficiency of 20 per cent., then 250,000 tons of nitrogen is required in the soil. Of this nitrogen it is assumed that 50,000 tons go into vegetable foods 8,000 tons into straw which forms farmyard manure, and the rest 192,000 tons are washed out of the ground by rain water and lost to the rivers and seas.

It will be noticed that I have assumed that the wastage of nitrogen derived from humus is the same as the wastage of nitrogen from artificial fertilisers. I have no direct evidence for this. I have no evidence at all, but I cannot think of a more reasonable assumption than the one I have made.

I have taken no account of the animal and vegetable life on the moors and mountains except so far as it provides human food. Probably I have neglected some other important factors, but I make no apology for offering you the first attempt at a nitrogen flow sheet for the country.

We are now getting much better statistics of agricultural production than formerly, and I believe that consideration of these statistics with other statistics now available has opened up new fields of study in agricultural economics.

I have calculated the average amount of nitrogen obtained from an acre of crops in different countries.

The figures in the next table are for the year 1928. They were obtained by calculating the weight of nitrogen in each crop for each country and then adding up the total amounts of nitrogen for each country. This weight of nitrogen is then divided by the area on which the crops grew, and we get the weight of nitrogen in the crop in lbs. per acre average over the whole country. By crop we mean the portion of the crop taken away for consumption by man or animals, for instance of wheat the grain, of potatoes the tubers, &c. The rest of the crop usually goes back to the land and is considered as part of the agricultural system of the country.

TABLE IV.

| Year 1928. | lbs. per acre. | | |
|---|---|---|---|
| Country. | 1. Total Nitrogen in crops. | 2. Nitrogen in crops from artificial fertilisers. | 3. Nitrogen in crops from humus. |
| Denmark | 52·0 | 2·4 | 49·6 |
| Holland | 49·0 | 17·6 | 31·4 |
| Belgium | 47·2 | 11·5 | 35·7 |
| United Kingdom | 40·2 | 2·6 | 37·6 |
| Japan | 34·2 | 4·2 | 30·0 |
| Germany | 32·5 | 5·6 | 26·9 |
| Egypt | 31·6 | 2·3 | 29·3 |
| France | 23·5 | 1·7 | 21·8 |
| Canada | 21·8 | 0·1 | 21·7 |
| U.S.A. | 20·8 | 0·6 | 20·2 |
| Italy | 20·5 | 1·2 | 19·3 |

Since some crops give a larger yield of nitrogen in the useful part of the crop than others, the figures in the table are to some extent affected by the different crops and different proportions of each crop grown in a country. But, so far as I can see, the effect of the different crops grown is of only minor importance, and as the production of proteins is the farmer's business, we are not far wrong in considering column (1) as an index of the agricultural efficiency of that part of the country under crops. In the second column is given that part of the nitrogen in the crop which has been supplied by artificial fertilisers. It is assumed that on an average 25 per cent. of the nitrogen supplied to the land as fertiliser is found in the useful portion of the crop. The third column is the difference between the other two columns, and is the weight of nitrogen in the crop which has been supplied by the land. In countries with a good system of farming and a good rotation of crops this quantity is high. We see that the system of agriculture in Denmark produces more than twice as much as that in Canada, U.S.A. and Italy, and that in this country we are a little better than Belgium and considerably better than Holland in our agricultural system, apart from the use of artificial nitrogen fertilisers. But since Holland uses seven times, and Belgium four times, as much nitrogen fertiliser per acre, these two countries obtain crops which are greater than those obtained in this country, as is shown in column 1. In Holland one-third of the crops appears to be grown from nitrogen fertiliser. There seems to be no climatic or other physical reason why fertilisers should not be used to a greater extent in this country. If we used as much per acre as in Holland we should consume 420,000 tons of nitrogen per year; if as much as in Belgium, 272,000 tons; and if as much as in Germany, 132,000 tons. The reason that we do not use more fertilisers does not appear to be economic. The use of sulphate of ammonia yields 100 per cent. to 300 per cent. on the money invested within a year.

Let us consider the special case of the fertilisation of wheat. If we compare the price of wheat and of sulphate of ammonia it will be seen that for some years the price of these commodities has been practically equal. Since 1 cwt. of sulphate of ammonia gives an increased yield of wheat of 2·4 cwt., it is easy to see that, even after paying for phosphatic and potash fertilisers, 100 per cent. will be earned on the money spent on nitrogen.

I have already told you that I think that there is no need for us to be uneasy about the world's food supply. I realise that in the past there has been very real grounds for foreboding. Consider the population figures of England for a thousand years. The first increase of population was due to the increase in transport and trade in Tudor times, but the great increase is the effect of the industrial revolution. The industrial revolution took place first in this country, but other countries have since undergone similar changes, and similar increases of population have taken and are taking place.

In every district there is each year either an over-production of foodstuffs or a famine. Since the means of transport in the world are now so adequate we may consider, with very few exceptions, the whole world as one district. At present there is no famine in the world; there is therefore over-production of foodstuffs, and I hope there always will be such over-production. It is a pity that our present social system penalises the farmers and agricultural workers by giving them a standard of living lower than the rest of the community, because they have produced the crops we require for food and the necessary margin to insure us against famine.

We have seen that the development of the nitrogen fixation industry has lowered the price of nitrogen fertilisers, so that we can expect the needs of the increasing population of the world to be met first by more intensive fertilisation of land close to the markets for food, rather than by extension of the cultivated area. We have seen that less capital is required to build a nitrogen factory than to bring new ground under cultivation.

Sir William Crookes said 'The fixation of nitrogen is vital to the progress of civilised humanity,' and again, 'The fixation of nitrogen is a question of the not far distant future. Unless we can class it among the certainties to come the great Caucasian race will cease to be the foremost in the world, and will be squeezed out of existence by the races to whom wheaten bread is not the staff of life.'

To-day we can answer back to Crookes through thirty-two years that science and industry have completed the task he set them, and have assured the food supply of the world for several generations.

# CONFERENCE OF DELEGATES OF CORRESPONDING SOCIETIES.

The Conference met in the Victoria Rooms, Bristol, on September 4 and 9, under the presidency of Prof. Patrick Abercrombie.

Thirty-nine delegates signed the register, representing forty-four societies.

SESSION OF SEPTEMBER 4.

## NATIONAL PARKS: A RÉSUMÉ OF THE POSITION.

By PROF. PATRICK ABERCROMBIE, M.A., PRESIDENT OF THE CONFERENCE.

§ 1.

So much has been written and spoken on the subject of National Parks both before and since the appointment of the Prime Minister's Committee that it is difficult to say anything new while we are waiting for the Committee's report; but it may be useful to summarise some of the principal points that have emerged in discussions and writings upon the subject. All the important societies whose work touches the maintenance of our great open spaces have given evidence before the Committee, several of them have issued valuable pamphlets[1] summarising that evidence, and Dr. Vaughan Cornish has published an admirable brief book devoted chiefly to the scenic aspect of the case.

It seems to be generally agreed that by using the term 'National' it is implied that the area should be a large one. A small reservation, however beautiful or famous, while making a most suitable object for preservation under the National Trust, does not fulfil the requirements of a National Park. Several such areas of limited size, which have been brought forward, should probably be ruled out on that score; and those who are enthusiastic for their preservation should endeavour to secure them either as public open spaces to local authorities or to have them vested under the National Trust. Again, the very large area still remaining in the nature of common lands, though they may enter into and largely form some of the proposed National Parks, do not themselves actually rise to the importance implied in the term.

Many people dislike the name 'Park' as suggesting not only something sophisticated but definitely railed off and enclosed; but the American usage of the word appears to have become fixed, and there is perhaps a

[1] National Trust: C.P.R.E.: C.P.R.W., &c.

feeling about it of the heightening of interest such as one experiences in a private park in the country, which is not amiss; 'Domain,' however, used as it is in Ireland for a private park, also possesses this significance. It is important to remember that a National Park is or should be something more than a simple stretch of English country, however normally beautiful. To say that all England, except its towns, should be one great National Park is a mere rhetorical expression. The National Park or Domain should have a certain heightened interest, a quickening of the pulses in appreciation, and it will chiefly be found to coincide with areas of wild scenery. In America, it is true, the greatest National Parks have consisted of wild tracts untouched by man (except for his use *as* a National Park). In this country such unmitigated wildness can hardly be said to exist, except over very limited areas. The size necessary for a National Park will therefore generally be found to include a certain amount of sophisticated scenery and cultivated country; indeed, it will be necessary to include villages and even towns if there are not to be a number of lacunæ, which would become danger spots, unless they were controlled in order to be in keeping with the surrounding wildness. It is even rare to find English *wild* scenery entirely free from human additions or intrusions: the stone walls, so frequent in the Lake District and North Wales, quarries in use or abandoned, tracks and footpaths.

§ 2.

It might be suggested that a National Park should combine a threefold use; first the preservation of scenery, second the maintenance of flora & fauna, and third the provision of recreation. Of these it might be suggested that the scenic aspect would involve as far as possible the preservation of the *status quo*, with the provision, however, of increased access in order that it may be enjoyed more fully. The flora and fauna object suggests preservation of the *status quo* without access at all (if possible); and the third, normal recreational use, calls for a definite provision for change and growth. Are these three objects incompatible? Provided the area is sufficiently large they need not be; but it is clear that to satisfy and adjust the requirements of each requires the nicest sense of proportion and delicacy of touch. To over-emphasise the scenery and flora & fauna aspect would defeat the whole object of the human use of these glorious open spaces; while, on the contrary, to allow them to be overrun or exploited and especially to cater for a type of amusement which might be just as easily satisfied in less beautiful surroundings, would be equally to destroy the first two objects. The economic use of wild country, generally so rich in minerals, at once comes into conflict with scenic preservation. It requires a most careful and broadminded study in order to determine in certain cases which is in the real national interest, namely, to exploit a certain mineral or stone or to preserve untouched a certain piece of scenery or object of historic interest. At present the first is almost invariably given an overriding importance, which has to be upset very often at great expense; but the best interests of the country may quite possibly be served by obtaining the same product elsewhere. On the other hand, nothing could be more short-sighted than to prohibit all quarrying, &c., in certain districts. For example, in the

Lake District the buildings in the villages and surrounding can be most suitably roofed from slate quarries, which are located in the midst of the mountains and sometimes amid most beautiful scenery. As to flora & fauna, there should be no great difficulty in a large area in obtaining certain tracts of sufficient remoteness and refraining from making them accessible. Even a little active obstruction, provided it does not lead to unscaleable fences, could well be practised. Everyone knows certain mountain pastures which are almost inaccessible owing to bogginess or to extremely rough going through loose stones or scrub. The normal wanderer sticks to a well-known path.

As regards the recreative object, access to the National Park should be made as rapid and comfortable as possible, but once arrived in the domain reasonable facilities for getting about should be aimed at, but no more. Speed tracks should not be encouraged, and people who have almost forgotten how to walk must be content to miss some of the choicer beauties of the area. A comparison of the Lake District and North Wales will show how more fortunate is the former in that the main roads that traverse it are so few and have to rise up so high as compared with the more level valley bottoms that lead through Snowdonia in nearly all directions. At the same time it is interesting to note that there is a larger massif of unpenetrated mountain area in North Wales than there is in the Lakes.

§ 3.

With regard to the methods of obtaining areas, these fall under three headings. In the first place and simplest there is the use of existing Crown lands; thus it might be said that the New Forest and the Forest of Dean are practically National Parks already. At most a slightly different attitude of the Crown Departments to their property would be necessary, such as with regard to economic afforestation or the selling of land for building purposes. Both these uses of the land might be changed, radically or imperceptibly, if the areas were to be primarily used for recreation.

The second method, the most complete for the purpose, would be the purchase of private property, giving more thorough possession even than the Crown lands, because the latter are all subject to very considerable easements, whereas the rights for private property, including minerals, would be practically entire. But, attractive though this would be, the expense involved even for wild and barren land would be prohibitive. It is probable and desirable that large areas should continue to be purchased even within the National Park, and to be vested in the National Trust, as has been done in the Lake District; and it is hoped that these trust ownerships will continue and will include both agricultural as well as wild country, as is the case in the Langdale Valley. There are certain places which everyone must feel should be in full public ownership.

For the rest the third type of holding for a National Park appears to be inevitable, namely, a control of private ownership on somewhat the same lines as that exercised under the Town Planning Act for growing areas. Such control of private ownership would have to be of a stringent nature in order to safeguard the special amenities of a National Park, and it is therefore more than probable that questions of compensation to owners will arise. If the park is for national use this compensation should be found nationally rather than locally. This would be the chief distinction between a National Park and land normally controlled under the Town Planning Act. In considering the sort of scheme which might be prepared for the typical composite area,[2] which would form a National Park, the following division of its ground into different types of area or zone has been suggested. It will be found that this composite area, as has been already hinted, may contain both Crown lands, Common lands and National Trust property already. These properties would fall into place and would create no financial question; they would most of them be found in the first type of zone or reservation, which is to be now described.

§ 4.

The *first* zone would be that which should, so far as practicable, be untouchable; it would apply in a mountain area to the high ground, and could in certain cases be delimited by a fairly constant contour line. This type of ground will not usually be expensive to sterilize—it is for the most part inaccessible and also difficult to build upon. At the same time there may be places where a main road crossing a path invades it and brings along with it all the perils of accessibility and even attractiveness for certain uses, e.g. petrol filling stations, refreshment places and the Germanic vice of Aussichtpunkt sophistication. Minerals, also, are no respecters of solitude, and electric power and overhead railways render places exploitable. Water catchment areas work both ways: in one way they are an effective check to development, and in one case have led to the purchase of mineral rights over an area of 30,000 acres. In other ways, however, they alter scenery by flooding valleys and by planting, which may be necessary for water conservation. The other characteristics of this zone would be its unobstructedness; it should be open to human beings and to wild life, though not everywhere concurrently.

The *second* zone, while scenically of as great importance as the first, would not necessarily be as accessible. For most composite areas in this country quite highly cultivated farming land will be found running up the valley bottoms into the troughs of the hills. Prof. Trevelyan clearly demonstrated this when he gave the valley farms of Langdale to the National Trust: they were not to be diverted from their present use. But in these valleys not only are farm buildings required, but an occasional outlander's house is found; and if the National Park is to be fully used, hostels or other accommodation will have to be placed somewhere in them. The greatest care is therefore necessary that every change and addition shall not damage the natural setting; the human additions shall here be relegated to an unobtrusive position in the scheme of things, as recommended by Wordsworth in the excellent rules he drafted for building in the Lake District. In working out this zone the normal considerations governing land suitable for building should not apply: a much more drastic policy is required. The area is not a residential one nor a manufacturing one, but a National Park, and its prime function must not be lost sight of. Thus it is not wise to lay down long lines of electric power

[2] This, as mentioned above, does not refer to those areas which are almost entirely of Crown property, such as the New Forest and the Forest of Dean.

(unless generated on the spot by water) which at a later date may tempt the purveyors of current to seek for a local market, and promote development.

The *third* type of zone would be in the nature of normal village or town areas, with extensions to include tracts which are suitable for what are called 'residential areas.' The proper 'industry' of a National Park will require headquarters, and there are always certain people who wish and are able to retire to live amid scenery which naturally falls within the National Park's periphery. Both these needs can be supplied, and a good example of what is meant can be seen in Keswick for the former, and the area between Keswick and Bassenthwaite for the latter. Full town-planning control, including of course that of the design and materials of elevations, must be exercised with thoroughness and reality, and there must be no indeterminate straggling and sprawling, just because road frontage may happen to be available. Here, clearly, there will be probable claims for compensation; in normal parts of the country the axiom may be stated that to group building on certain lands and to banish it from others will not diminish the total value of the land. What is diminished in one part is enhanced in another; the only difficulty is the unevenness of incidence on private ownership. But in a National Park it may be essential to limit development for the sake of the major object—the national enjoyment of wild scenery and country.

§ 5.

It is worth while considering for a moment what sort of organisation should plan and control an area which it is decided should be a National Park. Clearly the usual group of local authorities which join together for a regional planning scheme would be insufficient for the purpose. If funds for compensation or purchase or maintenance are to be forthcoming from national sources, the nation will require some say in the way they are used. There should be a central body to co-ordinate the work but not to control all the areas: these should be under local management, upon which should be represented the local authorities, including, of course, the county councils; the specially interested national societies such as the C.P.R.E., National Trust, Rural Community Councils, Commons, Open Spaces and Footpaths Preservation Society, Society for the Promotion of Nature Reserves, Society of Antiquaries, Society for the Preservation of Ancient Buildings, the Holiday Fellowship, &c., also the local societies where these are of sufficient standing. It would probably also make for smooth working to have representatives of the landowners and of the owners of water and mineral rights. A comparatively simple addition to the existing Town Planning Act (which is about to be amended) could be made to provide for this widened type of 'Responsible Authority.' The bulk of the machinery of this Act (in spite of its misnomer) is applicable to such special areas; and especially valuable is the fact that each scheme prepared, when approved, has the status of an Act of Parliament.

§ 6.

It would require a long paper to describe and to give reasons for the most suitable areas for National Parks; the C.P.R.E., in its evidence [3]

[3] Published by C.P.R.E., 17 Great Marlborough Street, London.

before the Committee, gave a comprehensive list to which Dr. Vaughan Cornish has now added an enlarged version of the Northumberland area, which might rank as a central park for Great Britain.

In selecting suitable areas the threefold use must be always in view, and especially the human recreational one. The relation of the population map to the wild country map must therefore be carefully studied; it would not do to choose one or two areas of remote wildness which would leave vast tracts of Britain without any area at a convenient distance. Nature and man, it is true, have not always concentrated their peak developments near the same spots, though in the north and west this *has* occurred. The Metropolis and East Anglia present the chief problems as to provision at a convenient distance. The South Downs and Norfolk Broads, both fulfilling many of the requirements, may need supplementing in extent.

§ 7.

The finance of the National Parks will doubtless receive full attention from the Committee in their report; but if for the so-called composite areas control rather than purchase is recommended, there are not really large sums, nationally considered, involved. It is surprising for what a small amount of money large areas even of good building land not far removed from centres of population could be reserved, to bring in as good a return to the landowner as sales, scattered as they are in distance and time. After all there is only a strictly limited amount of land accessible from existing roads without enormous expenditure on estate development. In wilder country this is intensified; except for a few feet depth from a few roads and a few valleys that lend themselves to exploitation, the building value in relation to anticipated rate of development is extremely low. Mineral rights are in most cases far more serious, and in some cases (fortunately not many) sporting rights are likely to cause heaviest compensation. The Peak and other moorland areas of Derbyshire which, from a purely population point of view, have perhaps first claims of urgency, are extremely highly preserved. The best behaviour in the world on the part of large numbers of users will not consort with the habits of stimulated grouse breeding.

If national purchase is to any degree necessary, a case can be made out for mineral and water power rights. The recent mineral purchase already alluded to was carried out to the satisfaction of both owner and purchaser; there is by this means an end to the most likely cause of disagreement in the administration of an area. But again it is necessary to state that nationalisation of minerals and water power does not mean the prevention of their working; it means the adequate consideration of the expediency of working from all points of view.

The Welsh evidence[4] contained an interesting financial proposal contributed by Mr. Clough Williams-Ellis, chairman of the executive, for the formation of a Statutory Company or Trust, in which all property owners should be invited to join, being issued with shares to the equivalent value of their holdings. This scheme presupposes no diminution, even an enhancement of values as a result of the establishment of a National Park.

[4] Council for the Preservation of Rural Wales: National Parks, pamphlet 4.

There is much to be said for this optimistic view, which would not then contemplate any drain upon the national exchequer.

§ 8.

Two further essentials of the scheme for providing National Parks are to be noted: firstly, the whole surroundings and setting of the area, however large, must be under the control of regional planning schemes. The approaches to nationally safeguarded property are frequently made the object of disgraceful speculation and the defacement of advertisements and litter; on a small scale the proposed amendment of the Ancient Monument Act is intended to protect the setting as well as the object itself. Similarly, the setting of the park—its approaches and the settlements upon its periphery—must be properly planned and safeguarded.

And this solicitude for the setting naturally leads to the final conclusion that, rightly considered, the provision of National Parks is merely one aspect of National Planning. It is quite true that many aspects of national existence are being planned on a national scale: roads and rail transport, electric power, afforestation, water supply (or if not yet, this soon will be). But these different aspects of the national plan are not yet considered *in conjunction*, nor are they adequately related to a national scheme of industrialism. Thus, we find attempts made to drive a through trunk route across a mountain region, without considering the damage it will do to it as a National Park. Ease of communication, minimising distances, has so reduced the apparent size of the country that a plan is now essential. And as playgrounds are to the towns so are National Parks to England, Scotland and Wales.

A hearty vote of thanks was accorded to Prof. Abercrombie for his valued address.

In the discussion following the Presidential Address Mr. J. A. Leckie, representing the Cannock Chase National Park Committee, read a paper dealing at length with the claim of Cannock Chase as a National Park.

Dr. Vaughan Cornish read a communication proposing the Roman Wall (Northumberland) as a National Park for Central Britain, in which he gave details of an area of approximately one hundred square miles in extent, embracing the Tynedale and Hexhamshire moors in Northumberland with the Roman Wall as the centre, and bounded on the east by the North Tyne River from Wark to the junction of the South Tyne, which, with its tributary the Tipalt Burn and some two and a half miles of road, forms the southern boundary, via Gap Shields as far as Gilsland. Thence the river Irthing forms the western boundary as far as Lampert, while the northern is bounded by the Wark Burn. Dr. Cornish referred especially to the history and amenities of the area and to its central position, and concluded that the proposed park would be accessible from all parts of Great Britain.

Dr. G. F. Herbert Smith, representing the Geological Society, also spoke.

Arising out of the President's Address and the discussion which followed, the following resolutions were approved and adopted:—

1. 'That the Council of the British Association be asked to represent to His Majesty's Government the need for the establishment of Nature Reserves in any project for the creation of National Parks.
2. 'That the Council be asked to appoint a Committee to take cognisance of proposals relating to National Parks by the Government and other authorities and bodies concerned, and to advise the Council as to action if desirable.'

The Conference recommended the appointment of Prof. Abercrombie, Mr. T. Sheppard, Prof. W. M. Tattersall, and Dr. C. Tierney (representing the Corresponding Societies), and that Section C (Geology), D (Zoology), E (Geography), H (Anthropology) and K (Botany) be asked to nominate a representative to serve on the Committee proposed in Resolution 2.

The Conference then considered the subject of Folk Museums and their need of establishment in this country.

Miss G. V. Barnard, representing the Norfolk and Norwich Naturalists' Society, gave an interesting account, illustrated by lantern slides, of the Strangers' Hall at Norwich, an attractive fifteenth-century domestic building now a folk museum, and of the successful endeavour to exhibit therein objects relating to the rapidly disappearing popular arts and crafts of the British people in what corresponds to their original setting.

A communication was received from Dr. Cyril Fox, of the National Museum of Wales, describing a visit to Scandinavia for the study of the character, extent and mode of exhibition of collections in the north illustrating national ethnography. These collections not only contain material exhibiting the pre-history and early history, but also material illustrating the finer craftsmanship of the country in historical sequence. In addition, buildings, domestic dwellings, barns, stables, churches, mills, workshops, &c., all appropriately furnished, are spaciously and fittingly set out illustrating the mode of life and the immediate environment of the people from the earliest times for which such collections are available down to the nineteenth century.

The interest aroused in Scandinavia among all classes of the population by these folk museums is remarkable. They form regional and national centres for folk dancing, folk music and the maintenance or revival of craftsmanship, and are visited by architects, craftsmen and others for ideas. In one case instruction is given in no fewer than forty separate crafts, covering the whole range of normal human needs, resulting in the well recognised development of the arts and crafts on traditional lines. There are no parallels to these field museums in this country, and Dr. Fox urged the establishment of these in Britain.

A discussion followed, in which Miss Marion Frost, Prof. J. L. Myres, Mr. W. E. Swinton and others took part, and the following resolution received from Section H (Anthropology) was approved and adopted without expressing any opinion as to the best site :—

> 'In view of the increasingly rapid disappearance of material relating to the popular arts and crafts of the British people, the Committee of Section H requests the Council to ask His Majesty's Government to put into effect the recommendation of the Royal Commission on National Museums and Galleries for the establishment of a National Open-air Folk Museum in London.
>
> 'The Committee further suggests that the Government might consider the possibility of utilising the Royal Botanic Garden in Regent's Park for the purpose, in view of its admirable situation and the proximity of a building (St. John's Lodge) suitable for exhibition purposes and offices, providing this can be done without interfering with the scientific work already in progress on the site.'

## Session of September 9.

At the resumed session of the Conference held at 2 p.m. on Tuesday, September 9, Mr. T. Sheppard, Vice-President, in the chair, Dr. J. R. Ashworth, representing the Rochdale Literary and Scientific Society, directed the attention of delegates to the importance of the registration of ultra-violet intensity of sunlight in towns and country. He exhibited charts showing the seasonal variations registered in various districts and urged the desirability of obtaining regular and adequate records throughout the British Isles, especially in industrial areas, and invited those members of the Corresponding Societies willing to assist herein to communicate with him at 55 King Street South, Rochdale.

The Conference then discussed the subject of co-operation between scientific societies. The chairman, in opening the discussion, gave an account of the history and development of the first union of naturalists, now known as the Yorkshire Naturalists' Union, which was founded at Heckmondwike in 1862. This union consists of some forty affiliated societies, covering the whole of the five Watsonian vice-counties into which Yorkshire is divided, and has an aggregate membership of approximately four thousand. In addition to the annual meeting of the Union, which is attended by representatives of the societies affiliated thereto, the Union organises through its committees visits to each of the five vice-counties for the special purpose of botanical, zoological, geological and other scientific investigations, the results of which are published in *The Naturalist*, the official organ of the Union. Several monographs on local and regional distributions have also been published as a result of these researches. Mr. Sheppard regretted the declining interest taken in these field investigations, and urged their importance and the desirability of co-operation as the only means of obtaining co-ordinated results. Mr. Sheppard's remarks are published *in extenso* in *The Naturalist* for December 1930.

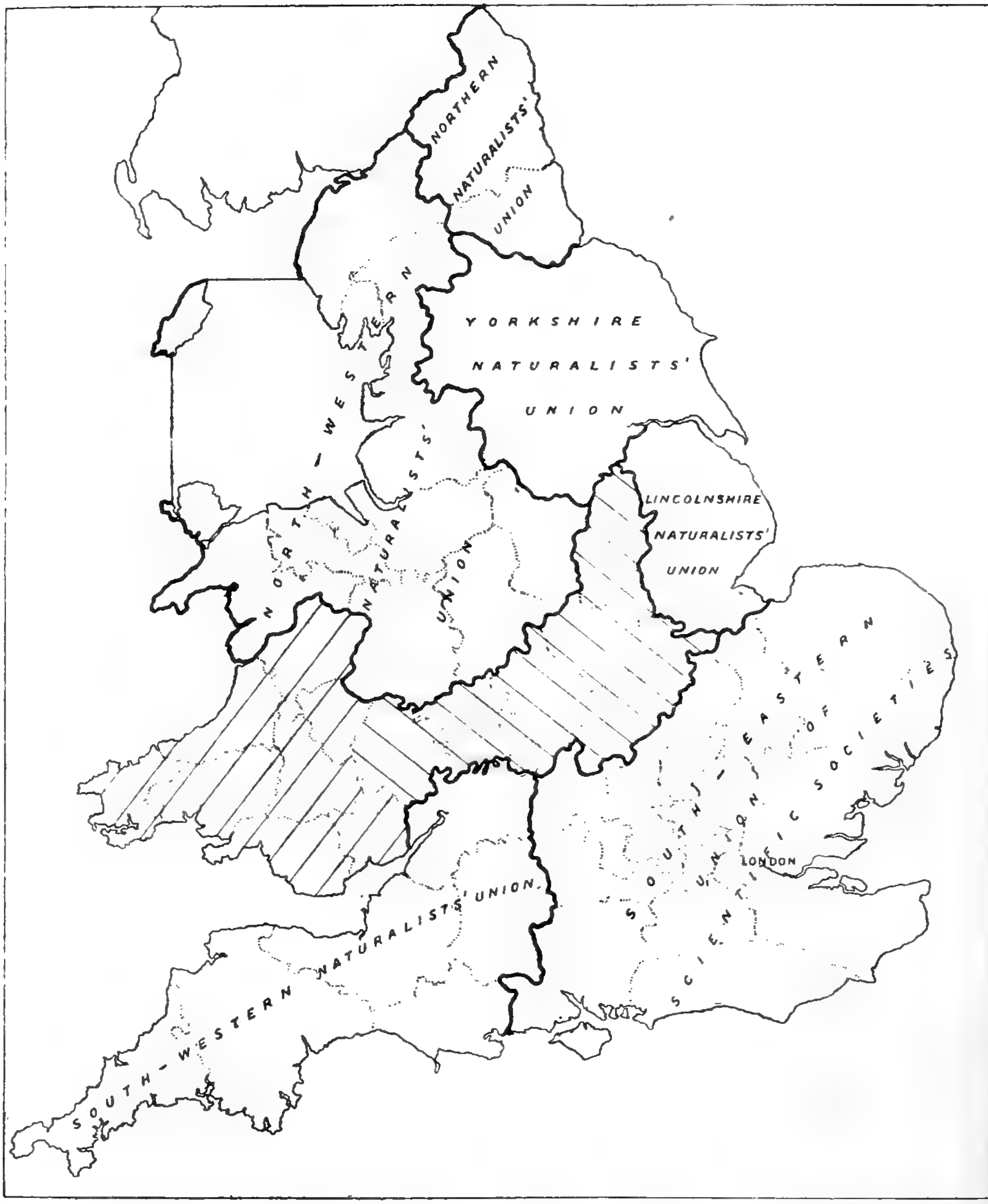
Mr. J. V. Pearman, representing the South-Western Naturalists' Union, summarised the constitution and activities of that body, urging those societies in the south-western counties of England not already affiliated thereto to become members of the Union and thus strengthen the organisation.

Mr. J. H. Danvers, representing the Southport Society of Natural Science, gave a brief summary of the newly-formed North-Western Naturalists' Union and of its contemplated work, while Dr. B. Millard Griffith, representing the Northern Naturalists' Union, similarly summarised the aims and objects of that body.

Dr. C. Tierney, representing the South-Eastern Union of Scientific Societies, called the attention of the delegates to a map of England he had prepared showing that the whole country was already divided into effective federations or unions of scientific societies, viz. the Northern Naturalists' Union, which embraced the northern counties, the Yorkshire Naturalists' Union, embracing the whole of Yorkshire, the North-Western Naturalists' Union, embracing the north-western counties, including the Isle of Man, the South-Western Union, embracing the south-western counties, and the South-Eastern Union, which included the south-east of England. Dr. Tierney drew attention to a remarkable gap across the middle of England, with Birmingham as the centre, including the counties of Hereford, Worcester, Warwick, Northampton, Leicester and Nottingham, where no organisation exists, so far as can be ascertained, for the co-ordination of scientific effort in that area, and he urged delegates present from those counties to bring the matter to the notice of their respective societies with a view to considering the desirability of co-operation and the establishment of a union in that area, to their own mutual advantage.

Dr. Tierney then described the constitution and working of the South-Eastern Union of Scientific Societies, an organisation comprising some seventy affiliated societies throughout the south-eastern counties, having an aggregate membership of approximately ten thousand. He dealt especially with the merits and advantages to be derived by the participating bodies as a result of co-operative and co-ordinated effort in scientific work, especially in matters pertaining to local records and regional distributions, as has been exhibited by the valuable regional survey work conducted by the South-Eastern Union. As an instance of the mutual value and importance of such organisations, Dr. Tierney referred to a request received some little while ago from the Ministry of Health for information as to the prevalence and distribution of a little-known species of mosquito, *Anopheles plumbeus*, throughout the south-eastern counties of England, which was thought to be a potential vector of introduced malaria. For this reason a knowledge of its prevalence and distribution was of obvious importance to the Ministry and to the community. The South-Eastern Union undertook the task of obtaining the information required, and through the loyal co-operation of its affiliated societies was able in due course not only to issue a very adequate report thereon, which was officially acknowledged as very valuable, but also to describe hitherto unknown details as to the development and life-history of the organism.

Dr. Tierney further directed the attention of delegates to the responsibilities of local societies as custodians of local records of scientific importance, such as geological exposures, botanical, archæological and other surveys, as well as the natural amenities of their own areas. He emphasised the desirability of securing the advantages and support to be derived from affiliation to such an organisation as a union of scientific societies, especially in matters where local amenities are threatened. He drew attention to the benefits accruing to local societies from such affiliation, not only in matters of co-operative investigation of regional problems and the publication of scientific results, but also in affording personal contact with competent workers and lecturers in districts other than their own.




Map to illustrate existing unions of scientific societies in England, Wales, and Isle of Man. County boundaries, so far as not coincident with union boundaries (heavy lines), are shown in dotted line. Areas shown by cross-shading are in no union.

In conclusion, he pointed out that the strength and effectiveness of such unions is largely dependent upon the support they receive from the local societies within their areas. The existing unions, particulars of which are given below, cover the whole of England with the exception of the Midland area previously referred to, and he urged that those local societies not already co-operating with their respective union should affiliate therewith to their own mutual advantage and to the advancement of scientific knowledge and investigation by co-operative and co-ordinated effort.

The Chairman, in summing up what had proved to be an interesting and valuable discussion, reminded delegates of the useful service they could render in reporting these proceedings to their respective societies, in order that the suggestions which had been so well and adequately put before them might be considered with a view to their effective adoption.

Existing Unions of Scientific Societies.

| *Name.* | *Area.* | *Secretary.* |
|---|---|---|
| Northern Naturalists' Union | Durham and Northumberland. | F. C. Garrett, D.Sc., Westcroft, Hexham. |
| Yorkshire Naturalists' Union. | Yorkshire. | F. A. Mason, F.R.M.S., 29 Frankland Terrace, Chapel Town, Leeds. |
| North-Western Naturalists' Union. | Cheshire, Cumberland, Derbyshire, Lancashire, Shropshire, Staffordshire, Westmorland, N. Wales and the Isle of Man. | F. J. Brown, M.Sc., Zoology Dept., The University, Manchester. |
| Lincolnshire Naturalists' Union. | Lincoln. | A. Smith, F.L.S., The Museum, Greyfriars, Lincoln. |
| The South-Western Naturalists' Union. | Cornwall, Devonshire, Dorsetshire, Gloucestershire, Somersetshire and Wiltshire. | J. V. Pearman, F.E.S., 32 Cornwallis Crescent, Clifton, Bristol. |
| The South-Eastern Union of Scientific Societies. | Bedfordshire, Berkshire, Buckinghamshire, Cambridgeshire, Essex, Hampshire, Hertfordshire, Huntingdonshire, Kent, London, Middlesex, Norfolk, Oxfordshire, Suffolk, Surrey and Sussex. | E. A. Martin, F.G.S., 10 Howden Road, S. Norwood, S.E. 25. |

# REFERENCES TO PUBLICATION OF COMMUNICATIONS TO THE SECTIONS

## AND OTHER REFERENCES SUPPLIED BY AUTHORS

The names of readers of papers in the Sections (pp. 293–425), as to which publication notes have been supplied, are given below in alphabetical order under each Section.

References indicated by 'cf.' are to appropriate works quoted by the authors of papers, not to the papers themselves.

General reference may be made to the issues of *Nature* (weekly) during and subsequent to the meeting, in which summaries of the work of the Sections are furnished.

### Section A.

Brett, Dr. G. F.—*Proc. Roy. Soc.* A, Oct. 1930.

Bureau, R.—Expected to appear in *Quart. Journ. Roy. Meteorol. Soc.*; cf. *Comptes Rendus, Assoc. Française pour l'Avancement des Sciences,* 1929, p. 287; *Comptes Rendus, Académie des Sciences*; **191,** p. 64 (1930).

Dirac, Dr. P. A. M.—Expected to appear in *Nature*; given more mathematically in *Proc. Roy. Soc.* A., **126,** p. 360 (1930).

Herzberg, G.—Prelim. notes in *Nature* (July 26, 1930, and Aug. 16, 1930), will probably appear in extended form in *Zeitschrift für Physik.*

Jackson, Dr. L. C.—Will appear in *Proc. Roy. Soc.*

Ower, E.—Cf. *Encyc. Brit.* 'Anemometry,' 14th Edition; 'Measurement of Air Flow' (Chapman & Hall, Ltd.); *Reports & Memoranda of Aeronaut. Res. Cttee.,* No. 981; *Phil. Mag.,* Nov. 1926; *Journ. Sci. Instruments,* Jan. 1926, and Dec. 1927; *Colliery Engineering,* Apr. 1927; *Phil. Mag.,* Oct. 1930.

Powell, Dr. C. F.—On 'Mobility of Ions,' *Proc. Roy. Soc.,* **129,** p. 162 (1930); on 'Metastable Molecules in Active Nitrogen,' to appear in *Proc. Roy. Soc.*

Stoneley, R.—Expected to appear in *Gerlands Beiträge für Geophysik.*

Sucksmith, W.—*Proc. Roy. Soc.* A, **128,** p. 276 (1930).

Tyndall, Prof. A. M.—*Proc. Roy. Soc.,* **129,** p. 162 (1930).

Watt, R. A. W.—*Quart. Journ. Roy. Meteorol. Soc.,* Jan. 1931.

### Department A*.

Berwick, Prof. W. E. H.—Cf. *Proc. Lond. Math. Soc.* (1920); expected to appear in *Math. Gazette,* Jan. or March (1931).

Bosanquet, Dr. L. S.—Expected to appear in *Math. Gazette* (Dec. 1930); cf. *Proc. Lond. Math. Soc.* (2) **31,** pp. 144–164; *Journ. Lond. Math. Soc.*; further paper to be presented to *Lond. Math. Soc.*

Chaundy, T. W.—*Quart. Journ. Maths.* (Oxford Series), **1,** pp. 187–195 (1930).

Comrie, Dr. L. J.—Cf. *Monthly Notices, Roy. Astron. Soc.,* **88,** 506 (Apr. 1928); **88,** 447 (Mar. 1928); *Office Machinery Users' Assn. Trans.* (1927–8); to appear in extended form in *Monthly Notices, Roy. Astronom. Soc.*

Dixon, Prof. A. C.—Cf. *Proc. Lond. Math. Soc.,* 2, **22,** pp. 201–222; **27,** pp. 233–272.

Fisher, Dr. R. A.—*Proc. Camb. Phil. Soc.,* **26,** pp. 528–35 (1930); cf. *Phil. Trans.* A, **222,** pp. 309–368 (1922); *Proc. Camb. Phil. Soc.,* **22,** pp. 700–25 (1925).

Henderson, Dr. J.—To appear in *Math. Gazette,* **15**; cf. *Tracts for Computers,* **13,** Camb. Univ. Press (1926).

Hodge, W. V. D.—To appear in *Journ. Lond. Math. Soc.*

Irwin, J. O.—Cf. *Tracts for Computers*, **10**, Camb. Univ. Press (1923).

Titchmarsh, Prof. E. C.—*Quart. Journ. Math.* (Oxford Series), **1** (1930).

Wishart, Dr. J.—Paper on 'The Derivation of Pattern Formulæ of Two-way Partitions from those of Simpler Patterns' expected to be published in *Proc. Lond. Math. Soc.*; cf. Fisher, *Proc. Lond. Math. Soc.*, **2**, 30 (1929), pp. 199–238; Wishart, *Proc. Lond. Math. Soc.*, **2**, 29 (1929), pp. 309–321: *Proc. Roy. Soc. Edin.*, **49** (1929), pp. 78–90; *Biometrika*, **22** (1930).

Wrinch, Dr. D. M.—Expected to appear in *Phil. Mag.*

### Section B.

Baly, Prof. E. C. C.—Cf. *Proc. Roy. Soc.*, A, **116**, pp. 197, 212, 219 (1927); **122**, p. 393 (1929).

British Dyestuff Industry.—Discussion published by *Assn. Brit. Chem. Manufacturers; Chemical Age; Journ. Soc. Chem. Industry.*

Britton, Dr. H. T. S.—Cf. *Journ. Chem. Soc.* pp. 1249–1261, pp. 1261–1274, pp. 2154–2166 (1930), and in issue of Nov. 1, 1930.

Francis, Prof. F. E.—*Proc. Roy. Soc.*, A, **128**, pp. 214–252 (1930).

German, W. L.—*Journ. Chem. Soc.*, pp. 1249–1261, and pp. 2154–2166 (1930).

Henry, Dr. T. A.—*Biochemical Journal*, **24**, No. 4, pp. 874–890 (1930).

King, Dr. H.—*Chemistry and Industry*, **49**, 786 (1930).

Malkin, Dr. T.—*Proc. Roy. Soc.* A, **128**, pp. 214–252 (1930).

Piper, S. H.—*Proc. Roy. Soc.* A, **128**, pp. 214–252 (1930).

Pope, Prof. Sir W., and Whitworth, J. B.—*Chemistry and Industry*, Sept. 5, 1930, p. 748.

Pyman, Dr. F. L.—*Chemistry and Industry*, Sept. 12, 1930, p. 757.

Robinson, Dr. R. A.—*Journ. Chem. Soc.*, 1261–1274 (1930); *ibid.*, Nov. 1, 1930.

Soper, Dr. F. G.—Expected to appear in *Journ. Chem. Soc.*

### Section C.

Heard, Dr. A.—Expected to appear in *Quart. Journ. Geol. Soc.*

Jones, J. F.—Expected to appear in *Quart. Journ. Geol. Soc.*

Trueman, Dr. A. E.—Cf. *Naturalist*, p. 321 (1930).

Versey, Dr. H. C.—Expected to appear in *Naturalist.*

### Section D.

Cannon, Prof. H. G.—On 'Cypridina (Doloria) levis,' and on 'Nebaliopsis typica,' to be published in *Discovery Reports* by Discovery Committee of Colonial Office.

de Lange, Dr. D.—Cf. *Koninkl. Akad. v. Wetenschappen te Amsterdam, Proc.*, **18**, 1916; *Compte Rendu, Assoc. d. Anat.*, Lyon, 18e Réunion (1923); *Anat. Anzeiger*, 58 (1924); *Compte Rendu du 3e Congrès Internat. des Anatomistes, Amsterdam* (1930).

Eales, Dr. N. B.—*Proc. Zool. Soc.*; cf. *Trans. Roy. Soc. (Edin.)*, **54** (1926); **55** (1928); **56** (1929).

Jenkin, Miss P. M.—In more detailed form in *Internat. Revue des ges. Hydrobiologie u. Hydrographie*; cf. *ibid.*, **24**, H.1/2, p. 25 (1930).

Kerr, Prof. J. G.—Cf. 'The work of John Samuel Budgett,' Camb. Univ. Press, 1907.

Manton, Miss S. M.—Expected to appear in *Proc. Zool. Soc.*

Orr, A. P.—Report of Great Barrier Reef Expedition published by Trustees of Brit. Mus.

Pearman, J. V.—On 'Life Histories': *Proc. Bristol Nat. Soc.*, **6**, pt. v., p. 384 (1927); *Ent. Month. Mag.*, **63**, p. 197 (1927); **64**, pp. 209, 239, 263 (1928); **65**, p. 89 (1929). On 'Sound Production': *Ent. Month. Mag.*, **64**, p. 179 (1928).

Regan, Dr. C. Tate.—*Ann. Mag. Nat. Hist.* (10), **6**, pp. 383–392 (1930).

Russell, F. S.—Report of Great Barrier Reef Expedition published by Trustees of British Museum.

Sladden, Miss D. E.—Expected to appear in *Proc. Roy. Soc.*; cf. *ibid.*, B, **106** (1930).

Swinton, W. E.—To appear in 'Liassic Plesiosaurs' (Trustees of Brit. Mus.); cf. *Ann. Mag. Nat. Hist.*, Ser. 10, **6**, p. 206; *Nat. Hist. Mag.*, **2**, No. 16, p. 271 (Oct. 1930).

Uvarov, Dr. B. P.—Cf. *Bull. Ent. Res.*, **12**, pp. 135–168; **14**, pp. 31–39; **20**, pp. 261–265; Handbook published by Imperial Bureau of Entomology.

Williams, C. B.—In extended form as 'Migration of Butterflies' (Williams), Oliver and Boyd (Edinburgh).

Yonge, Dr. C. M.—The Great Barrier Reef Expedition, 1928–29, **1**, 1930, Brit. Mus. (Nat. Hist.).

### Section E.

Brooks, Dr. C. E. P.—*Quart. Journ. Roy. Meteorol. Soc.* (1931) (Jan. or April).

Fogg, W.—Expected to appear in *Scottish Geog. Mag.*

Lennard, Col. E. W.—To appear in *Year Book, Royal Empire Soc.* (Bristol Branch) (1930); expected to appear in *Geography*; cf. 'Bristol, Empire City,' *Journ. Roy. Colonial Inst.*, **15** (July 1924).

### Section F.

Cathles, A.—Cf. 'Principles of Costing' (Gee & Co.); 'Accounts, Ancient and Modern' (*Accountants' Mag.*); 'Costing' (*The Accountant*); 'Devising a Costing System' (*Incorporated Accountants' Journal*).

Jones, D. Caradog.—Cf. *Jour. Roy. Statist. Soc.*, **93**, pt. iv. (1930) (two papers).

Mess, Dr. H. A.—Cf. 'Industrial Tyneside' (Ernest Benn, Ltd.).

### Section G.

Abell, C. F.—*Engineering*, Sept. 5, p. 308 (1930).

Batho, Prof. C.—*Engineering*, Oct. 3, p. 445 (1930); Sept. 26, p. 407 (1930).

Cook, R.—*Engineering*, Sept. 5, p. 290 (1930).

Damant, Capt. G. C. C.—*Engineering*, Sept. 19, p. 351 (1930).

Davies, Dr. S. J.—*Engineering*, Oct. 24, 1930; cf. *ibid.*, Sept. 12, p. 318 (1930).

Davis, R. H.—*Engineering*, Sept. 19, p. 351 (1930).

Doerr, Herr Direktor W. E.—To appear in *Engineering*; *ibid.*, Sept. 19, p. 353 (1930),

Fowler, Sir H.—*Engineering*, Sept. 19, p. 380 (1930).

Giffen, E.—*Engineering*, Oct. 24, 1930; cf. *ibid.*, Sept. 12, p. 318 (1930).

Hill, Prof. Sir L.—*Engineering*, Sept. 19, p. 351 (1930).

Hurley, T. F.—*Engineering*, Sept. 5, p. 290 (1930).

Moir, Sir E. W.—*Engineering*, Sept. 12, 1930, p. 335; Sept. 19, 1930, p. 351.

Orrok, G. A.—*Engineering*, Sept. 12, 1930, p. 339; cf. paper 249, World's Power Conference, Berlin (1930); paper 124, World's Power Congress, Japan Sectional Meeting (Nov. 1929).

Richmond, Lt.-Col. V. C.—*Engineering*, Sept. 12, 1930, p. 341; Sept. 19, 1930, p. 353.

Stanton, A. L.—*World Power*, Sept. 1930, p. 208 (with charts); cf. *Engineering*, Sept. 26, 1930, p. 407; *Elect. Times*, Sept. 18, 1930.

Stevens, S.—*World Power*, Sept. 1930, p. 208; cf. *Engineering*, Sept. 26, 1930, p. 407.

Wallis, B. N.—Expected to appear in *Proc. Roy. Aeronaut. Soc.*; *Engineering*, Sept. 19, 1930, p. 353.

Wilson, J. S.—To appear in *Engineering*.

SECTION H.

Bowen, E. G.—To appear in *Man*.

Cammiade, L. A.—Cf. *Antiquity*, Sept. 1930.

Clifford, Mrs. E.—*Man*, Nov. 1930; to appear in *Anthropol. Mag.*; to appear in extended form in *Trans. Bristol & Glos. Archæol. Soc.* (1931).

Dobson, Mrs. D. Portway.—In extended form as 'The Archæology of Somerset' (Methuen, 1931).

Evans, E. E.—Expected to appear in *Man*; cf. *Antiquity*, pp. 157–172 (1930).

Gray, H. St. G.—*Proc. Som. Arch. Soc.*, **50,** ii, pp. 32-49; **54,** ii, pp. 131–143; **59,** ii, pp. 1–24; **68,** pp. 8–20; **56,** ii, pp. 50–61; **69,** pp. 49–53; **70,** pp. 104–116; **71,** pp. 57–76; **72,** pp. 55–68; **49,** ii, pp. 23–53; *Archæologia*, **80**; *Trans. Devon. Assn.*, **44,** pp. 703–717; *Antiquaries' Journal*, **10,** pp. 156–7.

Horsfield, G.—In extended form in *Journ. Roy. Geog. Soc.*, Nov. 1930.

Leakey, S. B.—On 'Kikuyu System of Land Tenure'; cf. 'Report of the Kikuyu System of Land Tenure,' published by Kenya Govt., 1930. On 'Human Types and Stone Age Cultures in Kenya,' to be published in book form in 1931 by Camb. Univ. Press.

Murray, Miss M. A.—Shortly to be published in book form.

Parry, R. F.—*Proc. Somerset Archæol. and Nat. Hist. Soc.*, **14** and **16** (Fourth Series).

Petrie, Sir Flinders.—On 'Beth-Pelet'—with plates, in *Beth-Pelet*, ii, (1931).

SECTION I.

Drever, Dr. J.—Expected to appear in *Brit. Journ. Psych.*

Gates, Prof. R. R.—On 'Blood Groups,' cf. ch. 9, 'Heredity in Man' (Gates), Constable, 1929.

Kent, Prof. A. F. S.—*Nature*, p. 629, Oct. 18 (1930); cf. *Proc. Physiol. Soc.*, Nov. 1892; *Journ. of Physiol.*, **14**, pts. 4 and 5 (1893).

McDowall, Prof. R. J. S.—*Proc. Physiol. Soc.*, Dec. 14, 1929; *Journ. Physiol.*, **69**; *Edin. Med. Journ.*, August 1930.

Nixon, Prof. J. A.—Cf. *Brit. Med. Journ.* (1924), **1,** p. 53; **1,** p. 77 (1926); *Brit. Med. Chir. Journ.*, **43** (No. 162), p. 199 (1926); *Brit. Med. Journ.*, **1,** p. 326 (1930).

Roaf, Prof. H. E.—Expected to be published in *Nature*; cf. *Quart. Jour. Exp. Physiol.*, **14,** p. 151 (1924); **15,** p. 447 (1925); **16,** p. 379 (1927); **18,** p. 243 (1928); *Proc. Roy. Soc.* B, **106,** p. 276 (1930); *Jour. Physiol.*, **69,** p. 1 (1929); *Proc. Roy. Soc.*, B, **105,** p. 371 (1929).

Shaxby, Dr. J. H.—Expected to appear in *Journ. of Physiol.*

SECTION J.

Banister, Dr. H.—*Brit. Journ. of Tuberculosis*; cf. *Lancet*, April 5, 1930.

Binns, H.—Expected to appear in *Brit. Journ. Psych.*; cf. 'Personal Judgment in Industry and Commerce,' *Proc. Ninth Internat. Cong.* (Yale, 1929).

Isaacs, Mrs. S.—Embodied in 'Intellectual Growth in Young Children' (Isaacs), Routledge.

Phillips, Miss M.—*The Forum*, Nov. 1930; cf. 'The Young Industrial Worker,' Oxford Univ. Press.

Simmins, Miss C. A.—To appear in *Modern Languages*, **12**, No. 2 or 3; cf. *ibid.*, **9**, No. 2, Dec. 1927.

Wheeler, Prof. Olive A.—Expected to appear in first number of *Brit. Journ. of Educ. Psych.* or in *Forum*; cf. 'Youth: The Psychology of Adolescence' (Univ. Lond. Press, 1929).

Wohlgemuth, Dr. A.—*Journn. Mental Sci.*, Oct. 1930.

### Section K.

Alcock, Mrs. N. L.—Prelim. note in *Journ. Dept. Agric. for Scotland*, July 1930; expected to appear in full in *Ann. of App. Biol.*

Barnes, Dr. B.—Cf. B.A. Report, p. 388 (1927); *Annals of Botany*, **42** (1928); **44** (1930).

Brenchley, Dr. W. E.—On plant-nutrition; cf. *Annals of Botany*, **41**, No. clxi, pp. 167–87 (1927).

Cook, Dr. W. R. I.—Expected to appear in *Trans. Brit. Mycological Soc.*; prelim. account in *Arch. f. Protistenkunde*, **66**, 285–289 (1929), Pl. 7.

Gates, Prof. R. R.—On 'Haploid Plants,' *Journal of Genetics*, **23**, pp. 123–156; On 'Blood Groups'; cf. Ch. 9, 'Heredity in Man' (Gates), Constable, 1929.

Gwynne-Vaughan, Prof. Dame Helen.—Expected to appear in extended form in *Annals of Botany*, March 1931.

Marsden-Jones, E. M.—*Report of Botanical Soc.*

Turrill, Dr. W. B.—*Report of Botanical Soc.*

### Department K*.

Bryan, J.—*Timber Trades Journ.*, Sept. 20, 1930; expected to appear in *Empire Forestry Journ.*

Clarke, S. H.—Expected to appear, with additions, in *Journ. of Soc. of Foresters of Gt. Britain*; cf. 'Home Grown Timbers,' Bulletin 7, Forest Products Research Laboratory.

Day, W. R.—Expected to appear in *Forestry.*

Howard, A. L.—*Timber Trades Journal*, No. 2819, **114**, pp. 664–5; *Nature*, Oct. 4, 1930, p. 543.

Macdonald, J.—*Timber Trades Journal*, Nov. 8, 1930; fuller account in *Scottish Forestry Journal* in Spring, 1931.

Rayner, Dr. M. C.—*Forestry*, **4**, No. 2 (1930).

Taylor, W. L.—*Empire Forestry Journal*; cf. *Surveyors' Institution Journal* (1926); *Empire Forestry Journal*, No. 1 (1927); *Quarterly Journal of Forestry*, April 1930.

### Section L.

Blatz, Dr. W. E.—Cf. 'Parents and the pre-School Child' (J. M. Dent & Sons): 'The Management of Young Children' (J. M. Dent & Sons).

Brockington, W. A.—*Education*, Sept. 12, 1930.

Goodenough, Sir F.—Privately printed by Knapp, Drewett & Sons; cf. *Education for Commerce* (Address to Headmasters' Conference, 1929), privately printed, Knapp, Drewett; *Interim Reports of Committee on Education for Salesmanship* (Overseas Marketing, 1929; Mod. Languages, 1930), H.M. Stat. Office.

Gordon, Miss V. E. Carr.—*Education*; *Journ. of Educ.*

Pringle, W. H.—Cf. 'Commercial Education and Requirement of Industry' (Ass. for Educ. in Industry & Commerce: 1926).

Richardson, Dr. W. A.—Cf. 'Technical College and Local University,' Assn. of Tech. Institutions, Loughborough (1929).

Wodehouse, Miss H. M.—To appear in *British Journ. of Educ. Psych.*

SECTION M.

Bishop, Dr. L. R.—*Journ. Inst. of Brewing*, **36** (1930); cf. 'The proteins of barley during development and storage,' pp. 336–349; 'The Prediction of Extract,' pp. 421–434; 'The Nitrogen content and quality of Barley,' pp. 352–364; 'A prelim. study of Wort Composition' (in press).

Brenchley, Dr. W. E.—On 'Influence of Fertilisers'; cf. *Journ. Minst. Agric.*, pp. 665–73 (1930).

Godden, W.—Expected to appear in *Journ. Agr. Sci.*; cf. *ibid.*, **16.**

Lamb, Commr. D. C.—Cf. *Western Daily Press*, Sept. 10, 1930.

Ling, A. W.—Cf. *Univ. Bristol Bulletins I, II*, 'Sugar Beet Expts.,' 1926–7–8; *Agric. Prog.*, **4**, 1927; *ibid.* (1928); *Brit. Sugar Grower*, Feb. 1927; *Internat. Sugar Journ.*, Feb. 1927, June 1927, June 1930; *Brit. Beet Grower*, **2**, No. 6; *Journ. Ministry Agric.*, Jan., Feb., Mar. 1930.

Orr, Dr. J. B.—On Grassland; cf. 'Mineral Content of Pastures,' Orr (Lewis); *Trans. Highland & Agric. Soc. Journ.*, **41** (1929).

Tutin, F.—Cf. Annual Report, Long Ashton Research Station, p. 81 (1927); *Journ. Pomology and Hort. Sci.*, p. 129 (1930); *Journ. Min. Agric.*, p. 475 (1930).

Wallace, T.—Cf. *Journ. Pomology and Hort. Sci.*, Nos. 3 & 4, **4,** (1925); **5,** No. 1 (1925); **8,** No. 1 (1930); *Ann. App. Biol.*, **18,** No. 3 (1930).

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*References to addresses, reports, and papers printed in extended form are given in italics.*

* *Indicates that the title only of a communication is given.*

*When two references to a paper are given, the second is to a note of its publication elsewhere, or to a note of other publications by the author on the same subject.*

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# The following LIST OF PUBLICATIONS

*refers mainly to those issued since* 1900. *A new series of* "British Association Reprints" *was begun in* 1922, *in standard paper covers; these are indicated by* **heavy type.** Enquiries for earlier Reports, etc., and for shorter papers for recent years not included in the following list, should be addressed to the office.

Lalande's Catalogue of Stars, £1 1*s.*

Stellar Distribution and Movements, by A. S. Eddington, M.Sc., 1911, 3*d.*

Seismology, 1900, 1*s.*; 1904-1905, 1*s.* each; 1907, 6*d.*; 1908, 1*s.*; 1910, 1*s.*; 1912-1915, 1*s.* each; 1918, 6*d.*; 1922, 1*s.*; 1923-1928, 6*d.* each; 1930, 6*d.*

Catalogue of Destructive Earthquakes, A.D. 7 to A.D. 1899, by Dr. J. Milne, F.R.S., 1912, 5*s.*

**Catalogue of Earthquakes,** 1918–24, by Prof. H. H. Turner (B.A. Reprints, n.s., No. 21), 2*s.*

Investigation of the Upper Atmosphere, 1927, 6*d.*

Bibliography of Spectroscopy, in continuation of 1894 Report, 1898, 1901, 1*s.* each.

Note sur l'Unité de Pression, par le Dr. C. E. Guillaume, 1901, 3*d.*

Note on the Variation of the Specific Heat of Water, by Prof. H. L. Callendar, 1901, 4*d.*

On Threefold Emission Spectra of Solid Aromatic Compounds, by Prof. E. Goldstein, 1909, 3*d.*

Anode Rays and their Spectra, by Dr. Otto Reichenheim, 1909, 3*d.*

Report on the Determination of Gravity at Sea, 1916, 1*s.* 6*d.*; 1919, 1*s.* 6*d.*

Report on Tides, 1923, 1*s.*

Calculation of Mathematical Tables, 1923–29, 1*s.* each; 1930, 6*d.*

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Wave-lengths, 1899, 1*s.*; 1900, with Index to Tables from 1884 to 1900, 1*s.*; 1901, 1*s.*

Chemical Compounds contained in Alloys, by F. H. Neville, F.R.S., 1900, 6*d.*

The Constitution of Camphor, by A. Lapworth, D.Sc., 1900, 1*s.*

**Absorption Spectra and Chemical Constitution of Organic Compounds,** 1922 (B.A. Reprints, n.s., No. 12), 1*s.* 6*d.*

The Methods for the Determination of Hydrolytic Dissociation of Salt Solutions, by R. C. Farmer, 1901, 6*d.*

The Application of the Equilibrium Law to the Separation of Crystals from Complex Solutions and to the Formation of Oceanic Salt Deposits, by Dr. E. Frankland Armstrong, 1901, 1*s.*

Our Present Knowledge of Aromatic Diazo-compounds, by Dr. Gilbert Thomas Morgan, 1902, 6*d.*

The Present Position of the Chemistry of Rubber, by S. S. Pickles, 1906, 6*d.*

The Present Position of the Chemistry of Gums, by H. Robinson, 1906, 3*d.*

Solubility, by J. Vargas Eyre, Ph.D. Part I., 1910, 1*s.*; Part II., 1913, 1*s.*

Fuel Economy, 1916, 6*d.*; 1919, 6*d.*; 1922, 1*s.*

**The Structure of Molecules** (Discussion), (B.A. Reprints, n.s., No. 2), 1921, 9*d.*

**The Nitrogen Industry** (Discussion), (B.A. Reprints, n.s., No. 14), 1922, 9*d.*

The Botanical and Chemical Characters of the Eucalypts and their Correlation, 1915, 1s.

Non-aromatic Diazonium Salts, 1921, 6*d.*

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Changes in the Sea Coast, 1903, 1*s.*

Life-zones in the British Carboniferous Rocks, 1901, 6*d.*; 1902, 6*d.*; 1904, 6*d.*

The Formation of "Rostro-Carinate" Flints, by Professor W. J. Sollas, F.R.S., 1913, 3*d.*

Lower Carboniferous Zonal Nomenclature, 1925, 1*s.*

**A List of Characteristic Fossils** (B.A. Reprints, n.s., No. 18), 1924, 1*s.*

Photographs of Geological Interest, 1919, 1921, 1923, 1926-1928, 1930, 6*d.* each.

**Discussion on The Relation between Past Pluvial and Glacial Periods** (B.A. Reprints, n.s., No. 27), 1930, 1*s.*

**Discussion on The Validity of the Permian as a System** (B.A. Reprints, n.s., No. 28), 1930, 6*d.*

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Rules of Zoological Nomenclature, 1*s.*

Digest of Observations on the Migration of Birds, made at Lighthouses, by W. Eagle Clarke, 1896, 6*d.*

Migratory Habits of the Song-thrush and the White Wagtail, by W. Eagle Clarke, 1900, 6*d.*

Migratory Habits of the Skylark and the Swallow, by W. Eagle Clarke, 1901, 6*d.*

Migratory Habits of the Fieldfare and the Lapwing, by W. Eagle Clarke, 1902, 6*d.*; 1903, 6*d.*

Melanism in Yorkshire Lepidoptera, by G. T. Porritt, 1906, 6*d.*

Biological Problems incidental to the Belmullet Whaling Station, 1912, 3*d.*; 1914, 1*s.*

On the Phylogeny of the Carapace, and on the Affinities of the Leathery Turtle, *Dermochelys coriacea*, by Dr. J. Versluys, 1913, 6*d.*

Zoology Organization, 1921, 3*d.*

Biological Measurements, 1927, 6*d.*

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Amount and Distribution of Income (other than Wages) below the Income-tax Exemption Limit in the United Kingdom, 1910, 6*d.*

**Effects of the War on Credit, Currency, and Finance,** 1915, 6*d.*; 1921 (B.A. Reprints, n.s., No. 3), 1*s.* 6*d.*; 1922 (B.A. Reprints, n.s., No. 15), 6*d.*

The Question of Fatigue from the Economic Standpoint, 1915, 6*d.*; 1916, 6*d.*

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Second Report on a Gauge for Small Screws, 1884, reprinted 1895, 6*d.*

Report on giving practical effect to the Introduction of the British Association Screw Gauge, 1896, 6*d.*; 1901, 6*d.*; 1903, 6*d.*

Report on Proposed Modification of the Thread of the B.A. Screw, 1900, 6*d.*

Resistance of Road Vehicles to Traction, 1901, 3*d.*

The Road Problem, by Sir J. H. A. Macdonald, 1912, 3*d.*

Standardisation in British Engineering Practice, by Sir John Wolfe-Barry, K.C.B., 1906, 3*d.*

The Proper Utilisation of Coal, and Fuels derived therefrom (Discussion), 1913, 6*d.*

Liquid, Solid, and Gaseous Fuels for Power Production, by Professor F. W. Burstall, 1913, 3*d.*

The Standardisation of Impact Tests, 1918, 9*d.*

**Stress Distributions in Engineering Materials,** 1921 (B.A. Reprints, n.s., No. 4), 3*s.* 6*d.*; 1923 (B.A. Reprints, n.s., No. 17), 3*s.*

**Aeronautical Problems of the Past and of the Future,** by R. V. Southwell, F.R.S. (B.A. Reprints, n.s., No. 19), 1925, 1*s.* 6*d.*

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Progress of Anthropological Teaching, 1923, 6*d.*

Ethnological Survey of Canada, 1899, 1*s.* 6*d.*; 1900, 1*s.* 6*d.*; 1902, 1*s.*

Physical Characters of the Ancient Egyptians, 1914, 6*d.*

The Age of Stone Circles, 1922, 1*s.*

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The Claim of Sir Charles Bell to the Discovery of Motor and Sensory Nerve Channels (an Examination of the Original Documents of 1811–1830), by Dr. A. D. Waller, F.R.S., 1911, 6*d.*

Heat Coagulation of Proteins, by Dr. Chick and Dr. Martin, 1911, 3*d.*

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Curricula of Secondary Schools, 1907, 3*d.*

The Influence of School Books upon Eyesight, 1913 (Second Edition, revised), 4*d.*

The Teaching of Botany in Schools, 1903, 3*d.*

Report on Atlas, Textual, and Wall Maps for School and University use, 1915, 6*d.*

Report on Popular Science Lectures, 1916, 6*d.*

Museums in relation to Education, 1920, each 6*d.*, or for 6 or more copies, 2*d.*

**Training in Citizenship,** 1920, 1*s.* (9*s.* per doz.); 1921, 6*d.* (5*s.* per doz.); 1922, 6*d.* (4*s.* per doz.). (B.A. Reprints, n.s., Nos. 8, 9, 11.)

**Imperial Citizenship,** by the Rt. Hon. Lord Meston, 1922 (B.A. Reprints, n.s., No. 13), 9*d.* (6*s.* per doz.).

**Science and Ethics,** by Dr. E. H. Griffiths, F.R.S. (B.A. Reprints, n.s., No. 1), 1921, 9*d.*

**Charts and Pictures for use in Schools** (B.A. Reprints, n.s., No. 5), 1921, 1*s.*

**An International Auxiliary Language** (B.A. Reprints, n.s., No. 6), 1921, 1*s.*

**Geography Teaching** (B.A. Reprints, n.s., No. 16), 1*s.* (10*s.* per doz., £4 per 100).

Educational Training for Overseas Life, 1924, 1925, 1927, 1929, 6*d.* each.

Report of a discussion on **Educational Training for Overseas Life** (B.A. Reprints, n.s., No. 20), 1926, 6d.

**Science in School Certificate Examinations** (B.A. Reprints, n.s., No. 23), 1928, 1*s.*

**Animal Biology in the School Curriculum** (B.A. Reprints, n.s., No. 24), 1928, 1*s.*; 1930, 6*d.*

Report on Formal Training, 1929, 6*d.*; **1930,** (B.A. Reprint, n.s., No. 25), 6*d.*

**Report on Educational and Documentary Films** (B.A. Reprint, n.s., No. 26), 6d.

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**On Inbreeding in Jersey Cattle,** by A. D. Buchanan Smith (B.A. Reprints, n.s., No. 22), 1928, 6*d.*