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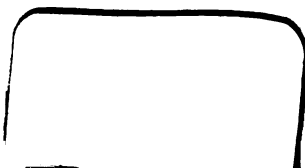
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PROCEEDINGS  
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ROYAL SOCIETY OF EDINBURGH.






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PROCEEDINGS  
OF THE  
ROYAL SOCIETY OF EDINBURGH.

VOL. XXV.

1903-4.

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PROC. ROY. SOC. EDIN.—VOL XXV.

1

**The People of the Faroes.** By Nelson Annandale, B.A. (Oxon.). *Communicated by Professor D. J. CUNNINGHAM, F.R.S.*

(MS. received Oct. 7, 1903. Read Nov. 2, 1903.)

PART I.—ANTHROPOMETRICAL.

The physical anthropology of the Faroes has recently been described in a very elaborate manner, as far as the island of Suderoe is concerned, by Dr F. Jørgensen (1), who was resident there as a medical man for some years. While pointing out, however, that the people of Suderoe differ considerably from those of the 'northern islands,' he only gives a comparatively small series of data regarding the latter, nor does he state to which of the northern islands the men he examined belonged, or even whether they came from one island or from several. Apart from Suderoe, there are sixteen inhabited islands (fig. 1) in the group, and between some of them very little communication exists even at the present day. In historical accounts of the Faroes the six following islands are usually called the 'northern isles,'—viz., Kalsoe, Kunoe, Boroe, Wideroe, Fugloe, and Svincoe,—but I take it that Dr Jørgensen would include at least Osteroe, Stromoe, and Waagoe also. His elaborate, laborious, and presumably accurate tables serve so well to point the moral that until a uniform method, a uniform standard, and a uniform set of anthropometrical instruments are adopted by anthropometrists of all nationalities *final* work in this branch of science will be impossible, that I have thought it well to put on record a small series of measurements taken by myself in the Faroes recently, and at the same time to point out wherein some of the data pretty generally adopted fail in accuracy, differing with the observer as well as the observed.

My measurements were taken in Thorshavn, the chief town in the islands, in August 1903, upon twenty adult males. The only value that can be claimed for so small a series is that it was obtained at a definite period and within a very limited area, for the men examined were all resident in the town. The length and breadth of the head, the length and breadth of the nose, the



length of the face, the bizygomatic and bigonial breadths, were taken with callipers of a simple type, while the height of the head, the auriculo-nasal and the auriculo-alveolar lengths were taken by means of Professor D. J. Cunningham's craniometer; all these measurements, therefore, were obtained directly, not by projections or estimations. The statures given can only be approximate, as all my subjects were measured with shoes or boots on their feet, and I was obliged to extract a varying number of millimetres in accordance with the kind of footwear worn.

The individuals measured are too few to make a rigid mathematical examination of the data regarding them legitimate, and they can give at best but an approximation to the race characters of the people of Thorshavn. With so small a series perhaps the rough and ready method of examination by the aid of means and extremes is the best, as having the least appearance of finality.

The length of the head, as may be seen by the table, varies in the twenty adult men from 176 to 157 millimetres, while the mean of the series is 166, only  $\cdot 5$  less than the mean of the two extremes. Though the extremes in the breadth of the head are less divergent from one another than those of the length, their mean is more divergent from that of the series, the former exceeding the latter by  $\cdot 9$ , and the variation is also greater. The mean index derived from these two measurements varies from 86.8 to 76.3; twelve of the men are brachycephalic, though five of these have an index between 80 and 81, while the remaining eight are mesaticephalic, only three being between 78 and 80; the mean, 80.6, is brachycephalic. If the skulls of these twenty men had been examined instead of their heads, it is probable that not more than four would have been brachycephalic, and that two would have been dolichocephalic; the mean index would certainly have been mesaticephalic. The mean cephalic index of Dr Jørgensen's series of thirty-three men above the age of twenty from the northern islands is 80.4, and the extremes are 75.4 and 85.3; and the variation, as might be expected in a larger series, is slightly greater than in mine, while the difference between the mean of the series and that of the extremes is less. Taking the two series together, the mean is 80.7, and the mean of the extremes

TABLE of Individual Measurements and Indices.  
Men of Thorshavn, Capital of the Faroes.

Serial Number	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	
Stature (centimetres), . . .	171	160	158	175	163	163	164	164	162	165	157	163	162	164	167	167	170	173	176	172	
Head—																					
Length, . . . . .	mm.	mm.	mm.	mm.	mm.	mm.	mm.	mm.	mm.	mm.	mm.	mm.	mm.	mm.	mm.	mm.	mm.	mm.	mm.	mm.	mm.
Breadth, . . . . .	196	196	194	189	188	190	202	186	199	190	188	190	186	192	190	186	197	192	200	202	
Height, . . . . .	136	157	150	166	146	156	166	143	155	150	153	146	149	154	156	152	170	162	172	168	
	135	134	126	133	132	141	137	127	140	131	143	144	132	141	131	136	146	140	134	131	
Face—																					
Length, . . . . .	130	125	114	123	123	124	118	112	123	116	121	118	116	124	106	126	130	122	134	126	
Biygomatic breadth, . . .	156	123	130	130	126	133	136	130	133	133	123	134	123	140	136	123	140	131	134	130	
Bigonial breadth, . . . .	128	104	109	100	112	106	116	110	118	106	108	112	104	110	126	112	120	116	114	106	
Nose—																					
Length, . . . . .	56	60	52	51	52	50	56	48	53	56	49	59	51	57	52	56	58	56	62	53	
Breadth, . . . . .	40	33	35	33	35	35	36	34	36	34	33	39	34	34	41	37	41	33	35	33	
Auriculo-nasal length, . .	106	110	105	94	100	100	106	97	111	102	100	104	101	105	100	110	112	110	106	108	
Auriculo-alveolar length, .	96	116	110	93	106	96	102	96	105	98	100	104	97	96	103	106	105	101	99	102	
Cephalic Index, . . . . .	85.1	80.1	77.3	81.4	77.6	82.1	82.2	76.3	77.9	78.9	80.8	76.8	80.5	80.2	82.1	80.8	86.8	79.3	86.0	78.2	
Vertical Index, . . . . .	69.7	68.3	65.1	67.7	70.3	74.4	67.8	68.3	70.3	68.9	75.5	75.8	71.3	73.9	68.9	72.3	74.1	72.9	67.0	74.7	
Frontal Index, . . . . .	83.3	101.8	87.7	99.2	101.5	90.6	86.3	86.1	92.7	84.1	94.5	88.1	95.1	88.6	77.9	98.4	92.8	93.1	100.0	90.8	
Bigonial Index, . . . . .	82.0	84.5	83.8	76.9	89.7	86.8	85.3	84.6	85.5	78.6	84.4	83.1	85.3	78.6	92.6	87.5	85.7	88.5	85.1	80.8	
Nasal Index, . . . . .	71.4	55.0	67.3	70.0	67.2	70.0	64.3	70.8	67.9	66.7	67.3	66.1	66.7	30.6	78.8	66.1	70.7	58.9	56.4	65.5	
Gnathic Index, . . . . .	89.6	103.5	104.8	98.4	103.0	96.0	96.2	99.0	94.7	96.3	100	100	95.1	90.4	103	96.4	93.7	92.7	94.3	94.4	

TABLE of Means and Extremes.

Twenty Men of Thorshavn.

<i>Gnathic Index.</i>		105.5	89.6	97.17
<i>Nasal Index.</i>		78.8	55.0	65.66
<i>Bigonial Index.</i>		92.6	76.8	83.87
<i>Facial Index.</i>		101.8	77.9	91.9
<i>Vertical Index.</i>		75.8	65.1	71.35
<i>Cephalic Index.</i>		86.8	76.8	80.62
Auriculo-alveolar Length.	mm.	116	98	101.4
Auriculo-nasal Length.	mm.	112	94	104.2
Breadth of Nose.	mm.	41	82	35.7
Length of Nose.	mm.	62	48	54.6
Bigonial Breadth.	mm.	128	100	111.8
Bizygomatic Breadth.	mm.	156	122	133.4
Length of Face.	mm.	184	106	122.3
Height of Head.	mm.	151	126	136.4
Breadth of Head.	mm.	172	142	155.2
Length of Head.	mm.	202	186	192.6
Stature.	cm.	176	157	166
	Highest,			
	Lowest,			
	Mean,			

TABLE of other Particulars.

Serial Number.	Name.	Age.	Colour of Eyes.	Colour of Hair.
1	Andreas Djurhuus, . . .	65	grey	brown
2	Jacob Jacobsen, . . .	63	blue	brown
3	Andreas Jacobsen, . . .	44	blue	brown
4	Jacob Mikkelsen, . . .	36	blue	fair
5	Christian Christiansen, . . .	42	blue	fair
6	Ole Hansen, . . .	40	blue	brown
7	Rasmus Andreassen, . . .	46	blue	fair
8	Paul Nichodemussen, . . .	55	blue	fair
9	Joen Gjoueraa, . . .	44	blue	fair
10	Paul Hansen, . . .	40	blue	dark
11	Tomas Yule Nichalsen, . . .	37	blue	red
12	William Paulsen, . . .	55	blue	brown
13	Daniel Samuelsen, . . .	34	blue	fair
14	Peter Hans Sørensen, . . .	59	grey	fair
15	Andreas Olsen, . . .	24	dark brown	dark
16	Peter Haraldsen, . . .	63	blue	brown
17	Hans Mikkelsen, . . .	52	dark grey	black
18	Nils Joensen, . . .	27	blue	fair
19	Djone Isaksen, . . .	54	blue	brown
20	August Mouriksen, . . .	...	blue	brown

is 81.1. If we consider 75 as the upper limit of dolichocephaly and 80 of mesaticephaly, eighteen of Dr Jørgensen's are brachycephalic and fifteen mesaticephalic. We may say, therefore, that were a large series of skulls of the people of the Faroes, leaving the island of Suderoe out of account, to be examined, it is probable that the great majority of them would be found to be mesaticephalic, while a comparatively small number would be dolichocephalic, and a less small number brachycephalic. Dr Jørgensen's data show that the proportion of individuals with dolichocephalic or low mesaticephalic heads would be greater in Suderoe than elsewhere in the Faroes, as is noted below.

The vertical height of the head, measured between the vertex and a line joining one external auditory meatus to the other, is, in every individual in my series, less than the greatest parieto-squamosal breadth, and in every case but two, very considerably so. Professor Cunningham's craniometer permits this measurement to be taken on the living person with considerable accuracy, but the question how far it corresponds to the basi-bregmatic height of the skull is a difficult one. The centre of the external auditory meatus is certainly, in most cases, several millimetres higher than the



basion, but the limits within which this difference in level varies will be discussed later. At any rate, the thickness of the soft tissues of the scalp and the hair must quite compensate for it, if they do not cause the vertical height, taken as described, to be slightly greater, as is possible, than the true basi-bregmatic height. It is very unlikely, however, that in more than two cases at most the basi-bregmatic height of the individuals under discussion would equal their parieto-squamosal breadth in the skull, and it is improbable that this would be found to be the case, could the skulls be measured, in a single instance. In the living men the mean breadth-height index of the head is 87·9, and the extremes are 98·6 and 77·9; the mean height is 136·4, and the extremes are 151 and 126 mm.

The length of the face, measured directly with the callipers between the bridge of the nose and the tip of chin, varies from 134 to 106 mm., with a mean of 122·3 mm., while the interzygomatic (or bizygomatic) breadth varies between 156 and 152 mm.; in two cases out of twenty the length of the face is greater than the bizygomatic breadth, and in one the two measurements are equal. The complete facial index, calculated from these two measurements, varies from 101·8 to 77·9, and the man with the shortest face, which is considerably shorter than any other in the series, has the lowest index, though the man with the longest face, which is not so much longer than any other, has only the third index, the breadth being equal to the length. The measurements for the cephalic and vertical indices are easy to take with a fair degree of accuracy, and do not depend upon the play of the subject's features; but it is far otherwise with those for the facial index—an unfortunate fact, seeing that, provided all the measurements are taken by the same person, no index is of greater importance as a racial character. It makes all the difference in the world whether the length of the face is taken directly, or by projection from the vertex to the nasion and to the chin and by subsequent calculation, and it makes just as much difference whether the features of the subject are perfectly at rest or in any way distorted. I am not aware in what manner exactly Dr Jørgensen obtained what he calls the "longitudo naso-menthalis," or what degree of pressure he exerted in measuring his "latitudo bizygoma-

ticus," but the fact remains that the facial index he calculates from these measurements differs considerably from that which I obtain from the nasio-mental length and bizygomatic breadth. Of course we measured different individuals, possibly from different islands—though at present I am only considering the thirty-three men from his series to whom I have referred—and I have known the facial index to be very different in two villages no further apart than, say, Thorshavn and Klagsvig, but this was in the Malay Peninsula, in a district where there was far more reason to suspect admixture of foreign blood in different degrees in neighbouring localities; and the difference in the figures between the two series from the Faroes, without including Suderoe, is so great that I cannot help thinking that either my own measurements, Dr Jørgensen's, or both, must have been taken in a manner not altogether satisfactory. The mean index of his thirty-three subjects, calculated from the figures he gives, is about 11 per cent. lower than that of my series; and while he makes a very large proportion of his subjects mesoprosopic,<sup>1</sup> and a considerable proportion actually chamæoprosopic,<sup>1</sup> eleven out of my twenty are leptoprosopic,<sup>1</sup> five mesoprosopic, and only four chamæoprosopic. In his series no man has a face of which the length even approaches closely to the breadth, and the mean of his series is chamæoprosopic, while that of mine is leptoprosopic. This is a very considerable difference; and although the facial index taken on the skull is probably, at any rate in normal individuals, considerably higher than if taken on the living individual, as the combined thickness of the soft tissues on both sides of the face is probably greater than that of the soft tissues at the tip of the chin, yet I am inclined to think that the Faroe men have narrower faces than Dr Jørgensen's figures would suggest, though it is quite possible that my own data may err in the opposite direction. What strikes one in a visual examination in the faces of a group of Faroemen, as distinguishing them at a glance from those of the Icelanders, and, to a less extent, from that of one type of Dane, is the narrowness of the zygomata, and the oval outline longitudinally. It should be noted, however, that in Icelanders the cheek bones

<sup>1</sup> My usage of these terms is that adopted by Sir William Turner in his recent papers (*Trans. Roy. Soc. Edinburgh*, vol. xl, 1903, part iii. pp. 605, 606).

are often very prominent, and the face is frequently so flat, the eyes are so narrow, and the mouth is so big, that one is inclined to speculate as to the possibility of environment having induced some latent Mongoloid strain, inherited from prehistoric times, ere Iceland was colonised, to develop, or even whether environment alone could possibly have produced a similarity to the Esquimaux, not inherited at all. However, the time has not come to settle, or even to seriously discuss, such questions, and, in any case, they are beyond the point in dealing with the Faroemen, in whom there is little, if any, trace of any such phenomenon. All that can be said with reference to the point at issue is, that two observers who have examined the faces of the Faroemen get very different results with regard to the facial index, and that there is reason to believe that were a large number of Icelanders examined, they would be found to have considerably broader and flatter faces than the Faroemen.

The bigonial breadth is another measurement that depends very largely upon the individual observer, and probably has a very different relationship to the same measurement on the skull in different subjects. In taking it on the living person it is by no means easy to regulate the pressure exerted by the points of the callipers upon the soft tissues, and the degree or absence of such pressure makes a very great difference in the results obtained, while the extent to which the muscles which work the jaw are developed also influences them considerably. Personally, I now make it a practice to draw the skin as tight as possible in taking this measurement, and to press in the points of the callipers as far as they will go without injuring the subject, believing that in this way it is possible to get a more uniform standard of comparison, both as regards different individuals and as regards the difference between the skull and the living head. It is probable, however, that many anthropometrists take care to exert as little pressure as possible, though it is obvious that if this be done, the measurement must vary even more with the muscular development and the amount of adipose tissue than with the true breadth of the skeletal support. The mean bigonial breadth in my series, taken as described, is 111·8 mm.—21·6 mm. less than the mean bizygomatic breadth—and the extremes are 128 and 100. The bigonial index, that is, the index obtained by the

formula  $\frac{\text{bigonial breadth} \times 100}{\text{bizygomatic breadth}}$ , varies within narrower limits than the facial index, or than either of the separate measurements from which it is calculated, showing that the longitudinal shape of the face is fairly constant; the mean is 83·87, and the extremes are 92·6 and 76·8. This is by no means a high index, and it probably shows that the faces of the Faroemen, as might be expected from a visual examination, narrow considerably from above downwards, though they are by no means broad across the cheek bones; but it must be borne in mind that my method of taking the bigonial breadth is very possibly not the general one, and I have been able to find very little information as to how it is obtained by other anthropometrists.

The measurements of the nose, again, seem to vary considerably with the individual observer; and, as the figures which express them are comparatively small, the variation in the index is magnified proportionately by an error or difference of method. In European peoples there is rarely any difficulty in finding the points of measurement with fair approximation, but this is always provided that the subject's face is in a state of perfect repose, and that no undue pressure is exerted on the callipers, especially in taking the breadth. In my opinion, it is quite impossible for the ordinary observer to take these measurements to within half a millimetre, as it has been suggested by Professor Haddon (2) that he should do. These things being so, I am surprised at the extent of agreement, rather than disagreement, with regard to the nasal index, as estimated on the living person by different observers. The mean nasal index of my series of Faroemen is 65·66, and the extremes are 78·8 and 55·0, so that they appear to be a very distinctly leptorhine people. The mean of Dr Jørgensen's series from the northern islands is 67·5, and the extremes are 81·1 and 58·6. In shape the nose is generally straight and prominent, the rather flat, coarse type common in Iceland occurring but seldom, and the Roman or aquiline being rarely if ever seen, in the Faroes.

As already stated, the auriculo-nasal and the auriculo-alveolar lengths were taken by means of Professor Cunningham's craniometer between the external auditory meatus (or rather

the line joining the centre of this opening on one side of the head to the same point on the other) and the bridge of the nose, and the central point of the upper jaw between the two central incisor teeth, respectively, the upper lip being lifted out of the way in the latter measurement. The index calculated from these two measurements appeared to make the people far more prognathic than I would have expected, if the centre of the auricular orifice, as has often been assumed, corresponded, as far as the measurements from which the gnathic and vertical indices are calculated are

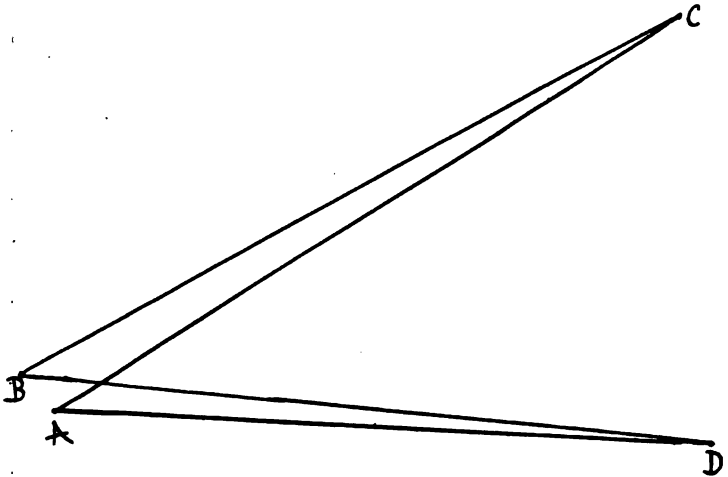


FIG. 2.—Diagram illustrating the relation of measurements taken from the basion to those taken from the auricular point. A = basion. B = auricular point. C = nasion. D = alveolar point.

concerned, in some degree with the basion; and, at Professor Cunningham's suggestion, I commenced a series of measurement on skulls in the Anatomical Museum of the University of Edinburgh, in order to see how far this assumption was legitimate. Before I had gone far in this investigation—indeed, on the same morning on which it was commenced—the solution of the problem became obvious, with the result that I find that the two points do not correspond with one another, for the following reasons, which are made clear by the diagram (fig. 2). In every skull examined I discovered that while the centre of the auricular orifice was several millimetres higher than the basion, it was also several millimetres posterior to it, so that while the auriculo-nasal length and the basi-

nasal length were approximately equal, the former being very slightly the longer of the two, the auriculo-alveolar length was considerably longer than the basi-alveolar. In five Irish skulls the difference between the vertical index when the height was taken from the basion and when it was taken from the auricular point, that is to say, from the centre of the external auditory meatus, varied from 2·9 to 6·3, so that it is very evident that the two measurements have little relationship to one another, except that the auricular height is, probably in all cases, the less of the two. In the same skulls the two gnathic indices obtained in a similar way differed by from 5·4 to 14·7, but in this case the auricular index was the greater of the two. It must be remembered, however, that measurements taken on the living head differ considerably from those taken on the skull; while the thickness of the soft tissues of the scalp and of the hair must go far in bringing the auriculo-bregmatic height up to the same figure as that of the basi-bregmatic, if they do not, in some cases, cause the former to surpass the latter, yet the comparatively greater thickness of the soft tissues and of the hair on the occiput and of the forehead must again reduce the vertical index, in whatever way it is obtained, to a result of which the degree cannot ever be arrived at with exactitude. In the gnathic index, on the other hand, the soft tissues that cover the nasion must make the index on the skull considerably higher than one obtained from the same measurements taken on the living head, and it is obvious that thickness of the fleshy coating on the nasion differs considerably in different persons; so that persons with thin faces will have, *cæteris paribus*, a gnathic index higher than that of persons with fleshy faces. It is therefore worth noting that the Faroeman in my table with the highest gnathic index was a very thin and unhealthy man, who suffered greatly from asthma. I do not see that there is any possibility of reducing measurements taken on the living head, as far as the vertical and gnathic indices are concerned, to a common denominator with those of the skull, no matter what the points may be from which the lengths are measured, and it would be difficult to persuade craniologists to give up measuring from the basion, even though the auricular point is one which can be found with equal ease in both cases.

The statures given in my table can only be regarded as approxi-

mate, for all of them were taken, as mentioned above, on men who were not barefooted, and allowance had to be made for different kinds of footgear in different individuals; for these reasons I have only given the results in centimetres, though the measurements were originally taken in millimetres, and I believe that when recorded thus they are fairly accurate. The statures seem to fall into two very distinct series, those of 170 cm. and above and those below that figure; it is noteworthy that the last four men examined fall within the former category, showing how necessary a large series of measurements must always be in estimating the mean stature of a race. Dr Jørgensen's series of thirty-three men from the northern islands gives a mean of 169 cm., with extremes of 155 and 178 mm. Again, a very serious discrepancy exists between my measurements and his, for my mean is 166 cm., and my extremes are 157 and 176 cm., but I have not been able to discover whether his measurements were taken on barefooted subjects, or, if not, whether allowance was made for footgear. In any case, a visual inspection of the Faroemen makes it obvious that they are a very short race, perhaps as a result of in-breeding, though they are robust and well-built, and not, so far as I have been able to discover, degenerate in any other way. It is difficult, however, to discover to what extent insanity prevails among them, as all bad cases of madness are removed to Denmark; but on the little island of Naalsoe, where several families, considering themselves to be descendants of the kings of Scotland, refused to marry the inhabitants of other islands, imbecility and total hereditary deafness are said to have been unusually common (3).

I have not thought it worth while to record my observations on the skin colour in detail, as I believe that this is due far more to the degree of exposure to which the individual has been subjected, to climate, and even to altitude, than to race, at any rate within reasonable limits; for no amount of protection from the elements, no cold, and no altitude would make a Negro white, or even give an Italian the complexion of a Dane. All that can be said on this point as regards the Faroemen is, that those men who have dark hair have also a dark skin, which in some cases is as dark as that of an Italian, and that such persons have frequently

features<sup>1</sup> more marked, and especially a more pronounced prominence, often combined with a tendency to be hooked, of the nose, than the majority of their fellow islanders.

It is probable that the twenty persons examined give a very fair approximation, at any rate as far as the island of Stromøe is concerned, to the general colour of the hair and eyes of the Faroemen, but the series of observations is not sufficiently extensive to permit the calculation of a percentage index of nigrescence on Beddoe's system (4). They show, however, that while the great proportion of the people have light eyes and light or neutral hair, there is a distinct dark element among them, which, as Dr Jørgensen has shown, and as Landt (5) had anticipated, is more pronounced in Suderoe than in the northern islands of the group. The danger of drawing conclusions, however, regarding this point is well illustrated by a fact in the history of a family living near Thorshavn. Several members of this family are very dark indeed, and have almost an Oriental appearance, which I was inclined, before I knew their history, to put down as due to an extreme development among them of the dark type that occurs sporadically in all Scandinavian countries, and is far from uncommon in the Faroes and Iceland. Quite incidentally, however, I learnt that the grandmother of the present head of the family came from somewhere in Eastern Europe, and that her grandchildren took after her. It would seem, on *primâ facie* evidence, that hardly any place in the world was more unlikely to harbour an Oriental European than the Faroes, but facts are liable to run counter to evidence of the kind, and it is, moreover, certain that this unlikely importation, who was met by her husband when both were being educated, I believe in Switzerland, has proved, in zoological language, prepotent, and may conceivably have an ultimate effect on the population of the Faroes, though, the present head of the family having married an Icelander, also met in the course of education, the problem becomes even more complicated. I may also say that this family is one which prides itself on keeping up the old customs of the Faroes, though some people in Thorshavn have told me that the conspicuous

<sup>1</sup> Some excellent photographs of Faroemen are reproduced in a paper just published by Dr Burmeister Norburg (*Globus*, vol. lxxxiv., 1903, No. 14, pp. 219-222). Oct. 29.



'old-fashioned' costume, which the men of the family delight to wear on festive occasions, is partly the result of their own imagination.

Having now dealt with the measurements and observations in my tables severally, I propose to inquire whether there are any obvious correlations between them, such as can be shown in even so small a number of individuals as twenty. If we take the mean stature of the five tallest men, the mean stature of the five who come nearest to them, of the next five, and finally of the five shortest, and if we take the mean of all the head indices of the same five individuals in each of the four batches, we get the following results:—

	Stature.	Cephalic Index.	Vertical Index.	Facial Index.	Nasal Index.	Gnathic Index.
Five tallest, . . .	173·4	82·0	70·4	94·5	63·0	93·9
Next five, . . .	167·4	81·1	72·0	88·1	68·5	98·4
Next five, . . .	163·4	79·8	70·9	90·1	66·4	96·9
Five shortest, . . .	159·8	79·3	70·6	94·4	64·8	99·3

As one figure is apt to throw out the mean in batches so small as five, we may further consider the head indices in the same way from the point of view of the cephalic index, as the five tallest men are not those who have the five shortest indices:—

	Cephalic Index.	Vertical Index.	Facial Index.	Nasal Index.	Gnathic Index.
Five shortest heads, . . .	84·4	70·6	90·7	70·6	94·0
Next five, . . .	81·1	71·1	93·0	68·3	98·6
Next five, . . .	79·3	70·8	90·2	61·4	96·8
Five longest heads, . . .	77·2	69·8	91·2	67·9	100·3

From these tables it would seem that there is a certain relationship between the stature and the shape of the head, and also, possibly, between the cephalic index and the gnathic index. Jørgensen's data for Suderoe appear to indicate no connection

between the stature and the cephalic index in that island, but it is clear that a longer head and a shorter stature differentiate the population of Suderoe as a whole from that of the northern islands, for all observers agree that the former are distinguished from the latter by being smaller and darker, while the following details exhibit the difference in the cephalic index in a sufficiently striking manner. Dr Jørgensen, who adopts the number 77·5 as the lower limit of mesaticephaly, states that of the adult males of Suderoe 44 per cent. are brachycephalic, 27 per cent. mesaticephalic, and 29 per cent. dolichocephalic. If my twenty observations from Thorshavn are combined with his thirty-three from the northern islands, and if the same standard of brachycephaly is adopted for the sake of comparison, we get as a result that in the two series together, decimals omitted, 56 per cent. are brachycephalic, 32 per cent. mesaticephalic, and 12 per cent. dolichocephalic.

#### PART II.—HISTORICAL.

Before discussing the history of the Faroes and the traditions current among the people as regards their origin, it may not be superfluous to consider for a moment the personal names given in my table. With two exceptions the second or third name of each man is a patronymic, but one adapted to modern Danish orthography, and become a regular surname, which, at any rate in Thorshavn, is not changed either from generation to generation or according to the sex of the person who bears it. Mr Henry Balfour has called my attention to the fact that the initials carved on objects from the Faroes, even if these be women's belongings, are the first letters of Christian names and surnames, not, as would be the case on Icelandic objects, those of a Christian name, another Christian name and an *s* (for *son*) or a *d* (for *dóttir*), according to the sex of the owner, and that there is no special indication of the name of the woman's husband, as would be the case on objects from the country districts of Norway. In a list of names of people<sup>1</sup> living in the Faroes between the years 1600 and 1709 there appear to be but a few real surnames, but married women adopt their husbands' patronymics without change; single women are known

<sup>1</sup> N. Andersen, *Færøerne*, 1600-1709. Copenhagen, 1895.

by their personal names, followed by those of their fathers with *dattir* added; men are for the most part referred to in the same manner, but with *sen* instead of *dattir*, while occasionally they adopt the name of their place of abode or birth instead of a patronymic. In the present list one man has a surname which has probably been introduced from southern Denmark or from the Schleswig-Holstein provinces, namely Djurhuus, while another has simply taken the name of his birthplace, Gjoueraa, a small village on the island of Stromoe, surnames being by no means a fixed institution in the country districts of the Faroes even at the present day, though they have gone far further in this direction than in Iceland. It is also worthy of note that a very large proportion of the names in my list are Biblical, and only a very small proportion Norse; while in a similar number of names from Iceland the majority would probably be found to be such as Gisli, Herjolfur, Arni, or the popular Magnus—a name introduced into Scandinavian countries through a misunderstanding of the latinized name of Charlemagne, a very popular hero in the ballads of the Faroes as in other Norse folk-lore.

The Faroes, we know, were colonised by vikings of Norse extraction, many of whom were also descended from the Iberian chieftains of the Hebrides and Ireland. There is no reason whatever to think that the islands had other human denizens when the vikings came, except perhaps occasional anchorites seeking to outdo the records of their fellows in the way of finding 'solitudes.' There is good reason, however, to believe that Faroe, or, as it is properly spelt, Færoe, means 'sheep island,' though Landt (5) gives other derivations, and that the group got its present name because the vikings found a breed of sheep already established there; and if this assumption be correct, the fact is difficult of explanation without supposing either that the island had already been colonised by some race which had disappeared, or else that the sheep had originally been accidentally introduced by a wreck, as was the case with the "great" or brown rat (5) in 1768. The breed appears to have been similar to that of Soa in St Kilda, but is now quite extinct, having been purposely exterminated by the islanders; it could hardly have come spontaneously into being on small islands separated by a very deep channel from any consider-

able mass of land, but its origin must, for the present, remain a mystery, and its existence in no way militates against the view that the Faroes were devoid of human inhabitants when they were first visited by the wanderers of more or less mixed race who are known in British history as the 'Danes,' although comparatively few of them had any connection with Denmark. Professor York Powell (6), in the introduction to his translation of the *Færeyinga Saga*, shows that during the early history of the Faroes their Norse families were closely related to several of the Icelandic chiefs both by blood and marriage, and it is probable that the Faroes were colonised in the first half of the tenth century, a little later than Iceland, which commenced to be peopled in 874 A.D.

In Icelandic history the people known to the vikings as 'men of the West,' that is to say, Irishmen and inhabitants of the outer Hebrides, occasionally make their appearance, chiefly as captives of war; it is to them that the Westmann Isles, off the south coast of Iceland, owe their name, a party of mutinous slaves having occupied them after slaying their master on the mainland, whence his avengers soon came to exterminate the murderers. In the Faroes, Westmannhavn, a fine natural harbour near the north-west corner of Stromoe, is said to have at one time been a favourite resort of the Western ships, while Saxen, a place with a similar but smaller harbour a few miles to the north, is believed to have attracted Scotch and Dutch smugglers until comparatively recent times, when the land-locked bay became silted up in the course of a single storm. The people of Suderoe claim themselves to be of Western descent, and a curious story (3), told me some years ago in Stromoe to account for their physical and dialectic peculiarities, makes them to be descended from an Irish captain's wife who was kidnapped from her husband's vessel by a native chief. The story has evidently been embellished by an ignorant person in order to account for the name of a village in Suderoe, but, for all that, may contain a germ of truth.

A far more circumstantial tradition links the island of Naalsole with Scotland. Certain families on this island, which has a population at the present day of about two hundred souls, believe implicitly that they are the direct descendants of 'Jacobus the Second of Scotland,' whose daughter eloped with a page of her father's

court named Eric and came with a great following to the Faroes. Naalsoe had been utterly depopulated by the Black Death, which raged in the islands at that date, and so the princess and her followers settled there. There she bore a son to Eric. Years later her father followed her, and when he came to Naalsoe he saw his grandson, whom he recognised because he was very like her, playing on the shore. Struck by the boy's beauty and manly appearance, he offered to forgive his daughter and her lover if they would return to Scotland with him. This they refused to do, remaining in the Faroes and having many other children there. The first-born son fell on a knife with which he was playing and killed himself, then the king of Denmark confiscated half the island from the princess because she was a Roman Catholic, but she and her other children, her followers and their descendants, peopled the island, and some of her descendants still refuse to marry outside the families who claim her as their ancestress. The present *amptmand* of the Faroes, the first native to be appointed to this position by the Danish Government, is of her kin. The whole story is, from the point of history, ridiculous, but I am inclined to agree with Robert Chambers (7), who heard the outlines of the tradition on a visit to the Faroes in the middle of last century, that in the main it may be true, any foreign lady of birth and wealth being easily transformed into a 'king's daughter' in a region so remote as the Faroes.

All these floating traditions, in any case, probably set forth a real fact, viz., that there was, subsequent to their original colonisation, a considerable influx of blood other than Norse into the Faroes; but whether the immigrants came as single individuals or in parties we cannot say with any more accuracy than we can give their advent an exact date. Throughout the later Middle Ages, and as late as 1874, the crown trading monopoly, instituted by the kings of Denmark, shut off the Faroes from commerce with Iceland on the one hand, and with the rest of Europe on the other; and though extensive smuggling doubtless occurred, smuggling is not a form of trade likely to lead to intermarriage. The fishermen of the Faroes met with fishing-smacks from Shetland on the high seas, and frequently hired themselves out to Shetland shipowners, learning to speak English from their mates, but they came home

with a scorn of Shetlanders as intense as the Icelanders' scorn of Faroemen, and it is worthy of note that the old dialect of Shetland, recently extinct, took a totally different line of development from that of the Faroes (8), though both sprang in the early Middle Ages from the old Norse, a language practically identical with the Icelandic of to-day. Young Faroe men and women who are anxious to make a little money still visit the west coast of Iceland during the fishing season, to help on the boats and with the preparation of salted fish, but the men rarely, if ever, bring home an Icelandic wife, and if a girl marries an Icelander she stays in Iceland.

As I have frequently heard it hinted that the dark strain in the population of the Faroes, especially of Suderoe, is due either to casual intercourse with Breton fishermen or to the raids of the Barbary corsairs, it may be well to consider whether there can be any truth in either or both of these insinuations. With regard to the Bretons' visits to the Faroes I have no information, but I have never heard it said that any of them settled in the islands; and the Faroe women are extremely modest, viewing the custom, so common in Iceland, of postponing marriage until a child is born or expected, with abhorrence. In Iceland, however, it is just possible that temporary connections formed between these foreign seamen and native women may have made dark complexions commoner in Reykjavik, as they certainly appear on casual inspection to be, than in the country districts, although, of course, a dark strain existed among the vikings themselves, and still exists in parts of Norway where Bretons and Algerians alike have been unknown, whether as a remnant of the aboriginal population, as is very possible, or as a result of intermarriage in the ninth century or earlier between the Norse raiders and their Irish captives, is very hard to say; probably its origin is mixed, perhaps even more mixed than has been suggested.

As regards the Barbary corsairs, I am doubtful whether they ever raided the Faroes. There is a tradition, it is true, on Naalsole to the effect that once, while all the men of that island were away at the fishing, the 'Turks' visited their homes and seized their women, but the women leapt into the sea from the ships to which they were hurried, and the 'Turks' cut off their breasts in the

water, so that they sank and were drowned. Mr Stanley Lane-Poole (9), moreover, in his *Barbary Corsairs*, states that Murād, a German renegade, "took three Algerine ships as far north as Denmark and Iceland, whence he carried off four hundred, some say eight hundred, captives . . .," and I have heard it stated in the Faroes that this expedition also visited these islands. Some years ago, while staying in the Westmann Isles, I took the trouble to translate the contemporary Icelandic accounts of Murād's raid, and of another, led by three Moorish captains, which also took place on the coast of Iceland in the same summer, that of 1627. These records (10) were collected and printed in Reykjavik about half a century ago. They contain no mention of a visit to the Faroes, and show that it is exceedingly improbable that any admixture of Algerian blood now exists even in Iceland. Between three and four hundred persons were taken prisoners by the two expeditions, and not more than forty, some of whom were women, got back to Iceland, the great majority being from the Westmann Isles, to which those who were ransomed by their friends or by the subscription raised for the purpose in Denmark returned. It is just possible that the women may have brought home with them children by Algerian masters, but it is exceedingly improbable that this would have been permitted; and even if they did, those who returned to the Westmann Isles, at any rate, have almost certainly left no descendants behind them, for all children, almost without exception, who were born there died within a fortnight after birth of *tetanus neonatorum*<sup>1</sup> until quite recently, and the islands were constantly being re-peopled from the north of Iceland, a region which the corsairs did not visit (11, 12).

#### CONCLUSIONS.

My object, as regards the first part of this paper, has been critical rather than constructive, for I do not believe that measurements on the living person, even in series of considerable magnitude, can give more than a rough sketch of the physical

<sup>1</sup> The islanders ascribe the recent extinction of this disease to the fact that while new-born children were formerly laid on a mass of uncovered feathers, they are now placed on a covered mattress.

characters of a race, and we do not yet know at all what is the physical result of crosses in the human species. The fact noted regarding the Faroese family whose ancestress came from Eastern Europe is of interest in this connection, although I am not able to give statistical details, for it shows how necessary it is that anthropologists should pay attention to that mysterious quality inherent in certain races and certain individuals — prepotency. Personally, I must express the great debt I owe to Professor D. J. Cunningham for calling my attention to this factor in ethnology, though it does not make the ethnologist's task the easier. With regard to the measurements themselves, it must be remarked how great an allowance must always be made for the idiosyncrasy of the observer in anthropometry on the living person. Some men naturally measure too short, some too long, and a couple of millimetres' divergency from ideal accuracy will often make a very much greater proportionate difference in an index where the numbers combined are small. If the observer would have even his own measurements of equal value on different occasions, he must take care to reproduce the conditions exactly, not only as regards his subjects, but also as regards himself; and above all, he must not attempt to measure more than a very few individuals at a sitting, for no other kind of purely mechanical investigation is more fatiguing to the mind and body, and a tired man is not in a condition to measure accurately.

By the combination of anthropometry with history and tradition it is possible to arrive at legitimate conclusions regarding the ethnology of the Faroes. The people, descended in the main from ancestors whose blood was somewhat mixed, but chiefly Norse, have remained more or less isolated for about a thousand years, except for casual immigration of persons and parties, who were probably 'Celtic' or Iberian, and who, it is safe to say, came either from Scotland, from Ireland, or from the intermediate isles. This casual admixture has taken place more frequently or in greater proportion, or the immigrants may have been more prepotent, in the most southerly island of the group. In-breeding may possibly have dwarfed the stature of the race, but details regarding imbecility and deafness are so indefinite that they may be well ignored, and after many weeks spent on different occasions in



different Faroe villages, I see no reason to believe that the race is physically or mentally degenerate. A point which needs investigation even more urgently than the ethnology of the Faroes is the development of the Icelandic race, which has been more strictly isolated than the Faroemen, and in which some interesting peculiarities, I believe myself, might be discovered, even with so rough a method of examination as a large series of measurements of living individuals.

It only remains for me to express my thanks to Sir William Turner for his encouragement in the study of physical anthropology, and to Professor D. J. Cunningham, at whose suggestion the investigations embodied above were undertaken.

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(Issued separately November 30, 1908.)

**Seiches observed in Loch Ness.** By Mr E. Maclagan-Wedderburn. *Communicated by Professor CHRYSTAL.*

(Read November 16, 1903. MS. received December 22, 1903.)

(*Abstract.*)

The first observations on seiches in Scotland were made last summer by members of the Lake Survey, the differences in level having been measured by a foot-rule. A Sarasin limnograph was procured by the Survey and was set up at Fort Augustus on Loch Ness in June of this year, and has been recording since then, with only a few stoppages. The biggest seiche so far recorded had an amplitude of about 9 cm. The boat-house of St Benedict's Abbey, kindly put at Sir John Murray's disposal by the Lord Abbot, gave shelter to the instrument both from wind and waves.

Three types of seiches are common on Loch Ness, with periods of approximately 31·5, 15·3, and 8·8 minutes. The first of these is probably the uninodal seiche. It seldom occurs pure, or of any considerable magnitude. This may be due to the influence of Loch Dochfour, which is a continuation of Loch Ness at the north-east end. The two lochs are connected by a narrow channel about 20 ft. deep, through which a strong current sometimes flows, and for this reason, in calculating the theoretical period of the seiche, it was thought proper to omit Loch Dochfour.

The period was calculated in two ways. First, by the formula  $t = 2 \int dl \sqrt{b/ag}$ , where  $b$  is the breadth and  $a$  the area of a cross section at any particular point. This is the formula obtained by assuming the hypothesis of parallel sections. The value obtained was 42 minutes, which is considerably in the excess of the observed value. The period was then calculated by the formula  $t = 2 \int dl / \sqrt{gh}$ , and the value obtained for  $t$  was 30·9 minutes, which agrees very closely with the observed value. This method assumes that the period of the seiche would be the same if the shores of the loch rose perpendicularly instead of obliquely.

The binodal seiche, whose period is about 15·3 minutes, is usually very well marked. It is the commonest type, and lasts longer than the uninodal seiche. The node is probably somewhere in the neighbourhood of Inverfarigaig, but has not yet been accurately determined. It is also interesting because its period is less than half the period of the uninodal seiche, although, according to Du Boys, it ought always to be greater than half; and in most lochs it is so, the most notable exception being Lake Geneva. The basin of Loch Ness is so regular that it is difficult to explain it, as was attempted in the case of Lake Geneva, by assuming an oscillation of part of the loch.

The polynodal seiche, whose period is 8·8 minutes, is always of small amplitude, but sometimes very regular. There are also oscillations of shorter period, but they do not occur regularly enough to allow of their measurement with any degree of accuracy. On one or two occasions there were embroideries on the curve, which may have been due to transverse seiches. Owing to the narrowness of the loch, the period of such a seiche would only be about 1 minute. These embroideries may be due to a variety of causes, such as the wash of steamers, the opening of the lock gates in the canal, etc. It will only be possible to determine whether they are vibrations or transverse seiches by simultaneous observations at the two sides of the loch.

The range of atmospheric conditions at Fort Augustus included thunderstorms and earthquakes, but these had no very marked influence on the loch. It seems probable that the cause of seiches is sudden local variations of atmospheric pressure; and this view is supported by the records of a barograph at Fort Augustus. The polynodal seiches, and perhaps the uninodal and binodal seiches also, may be started by sudden gusts of wind. The wind blows down the various glens in strong, almost vertical gusts, and this may be sufficient to start the oscillation.

All the speculations, however, regarding the causes of seiches can only be satisfactorily tested by quantitative measurements of the forces at work, and it is hoped that something will be done in this direction in the summer of 1904.

*(Issued separately January 15, 1904.)*

### The Bull Trout of the Tay and of Tweed.

By W. L. Calderwood. (With a Plate.)

The particular bull trout with which I desire to deal in this paper are the important migratory fishes which are commonly referred to by this name in Scotland. I make no mention of more or less monstrous examples of the common brown trout, or even of those examples of *S. fario* which have assumed a semi-migratory habit, and have become much modified by reason of their life in the estuaries of our larger rivers.

Amongst the true migratory salmonidæ are two fishes which I hope to show are distinct from one another, but concerning which considerable confusion seems at present to exist, because they are both called bull trout. This somewhat ambiguous term 'bull trout' is a familiar one throughout Scotland, but the two forms to which I here refer are well represented, the one in the Tay and the other in the Tweed, and it is convenient, therefore, to mention these two rivers specially, since they are, as it were, the homes of the separate forms. Parnell, in his essay on the Fishes of the Firth of Forth, describes and figures eight bull trout, to some of which he gives the name of 'salmon bull trout.' These fishes are placed as varieties of the species *S. eriox*, and are, curiously enough, included in part by Günther under his species *S. trutta* (*Brit. Mus. Cat.*, vol. vi. p. 26).

During last summer I had the opportunity of examining many Tay bull trout, and I am satisfied that this fish is the same as the 'salmon bull trout' of Parnell; and further, that it cannot be referred either to *S. eriox* or to *S. trutta*.

The bull trout of the Tay grows to a size beyond that ever attained by any variety of sea trout. Examples occur from 5 lbs. to 60 lbs. I have not myself seen any example approaching 60 lbs., and such are naturally extremely rare, but records in the possession of the Secretary of the Tay Salmon Fisheries Co. are sufficient to show that the fish attains as great weights as the salmon. During the past season two or three occurred well over 40 lbs., the heaviest

salmon being 51 lbs. On 6th July of this year (1903) seven bull trout were weighed together, and turned the scale at 214 lbs., showing the high average of 30 lbs. A small run of fish between 5 lbs. and 8 lbs. appeared with the grilse in July; and I may remark in passing that the Tay grilse are heavy as compared with the grilse of other rivers.

In general outline this so-called bull trout is in no way different from the shapely Tay salmon, and the appearance of the head, the outline of the gill cover, and shape of the preoperculum are identical. This is seen in Pl. fig. 1. The caudal fin also and the caudal peduncle are alike in like sizes of fish. The opportunity given me of viewing salmon interspersed with bull trout laid out in rows upon the sloping cement floor of the Tay Fisheries Co. Fish House at Perth enabled one not only to compare bull trout and salmon, but to note the variations which occur in both; and those variations I found to be in no way dissimilar.

The distinguishing feature of the bull trout is primarily one of surface marking. The dorsum is more or less thickly speckled with small black spots, and these are also to a varying extent displayed on the side, and more especially on the 'shoulder' of the fish below the lateral line. A well-marked bull trout has the spots below the lateral line continued backwards as far as the level of the dorsal fin. But when one examines a large number of fish, examples are readily found with few spots; and one notices that a diminishing gradation blends ultimately into an appearance which in no way differs from that seen in fish which are unquestionably pure salmon.

A peculiar characteristic of these fish, however, is the presence of 'maggots' (*Lerneopoda salmonea*, Linn.) on the gills, the parasite which commonly infests the gills of salmon kelts in fresh water. These bull trout coming from the sea into the river, and with tide lice (*Lerneophthirus*) upon them to prove their comparative cleanliness, are nevertheless usually infested by gill maggots.

I know of no other special features other than the two just mentioned whereby this so-called bull trout may be distinguished from salmon, and in my opinion no real structural difference exists.

A detailed examination reveals nothing in the dentition, fin-ray

formulæ, number of scales, from adipose fin to lateral line, or in the relative proportions of the head, which can be regarded as of any specific importance.

By fishermen these bull trout are judged by their spotted or speckled appearance and by the presence of maggots on the gills. In cases where the spots are so few as to render decision doubtful, the gills are examined, when, if maggots are present, the fish is regarded as a bull trout. For the table, the fish is considered as of inferior quality to the salmon, and it does not realise quite as high a price in the market.

I subjoin particulars of eleven of these fish examined at Perth on 15th August last, two of the examples being from Loch Ness, the others from the Tay. Length measurements are in each case made on the flat, without taking into account the round surface of the fish. Scales are counted from posterior margin of adipose fin obliquely forwards and downwards to lateral line.

No. 1. Female.

Length  $32'' \times 7\frac{1}{2}''$  ( $81.7 \times 18.4$  cm.);  
weight  $14\frac{1}{2}$  lbs.  
Length of head 15 cm.  
Post. margin of gill cover to back of  
eye 8.5 cm.  
Teeth only on head of vomer.  
Tail straight; caudal peduncle 5.8  
cm.  
Spots below lat. line.  
Scales 12.  
Fin rays, D 13, P 13.  
Maggots on gills.

No. 2.

Length  $39\frac{1}{2}'' \times 8\frac{1}{2}''$  ( $101 \times 21.7$  cm.);  
weight  $26\frac{1}{2}$  lbs.  
Length of head 20 cm.  
Eye to post. margin of gill cover  
11 cm.  
Teeth absent from vomer.  
Tail straight; caudal peduncle 7.2  
cm.  
Spots, none below lat. line or on  
head.  
Scales 12.  
Fin rays, D 13, P 13.  
Maggots on gills.

No. 3. Female.

Length  $42\frac{1}{2}'' \times 9''$  ( $107.8 \times 23$  cm.);  
weight 33 lbs.  
Length of head 21.3 cm.  
Eye to post. of gill cover 12.3 cm.  
Teeth absent from vomer.  
Tail straight; caudal ped. 8 cm.  
Spots numerous below lat. line and  
on head.  
Scales 12.  
Fin rays, D 14, P 12, A 12, V 8.  
Maggots on gills.  
A well-marked example.

No. 4. Female (Tay).

Length  $80\frac{3}{4}'' \times 6\frac{1}{2}''$  ( $78.5 \times 15.9$  cm.);  
weight  $11\frac{1}{2}$  lbs.  
Length of head 14 cm.  
Eye to post. of gill cover 8 cm.  
Teeth on head and one on shaft of  
vomer.  
Tail concave; caudal ped. 5.2 cm.  
Spots, only two on shoulder, below  
lat. line.  
Scales 11.  
Fin rays, D 14, P 12, V 9.  
Maggots numerous on gills.  
A shapely, salmon-like example.

## No. 5. Female (Tay).

Length  $31'' \times 7''$  ( $79.2 \times 17.8$  cm.);  
weight 13 lbs.

Length of head 14.3.

Eye to post. of gill cover 8.2 cm.

Teeth absent from vomer.

Tail straight; caudal ped. 5.6 cm.

Spots below lat. line to level of  
dorsal fin.

Scales 12.

Fin rays, D 12, V 9.

Maggots, very few.

## No. 6. Female (Tay).

Length  $28\frac{1}{2}'' \times 6\frac{1}{4}''$  ( $73.3 \times 16$  cm.);  
weight  $10\frac{1}{2}$  lbs.

Length of head 13.3 cm.

Eye to post. of gill cover 7.8 cm.

Teeth on head of vomer.

Tail concave; caudal ped. 5.2 cm.

Spots on shoulder below lat. line.

Scales 12.

Fin rays, D 13, V 9, P 12.

Maggots numerous.

This example had a marked  
salmon appearance.

## No. 7. Female (Tay).

Length  $32'' \times 7''$  ( $81.7 \times 17.8$  cm.);  
weight  $12\frac{1}{2}$  lbs.

Length of head 15 cm.

Eye to post. of gill cover 8.8 cm.

One tooth on head of vomer.

Tail straight; caudal ped. 5.8 cm.

Spots below lat. line to level of post.  
margin of dorsal fin.

Scales 12.

Fin rays, D 13, P 12, V 9, A 10.

Maggots, only two present.

## No. 8. Female (Tay).

Length  $36\frac{1}{2}'' \times 7\frac{1}{2}''$  ( $93.2 \times 19.2$  cm.);  
weight  $18\frac{1}{2}$  lbs.

Length of head 17.7 cm.

Eye to post. of gill cover 10 cm.

One tooth on head of vomer.

Spots, a small patch on shoulder  
only.

Scales 11.

Fin rays, D 13, P 12, V 9.

Maggots not numerous.

## No. 9. Female (from Loch Ness).

Length  $34\frac{1}{2}'' \times 8''$  ( $88 \times 20.3$  cm.);  
weight  $19\frac{1}{2}$  lbs.

Length of head 17 cm.

Eye to post. of gill cover 10 cm.

Teeth absent from vomer.

Tail straight.

Scales 12.

Fin rays, D 14, V 9.

Spots all along dorsum and also below  
lat. line to level of front of dorsal  
fin.

Maggot, only one present.

## No. 10. Female (from Loch Ness).

Length  $31'' \times 7''$  ( $79.2 \times 17.8$  cm.);  
weight  $12\frac{1}{2}$  lbs.

Length of head 15 cm.

Eye to post. of gill cover 8.5 cm.

Tail straight; caudal peduncle 5.8.

Scales, 11 on right side, 10 on left,  
distinct.

Fin rays, D 13, P 11, V 9.

Spots below lat. line to level of dorsal  
fin.

Maggots absent.

## No. 11 Female (Tay).

Length  $34\frac{1}{2}'' \times 7\frac{3}{4}''$  ( $88 \times 19.7$  cm.);  
weight  $17\frac{1}{2}$  lbs.

Length of head 17 cm.

Eye to post. of gill cover 9.3 cm.

Teeth absent from vomer.

Scales 12.

Fin rays D 14, V 9.

Spots, very few below lat. line ( $\frac{1}{2}$ ).

Maggots numerous.

Had appearance of ill-  
conditioned salmon.

In this series some fish were selected as having specially noticeable bull trout markings, others were less distinctly marked, while No. 6, when selected from amongst the other fish, gave rise to much discussion amongst the men present as to whether it

was a bull trout or salmon. It is the smallest fish of the series, being only 28 $\frac{3}{4}$ " long and 10 $\frac{3}{4}$  lbs. in weight, but it is interesting to compare it with the distinct bull trout nearest it in size, viz., No. 10, which is 31" long and 12 $\frac{1}{2}$  lbs. in weight—a Loch Ness fish.

	Head.	Tail fin.	Scales.	Fin formulæ.	Head in length of fish.
No. 6	13.3	concave	12	D 13 P 12 V 9.	5 $\frac{3}{4}$ times
10	15.0	straight	11/10	D 13 P 11 V 9.	5 times

No. 6 had a few spots on the shoulder below the lateral line and numerous maggots in the gills.

No. 10 had spots along the side to a level of the posterior margin of dorsal fin, but had no maggots.

The total absence of maggots is, I believe, rare.

That the bull trout of the Ness is quite similar to the Tay bull trout is well seen by comparing Nos. 9 and 11.

	Length.	Depth.	Weight.	Head.	Scales.	Fins.
No. 9	34 $\frac{1}{2}$ "	8"	19 $\frac{1}{2}$	17 cm.	12	D 14 V 9.
11	34 $\frac{1}{2}$ "	7 $\frac{3}{4}$ "	17 $\frac{3}{4}$	17 cm.	12	D 14 V 9.

The measurements of the head in each case show that, in the series, the length of the head is contained in the length of the fish from 5 times to 5 $\frac{1}{4}$  times, all measurements being of females. In the same way, the vertical measurement of the caudal peduncle is contained in the length of the fish from 13 $\frac{1}{2}$  to fully 15 times.

The belief that these Tay bull trout are in reality salmon receives what I think may almost be considered practical confirmation from certain recaptures of marked salmon which have recently been reported to me. Six Tay fish have been recaptured as bull trout which, when marked, were not noticed to show any trace of bull trout characteristics, but to be ordinary salmon.

No. 8161 { marked 14 lbs. : 36" : kelt : ♀ : 17th Jan. 1902 : at Battleby.  
 recaptured 33 lbs. : 48" : clean : 27th July 1903 : "Skin the Goat" station, near Newburgh.

(This fish may have ascended, spawned, and descended in the interval.)

No. 8311 { 13 lbs. : 32" : kelt : ♀ : 23rd Jan. 1902 : East Haugh, r. Tummel.  
 17 lbs. : 38" : clean : 15th Apr. 1903 : Flookie station, in tidal water.

No. 8343 { 6 lbs. : 24" : kelt : ♂ : 10th Feb. 1903 : East Haugh, r. Tummel.  
 14 $\frac{1}{2}$  lbs. : 33 $\frac{1}{2}$ " : clean : 20th Aug. 1903 : Pyeroad station, in tidal water.



- No. 8882 { 24½ lbs. : 37½" : clean : ♂ : 14th Nov. 1902 : Westshot, Linn of  
 Campsie.  
 22½ lbs. : 40" : clean : 13th Feb. 1903 : Flookie, in tidal water.
- No. 9005 { 7¾ lbs. : 27" : unspawned grilse : ♀ : 22nd Nov. 1902 : Almond-  
 mouth.  
 12 lbs. : 31¼" : clean : 13th Aug. 1903 : Needle station.
- No. 9402 { 4 lbs. : 25" : grilse kelt : ♀ : 5th Feb. 1903 : Logierait, Upper Tay.  
 10½ lbs. : 30 : clean : 31st July 1903 : Flookie station.

The intervals of time are, in order, 556 days, 447 days, 191 days, 91 days, 295, and 176 days. In other words, we have one recapture after 18 months, and, at the other extreme, a recapture after only 3 months, but this latter is peculiar, since the fish was clean run when marked. It is just possible that this fish, No. 8882, may have been descending (without having spawned) when recaptured. The loss of weight is significant.

I have already noticed that the gill maggots are commonly found on kelts. *Lerneopoda salmonea* is usually believed to be exclusively a fresh-water parasite. My attention was first called to the fact that this may not be the case in the results obtained by the marking of salmon which has been conducted by the Fishery Board for Scotland during recent years. A grilse kelt, marked in the Deveron on 11th March 1901 by a silver label numbered 6508, was recaptured on 11th July of the same year, at Cove, just south of Aberdeen. To have travelled in four months round the coast, passing, as it had done, the mouths of the rivers Ugie, Ythan, Don, and Dee, is sufficient to show that the fish must have been some time in salt water, and between marking and recapture it had gained 2¾ lbs. in weight, yet quite a number of maggots were still attached to the gills when I received the fish. This induced a more careful examination of the gills of fish ascending rivers from the sea, and during the continuance of salmon marking, Mr H. W. Johnston, who kindly associates himself with me in all the Tay markings, has noted, as I also have noted, many autumn fish with a few maggots in their gills—indeed, late-running fish are very commonly found with maggots. In salmon and grilse proper the maggots are never so numerous as in 'bull trout,' or fish with certain bull trout markings, but I regard it as most significant that fish fresh from the tide-way in the lower Tay should be so found. Our marking experiments have shown

that in our large rivers kelts frequently remain for surprisingly long periods after spawning. During a prolonged stay in fresh water the maggots remain fixed to the gills, and in some cases the fish do not regain their silvery appearance before entering the sea. The suggestion which I would venture upon is, that if such fish remain only a comparatively short time in the sea, or, it may be, remain a considerable time in the vicinity of the mouth of a large river like the Tay, the maggots will still be found attached to the gills on their return. Further, I think it very probable that the peculiar spotted appearance may arise under similar conditions; that the fish having, as it were, failed to visit good feeding grounds, and being, it may be, less fully nourished than the average salmon, exhibits to a varying degree this peculiar speckled appearance.

Since examining these fish, I find that in an addendum to Günther's *Brit. Mus. Catalogue*, vol. vi., reference is made to his seeing other specimens of bull trout taken from the Beaully. He states that in Lord Lovat's opinion some of those Beaully fish are hybrids between the salmon and the sea trout, "yet," he adds, "the relative size of the scales on the tail is in all these bull trout the same as in the salmon. Captain H. Fraser believes that other specimens of 'bull trout' are true salmon, which, having gone down to the sea as kelts, return to fresh water before having attained to the condition of well-mended fish. Thus, as regards the river Beaully at least, fishes named 'bull trout' do not constitute a distinct species." This was written in 1866, and I gather from it that Dr Günther would afterwards have probably altered the position which he assigns to 'the salmon bull trout of Parnell' taken from the Forth.

Captain H. Fraser's surmise is, I think, a correct one, applied not merely to Beaully fish but also to the so-called bull trout found in the Forth, Tay, Spey, Ness, and other rivers.

#### TWEED FISH.

Turning now to the bull trout of the Tweed district, we find at once a very different fish, and in this case a trout in reality. We have seen that Parnell classed his salmon bull trout under *S. eriox*, and I have ventured to assert that *S. salar* would have

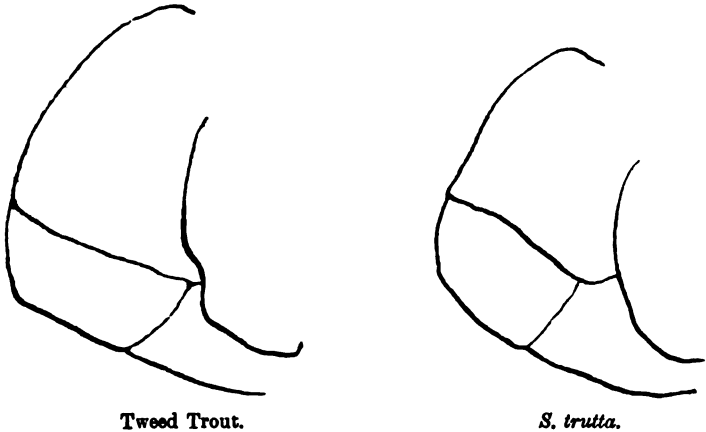
been a more appropriate title. This Tweed bull trout, otherwise known as the grey trout or round tail, is the *S. eriox*, as described by Yarrell, who, better I think than any other writer, seems to have recognised the rather distinct character of the fish. Günther refers to Yarrell's *S. eriox* under *S. cambricus*, the sewen, or English and Irish equivalent of our Scottish sea trout; and Day places the fish in the same category, with this difference, that he does not consider *cambricus* as specifically distinct from *trutta*.

Without entering at length into the wide question of the genealogy of migratory and non-migratory trout, it is advisable to recollect both the apparently great differences which exist between what I prefer to call local races of trout, and the infinite gradations which certainly exist to join such local races with one another and with the typical sea trout or the typical brown trout. The result of transporting brown trout eggs to New Zealand has shown how rapidly change of environment will produce a fish which our British Museum authorities diagnose as typical sea trout (*S. trutta*).

The late Sir James Maitland showed by different methods of feeding how Loch Leven trout could be made to resemble either *S. fario* or *S. trutta*; the beautifully silvery trout (*fario*) of some of our West Highland lochs inaccessible to ascending fish; the characteristics of estuary trout, of the Orkney trout, or, let us say, of the creature usually described as *Salmo ferox*, are enough to show that either we must have a great many species, in accordance with the view adopted by Günther, or, laying stress on the intermediate gradations, we must regard all trout as belonging to one species, and that a plastic, and therefore perhaps a comparatively recent species. The name *S. eriox* is as old as the thirteenth century. In 1824 Sir Humphrey Davy classed all our varieties under the name *S. eriox*; but it being maintained in 1878 that the fish referred to by Linnæus was in reality the young of *S. salar*, the term *eriox*, as applied to trout, was discarded, and by a process of gradual disentanglement from amongst the many specifically named creatures which in the interval had been described by naturalists, our present name of *S. trutta* has been brought into common use.

If we examine the Tweed bull trout, locally termed simply sea trout, as it comes from the sea at Berwick, its appearance is very different from that of the typical *S. trutta*. It is not a very silvery fish, and the sides are profusely spotted. This condition is constant in Tweed trout of all sizes. In examining a large number of these trout at Berwick last August, I was fortunate enough to find at the same time a single small specimen of the typical *trutta*, a fish of 2½ lbs. The brilliant sheen of this fish was very distinct from the rather faded grey appearance of the Tweed trout of the same size. The head had the conical appearance so characteristic of *S. trutta*—small in proportion to the length of the fish, with the maxillary bones well sunk into the surface, so as to give that smoothness and compact appearance which always seems to me a noticeable feature in typical examples of the species. The operculum and suboperculum united also in a rounded angle only slightly below the level of the eye. In the grey trout the head is flatter on the sides and the bones of the mouth more prominent, thus giving a coarser appearance to the head. The gill cover is more angular, and the angle is at a lower level, being in a line with, and sometimes even rather below, the level of the posterior extremity of the maxilla. On this account the lower margins of the suboperculum and interoperculum are straighter and more horizontal than in *trutta* or *salar*. A rather marked peculiarity of the preoperculum struck me, which does not appear to have been referred to by any of the authors whose works I have consulted. Instead of the posterior margin being gently curved or slightly sinuous, I found that the great majority of these fish have a crescent-shaped notch in the posterior margin of this bone. In a few cases two less distinct notches occurred, while in one or two examples three less deep notches were present, giving to the outline of this bone a rippling or undulatory appearance. In only one case out of the twelve or thirteen dozen fishes examined did I find no trace of indentations on the preopercular bones, while in one other case I found the bone of one side of the head with the usual deep single notch, while the bone of the other side of the head was unindented.

The typical gill cover I would represent thus :—



The general appearance of the head will be seen in the photographs of the male and female clean run fish (figs. 2 and 3). Relatively to the total length of the fish, I find that the head is contained from  $4\frac{1}{2}$  to  $5\frac{3}{4}$  times. The males examined in August varied from  $4\frac{1}{2}$  to  $4\frac{3}{4}$  times. The females in each case had the head measurement  $5\frac{3}{4}$  times in the length of the fish (measured on the flat).

The caudal fin is also a well-marked feature. At a comparatively early age this tail fin becomes truncate or rounded at its outer margin. In *salar* and in *trutta* proper this never happens, so far as I am aware, except in distinctly large fish. In the Tweed trout, however, fish between 6 and 7 pounds, or about 25 inches long, show this rounded tail—hence the name ‘round tail.’

The female specimen photographed is  $7\frac{1}{2}$  lbs. and 26 inches in length. The rounded tail is well seen. An example weighing  $2\frac{1}{2}$  lbs., and which was  $18\frac{1}{2}$  inches long, was found to have the caudal fin slightly forked when fully extended. From this slightly forked condition in young fish, the tail fin becomes first ‘straight,’ then, with increased size and age, the rounded outer border appears. Finally, in fish of 10 lbs. and upwards, a stunted aspect is frequently noticeable, the tail being not only rounded, but apparently so much thickened and grown-over by the caudal peduncle as to have the free portions of the caudal fin rays noticeably short. All large specimens have not this appearance, but it is

common amongst large examples; the tail is thick, short, and clumsy. The male of 12½ lbs. represented in the photograph has not this stunted tail. The Tweed trout is not often found of greater weight than 15 or 16 lbs. The heaviest fish of which I can find any record is one of 22 lbs., said to have been caught at Cornhill boat-house in either 1841 or 1842 (William Rochester, *Tweed Salmon Reports*, 1866, p. 102).

The caudal peduncle is, trout-like, comparatively broad, varying, I find, in the proportion of 12 to 13½ times the total length of the fish. In the finer-tailed salmon this measurement gives 13½ to 15 times.

The fish appears to retain its teeth on the shaft as well as on the head of the vomer to a more advanced age than is the case in the ordinary sea trout. No gill maggots were present in the fishes examined.

The following are particulars of a few selected specimens:—

No. 1.  
Length 31" × 6¾" (79·2 × 17·5 cm.);  
weight 13 lbs.  
Head 16·7 cm.  
Eye to post. margin of gill cover  
9·6 cm.  
Vomer teeth absent.  
Scales 14/14.  
Tail truncate; caudal peduncle 6·6 cm.  
Fins, D 12, P 13, A 10.

No. 2.  
Length 31½" × 6¾" (80·4 × 17 cm.);  
male; weight 13 lbs. 3 oz.  
Head 17·0 cm.  
Eye to gill cover 9·6 cm.  
Scales 12/13.  
Fins, D 11, P 13, A 10.  
Tail truncate; caudal peduncle 6·5  
cm.

No. 3.  
Length 30" × 6¾" (76·5 × 17 cm.);  
male; weight 12½ lbs.  
Head 16·0 cm.  
Eye to gill cover 9·0 cm.  
Teeth, two on head of vomer.  
Scales 13/13.  
Tail markedly truncate (2·2 cm.);  
caudal peduncle 6·2 cm.  
Fins, D 12, P 13, A 11.

The specimen photographed.

No. 4.  
Length 26" × 5¾" (66·3 × 14·6 cm.)  
female; weight 7¼ lbs.  
Head 11·7 cm.  
Eye to gill cover margin 7·1 cm.  
Teeth, two on shaft and two on head  
of vomer.  
Scales 14/14.  
Tail truncate; caudal peduncle 5·0 cm.  
Fins, D 12 (very distinct), P 12,  
A 10, V 9.  
Specimen photographed.

No. 5.  
Length 18" × 3½" (46 × 10 cm.);  
weight 2½ lbs.  
Head 8·5 cm.  
Eye to gill cover 5·0 cm.  
Teeth, 3 on shaft and also on head of  
vomer.  
Scales, R 13, L 11.  
Fins, D 11, P 12, A 10.

No. 6.  
Length 18" × 3½" (46 × 9·2 cm.);  
weight 2 lbs. 10 oz.  
Head 8·6 cm.  
Eye to gill cover 4·9.  
Teeth all along shaft of vomer and on  
head.  
Tail very slightly forked.

I am indebted to Sir Richard Waldie Griffith, Bart., Chairman of the Tweed Commissioners, for specimens in spawning condition taken later in the year.

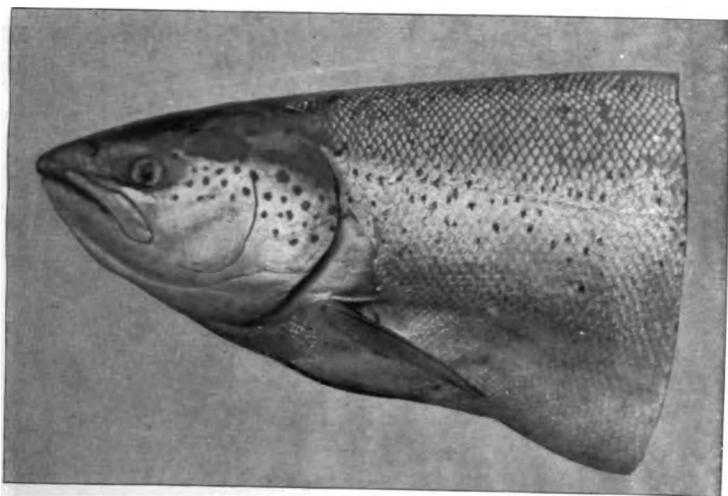
Though the Tweed trout cannot, in my opinion, be regarded as a species distinct from *trutta*, it is perhaps the best-defined variety of migratory trout in the British islands, and on this account might well, I think, retain the distinguishing name of *eriox*, in contradistinction to the variety *cambricus*. I am not familiar with the trout of the Coquet, but there seems no reason to doubt that the Tweed trout and the Coquet trout are of the same local race, and that Berwickshire and Northumberland form, as it were, the headquarters of the variety. Moreover, the history of the local fisheries seems to show that this variety has almost entirely superseded the sea trout proper. A point upon which more information is required is the relative distribution of this fish at the mouths of many of our Highland rivers, as referred to recently by Mr Harvie-Brown (*Fishing Gazette*, Oct. 10, 1903). In the Tweed, clean bull trout have been taken in January during netting for experimental purposes; and although the greatest runs are in early summer, and especially in late autumn, a certain number of fish are entering fresh water all the year round. They affect certain tributaries more than others, but push up to high spawning grounds.

In particulars of Estimated Annual Produce of the Fisheries of the River Tweed from 1808 to 1894, it appears that, whereas at the beginning of that period trout were less numerous than either salmon or grilse, in process of time trout became more numerous, first than salmon, and afterwards than grilse.

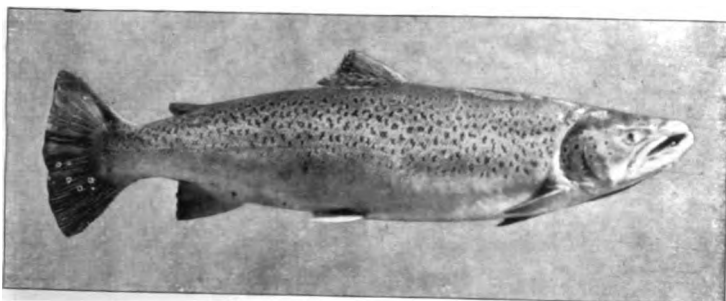
In 1808 the figures are 37,333 salmon, 25,324 grilse, and 21,033 trout. In 1844, the year of the maximum trout crop, there were 21,830 salmon, 88,003 grilse, and 99,256 trout. In 1894 we have a marked shrinkage, viz.—3271 salmon, 7877 grilse, and 18,535 trout.

The surprising manner in which this trout has asserted itself leads us more clearly to understand the well-defined character which the variety *eriox* now exhibits.

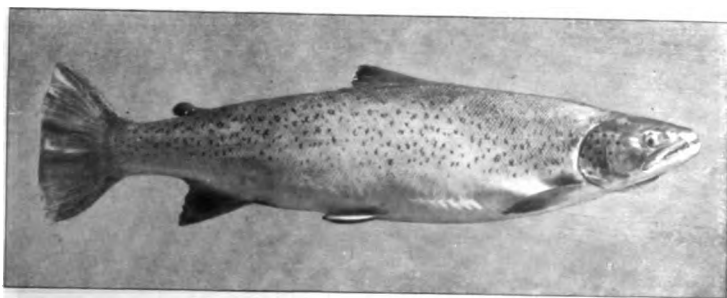
(*Issued separately January 30, 1904.*)



**FIG. 1.**



**FIG. 2.**



**FIG. 3.**

**MR W. L. CALDERWOOD.**





**The Relative Efficiency of certain Methods of performing Artificial Respiration in Man.** By E. A. Schäfer, F.R.S. (With a Plate.)

(Read December 21, 1903.)

Preliminary observations upon this subject, which were made by the author on behalf of a committee of the Royal Medical and Chirurgical Society of London, are published in a report presented by the committee and read on May 26th of this year before that Society.

The methods which were then investigated comprised traction by the arms with alternate relaxation, with and without chest compression; and pressure upon the chest walls alternating with relaxation from removal of the pressure; the subjects of the experiment being for each method placed successively in the supine, the prone and the lateral positions (in the last-named case one arm only being used for traction). In addition, the method of Marshall Hall was similarly tested. In this, the subject is alternately rolled over from the lateral to the prone position, expiration being assisted by pressure upon the back whenever the subject is brought to the prone position.

It was evident from those experiments that it is possible by nearly all the methods investigated to obtain an exchange of air per respiration as great as that of the tidal air, the sole exception being the methods in which traction alone, without alternating pressure upon the lower part of the chest, was employed.

The number of experiments which we were able to make at the time was, however, too limited to enable us to draw any positive conclusion regarding the relative value of the several methods of performing artificial respiration in man which have at various times been recommended, although the experiments clearly show the very important part which alternating pressure upon the

lower part of the chest plays in effecting the emptying and (by resiliency) the consequent filling of the lungs. It has seemed desirable, therefore, to supplement them by further experiments, having for their object the exact determination of the amount of air exchanged, not only per respiratory movement, but also per unit of time, a factor which was left out of account in the earlier experiments, but one, nevertheless, of considerable importance.

The apparatus which was used in the experiments referred to in the report consisted of a counterpoised bell-jar, filled with air and inverted over water; to or from this the air of respiration was conducted from the mouthpiece (or mask) by a curved tube which passed through the water and opened into the bell-jar. When, therefore, air was drawn by the movement of inspiration from the bell-jar this sank in the water, and when air was forced into it by the movement of expiration it rose. These movements of the bell-jar were recorded upon a slowly moving blackened cylinder, and the diameter and corresponding cubic contents of the bell-jar being known, the amount of air exchange was found by measuring the ordinates of the curves described on the cylinder. The readings, however, must be looked upon as only approximate, because, firstly, the bell-jar which was used was only approximately cylindrical; and secondly, because the counterpoised bell-jar acquired, with the somewhat rapid movements imparted to it, a swing of its own which must have affected the record.

In order to obtain more accurate measure of the amount of air exchanged in respiration, the apparatus which was employed in these earlier experiments has been discarded, and we have used a carefully constructed graduated gasometer (spirometer), counterpoised on the principle devised by the late Dr W. Marcet to avoid the error which arises from the fact that the more a gasometer is raised out of the water in which it is inverted, the greater is the pressure exerted upon its contents. The air which is pumped out of the chest is alone measured, but it is clear that an equal amount must afterwards pass in to take its place. The air is respired through either a mask or mouthpiece. In practice the latter is found to be the more convenient, as less liable to accidental leakage. When it is used, the nostrils must be occluded

by pinching the nose either by the fingers or by a spring clip. The tube which leads from the mouthpiece is forked, and each fork passes to a water valve, one for admitting air to the mouthpiece, and the other to enable the air which is driven out of the chest to pass through on its way to the gasometer. The air which is pumped into the gasometer can either be read off at once on a scale attached to the instrument, which is graduated in litres and tenths of a litre, or it can be graphically recorded by attaching a pen to the moving (ascending) gasometer, allowing this both to

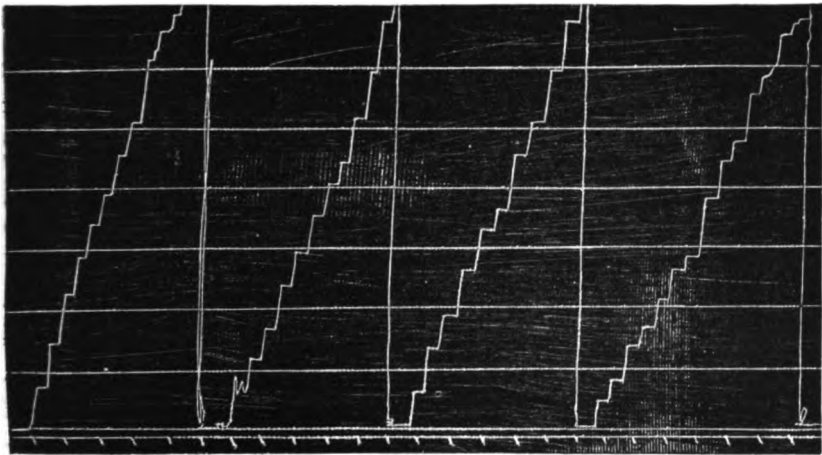


FIG. 3.—Silvester method.

register the extent of each movement and also the number of respiratory movements per minute upon a blackened drum revolving slowly by means of clockwork, and upon which a time tracing is also recorded. The tracings so obtained can be afterwards studied at leisure.

Fig. 1 is a photograph showing the arrangement of the apparatus.

Fig. 2 shows the manner in which any respiratory method is investigated by it. The method shown in the photograph is that of intermittent pressure upon the lower ribs, with the subject in the prone position.

Figs. 3, 4 and 5 are samples of tracings obtained by this method. The 'steps' upon each curve mark the successive

respiratory movements; each 'rise' gives the amount of air expired; inspiration occurs during the 'tread' of each step; the intervals between the horizontal lines represent 500 c.c.; the time tracing shows a mark every ten seconds.

The tracings reproduced in figs. 3, 4 and 5 were all taken at the same time and from the same individual. The experiment begins in each case at the bottom, and is continued until the pen has nearly reached the top of the paper. The drum was then stopped and the cylinder (and pen) lowered (continuous vertical

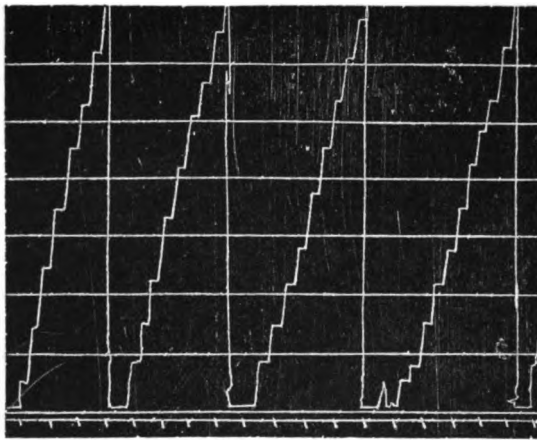


FIG. 4.—Supine pressure method.

line), and after a brief interval of natural respiration another record of the particular mode of artificial respiration which was being investigated was taken. Fig. 3 illustrates the amounts of air exchanged in the employment of the Silvester method\* (forcible raising and subsequently lowering the arms, followed by lateral pressure upon the chest); fig. 4, the amount exchanged when the Howard method† was used; and fig. 5, the amount exchanged by intermittent pressure over the lower ribs, with the subject

\* H. R. Silvester, *The Discovery of the Physiological Method of inducing Respiration in Cases of apparent Death from Drowning, Chloroform, Still-birth, Noxious Gases, etc. etc.*, 3rd edition, London, 1863.

† B. Howard, *Plain Rules for the Restoration of Persons apparently Dead from Drowning*, New York, 1869.

in the prone position. The amount of pressure used in the last two methods was approximately the same, having been produced by throwing the whole weight of the fore part of the body of the operator upon his hands, which were placed over the lowest part of the thorax of the subject, the only difference being that in the one case (Howard) the subject was supine, in the other prone. The pressure was in every case applied and removed gradually; a pressure of about 60 lbs. was thereby exerted.

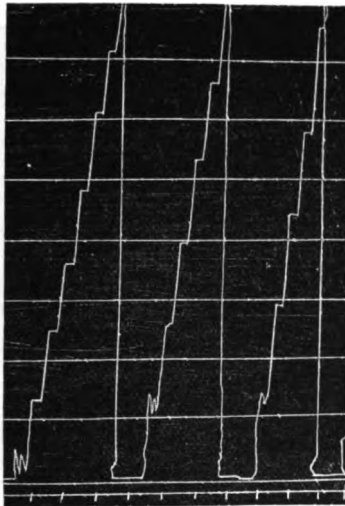


FIG. 5.—Prone pressure method.

Fig. 6 shows two tracings obtained by permitting the subject to breathe, under approximately natural conditions, into the spirometer, and the steps on these tracings give, therefore, an idea of the amount of tidal air. The rate of respiration on this occasion was about 16 per minute, and the average amount of air exchanged at each respiration (*i.e.* the amount of tidal air) was 385 c.c., or 6160 c.c. per minute. Before and after these two tracings, others were made with employment of the prone-pressure method; and these, which are also shown in the figure, illustrate well the efficiency of that method in providing a due exchange of air.

The following tables will serve to show the results yielded by the four principal methods which have been recommended for artificial respiration in man. In each case the respirations were performed during five minutes, but as the spirometer was only graduated to ten litres, it was necessary to take the amount of air yielded by each minute separately. In the intervals the subject was allowed to breathe naturally. There are also two tables (I. and II.) giving the amount of air breathed naturally into the spirometer, the circumstances being otherwise similar.

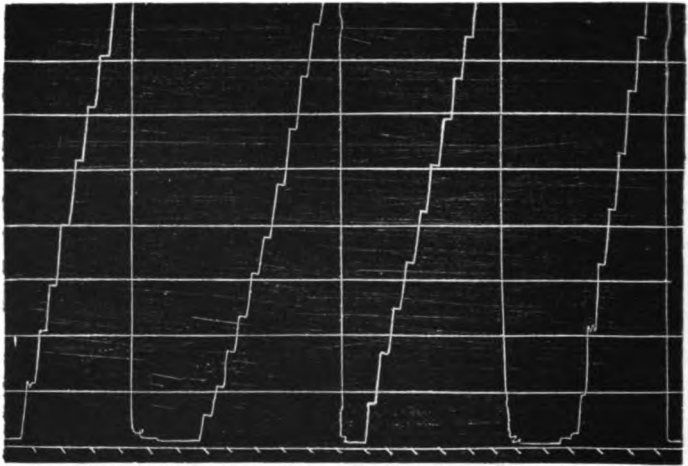


FIG. 6.—Two middle tracings, natural respiration ; two lateral tracings, artificial respiration by prone pressure method.

In the one series of these the subject was supine, in the other prone. Since, from the result recorded in these two tables, it appeared that the normal rate of respiration was about 13 per minute in the subject under the conditions of the experiment, this was the rate aimed at in performing artificial respiration. The same operator and the same subject took part in all the experiments. The amount of pressure produced by the weight of the upper part of the body of the operator when thrown forward on to his hands in performing the artificial respirations, shown in Tables IV. and VI., was determined to be about 60 lbs. The statistics of the subject of experiment are as follows:—

Male; age, 23; occupation, laboratory attendant; height, 5 feet 7½ inches (1.71 m.); chest measurement (at mammary line and in full inspiration), 38½ inches (0.978 m.); weight, 10 stone 1½ lbs. (64 kilog.); vital capacity, 4450 c.c.

TABLE I.—*Tidal Air of Natural Respiration—supine position.*

	Number of Respirations.	Amount of Air in Cubic Cent.
1st minute, . . . . .	14	6,700
2nd ,, . . . . .	13	6,200
3rd ,, . . . . .	14	6,500
4th ,, . . . . .	13	6,600
5th ,, . . . . .	12	6,300
In 5 minutes, . . . . .	66 respirations.	32,300 c.c. air respired.

*Remarks.*—The average number of respirations per minute was 13. The average amount of air exchanged per respiration was 489 c.c., and per minute 6460 c.c.

TABLE II.—*Tidal Air of Natural Respiration—prone position.*

	Number of Respirations.	Amount of Air in Cubic Cent.
1st minute, . . . . .	12	5,300
2nd ,, . . . . .	12	6,000
3rd ,, . . . . .	12	5,000
4th ,, . . . . .	13	4,200
5th ,, . . . . .	13	5,700
In 5 minutes, . . . . .	62 respirations.	26,200 c.c. air respired.

*Remarks.*—This gives about 12½ respirations per minute, with an air exchange per respiration of 422 c.c., and per minute of 5240 c.c.

Combining the results given in Tables I. and II., the tidal air of the individual under experiment averages 456 c.c.



TABLE III.—*Silvester Method.* (Forcible traction upon the arms, followed by bringing of the arms back to the side of the chest and pressure upon the chest.)

	Number of Respirations.	Amount of Air in Cubic Cent.
1st minute, . . . . .	13	3,700*
2nd ,, . . . . .	12	2,100
3rd ,, . . . . .	13	1,600
4th ,, . . . . .	13	1,700
5th ,, . . . . .	13	2,300
In 5 minutes, . . . . .	64 respirations.	11,400 c.c. air exchanged.

*Remarks.*—The average number of respirations per minute was 12·8, and the amount of air exchanged per respiration averaged 178 c.c., and per minute 2280 c.c.

The amount of physical exertion required to effect even this amount of air exchange was very great, and it would have been impossible to continue it for any length of time. Moreover, the subject could scarcely sustain the effort not to breathe, for the amount of air he was receiving was quite inadequate, his natural tidal air being about 450 c.c. per respiration, and 5850 c.c. per minute (see Tables I. and II.). The subject was on the ground, with a folded coat under the shoulders; the operator at his head, in a semi-kneeling posture.

TABLE IV.—*Supine Pressure (Howard's) Method.* (Intermittent pressure over the lower ribs, with the subject in the supine position.)

	Number of Respirations.	Amount of Air in Cubic Cent.
1st minute, . . . . .	14	4,000
2nd ,, . . . . .	14	4,100
3rd ,, . . . . .	14	3,900
4th ,, . . . . .	13	3,500
5th ,, . . . . .	13	4,600
In 5 minutes, . . . . .	64 respirations.	20,100 c.c. air exchanged.

\* The relatively large amount recorded here was probably due to the lungs having been unusually well filled by the subject just before the experiment commenced.

*Remarks.*—The average number of respirations was 13·6 per minute, and the amount of air exchanged works out at 295 c.c. per respiration, and 4020 c.c. per minute. Very little physical exertion is required with this method, especially with the patient on the floor, since it merely consists in throwing the weight of the operator's body forward upon his hands and alternately swinging back to relieve the pressure. The amount exchanged in this experiment, although far more than by the Silvester method, was not up to the tidal air standard, but the deficit was not sufficient to cause any feeling of distress to the subject of the experiment during the minute that each bout of respirations lasted.

TABLE V.—*Marshall Hall Method.\** (The patient is laid prone and rolled over to one side and back again, and so alternately. When in the prone position, pressure was during three of the five-minute intervals exercised upon the back of the chest.)

	Number of Respirations.	Amount of Air in Cubic Cent.
1st minute (with pressure), . . .	13	3,100
2nd ,, (with pressure), . . .	14	3,500
3rd ,, (without pressure; rolling only), . . .	12	2,400
4th minute (without pressure; rolling only), . . .	12	2,200
5th minute (with pressure), . . .	12	3,300
In 5 minutes, . . . . .	63 respirations.	14,500 c.c. air exchanged.

*Remarks.*—The average number of respirations was 12·6 per minute, and the amount of air exchanged per respiration comes to 230 c.c. If the three minutes during which pressure was alternated with the rolling over are alone taken into consideration, the exchange with each respiration works out at 254 c.c. The rolling without pressure gave 192 c.c. per respiration. Since the method as recommended by Marshall Hall embraces alternating

\* Marshall Hall, *Prone and Postural Respiration in Drowning, etc.*, London, 1857.

pressure upon the back, the highest of these three numbers may be adopted, viz., 254 c.c. per respiration (3300 c.c. per minute). This amount, as compared with the tidal air of 450 c.c. per respiration, and 5850 c.c. per minute, is obviously inadequate; and, conformably with this, the subject experienced distinct distress towards the end of each minute, even when pressure was used. In the experiments without pressure, the minutes had to be cut up on this account into two periods of half a minute each.

Although not a great deal of physical exertion is required to roll a body half over in this way some 12 or 13 times a minute and alternately to press upon the back, yet the labour is much greater than that required by the simple pressure method. Such efficiency as the method may have depends largely upon the alternating pressure, for without this the rolling is quite ineffective. The reason why this pressure produces less effect than in the method next to be considered appears due to the fact that the time taken up by the rolling enables less time to be given to the pressure, so that this is almost necessarily inadequately performed if the normal rate of respiration is kept up.

TABLE VI.—*Prone Pressure Method.*\*—(This is similar to the Howard method (intermittent pressure on the lower ribs), but the subject is in the prone position.)

	Number of Respirations.	Amount of Air in Cubic Cent.
1st minute, . . . . .	12	6,100
2nd ,, . . . . .	13	6,800
3rd ,, . . . . .	14	6,750
4th ,, . . . . .	12	7,000
5th ,, . . . . .	14	7,200
5 minutes, . . . . .	65 respirations.	33,850

*Remarks.*—The rate of respiration was on the average 13, and the amount of air exchanged averaged 520 c.c. per respiration,

\* This method is described in a paper communicated by the author to the Royal Medical and Chirurgical Society, which was read on December 8th, 1903, and will be published in the *Med. Chir. Trans.*

and 6760 c.c. per minute. It is the only method which, in this series of experiments, gave an amount equal to the normal tidal air of the individual—which was, in fact, somewhat exceeded. Not that it is impossible by other methods (especially those of Howard and Marshall Hall) to obtain larger figures for the exchange air than those given in the tables here shown—figures equal to or even larger than the tidal air—but merely because it is more difficult to do so at the rate of artificial respiration at which these experiments were carried on. The most important fact which the tables show is that at this rate (which is the normal rate of this particular individual, and not by any means a fast rate), it is easily possible to pump far more air into and out of the chest by the prone-pressure method than by any of the methods generally employed. The actual pressure exerted upon the prone subject was not greater, probably rather less, than upon the supine subject, in which the *full* weight of the fore part of the operator's body was certainly thrown upon the lower ribs, whereas in the similar experiments upon the prone subject the outflow of air on making pressure on these ribs was so abundant and easy that there was a tendency for the operator not to throw the whole weight on the hands; even more air, therefore, could have been exchanged if desired.

TABLE VII.—*The following Table gives the main results of all the foregoing Tables in a summarised form.*

Mode of Respiration.	Number per Minute.	Amount of Air exchanged per Respiration.	Amount of Air exchanged per Minute.
Natural (supine), . . .	13	489 c.c.	6,460 c.c.
Natural (prone), . . .	12·5	422 "	5,240 "
Prone pressure, . . .	13	520 "	6,760 "
Supine pressure, . . .	13·6	295 "	4,020 "
Rolling (with pressure), . . .	13	254 "	3,300 "
Rolling (without pressure), . . .	12	192 "	2,300 "
Traction (with pressure), . . .	12·8	178 "	2,280 "

Results similar in character to the above have been yielded by many experiments, both upon the same and upon different individuals. These experiments all show that by far the most efficient method

of performing artificial respiration is that of *intermittent pressure upon the lower ribs with the subject in the prone position*. It is also the easiest to perform, requiring practically no exertion, as the weight of the operator's body produces the effect, and the swinging forwards and backwards some thirteen times a minute, which is alone required, is by no means fatiguing.\* This statement also applies to the supine-pressure method when effected slowly and without undue violence. But not only is this method less efficient than the prone-pressure method, but there are undoubted dangers attending it, especially in those cases where the asphyxial condition is due to drowning. For in drowned individuals the liver is enormously swollen and congested, and ruptures easily, as Dr Herring and I found when endeavouring to resuscitate drowned dogs by this method of artificial respiration.† And further, the supine position is contra-indicated both in drowning and in asphyxia generally, since it involves the risk of obstruction of the pharynx by the falling back of the tongue, and also fails to facilitate the escape of water, mucus, and vomited matter from the mouth and nostrils.

The Silvester method, as compared with the others, has nothing in its favour. It has all the disadvantages of the supine position, is most laborious, and is relatively inefficient. As regards the Marshall Hall method, the most effectual part of that method is the exertion of pressure in the prone position; the rolling over is quite unnecessary, and attended by manifest disadvantages. The addition to this method which is advocated by Bowles,‡ consisting in raising the one arm over the head after the body is placed in the lateral position, has been found, in measurements we have made, to introduce no serious augmentation in the amount of air exchanged, but merely serves to render it still more difficult to perform the respiratory movements efficiently at the necessary rate.

\* I have on one occasion continued it for nearly an hour without experiencing the least fatigue, and without the subject having any desire to breathe naturally or feeling at all inconvenienced.

† Report of Committee of Royal Medical and Chirurgical Society, *op. cit.*

‡ R. L. Bowles, *A Method for the Treatment of the apparently Drowned*, London, 1903.

(Issued separately January 29, 1904.)



FIG. 1.

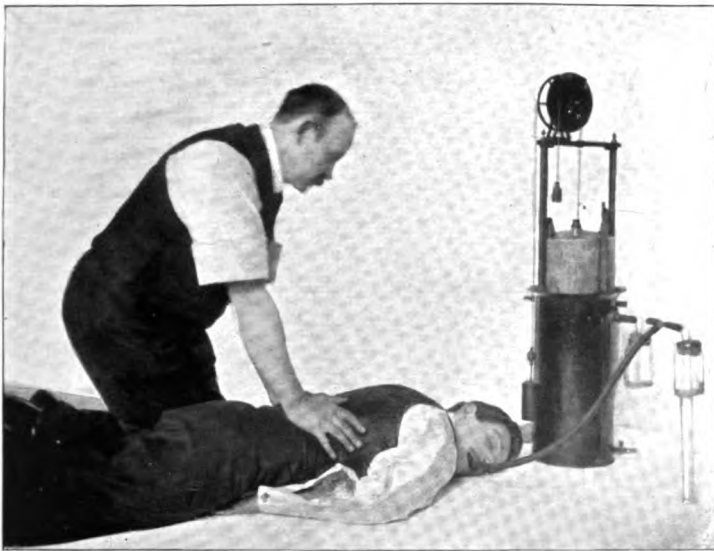


FIG. 2.

PROF. E. A. SCHÄFER.



**Physico-Chemical Investigations in the Amide Group.**By **Charles H. Fawsitt**, Ph.D., B.Sc. (Edin. and Lond.).*Communicated by Professor CRUM BROWN.*

(MS. received December 14. Read December 21, 1903.)

Some time ago, while studying the chemical dynamics of the changes which occur in solutions of urea or carbamide,\* I came upon some rather unexpected results which led me to hope that investigations conducted on somewhat the same lines with other substances of the amide group might prove to yield results of some interest. The amides referred to are those derived from carboxylic acids. While proceeding to this investigation I noticed some measurements,† obtained in connection with the viscosity of aqueous solutions of carbamide, which appeared of sufficient interest to demand an inquiry into the nature of solutions of this class of substances before proceeding further with the subject of inquiry in the manner at first intended.

*The Viscosity of the Amides in Aqueous Solution.*

The viscosity of solutions is a problem on which a considerable amount of work has been carried out, and the way in which the viscosity of a solution changes with the concentration of the substance dissolved has been found to be generally in agreement with the formula

$$\eta_x = A^x \text{ (i),}$$

where  $\eta_x$  is the viscosity of a solution of concentration  $x$ , the viscosity of water being taken as unity and where  $A$  is a constant. Some observers have shown that results occasionally follow the formula

$$\eta_x = 1 + ax \text{ (ii),}$$

where 'a' is a constant. It will be noticed, however, that if

\* *Zeit. für physikal. Chemie*, 41, 601 (1902).† Rudolf, *Zeit. für physikal. Chemie*, 43, 257 (1903).



' $a$ ' is small and also  $x$ , equation (ii) is really a particular case of equation (i); for we may put (i) in the form

$$\eta_s = 1 + x \log_e A + \frac{x^2 \log_e^2 A}{2!} + \frac{x^3 \log_e^3 A}{3!} + \dots$$

or, putting  $\log_e A = a$

$$\eta_s = 1 + ax + \frac{a^2 x^2}{2!} + \frac{a^3 x^3}{3!} + \dots \text{ (iii).}$$

Considering aqueous solutions, we may (roughly) divide the dissolved substances into electrolytes and non-electrolytes. In the former class substances are known, e.g. potassium chloride, which do not follow the above formula (iii), but possess what may be called a 'negative' viscosity. Thus the viscosity of  $\frac{1}{2}$  normal potassium chloride is less than that of water. Up to the present no non-electrolyte has been found to show this 'negative' viscosity. In the paper mentioned above, Rudolf drew attention to the fact that carbamide in dilute aqueous solution shows a 'negative' viscosity. I have repeated these measurements, and have also made determinations of the viscosity of acetamide in solution. These substances show a normal behaviour in their depression of the freezing-point.\*

#### Carbamide (Urea).

Concentration.	$\eta_1$	$\eta_2$	$\Delta$
$\frac{1}{2}$ mol. . . . .	1·005 ...	1·005 ...	·0
$\frac{1}{4}$ ,, . . . . .	1·012 ...	1·011 ...	-·001
$\frac{1}{3}$ ,, . . . . .	1·024 ...	1·022 ...	-·002
mol. . . . .	1·015 ...	1·045 ...	...
2 ,, . . . . .	1·089 ...	1·092 ...	+·003

#### Acetamide.

Concentration.	$\eta_1$	$\eta_2$	$\Delta$
$\frac{1}{2}$ mol. . . . .	1·018 ...	1·014 ...	+·001
$\frac{1}{4}$ ,, . . . . .	1·028 ...	1·028 ...	...
$\frac{1}{3}$ ,, . . . . .	1·057 ...	1·057 ...	...
mol. . . . .	1·117 ...	1·118 ...	+·001
2 ,, . . . . .	1·250 ...	1·250 ...	...

$\eta_1$  is the viscosity determined experimentally;  $\eta_2$  that calculated from equation (iii).  $\Delta$  is difference of the calculated value from that observed; ( $a$ ) in the case of carbamide being taken as ·044,

\* *Zeitschrift für physikal. Chemie*, 2, 491 (1889).

and in the case of acetamid as ·111. The calculated and observed values agree well with one another. There is no indication of any negative viscosity in the case of carbamide. As the substance employed was very pure, I have some difficulty in explaining the different result obtained by Rudolf. In case the solution used by him had undergone any decomposition (into ammonium cyanate), I heated some  $\frac{1}{4}$  mol. solution of urea for an hour at 100° C. to see whether the production of ammonium cyanate would affect the result: the solution had a viscosity almost identical with the result previously obtained for pure urea, an *increase* of ·002 being found.

#### THE CHEMICAL NATURE OF THE AMIDES.

The amides are above described as non-electrolytes, but I thought it might be of interest to inquire as to how far this was the case, and to what the amides owe such conductivity as they do possess. In the following measurements I have used urea as the amide.

The amides are known to form compounds with acids. Thus urea and hydrochloric acid give the compound  $\text{CO}(\text{NH}_2)_2 \cdot \text{HCl}$ .

These compounds are split up very largely into amide and acid again by dissolving in water.

Walker showed\* that the concentration of free acid in a solution is gradually decreased by the addition of urea, and the relations here may be represented by the formula

$$\frac{C_{\text{CO}(\text{NH}_2)_2} \times C_{\text{HCl}}}{C_{\text{CO}(\text{NH}_2)_2 \cdot \text{HCl}}} = K$$

where  $C_x$  is the concentration of the substance  $x$  and  $K$  is a constant.

He found that if the concentration of  $\text{H}^+$  ions in normal hydrochloric acid be represented by the number 315 (25° C.), then the concentration after addition of urea was as follows:—

Norm. HCl	.	.	.	315
„	+ $\frac{1}{2}$ mol. $\text{CO}(\text{NH}_2)_2$	.	.	237
„	+ 1 „	„	.	184
„	+ 2 „	„	.	114
„	+ 3 „	„	.	82
„	+ 4 „	„	.	60

\* *Zeit. für physikal. Chemie*, 4, 319 (1889).

I have represented these results in fig. 1.

The diminution in concentration of the  $H^+$  ions may be observed

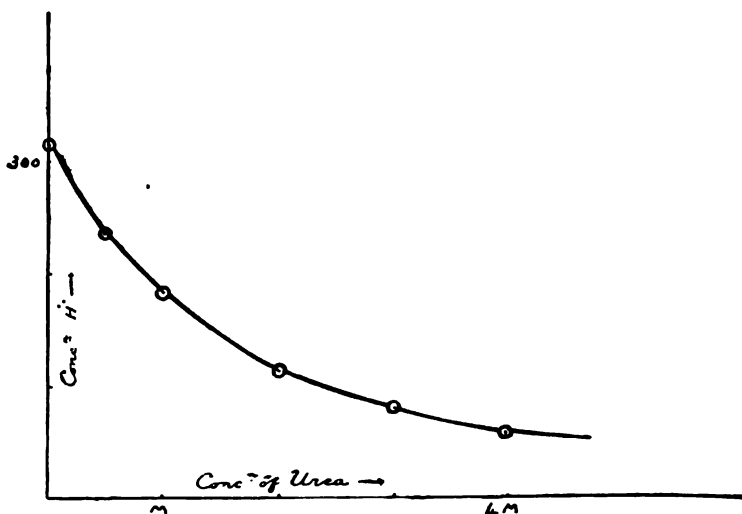


FIG. 1.

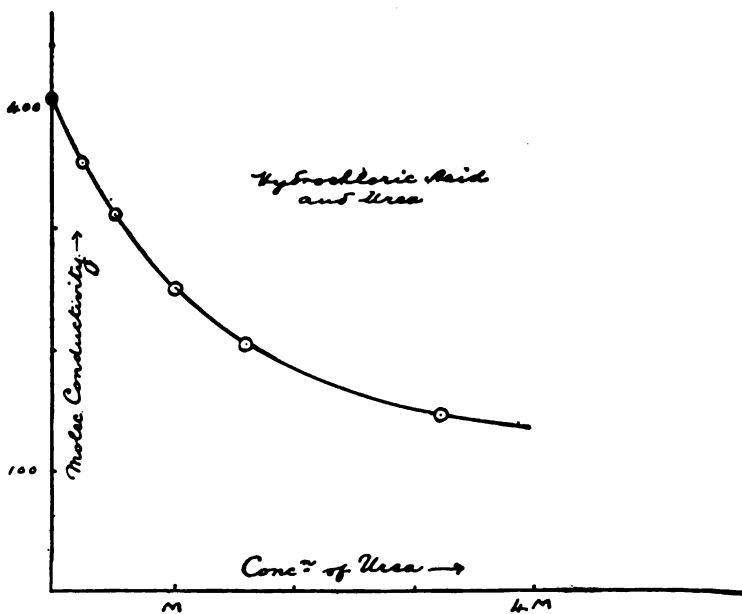


FIG. 2.

by making measurements of the electrical conductivity. On the addition of urea we have the ion  $\text{CO}(\text{NH}_2)_2\text{H}^+$  forming at the expense of the  $\text{H}^+$  ion, but the mobility of this new ion, as indeed of all other kations, is considerably less than that of  $\text{H}^+$ . Below are results I have obtained for urea and hydrochloric acid at  $34.2^\circ \text{C}$ . (The relations are practically unaltered at other temperatures between  $25^\circ$  and  $100^\circ \text{C}$ .)

*Urea and Hydrochloric Acid.*

Concentration.	Molec. Conductivity.
$\frac{1}{2}$ norm. HCl	406.8
„ $+\frac{1}{2}$ mol. $\text{CO}(\text{NH}_2)_2$ . . . .	353
„ $+\frac{1}{2}$ „ „ . . . .	312
„ $+1$ „ „ . . . .	250
„ $+1.6$ „ „ . . . .	206
„ $+3.2$ „ „ . . . .	147.6

These results are reproduced in figure 2, giving a curve very similar to that in figure 1. It will be noticed in these curves that the effect produced by the urea falls off greatly in the higher concentrations.\*

To show the effect of urea on the electrical conductivity of a neutral salt in solution, I have measured the conductivity of a solution of potassium chloride with varying additions of urea,

*Urea and Potassium Chloride;  $25^\circ \text{C}$ .*

Concentration.	Molec. Conductivity.
$\frac{1}{2}$ norm. KCl	116.4
„ $+\frac{1}{2}$ mol. $\text{CO}(\text{NH}_2)_2$ . . . .	115.3
„ $+\frac{1}{2}$ „ „ . . . .	114.6
„ $+1$ „ „ . . . .	111.9
„ $+1.6$ „ „ . . . .	108.7
„ $+3.2$ „ „ . . . .	100.1

It will be seen that the percentage decrease here is very much less than in the last case. The results are given in the curve (fig. 3). The form of the curve is also different from the last case, being almost a straight line, but slightly concave towards the abscissa axis.

In the present case we may assume that there is no measurable salt formation in solution. The decrease of conductivity may be

\* Compare also *Journ. Chem. Soc.*, 79, 707 (1901).

looked on as due to increased viscosity of the solution, as will be shown further on.

An amide is usually represented by the formula  $R - \overset{\text{O}}{\parallel} C - NH_2$ , where R stands for some radical. The formula  $R - \overset{\text{O}}{\parallel} C - OH$  has

also been suggested, although recent work \* favours the adoption of the former. In investigating the constitution of such sub-

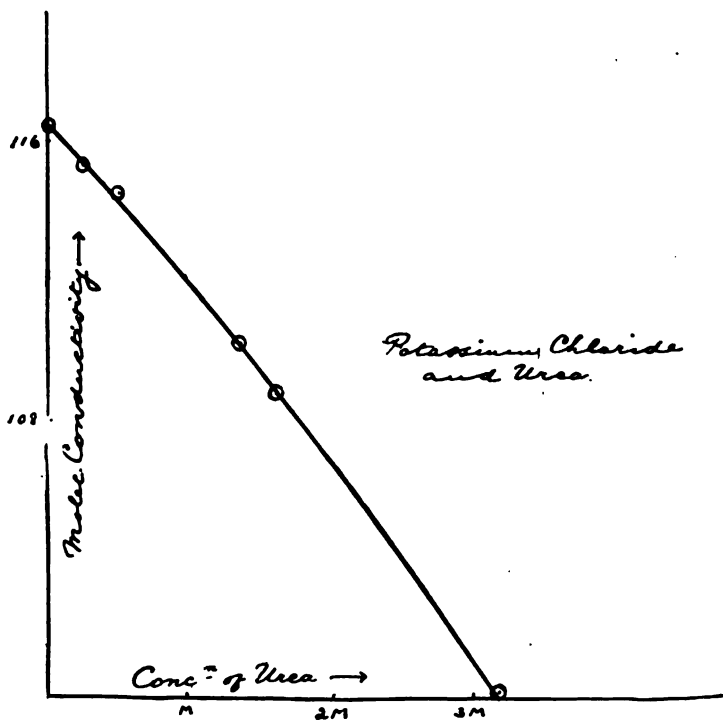


FIG. 3.

stances, it is generally agreed that physical methods give the most reliable results to draw conclusions from. Now, if  $R - \overset{\text{O}}{\parallel} C - OH$

represented the formula of an amide, we should expect a substance of this kind to show at least feebly acid properties. I have

\* *Berl. Berichte*, 34, 3142, 3161, 3558.

investigated this by measuring the electrical conductivity of sodium hydrate solution with varying additions of urea.\*

*Sodium Hydrate and Urea; 25° C.*

Concentration.	Molec. Conductivity.
$\frac{1}{2}$ norm. NaOH, . . . . .	194.2
" " + $\frac{M}{4}$ urea, . . . . .	191.8
" " + $\frac{M}{2}$ " . . . . .	188.7
" " + M " . . . . .	183.0
" " + 2M " . . . . .	172.0

By adding  $\frac{1}{2}$  mol. urea to hydrochloric acid, potassium chloride

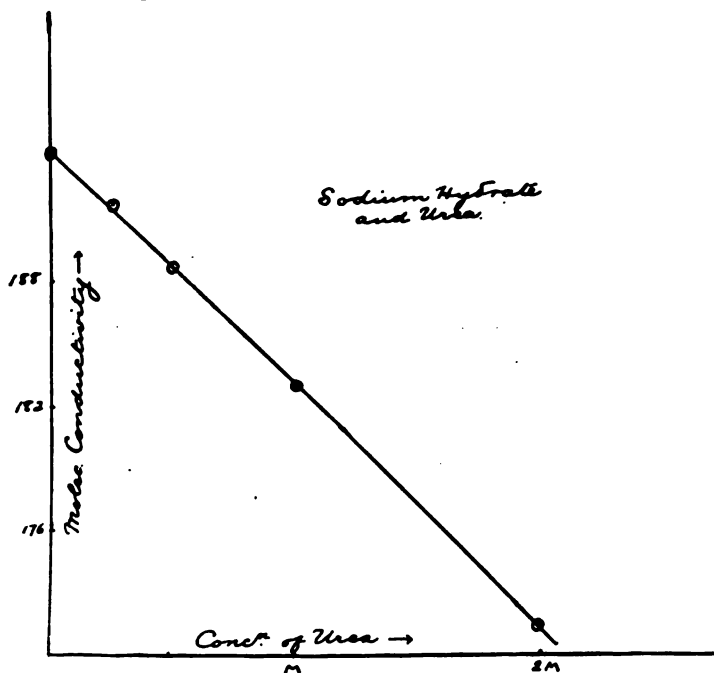


FIG. 4.

and sodium hydroxide, we obtain depressions of the conductivity by 23.6%, 1.6%, and 2.8% respectively. As, among anions, -OH' wanders faster than any other ion, we would have expected a much

\* Winkelblech (*Zeit. für physikal. Chemie*, 36, 576 {1901}) has experimented with dilute solutions  $\frac{1}{4}$  to  $\frac{1}{16}$  molec.; at these dilutions signs of salt formation could hardly be expected.

larger decrease in the last case than that actually found if there had been any acidic character at all about urea. Further, the form of the curve obtained here (figure 4) resembles very closely that obtained for the case of urea and potassium chloride. We conclude, then, that there is no measurable acid function in the amides. As the basic character is itself only a slight one, we should expect that aqueous urea solutions would conduct the electric current feebly. The ions here in the case of urea are  $\text{CO}(\text{NH}_2)_2\text{H}'$  and  $-\text{OH}'$ , and the dissociation constant  $K = \frac{\text{CO}(\text{NH}_2)_2\text{H}' \times \text{OH}'}{\text{CO}(\text{NH}_2)_2\text{H}_2\text{O}}$  has been calculated from the amount of salt formation between urea and hydrochloric acid\* to be  $1.5 \times 10^{-14}$  ( $25^\circ \text{C}$ ). The value of the dissociation constant for water is  $.8 \times 10^{-14}$ . Such water has a specific conductivity of  $.05 \times 10^{-6}$ , but it is impossible, under ordinary conditions, to prepare water anything like this. With water purified by ordinary methods we should be able to prepare a solution of urea having almost identically the same conductivity as the water used. Using water of spec. conductivity  $1.5 \times 10^{-6}$ , I have prepared urea solutions  $\left(\frac{\text{M}}{32}\right)$  having a conductivity indistinguishable from that of the water. The purest specimen of urea obtained by recrystallisation from alcohol gave a molecular solution (60 grams per litre) of spec. conductivity  $2.8 \times 10^{-6}$ . There is little doubt that this small amount of conductivity, in excess of that of the pure water, is due to impurity in the urea, but the determination is of interest in so far as it shows how pure such substances may be obtained by the ordinary process of recrystallisation. In preparing other amides in a pure state I have found the determination of electrical conductivity a very useful means of following the purification.

*The Viscosity of some of the above-mentioned Solutions.*

I next give some measurements of the viscosity of solutions containing (a) potassium chloride and urea, (b) hydrochloric acid and urea; and in making these determinations I have had the valuable assistance of Mr Clerk Ranken, B.Sc., to whom I wish

\* Wood, *Journ. Chem. Soc.*, 83, 484 (1903).

to express my thanks. With these solutions I have calculated values of the viscosity from the formula

$$\eta_x = R \left\{ 1 + ax + \frac{a^2 x^2}{2!} + \frac{a^3 x^3}{3!} + \dots \right\}$$

where R is the viscosity of the KCl or HCl and the other letters are as before.

*Potassium Chloride and Urea (25° C.).*

Concentration.	Viscosity		Δ
	Observed.	Calculated.	
½ norm. KCl . . . . .	.995	...	...
„ + ½ mol. urea, . . .	1.007	1.006	- .001
„ + ½ „ . . . . .	1.017	1.017	...
„ + .7 „ . . . . .	1.030	1.026	- .004
„ + 1 „ . . . . .	1.043	1.040	- .003
„ + 1.4 „ . . . . .	1.066	1.058	- .008
„ + 1.6 „ . . . . .	1.075	1.068	- .007

'α' is here taken equal to .044, as also in the next series.

*Hydrochloric Acid and Urea (25° C.).*

Concentration.	Viscosity		Δ
	Observed.	Calculated.	
½ norm. HCl . . . . .	1.033	...	...
„ + ½ mol. urea . . .	1.045	1.045	...
„ + ½ „ . . . . .	1.054	1.056	+ .002
„ + .7 „ . . . . .	1.063	1.065	+ .002
„ + 1 „ . . . . .	1.081	1.080	- .001
„ + 1.4 „ . . . . .	1.102	1.099	- .003
„ + 1.6 „ . . . . .	1.114	1.109	- .005

The observed and calculated values for the case of KCl and CO(NH<sub>2</sub>)<sub>2</sub> agree very well up to 1 mol. urea. For the case HCl and CO(NH<sub>2</sub>)<sub>2</sub> the agreement is also fairly good.

The viscosity of KCl and CO(NH<sub>2</sub>)<sub>2</sub> is represented in fig. 5: it appears as almost an exact reverse\* of the conductivity curve (fig. 3).

\* Compare also *Phil. Mag.*, 6, iii. 487 (1902).



*Summary.*—(1) The amides show no acid character, and according to this view they are better represented by the formula  $R - C - NH_2$  than by  $R - C - OH$ .



(2) The non-conductivity of the amides in aqueous solution is a good criterion for their purity.

(3) The viscosity of pure aqueous solutions of acetamide and

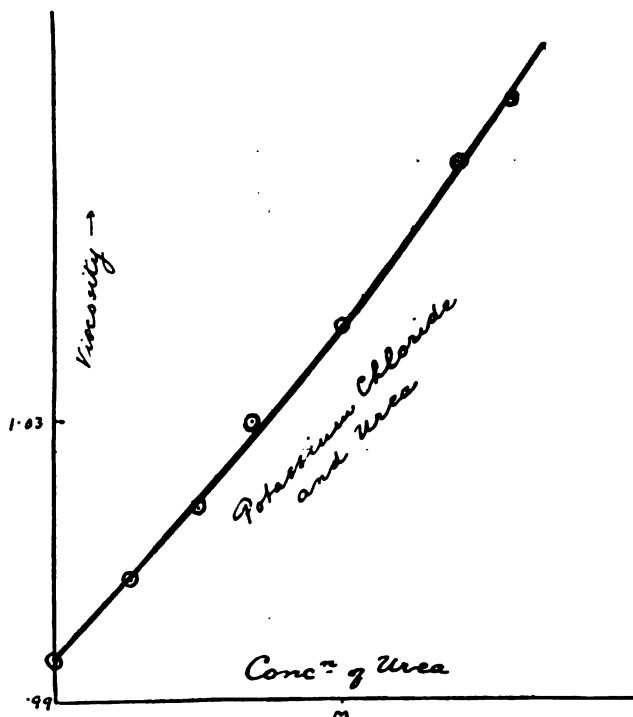


FIG. 5.

carbamide follows the formula  $\eta_x = A^x$ , where  $\eta_x$  is the viscosity of a solution of concentration  $x$  and  $A$  is a constant.

(4) A comparison of the viscosities and conductivities of a solution of potassium chloride, to which varying amounts of an amide were added, shows that the two are very closely related.

(Issued separately February 6, 1904.)

**The Theory of General Determinants in the Historical Order of Development up to 1846. By Thomas Muir, LL.D.**

(MS. received August 10, 1903. Read November 2, 1903.)

Since the year 1889, when the last of a series of six papers with a title similar to the above appeared, further research has led to the discovery of a number of writings belonging to the period then dealt with, viz., 1693-1844. Of those an account is now given before proceeding to the papers of later date than 1844.

FONTAINE (1748).

[Mémoires donnés à l'Académie Royale des Sciences, non imprimés dans leurs temps. Par M. Fontaine\* de cette Académie. 588 pp. Paris, 1764.]

These memoirs of Fontaine's, sixteen in number, cover a considerable variety of mathematical subjects: it is the seventh of the series which indirectly concerns determinants. There is not, however, even the most distant connection between it and the work of Leibnitz. The heading is "Le calcul intégral. Seconde méthode," the sixth memoir having given the first method. The date is indicated in the margin.

The matter which concerns us appears as a lemma near the beginning of the memoir (p. 94). The passage is as follows:—

"Soient quatre nombres quelconques

$$a_1, a_2, a_3, a_4,$$

\* The full name is *Alexis Fontaine des Bertins*. The very same collection was issued in 1770 under the less appropriate title *Traité de calcul différentiel et intégral*. Vandermonde is said to have been a pupil of Fontaine's (*v. Nouv. Annales de Math.*, v. p. 155).

et quatre autres nombres aussi quelconques

$$a_1, a_2, a_3, a_4 ;$$

faites

$$a_1 a_2 - a_1 a_2 = a^1 1,$$

$$a_2 a_3 - a_2 a_3 = a^1 2,$$

$$a_3 a_4 - a_3 a_4 = a^1 3,$$

$$a_1 a_3 - a_1 a_3 = a^2 1,$$

$$a_2 a_4 - a_2 a_4 = a^2 2,$$

$$a_1 a_4 - a_1 a_4 = a^3 1,$$

vous aurez

$$a^3 1 a^1 2 - a^2 1 a^2 2 + a^1 1 a^1 3 = 0."$$

Manifestly this is the identity which in later times came to be written

$$|a_1 b_2| \cdot |a_3 b_4| - |a_1 b_3| \cdot |a_2 b_4| + |a_1 b_4| \cdot |a_2 b_3| = 0,$$

and which, so far as we know, appeared first in its proper connection in the writings of Bezout.

It is curious to note that Fontaine was not satisfied with the lemma in this form, but proceeded to take "autant de nombres quelconques que l'on voudra  $a_1, a_2, \dots, a_{10}, \dots$ " and wrote the identity one hundred and twenty-six times before he appended "et cetera," the 126th being

$$a^9 6 a^1 7 - a^2 6 a^2 7 + a^1 6 a^1 8 = 0.$$

CAUCHY (1829).

[Sur l'équation à l'aide de laquelle on détermine les inégalités séculaires des mouvements des planètes. *Exercices de Math.*, iv. ; or *Œuvres* (2), ix. pp. 172-195.]

As the title would lead one to expect, the determinants which occur in this important memoir belong to the class afterwards distinguished by the name "axisymmetric," and thus fall to be considered along with others of that class. Since, however, the proof employed for one of the theorems therein enunciated is equally applicable to all kinds of determinants, it would be scarcely fair to omit here all mention of the said theorem.

In modern phraseology its formal enunciation might stand as follows:—

*S* being any axisymmetric determinant, *R* the determinant got by deleting the first row and first column of *S*, *Y* the determinant got by deleting the first row and second column of *S*, and *Q* the determinant got from *R* as *R* from *S*, then, if  $R = 0$

$$SQ = - Y^2;$$

and the theorem in general determinants whose validity is warranted by the proof given is in later notation—

If  $|b_2c_3d_4| = 0$ , then  $|a_2c_3d_4| \cdot |b_1c_3d_4| = -|a_1b_2c_3d_4| \cdot |c_2d_4|$ . This, it is readily seen, is not a very obscure foreshadowing of Jacobi's identity

$$|A_1B_2| = |a_1b_2c_3d_4| \cdot |c_3d_4|.$$

JACOBI (1829).

[Exercitatio algebraica circa discriptionem singularem fractionum, quae plures variables involvunt. *Crelle's Journ.*, v. pp. 344-364.]

In the ordinary expansion of  $(ax + by + cz - t)^{-1}$  there are evidently only negative powers of  $x$  and positive powers of  $y$  and  $z$ ; in the like expansion of  $(b'y + c'z + a'x - t')^{-1}$  there are only negative powers of  $y$  and positive powers of  $z$  and  $x$ ; and similarly for  $(c''z + a''x + b''y - t'')^{-1}$ . It follows from this that the ordinary expansion of  $(ax + by + cz - t)^{-1} \cdot (b'y + c'z + a'x - t')^{-1} \cdot (c''z + a''x + b''y - t'')^{-1}$ , looked at from the point of view of the powers of  $x, y, z$ , contains a considerable variety of terms; for example, terms in which negative powers of  $x$  occur along with positive powers of  $y$  and  $z$ , terms in which  $x$  does not occur at all, and so forth. There is thus suggested the curious problem of partitioning the fraction

$$\frac{1}{(ax + by + cz - t)(b'y + c'z + a'x - t')(c''z + a''x + b''y - t'')}$$

into a number of fractions each of which is the equivalent of the series of terms of one of those types. This is the problem with which Jacobi is here concerned.

In the case of two variables he counts three types of terms, viz., that in which the indices of both  $x$  and  $y$  are negative, that in which the index of  $x$  only is negative, and that in which the index of  $y$  only is negative. In the case of three variables he counts seven types, viz., that in which the indices of  $x, y, z$  are all negative, the three in which the index of only one variable is negative, and the three in which the index of only one variable is not negative. These two cases are gone fully into, with the result that the expressions for the three aggregates in the former are all found to contain the factor  $(ab')^{-1}$ , and the expressions for the seven aggregates in the latter the factor  $(ab'c'')^{-1}$ . The reciprocal of each of those factors is recognised as the common denominator of the values of the unknowns in a set of linear equations, a denominator "*quam quibusdam determinantem nuncupamus et designemus per  $\Delta$ .*" Its persistent appearance in the problem under discussion,—a persistency, in fact, sufficient to suggest the change of the numerator of the given fraction from 1 to  $(ab')$  in the case of two variables and from 1 to  $(ab'c'')$  in the case of three,—is remarked upon:—"Quam determinantem in hac quaestione magnas partes agere videbimus, videlicet omnes illas series infinitas, quas ut coefficientes producti propositi evoluti invenimus, ex evolutione dignitatum negativarum determinantis provenire." Then fixing the attention on a unique term of the expansion Jacobi ventures on the generalisation that the coefficient of

$$(x_1 x_2 \dots x_{n-1})^{-1}$$

in the expansion of

$$(u_1 u_2 \dots u_{n-1})^{-1},$$

that is to say, of

$$(ax + by + cz + \dots)^{-1} (b'y + c'z + \dots)^{-1} (c''z + \dots)^{-1} \dots$$

is the determinant

$$(ab'c'' \dots)^{-1}.$$

No proof, however, is given, save for the cases where  $n=2$  and  $n=3$ . The proposition is most noteworthy in that it supplies the generating function of the reciprocal of a determinant.

To obtain a generalisation in a different direction, viz., from  $(ax + by)^{-1} (b_1y + a_1x)^{-1}$  to  $(ax + by)^{-m} (b_1y + a_1x)^{-n}$ , Jacobi pro

ceeds in a very curious and interesting way. Denoting

$$\dots + \beta^{-3}a^2 + \beta^{-2}a + \beta^{-1} + a^{-1} + a^{-2}\beta + a^{-3}\beta^2 + \dots$$

or 
$$\sum_{m=-\infty}^{+\infty} a^{-m}\beta^{m-1}$$

by\*

$$\frac{1}{\beta - a} + \frac{1}{a - \beta},$$

since it is the sum of the infinite series for  $(\beta - a)^{-1}$  and  $(a - \beta)^{-1}$ , he proves after a fashion that its product by  $\beta - a$  or  $a - \beta$  is 0, and that therefore its product by

$$\frac{1}{\gamma + m(\beta - a)} \quad \text{or} \quad \frac{1}{\gamma + m(a - \beta)}$$

is simply its product by  $\frac{1}{\gamma}$ . Turning then from this lemma

to the product

$$\left( \frac{1}{u_0 - t_0} + \frac{1}{t_0 - u_0} \right) \left( \frac{1}{u_1 - t_1} + \frac{1}{t_1 - u_1} \right)$$

where  $u_0 = a_0x + b_0y$ ,  $u_1 = b_1y + a_1x$ , he substitutes for the first factor of it

$$\frac{b_1}{|a_0b_1|x - |b_1t_0| + b_0(u_1 - t_1)} + \frac{b_1}{|b_1t_0| - |a_0b_1|x - b_0(u_1 - t_1)}$$

his justification being the fact that

$$b_1(u_0 - t_0) = |a_0b_1|x - |b_1t_0| + b_0(u_1 - t_1);$$

but, on account of the said lemma, he leaves the term  $b_0(u_1 - t_1)$  out of both denominators. For the second factor there is thereupon substituted

$$\frac{|a_0b_1|}{|a_0b_1|y - |a_0t_1|} + a_1 \frac{|a_0b_1|}{|a_0b_1|x - |b_1t_0|}$$

$$+ \frac{|a_0b_1|}{|b_1t_0| - |a_0b_1|y} + a_1 \frac{|a_0b_1|}{|b_1t_0| - |a_0b_1|x}$$

\* Jacobi writes it  $\frac{1}{\beta - a} + \frac{1}{a - \beta}$  with the caution that the two parts are not to be taken as cancelling one another. Of course, also, lower down he does not write  $|a_0b_1|$  but  $a_0b_1 - a_1b_0$  or later  $(a_0b_1)$ .

on the ground that we have the identity

$$\alpha_0 b_1 | \cdot (u_1 - t_1) = b_1 \{ |\alpha_0 b_1 | y - |\alpha_0 t_1 | \} + \alpha_1 \{ |\alpha_0 b_1 | x - |b_1 t_0 | \},$$

the term  $\alpha_1 \{ |\alpha_0 b_1 | x - |b_1 t_0 | \}$  being subsequently left out of both denominators for the same reason as before. The result thus reached is consequently

$$\begin{aligned} & |\alpha_0 b_1 | \cdot \left( \frac{1}{u_0 - t_0} + \frac{1}{t_0 - u_0} \right) \left( \frac{1}{u_1 - t_1} + \frac{1}{t_1 - u_1} \right) \\ &= \left( \frac{|\alpha_0 b_1 |}{|\alpha_0 b_1 | x - |b_1 t_0 |} + \frac{|\alpha_0 b_1 |}{|b_1 t_0 | - |\alpha_0 b_1 | x} \right) \\ &\quad \cdot \left( \frac{|\alpha_0 b_1 |}{|\alpha_0 b_1 | y - |\alpha_0 t_1 |} + \frac{|\alpha_0 b_1 |}{|\alpha_0 t_1 | - |\alpha_0 b_1 | y} \right), \end{aligned}$$

or, if we write  $\xi, \eta$  for the values of  $x, y$  which make  $u_0 - t_0 = 0, u_1 - t_1 = 0,$

$$\begin{aligned} & \alpha_0 b_1 | \cdot \left( \frac{1}{u_0 - t_0} + \frac{1}{t_0 - u_0} \right) \left( \frac{1}{u_1 - t_1} + \frac{1}{t_1 - u_1} \right) \\ &= \left( \frac{1}{x - \xi} + \frac{1}{\xi - x} \right) \left( \frac{1}{y - \eta} + \frac{1}{\eta - y} \right) \cdot (a). \end{aligned}$$

Since the general terms of the four doubly-infinite series here are

$$\frac{t_0^m}{u_0^{m+1}}, \frac{t_1^n}{u_1^{n+1}}, \frac{\xi^{\mu}}{x^{\mu+1}}, \frac{\eta^{\nu}}{y^{\nu+1}},$$

we deduce

$$|\alpha_0 b_1 | \cdot \sum \frac{t_0^m t_1^n}{u_0^{m+1} u_1^{n+1}} = \sum \frac{\xi^{\mu} \eta^{\nu}}{x^{\mu+1} y^{\nu+1}},$$

$$\begin{aligned} \text{i.e.,} \quad & |\alpha_0 b_1 | \cdot \sum \frac{t_0^m t_1^n}{(\alpha_0 x + b_0 y)^{m+1} (b_1 y + \alpha_1 x)^{n+1}} \\ &= \sum \frac{|b_1 t_0 |^{\mu} \cdot |\alpha_0 t_1 |^{\nu}}{|\alpha_0 b_1 |^{\mu+\nu+1} \cdot x^{\mu+1} y^{\nu+1}}, \end{aligned}$$

where  $m, n$  on the one side and  $\mu, \nu$  on the other are to have all integral values from  $-\infty$  to  $+\infty$ . Since the coefficients of  $t_0^m t_1^n / x^{\mu} y^{\nu}$  on the two sides must be equal, we obtain the theorem:—

The coefficient of  $\frac{1}{x^\mu y^\nu}$  in the expansion of  $\frac{1}{(a_0x + b_0y)^{m+1}(b_1y + a_1x)^{n+1}}$

is the same as the coefficient of  $t_0^m t_1^n$  in the expansion of  $\frac{(b_1t_0 - b_0t_1)^{\mu-1}(a_0t_1 - a_1t_0)^{\nu-1}}{|a_0b_1|^{\mu+\nu-1}}$ , it being remembered that  $m$  and

$n$  are of the same sign as  $\mu$  and  $\nu$  respectively and that  $m + n = \mu + \nu - 2$ .

In similar fashion the author deals with the case of three functions  $u_0, u_1, u_2$  of three variables  $x, y, z$ , proving laboriously and not very neatly the neat result

$$|a_0 b_1 c_2| \cdot \left(\frac{1}{u_0 - t_0} + \frac{1}{t_0 - u_0}\right) \left(\frac{1}{u_1 - t_1} + \frac{1}{t_1 - u_1}\right) \left(\frac{1}{u_2 - t_2} + \frac{1}{t_2 - u_2}\right) \\ = \left(\frac{1}{x - \xi} + \frac{1}{\xi - x}\right) \left(\frac{1}{y - \eta} + \frac{1}{\eta - y}\right) \left(\frac{1}{z - \zeta} + \frac{1}{\zeta - z}\right) (\beta)$$

thence deriving

$$|a_0 b_1 c_2| \cdot \sum \frac{t_0^m t_1^n t_2^r}{u_0^{m+1} u_1^{n+1} u_2^{r+1}} = \sum \frac{\xi^\mu \eta^\nu \zeta^\rho}{x^{\mu+1} y^{\nu+1} z^{\rho+1}}$$

and ending with the theorem :—

The coefficient of  $\frac{1}{x^\mu y^\nu z^\rho}$  in the expansion of

$$\frac{1}{(a_0x + b_0y + c_0z)^{m+1}(b_1y + c_1z + a_1x)^{n+1}(c_2z + a_2x + b_2y)^{r+1}}$$

is the same as the coefficient of  $t_0^m t_1^n t_2^r$  in the expansion of

$$\frac{\{|b_1c_2|t_0 + |b_2c_0|t_1 + |b_0c_1|t_2\}^{\mu-1} \{|c_2a_0|t_1 + |c_0a_1|t_2 + |c_1a_0|t_0\}^{\nu-1} \{|a_0b_1|t_2 + |a_1b_2|t_0 + |a_2b_0|t_1\}^{\rho-1}}{|a_0b_1c_2|^{\mu+\nu+\rho-2}}$$

it being understood that  $m, n, r$  are of the same sign as  $\mu, \nu, \rho$  respectively and that  $m + n + r = \mu + \nu + \rho - 3$ .

The corresponding results for  $n$  functions of  $n$  variables are evident. They had already been enunciated in the introductory section of the paper, and Jacobi now merely adds "Omnino similia theoremata de numero quolibet variabilium, quae § 1 proposuimus, eruantur." It has to be noted, however, that belief



in the general fundamental theorem, viz., that which includes (a) and (β) above, is more strongly induced by the elegance of the form of the theorem than by the mode of proof. In § 1 it stands approximately thus—

$$\left(\frac{1}{u_0 - t_0} + \frac{1}{t_0 - u_0}\right) \left(\frac{1}{u_1 - t_1} + \frac{1}{t_1 - u_1}\right) \cdots \cdots \left(\frac{1}{u_{n-1} - t_{n-1}} + \frac{1}{t_{n-1} - u_{n-1}}\right)$$

$$= \frac{1}{\Delta} \left(\frac{1}{x_0 - p_0} + \frac{1}{p_0 - x_0}\right) \left(\frac{1}{x_1 - p_1} + \frac{1}{p_1 - x_1}\right) \cdots \cdots \left(\frac{1}{x_{n-1} - p_{n-1}} + \frac{1}{p_{n-1} - x_{n-1}}\right)$$

and then follows the passage containing the two deductions, viz.,

“quam aequationem etiam hunc in modum repraesentare licet:

$$\sum \frac{t_0^{a_0} t_1^{a_1} \dots t_{n-1}^{a_{n-1}}}{u_0^{a_0+1} u_1^{a_1+1} \dots u_{n-1}^{a_{n-1}+1}} = \frac{1}{\Delta} \sum \frac{p_0^{\beta_0} p_1^{\beta_1} \dots p_{n-1}^{\beta_{n-1}}}{x_0^{\beta_0+1} x_1^{\beta_1+1} \dots x_{n-1}^{\beta_{n-1}+1}}$$

designantibus  $a_0, a_1, \text{etc. } \beta_0, \beta_1, \text{etc.}$  numeros omnes et positivos et negativos a  $-\infty$  ad  $+\infty$ . E quo theoremate videmus, coefficientem termini

$$\frac{1}{x_0^{\beta_0+1} x_1^{\beta_1+1} \dots x_{n-1}^{\beta_{n-1}+1}}$$

in expressione

$$\frac{1}{u_0^{a_0+1} u_1^{a_1+1} \dots u_{n-1}^{a_{n-1}+1}}$$

aequalem fore coefficienti termini  $t_0^{a_0} t_1^{a_1} \dots t_{n-1}^{a_{n-1}}$

in expressione

$$\frac{1}{\Delta} p_0^{\beta_0} p_1^{\beta_1} \dots p_{n-1}^{\beta_{n-1}}."$$

The use here of  $\beta_0 + 1, \beta_1 + 1, \dots$  rather than the change made in the two special cases to the less natural  $\beta_0, \beta_1, \dots$  is worth noting.

The theorems of the remaining four pages of the paper have a less direct bearing on our subject.

JACOBI (1833).

[De binis quibuslibet functionibus homogeneis secundi ordinis per substitutiones lineares in alias binas transformandis, quae solis quadraticis variabilium constant: una cum . . . . *Crelle's Journ.*, xii. pp. 1-69.]

Jacobi's mode of proving his theorem regarding a minor of the adjugate occupies § 6 (pp. 9-11). Temporarily denoting by  $X_m$  the left-hand member of the  $m^{\text{th}}$  given equation

$$\alpha_1^{(m)}x_1 + \alpha_2^{(m)}x_2 + \dots + \alpha_n^{(m)}x_n = ym,$$

and by  $Y_m$  the left-hand member of the  $m^{\text{th}}$  derived equation

$$\beta'_m y_1 + \beta''_m y_2 + \dots + \beta^{(m)}_m y_n = \Delta x_m:$$

and explaining that by

$$\left[ \text{U} \right] \frac{1}{x_1 x_2 \dots x_n}$$

he means the coefficient of  $x_1^{-1}x_2^{-1} \dots x_n^{-1}$  in a certain specified expansion of U, he recalls his paper of the year 1829 on the "discerptio singularis," and affirms that he had there proved

"fore

$$\left[ \frac{1}{X_1 X_2 \dots X_n} \right] \frac{1}{x_1 x_2 \dots x_n} = \frac{1}{A}$$

sive etiam, quod idem est,

$$\left[ Y_1 Y_2 \dots Y_n \right] \frac{1}{y_1 y_2 \dots y_n} = \frac{1}{B}$$

ac generalius

$$\begin{aligned} & \left[ \frac{x_1^{\varepsilon_1} x_2^{\varepsilon_2} \dots x_n^{\varepsilon_n}}{X_1^{r_1+1} X_2^{r_2+1} \dots X_n^{r_n+1}} \right] \frac{1}{x_1 x_2 \dots x_n} \\ &= \frac{1}{A^{r_1+r_2+\dots+r_n+1}} \left[ \frac{Y_1^{r_1} Y_2^{r_2} \dots Y_n^{r_n}}{y_1^{\varepsilon_1+1} y_2^{\varepsilon_2+1} \dots y_n^{\varepsilon_n+1}} \right] \frac{1}{y_1 y_2 \dots y_n} \end{aligned}$$

designantibus  $r_1, r_2, \dots, r_n$  ac  $\varepsilon_1, \varepsilon_2, \dots, \varepsilon_n$  numeros quoslibet integros sive positivos sive negativos."

A glance, however, suffices to convince one that the concluding general theorem here given differs considerably from the theorem which he had previously enunciated and possibly proved. As originally stated the theorem was—

$$\left[ \frac{1}{u_0^{a_0+1} u_1^{a_1+1} \dots u_{n-1}^{a_{n-1}+1}} \right] \frac{1}{x_0^{\beta_0+1} x_1^{\beta_1+1} \dots x_{n-1}^{\beta_{n-1}+1}}$$

$$= \frac{1}{\Delta} \left[ p_0^{\beta_0} p_1^{\beta_1} \dots p_{n-1}^{\beta_{n-1}} \right] \frac{1}{t_0^{a_0} t_1^{a_1} \dots t_{n-1}^{a_{n-1}}}$$

which being altered into the notation of his present paper by the substitutions :

$$\begin{aligned} x_0, x_1, \dots &= x_1, x_2, \dots \\ u_0, u_1, \dots &= X_1, X_2, \dots \\ p_0, p_1, \dots &= \frac{Y_1}{A}, \frac{Y_2}{A}, \dots \\ a_0, a_1, \dots &= r_1, r_2, \dots \\ \beta_0, \beta_1, \dots &= s_1, s_2, \dots \\ \Delta &= A, \end{aligned}$$

becomes

$$\left[ \frac{1}{X_1^{r_1+1} X_2^{r_2+1} \dots X_n^{r_n+1}} \right] \frac{1}{x_1^{s_1+1} x_2^{s_2+1} \dots x_n^{s_n+1}}$$

$$= \frac{1}{A^{s_1+s_2+\dots+s_n+1}} \left[ Y_1^{s_1} Y_2^{s_2} \dots Y_n^{s_n} \right] \frac{1}{y_1^{r_1} y_2^{r_2} \dots y_n^{r_n}}$$

Using on both sides of this the fact that if an expanded function be multiplied by the product of certain powers of the variables, any particular coefficient in the original expansion has now for facient its original facient multiplied by the said product, we obtain

$$\left[ \frac{x_1^{s_1} x_2^{s_2} \dots x_n^{s_n}}{X_1^{r_1+1} X_2^{r_2+1} \dots X_n^{r_n+1}} \right] \frac{1}{x_1 x_2 \dots x_n}$$

$$= \frac{1}{A^{s_1+s_2+\dots+s_n+1}} \left[ \frac{Y_1^{s_1} Y_2^{s_2} \dots Y_n^{s_n}}{y_1^{r_1+1} y_2^{r_2+1} \dots y_n^{r_n+1}} \right] \frac{1}{y_1 y_2 \dots y_n}$$

—a statement differing from Jacobi's in having  $r$ 's and  $s$ 's on the right-hand side where he has  $s$ 's and  $r$ 's respectively. The oversight was probably not noticed by reason of the fact that in the special instances considered by him the values of any  $r$  and the corresponding  $s$  are the same.

In the first of these instances he puts

$$\begin{aligned} r_1 = r_2 = \dots = r_n &= -1 \\ s_1 = s_2 = \dots = s_n &= -1; \end{aligned}$$

and obtains

$$1 = A^{n-1} \left[ \frac{1}{Y_1 Y_2 \dots Y_n} \right] \frac{1}{y_1 y_2 \dots y_n} = \frac{A^{n-1}}{B},$$

thus arriving at Cauchy's theorem regarding the adjugate, viz.,

$$B = A^{n-1}.$$

In the second instance, he puts

$$\begin{aligned} r_1 = r_2 = \dots = r_m &= -1, & r_{m+1} = r_{m+2} = \dots = r_n &= 0, \\ s_1 = s_2 = \dots = s_m &= -1, & s_{m+1} = s_{m+2} = \dots = s_n &= 0, \end{aligned}$$

and obtains

$$\begin{aligned} & \left[ \frac{1}{X_{m+1} X_{m+2} \dots X_n} \right] \frac{1}{x_{m+1} x_{m+2} \dots x_n} \\ &= A^{m-1} \left[ \frac{1}{Y_1 Y_2 \dots Y_m} \right] \frac{1}{y_1 y_2 \dots y_m}. \end{aligned}$$

He then recalls the fact that by the conditions attaching to the expansion of the expressions enclosed in rectangular brackets the powers of  $x_1, x_2, \dots, x_m$  contained in the one and the powers of  $y_{m+1}, y_{m+2}, \dots, y_n$  contained in the other are all positive; and argues that as we are concerned only with terms that do not involve these variables, it is quite allowable to put them all equal to 0. This being done it is seen that

$$\left[ \frac{1}{X_{m+1} X_{m+2} \dots X_n} \right] \frac{1}{x_{m+1} x_{m+2} \dots x_n} = \frac{1}{\sum \pm a_{m+1}^{(m+1)} a_{m+2}^{(m+2)} \dots a_n^{(n)},}$$

and

$$\left[ \frac{1}{Y_1 Y_2 \dots Y_m} \right] \frac{1}{y_1 y_2 \dots y_m} = \frac{1}{\sum \pm \beta_1' \beta_2'' \dots \beta_m^{(m)'}}$$

so that there is obtained

$$\sum \pm \beta_1' \beta_2'' \dots \beta_m^{(m)'} = A^{m-1} \cdot \sum \pm \alpha_{m+1}^{(m+1)} \alpha_{m+2}^{(m+2)} \dots \alpha_n^{(n)},$$

as was expected.

JACOBI (1834).

[Dato systemate  $n$  aequationum linearium inter  $n$  incognitas, valores incognitarum per integralia definita  $(n - 1)$  tuplicia exhibentur. *Crelle's Journ.*, xiv. pp. 51-55.]

This short paper is, as it were, a by-product of the investigation which resulted in Jacobi's long memoir of the preceding year. Its only interest for us at present lies in the fact that values which are ordinarily expressed by means of determinants are here given in the form of definite multiple integrals. Indeed, instead of viewing the result obtained as being the solution of a set of simultaneous linear equations, it might be equally appropriate to consider the investigation as belonging to the subject of definite integration. It will suffice, therefore, merely to give a statement of the theorem arrived at. In Jacobi's own words, it is,—

“Sit propositum inter  $n$  incognitas  $z_1, z_2, \dots, z_n$  systema  $n$  aequationum linearium

$$\begin{aligned} b_{11}z_1 + b_{12}z_2 + \dots + b_{1n}z_n &= m_1, \\ b_{21}z_1 + b_{22}z_2 + \dots + b_{2n}z_n &= m_2, \\ \dots & \\ b_{n1}z_1 + b_{n2}z_2 + \dots + b_{nn}z_n &= m_n; \end{aligned}$$

statuamus

$$\begin{aligned} X &= [b_{11}x_1 + b_{21}x_2 + \dots + b_{n1}x_n]^2 \\ &+ [b_{12}x_1 + b_{22}x_2 + \dots + b_{n2}x_n]^2 \\ &\dots \\ &+ [b_{1n}x_1 + b_{2n}x_2 + \dots + b_{nn}x_n]^2, \end{aligned}$$

porro

$$M = m_1x_1 + m_2x_2 + \dots + m_nx_n$$

ubi

$$x_n = \sqrt{(1 - x_1^2 + x_2^2 - \dots - x_{n-1}^2)}$$

radicali positive accepto; porro ponamus

$$\nabla = \pm \sum \pm b_{11}b_{22} \dots b_{nn},$$

signo ancipiti, ante ipsum  $\Sigma$  posito, ita determinato, ut valor ipsius  $\nabla$  positivus prodeat. Quibus omnibus positis, erit

$$\frac{n}{2^{n-1}S} \cdot \frac{z_1}{\nabla} = \int \frac{M(b_{11}x_1 + b_{22}x_2 + \dots + b_{n1}x_n)\delta x_1 \delta x_2 \dots \delta x_{n-1}}{x_n X^{1/(n+2)}},$$

$$\frac{n}{2^{n-1}S} \cdot \frac{z_2}{\nabla} = \int \frac{M(b_{12}x_1 + b_{22}x_2 + \dots + b_{n2}x_n)\delta x_1 \delta x_2 \dots \delta x_{n-1}}{x_n X^{1/(n+2)}},$$

.....

$$\frac{n}{2^{n-1}S} \cdot \frac{z_n}{\nabla} = \int \frac{M(b_{1n}x_1 + b_{2n}x_2 + \dots + b_{nn}x_n)\delta x_1 \delta x_2 \dots \delta x_{n-1}}{x_n X^{1/(n+2)}},$$

integralibus  $(n-1)$  tuplicibus extensis ad omnes valores reales ipsorum  $x_1, x_2, \dots, x_{n-1}$  et positivos et negativos, pro quibus etiam  $x_n$  realis sit sive pro quibus

$$x_1^2 + x_2^2 + \dots + x_{n-1}^2 \leq 1;$$

et designante S aut

$$\frac{1}{2 \cdot 4 \cdot \dots \cdot (n-2)} \left(\frac{\pi}{2}\right)^{\frac{n}{2}} \text{ aut } \frac{1}{1 \cdot 3 \cdot 5 \cdot \dots \cdot (n-2)} \left(\frac{\pi}{2}\right)^{\frac{n-1}{2}}$$

prout  $n$  aut par aut impar."

MOLINS (1839).

[Démonstration de la formule générale qui donne les valeurs des inconnues dans les équations du premier degré. *Journ. de Liouville*, iv. pp. 509-515.]

The real object of Molins was simply to give a rigorous demonstration of Cramer's rules. His literary progenitors, so far as determinants were concerned, were apparently Cramer, Bezout, Laplace, and Gergonne, the last of whom, it may be remembered, wrote a paper which might well have borne the same title as the

above. The writer, however, whose work that of Molins most closely resembled was Scherk, and very probably the two were unknown to each other. Both had the same purpose in view, and both used the method of so-called "mathematical induction." The difference between them may most easily be explained by using a special example and modern notation.

To make the solution of the set of three equations

$$\left. \begin{aligned} a_1x + a_2y + a_3z &= a_4 \\ b_1x + b_2y + b_3z &= b_4 \\ c_1x + c_2y + c_3z &= c_4 \end{aligned} \right\}$$

dependent upon the already obtained solution of two, Scherk put the first pair of equations in the form

$$\left. \begin{aligned} a_1x + a_2y &= a_4 - a_3z \\ b_1x + b_2y &= b_4 - b_3z \end{aligned} \right\},$$

solved for  $x$  and  $y$ , and substituted the values in the third equation.

Molins, on the other hand, having used the multipliers  $m_1$ ,  $m_2$ , 1, with the equations of the given set, performed addition, solved the pair of equations

$$\left. \begin{aligned} m_1a_2 + m_2b_2 + c_2 &= 0 \\ m_1a_3 + m_2b_3 + c_3 &= 0 \end{aligned} \right\}$$

for  $m_1$  and  $m_2$ , and substituted the obtained values in the result

$$x_1 = \frac{m_1a_4 + m_2b_4 + c_4}{m_1a_1 + m_2b_1 + c_1}.$$

His exposition is laboured and uninviting.

BOOLE, G. (1843).

[On the transformation of multiple integrals. *Cambridge Math. Journ.*, iv. pp. 20-28.]

Boole had to use in his paper the resultant of a system of  $n$  linear homogeneous equations, and he therefore thought proper, by way of introduction, to state a mode of forming the resultant, and

to prove that the result was correct. As the mode is that in which the rule of signs is dependent on the number of interchanges,\* or, as Boole calls them, "binary permutations," any interest attaching to the little exposition is connected with the "proof." The first essential paragraph is:—

"The result of the elimination of the variables from the equations

$$\begin{aligned} a_1x_1 + a_2x_2 + \dots + a_nx_n &= 0, \\ b_1x_1 + b_2x_2 + \dots + b_nx_n &= 0, \\ \dots\dots\dots\dots\dots\dots\dots\dots\dots \\ r_1x_1 + r_2x_2 + \dots + r_nx_n &= 0, \end{aligned}$$

is an equation of which the second member is 0, and of which the first member is formed from the coefficient of  $x_1x_2\dots x_n$  in the product of the given equations, by assuming a particular term, as  $a_1b_2\dots r_n$ , positive, and applying to every other term a change of sign for every binary permutation which it may exhibit, when compared with the proposed term  $a_1b_2\dots r_n$ .

The curious point worth noting here is that we are directed first to form the terms of the expression afterwards denoted by  $\dagger |a_1 b_2 \dots r_n|$  and called a "permanent," and then to alter the signs of certain terms of it. Boole then proceeds:—

"The truth of the above theorem is shown by the following considerations. The elimination of  $x_1$  from the first and second equation of the system introduces terms of the form  $a_1b_2 - a_2b_1$ ,  $a_1b_3 - a_3b_1$ , etc., in which the law of binary permutation is apparent, and as we may begin the process of elimination with any variable and with any pair of equations, the law is universal. From the same instance it is evident that no proposed suffix can occur twice in a given term, which condition is also characteristic of the coefficient of  $x_1x_2\dots x_n$  in the product of the equations of the system, whence the theorem is manifest."

\* See Rothe's paper of the year 1800.



It will be observed that neither the word "determinant" nor the word "resultant" occurs: indeed, throughout the paper, instead of resultant he uses "final derivative," a term which probably may be traced to Sylvester.\*

CAYLEY (1843).

[Chapters in the analytical geometry of  $n$  dimensions. *Cambridge Math. Journ.*, iv. pp. 119-127; or *Collected Math. Papers*, i. pp. 55-62.]

Of the four short chapters which compose this paper, the only one which concerns us is the first, although in the others determinants are constantly made use of. At the outset an important notation is introduced which afterwards came to be generally adopted. The passage in regard to it is:—

"Consider the series of terms—

$$\begin{array}{ccccccc} x_1 & x_2 & \cdot & \cdot & \cdot & \cdot & x \\ A_1 & A_2 & \cdot & \cdot & \cdot & \cdot & A_n \\ \cdot & \cdot & \cdot & \cdot & \cdot & \cdot & \cdot \\ K_1 & K_2 & \cdot & \cdot & \cdot & \cdot & K_n, \end{array}$$

the number of quantities  $A, \dots, K$  being equal to  $q$  ( $q < n$ ). Suppose  $q+1$  vertical rows selected, and the quantities contained in them formed into a determinant,

this may be done in  $\frac{n(n-1) \cdot \dots \cdot (q+2)}{1 \cdot 2 \cdot \dots \cdot (n-q-1)}$  different ways.

The system of determinants so obtained will be represented by the notation

$$\left| \begin{array}{cccc} x_1 & x_2 & \cdot & \cdot & \cdot & x_n \\ A_1 & A_2 & \cdot & \cdot & \cdot & A_n \\ \cdot & \cdot & \cdot & \cdot & \cdot & \cdot \\ K_1 & K_2 & \cdot & \cdot & \cdot & K_n \end{array} \right|;$$

\* See Sylvester's paper of 1840.

and the system of equations, obtained by equating each of these determinants to zero, by the notation

$$(3) \quad \begin{vmatrix} x_1 & x_2 & \dots & x_n \\ A_1 & A_2 & \dots & A_n \\ \dots & \dots & \dots & \dots \\ K_1 & K_2 & \dots & K_n \end{vmatrix} = 0.$$

A theorem is next enunciated in regard to the expression of any one of the determinants in terms of  $n - q$  of them.

“The  $\frac{n(n-1) \dots (q+2)}{1 \cdot 2 \dots (n-q-1)}$  equations represented by this formula reduce themselves to  $n - q$  independent equations. Imagine these expressed by

$$(1) = 0, \quad (2) = 0, \quad \dots, \quad (n - q) = 0,$$

any one of the determinants is reducible to the form

$$\Theta_1(1) + \Theta_2(2) + \dots + \Theta_{n-q}(n - q)$$

where  $\Theta_1, \Theta_2, \dots, \Theta_{n-q}$  are coefficients independent of  $x_1, x_2, \dots, x_n$ .”

No proof is given.

The introduction of the notation is fully justified by two theorems which follow. The first is virtually to the effect that we may multiply both sides of (3) by the determinant

$$(5) \quad \begin{vmatrix} \lambda_1 & \lambda_2 & \dots & \lambda_n \\ \mu_1 & \mu_2 & \dots & \mu_n \\ \dots & \dots & \dots & \dots \\ \tau_1 & \tau_2 & \dots & \tau_n \end{vmatrix}$$

just as if (3) were a single equation instead of  $C_{n, q+1}$  equations, and as if the left-hand side were a determinant; and the result, written in the form

$$(6) \quad \begin{vmatrix} \lambda_1 x_1 + \dots + \lambda_n x_n & \mu_1 x_1 + \dots + \mu_n x_n & \dots & \tau_1 x_1 + \dots + \tau_n x_n \\ \lambda_1 A_1 + \dots + \lambda_n A_n & \mu_1 A_1 + \dots + \mu_n A_n & \dots & \tau_1 A_1 + \dots + \tau_n A_n \\ \dots & \dots & \dots & \dots \\ \lambda_1 K_1 + \dots + \lambda_n K_n & \mu_1 K_1 + \dots + \mu_n K_n & \dots & \tau_1 K_1 + \dots + \tau_n K_n \end{vmatrix} = 0$$

will be true; that is to say, we shall have a new set of  $C_{n, q+1}$  equations, which follows logically from the original set. Further, and conversely, if the set (6) hold, we can deduce the set (3) provided that the determinant (5) be not zero. The other theorem is quite similar, being to the effect that the equations (3) may be replaced by the set

$$(8) \begin{vmatrix} x_1 & x_2 & \cdots & x_n \\ \lambda_1 A_1 + \cdots + \omega_1 K_1 & \lambda_1 A_2 + \cdots + \omega_1 K_2 & \cdots & \lambda_1 A_n + \cdots + \omega_1 K_n \\ \cdot & \cdot & \cdot & \cdot \\ \lambda_q A_1 + \cdots + \omega_q K_1 & \lambda_q A_2 + \cdots + \omega_q K_2 & \cdots & \lambda_q A_n + \cdots + \omega_q K_n \end{vmatrix} = 0,$$

and that conversely from the set (8) the set (3) is deducible provided the determinant

$$\begin{vmatrix} \lambda_1 & \mu_1 & \cdots & \omega_1 \\ \lambda_2 & \mu_2 & \cdots & \omega_2 \\ \cdot & \cdot & \cdot & \cdot \\ \lambda_q & \mu_q & \cdots & \omega_q \end{vmatrix}$$

be not zero.

As the "derivation of coexistence" came prominently before us in examining Sylvester's early work, it may be noted here in passing that Cayley's second chapter, extending to about a page, consists of the enunciation of a theorem on this subject.

CAYLEY (1843).

[On the theory of determinants. *Trans. Cambridge Philosop. Soc.*, viii. pp. 1-16; or *Collected Math. Papers*, i. pp. 63-79.]

Up to this point Cayley had dealt with determinants, only, as it were, incidentally. Now, however, he devotes a memoir of sixteen quarto pages to the study of them.

The introductory page shows a pretty wide acquaintance with previous writings on the subject, the authors mentioned being Cramer, Bezout (1764), Laplace, Vandermonde, Lagrange,\* Bezout

\* As the memoir of Lagrange which Cayley refers to is not one of those brought into notice in the early part of our history, but is one bearing the title "Sur le problème de la détermination des orbites des comètes d'après trois

(1779), Gauss, Binet, Cauchy (1812), Lebesgue, Jacobi (1841), and Cauchy (1841).

The first section of the paper is said to deal with "the properties of determinants considered as *derivational functions*." As a matter of fact, however, a close examination shows that the functions whose properties are investigated are not strictly determinants, but belong to a class afterwards named *bipartites* by Cayley himself. It is true that it is the determinant notation which is employed in specifying the functions, but this is due to the fact that the bipartite under discussion is of a very special type, and so happens to be expressible as a determinant.

The function *U* from which he considers his three determinants to be "derived" is

$$\begin{aligned} & x(a\xi + \beta\eta + \dots) \\ & + x'(a'\xi + \beta'\eta + \dots) \\ & + \dots \end{aligned}$$

there being *n* lines and *n* terms in each line. This at a somewhat later date (1855) he would have denoted by

$$\left( \begin{array}{cccc|c} a & \beta & \dots & \xi & \xi, \eta, \dots, \xi x, x', \dots \\ a' & \beta' & \dots & \eta & \\ \dots & \dots & \dots & \dots & \end{array} \right)$$

and called a *bipartite*. A still later notation is

$$\begin{array}{cccc|c} \xi & \eta & \dots & & \\ \hline a & \beta & \dots & & x \\ a' & \beta' & \dots & & x' \\ \dots & \dots & \dots & & . \end{array}$$

from which each term of the final expansion is very readily

observations," it may be well to mention that the substance of the only sentence in it which concerns us had already appeared in the memoir of 1778. The sentence is

"De là il s'ensuit aussi qu'on aura

$$\begin{aligned} (t^m u' - t^m u'')^2 &= (x^m z' - x^m z'')^2 + (y^m z' - y^m z'')^2 + (x^m y' - x^m y'')^2, \\ &= (x'^2 + y'^2 + z'^2)(x''^2 + y''^2 + z''^2) - (x'x'' + y'y'' + z'z'')^2. \end{aligned}$$

—*Nouv. Mém. de l'Acad. Roy.* . . . . (Berlin), ann. 1778, p. 160.

obtained by multiplying an element,  $\beta'$  say, of the square array by the two elements  $(\eta, x')$  which lie in the same row and column with it but outside the array. The three determinants which are viewed as "derivational functions" of this function U are

$$\begin{array}{c} \left| \begin{array}{cccc} a & \beta & \dots & \\ a' & \beta' & \dots & \\ \dots & \dots & \dots & \dots \end{array} \right|, \\ - \left| \begin{array}{cccc} Ax + A'x' + \dots & Bx + B'x' + \dots & \dots & \\ R\xi + S\eta + \dots & a & \beta & \dots \\ R'\xi + S'\eta + \dots & a' & \beta' & \dots \\ \dots & \dots & \dots & \dots \end{array} \right|, \end{array}$$

and

$$- \left| \begin{array}{cccc} Rx + R'x' + \dots & Sx + S'x' + \dots & \dots & \\ A\xi + B\eta + \dots & a & \beta & \dots \\ A'\xi + B'\eta + \dots & a' & \beta' & \dots \\ \dots & \dots & \dots & \dots \end{array} \right|.$$

These are denoted by KU, FU,  $\mathfrak{F}$ U; and the closing sentence of the introduction is, "The symbols K, F,  $\mathfrak{F}$  possess properties which it is the object of this section to investigate."

KU, it will be observed, is what afterwards came to be called the *discriminant* of U; and FU,  $\mathfrak{F}$ U are the results of making certain linear substitutions for the elements of the first row and of the first column of the determinant

$$\left| \begin{array}{cccc} x & y & z & \dots \\ \xi & a & \beta & \gamma & \dots \\ \eta & a' & \beta' & \gamma' & \dots \\ \zeta & a'' & \beta'' & \gamma'' & \dots \\ \dots & \dots & \dots & \dots & \dots \end{array} \right|.$$

It is this determinant, therefore, which is under investigation and under comparison with U. That it is a bipartite function of  $x, y, z, \dots$  and  $\xi, \eta, \zeta, \dots$  is manifest when we think of expanding it according to binary products of the elements of the first row and of the first column, the expression for it in the notation of

bipartites being thus seen to be

$$\begin{array}{cccc|c}
 & x & y & z & \\
 \hline
 -|\beta'\gamma''\dots| & |\alpha'\gamma'''\dots| & -|\alpha'\beta''\dots| & \dots & \xi \\
 |\beta\gamma''\dots| & -|\alpha\gamma'''\dots| & |\alpha\beta''\dots| & \dots & \eta \\
 -|\beta\gamma'\dots| & |\alpha\gamma'\dots| & -|\alpha\beta'\dots| & \dots & \zeta \\
 \dots & \dots & \dots & \dots & \dots
 \end{array}$$

Now the properties of this which are investigated by Cayley are properties possessed by the more general bipartite

$$\begin{array}{cccc|c}
 & x & y & z & \dots & \\
 \hline
 a_1 & a_2 & a_3 & \dots & & \xi \\
 b_1 & b_2 & b_3 & \dots & & \eta \\
 c_1 & c_2 & c_3 & \dots & & \zeta \\
 \dots & \dots & \dots & \dots & \dots & \dots
 \end{array}$$

which is not expressible in the form of a determinant. So far, therefore, as this section of the memoir is concerned, it is evident that the title is somewhat misleading, and it is unnecessary to enter into detail regarding the properties in question.

In the course of the section, however, having occasion to use Jacobi's theorem regarding a minor of the adjugate, Cayley gives at the outset a formal proof which it is most important to note, as it is the natural generalisation of Cauchy's proof for the ultimate case, and consequently has since become the standard proof given in text-books. The passage is

“Let  $A, B, \dots, A', B', \dots$  be given by the equations

$$\begin{array}{l}
 A = \begin{array}{c} \beta' \ \gamma' \ \dots \\ \beta'' \ \gamma'' \ \dots \\ \dots \end{array} \quad B = \pm \begin{array}{c} \gamma' \ \delta' \ \dots \\ \gamma'' \ \delta'' \ \dots \\ \dots \end{array} \\
 A' = \pm \begin{array}{c} \beta'' \ \gamma'' \ \dots \\ \beta''' \ \gamma''' \ \dots \\ \dots \end{array} \quad B' = \begin{array}{c} \gamma'' \ \delta'' \ \dots \\ \gamma''' \ \delta''' \ \dots \\ \dots \end{array}
 \end{array}$$

the upper or lower signs being taken according as  $n$  is odd or even.

These quantities satisfy the double series of equations

$$\left. \begin{aligned}
 Aa + B\beta + \dots &= \kappa \\
 Aa' + B\beta' + \dots &= 0 \\
 \dots &\dots \\
 A'a + B'\beta + \dots &= 0 \\
 A'a' + B'\beta' + \dots &= \kappa \\
 \dots &\dots
 \end{aligned} \right\} \quad (6)$$
  

$$\left. \begin{aligned}
 Aa + A'a' + \dots &= \kappa \\
 A\beta + A'\beta' + \dots &= 0 \\
 \dots &\dots \\
 Ba + B'a' + \dots &= 0 \\
 B\beta + B'\beta' + \dots &= \kappa \\
 \dots &\dots
 \end{aligned} \right\}$$

the second side of each equation being 0, except for the  $r^{\text{th}}$  equation of the  $r^{\text{th}}$  set of equations in the systems.

Let  $\lambda, \mu, \dots$  represent the  $r^{\text{th}}, (r + 1)^{\text{th}}, \dots$  terms of the series  $a, \beta, \dots$ ;  $L, M, \dots$  the corresponding terms of the series  $A, B, \dots$ , where  $r$  is any number less than  $n$ , and consider the determinant

$$\begin{vmatrix}
 A, \dots, L \\
 \dots \\
 A^{(r-1)}, \dots, L^{(r-1)}
 \end{vmatrix}$$

which may be expressed as a determinant of the  $n^{\text{th}}$  order, in the form

$$\begin{vmatrix}
 A, \dots, L, 0, 0, \dots \\
 \dots \\
 A^{(r-1)}, \dots, L^{(r-1)}, 0, 0, \dots \\
 0, \dots, 0, 1, 0, \dots \\
 0, \dots, 0, 0, 1, \dots \\
 \dots
 \end{vmatrix}$$

Multiplying this by the two sides of the equation

$$\kappa = \begin{vmatrix}
 a, \beta, \dots \\
 a', \beta', \dots \\
 \dots
 \end{vmatrix}$$

and reducing the result by the equation (⊙) and the equations (6), the second side becomes

$$\begin{vmatrix} \kappa & 0 & \dots & \dots & \dots \\ 0 & \kappa & \dots & \dots & \dots \\ \dots & \dots & \dots & \dots & \dots \\ \dots & \dots & \kappa & 0 & 0 & \dots \\ \dots & \dots & \dots & 0 & \mu^{(r)} & \nu^{(r)} & \dots \\ \dots & \dots & \dots & 0 & \mu^{(r+1)} & \nu^{(r+1)} & \dots \\ \dots & \dots & \dots & \dots & \dots & \dots & \dots \end{vmatrix}$$

which is equivalent to

$$\begin{vmatrix} \kappa^r & \mu^{(r)} & \nu^{(r)} & \dots & \dots \\ & \mu^{(r+1)} & \nu^{(r+1)} & \dots & \dots \\ \dots & \dots & \dots & \dots & \dots \end{vmatrix}$$

or we have the equation

$$\begin{vmatrix} A & \dots & L \\ \dots & \dots & \dots \\ A^{(r-1)} & \dots & L^{(r-1)} \end{vmatrix} = \kappa^{r-1} \begin{vmatrix} \mu^{(r)} & \nu^{(r)} & \dots \\ \mu^{(r+1)} & \nu^{(r+1)} & \dots \\ \dots & \dots & \dots \end{vmatrix}$$

which in the particular case of  $r = n$  becomes

$$\begin{vmatrix} A & \dots & B \\ A' & \dots & B' \\ \dots & \dots & \dots \end{vmatrix} = \kappa^{n-1}."$$

The Second Section is said to concern "the notation and properties of certain functions resolvable into a series of determinants," and it is at once seen that the functions in question are obtainable from the use of  $m$  sets of  $n$  indices in the way in which a determinant is obtainable from only two sets. Sylvester spoke of them later (1851) as *commutants*.\*

CAYLEY (1845).

[On the theory of linear transformations. *Camb. Math. Journ.*, iv. pp. 193-209; or *Collected Math. Papers*, i. pp. 80-94.]

\* See Postscript to Cayley's paper "On the Theory of Permutants," *Camb. and Dub. Math. Journ.*, vii. pp. 40-51; or *Collected Math. Papers*, ii. pp. 16-26.



[Mémoire sur les hyperdéterminants. *Crelle's Journ.*, xxx. pp. 1-37.]\*

[On linear transformations. *Camb. and Dub. Math. Journ.*, i. pp. 104-122; or *Collected Math. Papers*, i. pp. 95-112.]

These memoirs, afterwards so famous in the history of what is now known as the algebra of quantics, contain exceedingly little on determinants. It is important, however, to direct attention to them, because the basis of them is a generalisation of determinants. Using language which came into vogue two or three years later, we may say that just as the idea and notation of determinants provided the means of expressing *one* of the invariants (viz., the discriminant) of a function, the idea and notation of hyper-determinants were brought forward for the purpose of expressing *all* the invariants.† The generalisation is of great width, hyper-determinants including as a very special case the generalisation previously made, viz., *commutants*.

The first memoir gives incidentally a more general mode of using what we may call the *notation of multiple determinants* than that specified in his paper of 1843. The first usage, it will be remembered, is exemplified by

$$\left\| \begin{array}{cccc} a_1 & a_2 & a_3 & a_4 \\ b_1 & b_2 & b_3 & b_4 \end{array} \right\| = 0$$

which is meant to signify that

$$\left| \begin{array}{cc} a_1 & a_2 \\ b_1 & b_2 \end{array} \right| = \left| \begin{array}{cc} a_1 & a_3 \\ b_1 & b_3 \end{array} \right| = \left| \begin{array}{cc} a_1 & a_4 \\ b_1 & b_4 \end{array} \right| = \left| \begin{array}{cc} a_2 & a_3 \\ b_2 & b_3 \end{array} \right| = \left| \begin{array}{cc} a_2 & a_4 \\ b_2 & b_4 \end{array} \right| = \left| \begin{array}{cc} a_3 & a_4 \\ b_3 & b_4 \end{array} \right| = 0.$$

A corresponding example of the new usage is

$$\left\| \begin{array}{cccc} a_1 & a_2 & a_3 & a_4 \\ b_1 & b_2 & b_3 & b_4 \end{array} \right\| = \left\| \begin{array}{cccc} x_1 & x_2 & x_3 & x_4 \\ y_1 & y_2 & y_3 & y_4 \end{array} \right\|,$$

\* This is stated to be a translation of the preceding paper, with certain additions by the author; and as such it is not reprinted in *Collected Math. Papers*. It also contains the substance of the paper which follows, the latter having been delayed in publication.

† And indeed the covariants also.

where six equations are again intended to be specified, viz.,

$$\begin{vmatrix} a_1 & a_2 \\ b_1 & b_2 \end{vmatrix} = \begin{vmatrix} x_1 & x_2 \\ y_1 & y_2 \end{vmatrix}, \quad \begin{vmatrix} a_1 & a_3 \\ b_1 & b_3 \end{vmatrix} = \begin{vmatrix} x_1 & x_3 \\ y_1 & y_3 \end{vmatrix}, \quad \dots$$

each determinant of the one group of six being meant to be equal to the corresponding determinant of the other group.

The example actually employed by Cayley is a result of the multiplication-theorem, and fully justifies the usage. It is

$$\begin{vmatrix} \lambda a + \lambda' a' + \dots, & \lambda \beta + \lambda' \beta' + \dots, \\ \mu a + \mu' a' + \dots, & \mu \beta + \mu' \beta' + \dots, \\ \dots & \dots \end{vmatrix} = \begin{vmatrix} \lambda & \mu & \dots \\ \lambda' & \mu' & \dots \\ \dots & \dots & \dots \end{vmatrix} \cdot \begin{vmatrix} a & \beta & \dots \\ a' & \beta' & \dots \\ \dots & \dots & \dots \end{vmatrix}$$

where, of course, the number of columns in the multiplier must be greater than the number in the determinant which is its cofactor.

It may be worth adding that the *Mémoire sur les hyper-déterminants* affords the first instance of the occurrence of Cayley's vertical-line notation in *Crelle's Journal*.\*

DE FÉRUSAC (1845).

[Sur la résolution d'un système général de  $m$  équations du premier degré entre  $m$  inconnues. *Nouv. Annales de Math.*, iv. pp. 28-32.]

This is a belated contribution, having no connection with any of those immediately preceding it. The author in all probability knew nothing of the subject, with the exception of Cramer's rule, which by this time was almost a century old.

The theorem which he seeks to establish is:—

“Connaissant les valeurs des inconnues d'un système de  $n$  équations à  $n$  inconnues, pour avoir le dénominateur commun des valeurs d'un système de  $n + 1$  équations à  $n + 1$  inconnues, on multiplie le dénominateur du valeur du premier système, par le coefficient de la nouvelle inconnue dans la nouvelle équation. Puis on en retranche les produits respectifs des

\* In *Liouville's Journal* brackets, [ ] or { }, were used in Cayley's own papers of the year 1845. See vol. x.

numérateurs des  $n$  inconnues du premier système par leurs coefficients, dans la dernière du nouveau système. Quant au numérateur il se forme toujours du dénominateur en remplaçant le coefficient de l'inconnue que l'on considère par le terme tout connu."

The method of proof is that known as "mathematical induction." The details of it need not be given, as they correspond closely with what are to be found in Scherk's paper of the year 1825, the main differences being that Férussac uses no special determinant notation, and, while clear and simple, is not nearly so lengthy nor so laboriously logical.

#### TERQUEM (1846).

[Notice sur l'élimination. *Nouv. Annales de Math.*, v. pp. 153-162.]

This is a continuation of Terquem's paper of the year 1842. Just as the previous portion dealt with Cramer and Bezout, this deals with Fontaine (des Bertins), Vandermonde, and Laplace, explaining concisely and clearly their main contributions to the subject.

The only portion of it calling for notice is that in which attention is drawn to the curious fact that Laplace makes no reference to Vandermonde's paper read to the Academy in the preceding year. In regard to this Terquem's remark is—

"Il est extrêmement probable que Laplace n'a pas pris connaissance du mémoire de son confrère : on sait, d'ailleurs, que les analystes français lisent peu les ouvrages les uns des autres. Ceci nous explique également comment la résolution de l'équation du onzième degré à deux termes, la plus importante découverte de Vandermonde, soit restée ignorée jusqu'à ce qu'elle ait attiré l'attention de Lagrange, après la découverte similaire de M. Gauss."

Not only, however, does this explanation not carry us far, but the question arises whether the point sought to be explained is really the point which stands most in need of explanation.

Vandermonde's paper was read at the very beginning of 1771 and Laplace's in 1772: yet in the History of the Academy for the latter year Laplace's occupies pp. 267-376 and Vandermonde's pp. 516-532, and neither refers to the other's work.

It may be noted here that, notwithstanding Terquem's knowledge of the early history of determinants and his manifest desire to induce his readers to take up the subject, he does not himself hold the new weapon with a very firm grasp. For example, in giving in this volume an account of a paper of Grunert's in *Crelle's Journal*, viii. pp. 153-159, in which the author says—

“Entwickeln wir nemlich  $x', y', z'$ , durch Elimination aus den Gleichungen :

$$\begin{aligned} x &= Ax' + By' + Cz', \\ y &= A'x' + B'y' + C'z', \\ z &= A''x' + B''y' + C''z'', \end{aligned}$$

so erhalten wir :

$$\begin{aligned} x' &= \frac{(B'C'' - B''C')x + (B''C - BC'')y + (BC' - B'C'')z}{L}, \\ y' &= \dots\dots\dots \\ z' &= \dots\dots\dots \end{aligned}$$

wenn wir

$$L = AB'C'' - A'BC'' + A''BC' - AB''C' + A'B''C - A''B'C$$

setzen ”—

he paraphrases the passage as follows :—

“ Les équations donnent

$$\begin{aligned} x' &= \frac{x[B'C''] + y[B''C] + z[BC']}{L}, \\ y' &= \dots\dots\dots \\ z' &= \dots\dots\dots \end{aligned}$$

où les crochets représentent des *binômes alternés* ;

$$[B'C''] = B'C'' - B''C',$$

et ainsi des autres : L est la *résultante*, dénominateur commun.”

The simultaneous use of *binôme alterné* and *résultante* is far from happy.\*

CATALAN (1846).

[Recherches sur les déterminants. *Bull. de l'Acad. roy. ... de Belgique*, xiii. pp. 534-555.]

As is known, Catalan had already dealt with determinants in the year 1839 in a memoir regarding the change of variables in a multiple integral. In the paper which we have now come to he leads up to examples of the same kind of transformation; but the greater part of it—seventeen out of the total twenty-two pages—is occupied with determinants pure and simple. Half of this amount consists of an elementary exposition of known properties, and calls for no remark save that what Cauchy called “produit principal” or “terme indicatif” is here called “terme caractéristique,” and that he makes constant use of the symbolism

$$\text{dét.}(A, B, C, \dots)$$

to stand for the determinant whose first row consists of *a*'s, second row of *b*'s, and so on: for example,

$$\begin{aligned} \text{dét.}(B, A, C, \dots) &= - \text{dét.}(A, B, C, \dots), \\ \text{dét.}(A, A, C, \dots) &= 0, \\ \text{dét.}(A + M, B) &= \text{dét.}(A, B) + \text{dét.}(M, B), \\ &\dots \dots \dots \end{aligned}$$

When we come to § 13, however, we find fresh ground struck. The exact words are:—

“Supposons maintenant qu'étant donné le système—

$$\left. \begin{array}{l} A_1, \\ A_2, \\ \cdot \\ \cdot \\ A_n \end{array} \right\} \dots \dots (A)$$

\* Two years later we find him, in referring to a paper of Cayley's where the determinant

$$\begin{array}{cccc|c} L & T & S & \xi & \\ T & M & R & \eta & \\ S & R & N & \zeta & \\ \xi & \eta & \zeta & & \end{array}$$

occurs, calling it a “fonction cramérienne,” and writing it

$$\left\{ \begin{array}{cccc} L & T & S & \xi \\ T & M & R & \eta \\ S & R & N & \zeta \\ \xi & \eta & \zeta & \end{array} \right\}.$$

dont le déterminant est  $\Delta$ , on ait combiné par voie d'addition et de soustraction les équations dont les premiers membres sont représentés par  $A_1, A_2, \dots, A_n$ ; et, par exemple, qu'on ait déduit du système (A) le système suivant

$$\left. \begin{array}{l} A_1 + A_2 + \dots + A_n, \\ A_1 - A_2, \\ A_2 - A_3, \\ \dots \dots \dots \\ A_{n-1} - A_n \end{array} \right\} \dots \dots (B)$$

dont la considération nous sera utile plus loin. Soit  $\Delta'$  le déterminant de ce nouveau système: d'après les n<sup>os</sup> (3) et (4), nous aurons

$$\begin{aligned} \Delta' = & \text{dét. } (A_1, -A_2, -A_3, \dots, -A_n) \\ & + \text{dét. } (A_1, A_2, -A_3, -A_4, \dots, -A_n) \\ & + \text{dét. } (A_1, A_2, A_3 - A_4, \dots, -A_n) \\ & + \dots \dots \dots \\ & + \text{dét. } (A_n, A_1, A_2, \dots, A_{n-1}). \end{aligned}$$

On sait que si l'on change les signes des termes d'une colonne horizontale, le déterminant change de signe; donc

$$\begin{aligned} \Delta' = & (-1)^{n-1} \text{dét. } (A_1, A_2, \dots, A_n) + (-1)^{n-2} \text{dét. } (A_2, A_1, A_3, \dots, A_n) \\ & + (-1)^{n-3} \text{dét. } (A_3, A_1, A_2, A_4, \dots, A_n) + \dots \dots \dots \\ & + (-\text{dét. } (A_{n-1}, A_1, A_2, \dots, A_{n-2}, A_n) + \text{dét. } (A_n, A_1, A_2, \dots, A_{n-1})). \end{aligned}$$

Dans la première parenthèse, il n'y a pas d'inversion; dans la seconde, il y a une inversion, etc.; donc

$$\Delta' = (-1)^{n-1} n \Delta."$$

The theorem thus reached may be enunciated as follows:—*If from a determinant  $\Delta$  of the  $n^{\text{th}}$  order, we form another  $\Delta'$  such that the first row of  $\Delta'$  is the sum of all the rows of  $\Delta$  and every other row of  $\Delta'$  is got by subtracting the corresponding row of  $\Delta$  from the row preceding it in  $\Delta$ , then*

$$\Delta' = (-1)^{n-1} n \Delta.$$

In Catalan's notation it is

$$\begin{aligned} & \text{dét. } (A_1 + A_2 + \dots + A_n, A_1 - A_2, A_2 - A_3, \dots, A_{n-1} - A_n) \\ & = (-1)^{n-1} n \cdot \text{dét. } (A_1, A_2, \dots, A_n), \end{aligned}$$

although, strange to say, it is never so formulated by him.

A generalisation of it is next given by saying :—

“Si la première ligne du système (B) avait renfermé seulement  $p$  des quantités  $A_1, A_2, \dots, A_n$ , nous aurions trouvé, pour la déterminant de ce système,

$$\Delta' = (-1)^{n-1} p \Delta,$$

and then there follow a number of applications to the evaluation of certain special determinants.

Thus, to take the simplest example, having

$$\Delta = \begin{vmatrix} 1 & . & . & . \\ . & 1 & . & . \\ . & . & 1 & . \\ . & . & . & 1 \end{vmatrix} = 1$$

the theorem gives

$$\begin{vmatrix} 1 & 1 & 1 & 1 \\ 1 & -1 & . & . \\ . & 1 & -1 & . \\ . & . & 1 & -1 \end{vmatrix} = (-1)^3 4 \Delta = -4.$$

The other illustrations all concern determinants of the special form afterwards known as “circulants”; for example,  $C(-1, 1, 1, \dots, 1)$ ,  $C(-1, -1, 1, 1, \dots, 1)$ , etc.,  $C(1, 1, \dots, 1, 0)$ ,  $C(1, 1, \dots, 1, 0, 0)$ , etc. They therefore fall to be dealt with in a different place.

SARRUS, P. F. (1846).

[Finck, P. J. E. *Éléments d'Algèbre*. Seconde édition. iv + 544 pages. Strasbourg.]

In the course of his discussion of the solution of a set of linear equations with three unknowns, the author interjects the following paragraph (No. 52, p. 95) :—

“ Pour calculer, dans un exemple donné, les valeurs de  $x$ ,  $y$  et  $z$ , M. Sarrus a imaginé la méthode pratique suivante, qui est fort ingénieuse. D'abord on peut calculer le dénominateur, et à cet effet on écrit les coefficients des inconnues ainsi

$$\begin{array}{ccc} a & b & c \\ a' & b' & c' \\ a'' & b'' & c'' \end{array}$$

On répète les trois premiers  $a \ b \ c$   
et les trois suivants  $a' \ b' \ c'$

Actuellement partant de  $a$ , on prend diagonalement du haut en bas, en descendant à la fois d'un rang, et reculant d'autant à droite,  $a'b'c''$ : on part de  $a'$  de même, et on a  $a'b'c''$ ; de  $a''$ , et on trouve  $a''b'c'$ ; on a ainsi les trois termes positifs (c'est-à-dire à prendre avec leur signes) du dénominateur. On commence ensuite par  $c$  et descendant de même vers la gauche on a  $c'b'a''$ ,  $c'b'a'$ ,  $c'b'a'$ , ou les trois termes négatifs (ou plutôt les termes qu'il faut changer de signe).”

This “méthode pratique” or mnemonic is the original form of the so-called “règle de Sarrus” which came later to have unnecessary prominence given to it by writers on determinants when dealing with those of the third order.\*

\* The date 1833 has been assigned to this “rule” in a recent German text-book on determinants (Weichold's): if 1833 be the correct date the “rule” probably will be found in a publication by Sarrus entitled *Nouvelle méthode pour la résolution des équations*, which appeared at Strasbourg in that year.

#### LIST OF AUTHORS

whose writings are herein dealt with.

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(Issued separately February 12, 1904.)



**Man as Artist and Sportsman in the Palæolithic Period.** By Robert Munro, M.A., M.D., LL.D. (With Eleven Plates.)

(An Address delivered at the request of the Council, Nov. 23, 1903.)

I. INTRODUCTION.

So long as *Homo sapiens* was believed to occupy a higher platform in the organic world than other animals by virtue of his special endowments, no one, apparently, thought of looking for evidence of his origin and history in the obscure vista of prehistoric times. The long cherished traditions and myths which had gathered around the inquiry left little room for any other hypothesis than that his apparition on the field of life was the last and crowning achievement of a long series of creative fiat which brought the present world-drama into existence. In the cosmogony thus conjured up, the multitudinous phenomena of the material world—animals and plants, the distribution of land and water, the recurrence of seasons, etc.—were regarded as having been specially designed and arranged to administer to the life-functions of this new being.

Nurtured in an environment so full of legendary romance, we need not be surprised that the philosophic schools of Britain, as well as of other countries, continued to teach some such theory of man's origin up to about half a century ago, when the doctrine of organic evolution captured the scientific mind of the day. But, notwithstanding the far-reaching significance of the evolution theory, the evolutionary stages of man's career on the globe remained almost as great a mystery as before; for, at the outset, the new doctrine appeared to go no further than to point to the direction in which the trail of humanity was to be looked for. The erect attitude, bipedal locomotion, true hands, and a unique handicraft skill, amply differentiated him from all other animals. But for a long time no rational explanation of how he acquired these distinguishing characteristics was forthcoming; and, even

now, their origin and development are among the most obscure problems within the whole range of anthropology.

In the address which I had the honour of delivering in 1893, as president of the Anthropological Section of the British Association for that year, I advocated the hypothesis that the origin of the higher mental manifestations of man was primarily due to the attainment of the erect attitude, which, by entirely relieving the fore-limbs of their primary function as locomotive organs, afforded him the opportunity of entering on a new phase of existence, in which intelligence and mechanical skill became the governing factors. With the completion of the morphological changes involved in the attainment of this attitude, the evolution of the present human form, with the exception of some remarkable modifications in the skull and facial bones, which will be subsequently referred to, was practically completed. As soon as bipedal locomotion became habitual and firmly secured on an anatomical basis, it does not appear that the osseous characters of the lower limbs would be sensibly affected by any subsequent increase in the quantity or quality of brain-matter. For example, the function of the femurs being henceforth to support a certain load, *i.e.* the entire weight of the body, it would not influence them in the least whether that load contained the brains of a fool or of a philosopher. The important and novel element which the permanent assumption of the erect posture was the means of introducing on the field of human life, was the use to which the eliminated fore-limbs were put. By substituting, for nature's means of defence and self-preservation, a variety of implements, weapons and tools made with their own hands, the subsequent well-being of these novel bipeds became dependent on their ability to interpret and utilise the laws and forces of nature. As time went on they began to recognise the value of the faculty of reasoning as the true source of inventive skill; and hence a premium was put on this commodity. In this way, stimulants to the production of new ideas and new inventions were constantly coming within the scope of their daily avocations, the result of which was a steady increase of human intelligence, and consequently of brain substance. Now, according to the well-established doctrine of the localisation of brain function, the

additional brain molecules and cells thus acquired had their seat of growth for the most part somewhere in the cerebral hemispheres which lie well within the anterior portion of the brain-casing. The mere mechanical effect of this increment to the physical organ of thought would be to increase the weight of the anterior half of the head, and so to upset its finely equipoised position on the top of the spinal column. But as any interference with the free and easy rotatory movements of the head would manifestly be disadvantageous to the individual in the struggle of life, it became necessary to counteract the influence of this disturbing element by some other concurrent morphological process, which would not be prejudicial to the general well-being of the human economy. This object was partly attained by a retrocession or contraction of the facial bones, especially the jaw bones, towards the central axis of the spinal column, and partly by a backward shifting of the cerebrum over the cerebellum. As the gradual filling up of the cranial cavity progressed necessarily *pari passu* with these cerebral modifications, we have, in the facial angle of Camper, a rough mechanical means of estimating the progress of mental development during the period of man's existence as a human being, *i.e.* since he attained the erect attitude.

One of the results of this retrocession of the facial bones was the gradual contraction of the alveolar borders of the jaws, thereby diminishing the space allotted to the teeth,—a fact which plausibly accounts for some of the peculiarities which differentiate the older fossil jaws from modern specimens. Thus, in the dentition of the former, the third or last molar is the largest, whereas in the latter it is the smallest. Not only so, but among Neolithic and some European races of to-day these four molar teeth (wisdom) make their appearance at a later date in the individual's life than formerly, so that they seem to be on the highway to become vestigial organs. It is interesting to note that the shortening of the dental portion of the human jaw attracted the attention of Mr Darwin, who, however, attributed it to "civilised men habitually feeding on soft, cooked food, and thus using their jaws less."

Another peculiarity of civilised races is the greater prominence of the chin, a peculiarity which may also be due to the contraction

of the alveolar ridges and the consequent more upright setting of the teeth in their sockets. But whatever the precise cause may have been, there can be no doubt that the gradual formation of the chin has a striking parallelism with the progressive stages of man's intellectual development, ever since he diverged from the common stem line from which he and the anthropoid apes have descended (see fig. 18).

From these general remarks it will be seen that there are two distinct lines on which investigations into the past history of mankind may be profitably conducted, both of which start from the attainment of the erect attitude. The evidential materials to be gathered from these different sources consist, in the one case, of some fragments of a few skeletons of former races, which, by some fortuitous circumstances, have to this day resisted the disintegrating forces of nature; and, in the other, of a number of specimens of man's handicraft works, which, being largely made of such enduring substance as flint, are more abundantly met with. The successive modifications which these respective materials have undergone during a long series of ages, though different in kind, are found to bear a decided ratio to the progress of human intelligence. Thus, taking the human skull at the starting-point of humanity as comparable to that of one of the higher apes, we know, as a matter of fact, that during the onward march of time it has undergone some striking changes, both in form and capacity, before reaching the normal type of modern civilised races—changes which can be largely classified in chronological sequence (see pp. 99-108). Similarly, the artificial products of man's hands show a steady improvement in type, technique, and efficiency, commensurate with his progressive knowledge of the laws of nature and his ability in applying them to mechanical and utilitarian purposes. Indeed, the trail of humanity along its entire course is strewn with the discarded weapons and tools which, from time to time, had to give way to others of greater efficiency. Such obsolete objects are now only collected as curiosities to be preserved in archæological museums (see pp. 109-117).

The main object of these preliminary remarks is to emphasise the nature and true significance of the methods by which anthropologists have been enabled to prosecute their researches far

beyond the limits of the historic period. Without a clear notion of the logic and grounds on which their deductions are founded, it would be impossible to enlist the attention of a general audience to an address involving data so different from those of ordinary scientific work.

The special subject on which I have to discourse consists of some exceptionally interesting human relics, chiefly belonging to the Later Palæolithic period in Europe. These remains have been most abundantly found among the culinary *débris* of a race of hunters who inhabited caves and rock-shelters in France, Switzerland, South of England, and other parts of Europe. Among the more remarkable objects collected in these localities are representations of various animals carved, and sometimes sculptured, on pieces of ivory, horn, bone and stone. As illustrations of most of these artistic productions have been published, I am enabled to exhibit some of the more characteristic specimens on the screen. But before doing so, there is one question which I had better dispose of at once, viz., that of their supposed age, because the answer is itself a typical object-lesson of the resourceful means by which anthropological investigations are being conducted.

Whatever views may be held as to the anthropological value of the famous skull of *Pithecanthropus erectus* (figs. 4 and 5), discovered some ten years ago by M. Dubois in the Upper Pliocene deposits of Java, the femur (fig. 6) found in the same stratum with it conclusively proves that there had been then in existence a being of the genus *Homo* which had assumed the erect attitude as its normal mode of locomotion—*i.e.* at a time prior to the advent of that great landmark in the physical history of the northern hemisphere known as the glacial period. Now it was only towards the end of that period, just when the ice sheet and its great feeding glaciers were creeping back to their primary centres of dispersion in the mountainous regions of Britain, Central Europe, and Scandinavia, that the European troglodytes, whose antiquity is now *sub judice*, flourished. Hence, they and their works must be assigned to an intermediate period between the present time and the starting-point of humanity. As the first part of this chronological range may be equated with nearly the whole duration of the glacial period, the task of converting it into so many cen-

turies or millenniums may be left in the hands of astronomers and geologists, who, in more recent times, have appropriated among them the solution of this part of the problem. It is with the second part of the range, viz., the time which has elapsed since the Palæolithic artists and hunters lived, that we are now chiefly concerned. It embraces the entire duration of the Historic, Iron, Bronze and Neolithic Ages, together with an interval of unknown length between the Neolithic and Palæolithic civilisations. It has long been supposed that during this obscure interval there had been a *hiatus* in the continuity of human existence in Western Europe—an idea which, however, is now justly discredited in face of more recent discoveries, throughout the same geographical area, of transition deposits containing human relics. Of these later discoveries the rock-shelter of Schweizersbild, near Schaffhausen, is one of the best examples known to me, as its *débris* indicates that the site was a constant *rendezvous* for bands of roving hunters from the Palæolithic period down to the Bronze Age. Dr Nüesch, its explorer, has expressed the opinion, founded on the relative thickness of the deposits and the character of the fauna represented in them, that the antiquity of its earliest human relics cannot be less than 20,000 years. Now, since the art-remains found in this station and in the adjacent cave of Kesslerloch are precisely similar to those of the analogous stations in France, we can accept the above estimate as equally applicable to the latter. The nature of the evidence on which Dr Nüesch founded his opinion is briefly as follows:—

According to Professor Nehring, who has made a special study of the animals now inhabiting the arctic and sub-arctic regions, those characteristic of the former are — Band-lemming, Obi-lemming, arctic fox, mountain hare, reindeer and musk-ox. With these are frequently associated a number of animals of migratory habits, such as northern vole, water-rat, glutton, ermine, little weasel, wolf, fox and bear. Now, the extraordinary fact was brought out that of these fourteen species only the Obi-lemming and the musk-ox were unrepresented in the lowest relic-bed of the Schweizersbild. The latter was, however, found in the *débris* of the Kesslerloch cave in the vicinity. It appears that the Band-lemming (*Myodes torquatus*) and the arctic fox are the most persistent animals of the arctic fauna, so that the

presence of the bones of these two animals in the *débris* of this rock - shelter was alone sufficient to prove that the climate of the period was of an arctic character. In the upper portion of this deposit relics of new animals, indicating a change to a sub-arctic climate, began to appear, and had their greatest development in the next succeeding layer.

The result of careful analysis of the contents of the other deposits showed that this arctic fauna became ultimately displaced by the true forest fauna of the Neolithic period. Among the newcomers were the badger, wild cat, hare, *Urus*, *Bos longifrons*, goat and sheep; while of those represented in the Palæolithic deposit a large number was absent. Thus both the arctic and sub-arctic fauna had given way to a forest fauna, and, synchronous with these changes, the Palæolithic hunters and reindeer vanished from the district.

Among the few art specimens found at the Schweizersbild is a stone tablet, having rude outlines of a wild ass and of a reindeer incised upon it. The whole collection, among which were 14,000 worked flints, 180 fragments of bone needles, 41 whistles, 42 pierced ornaments made of shells and of the teeth of the arctic fox, glutton, etc., is typical of the latest phase of Palæolithic civilisation of the Dordogne caves.

The chronological deductions founded on the investigations at the Schweizersbild are, from their very nature, more or less hypothetical. But, after all allowances for possible errors are made, I can see no objection to Dr Nüesch's lowest estimate of the date of man's first appearance into Northern Switzerland, viz., 20,000 years ago.\*

I now proceed to exhibit some illustrations selected from the evidential materials on which the opinions and conclusions advocated in this address are founded. The slides are arranged in two series, corresponding to the two lines of research on which, as mentioned in the preliminary remarks, anthropological investigations are most usually conducted. Afterwards I will add some further comments on the phase of human civilisation thus so singularly resurrected from the lumber-room of oblivion.

\* See *Neue Denkschriften der allgemeinen schweizerischen Gesellschaft für die gesammten Naturwissenschaften*, vol. xxxv.

## II. ILLUSTRATIONS.

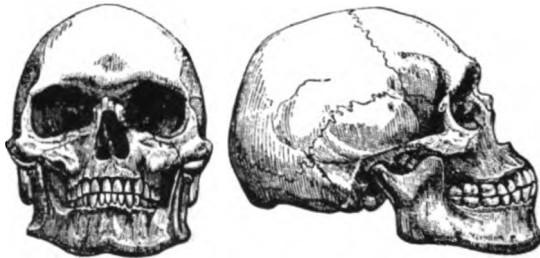
The following illustrations are not in all cases reproductions of those exhibited on the screen when the address was delivered, as it was impracticable to convert some of them into printing blocks. They are, however, with few exceptions, substantially the same, only grouped differently, and are specially selected to elucidate the various points touched upon in the text. The remains of fossil man are, as yet, too meagre to afford much choice of illustrative materials; but of the handiworks of the artists and hunters of the Palæolithic period there is no lack, as, indeed, most of the principal museums of the world contain more or fewer specimens in addition to casts of the most remarkable pieces. Even in the Scottish metropolis, anyone desirous of becoming conversant with their characteristic features has only to visit the ethnological department of either the Museum of Science and Art or of the National Museum of Antiquities. The literature of the subject is also voluminous and much of it readily accessible, among which I would particularly mention the recently issued Guide to the Antiquities of the Stone Age in the British Museum. Owing to the roundness of the beam of an antler, on which these engravings are generally executed, the whole of the incised outlines of an animal cannot always be seen from one point of view, and hence a drawing is sometimes more effective than a photograph. The illustrations here supplied are the result of a combination of all available sources—original specimens, casts, photographs and drawings of objects not at hand being requisitioned into the work.

A.—*Evidence of Progressive Changes in the Human Skull.*

Among the bodily features which distinguish man from other animals the following are particularly worthy of note, viz., the upright attitude, with the head balanced on the top of the spinal column; the double curvature of the spine; the great difference between the hands and feet; the power of firmly opposing the thumb to each of the other four fingers; the prominence of the frontal bone; and the almost vertical profile of the face. It may, however, be observed that, as regards the prominence of the forehead and degree of prognathism of the facial bones, some striking variations occur among the different existing races. To show the extent of these differences I reproduce, from Owen's *Comparative Anatomy* (vol. ii. pp. 558, 560), figures of two skulls, one (figs. 1 and 2) labelled "Cranium of a native Australian," and the other (fig. 3) "Skull of a well-formed European," from which it will be at once seen that the former has, relatively, a retreating forehead and a highly prognathic profile, while the latter has a well-filled forehead and an orthognathic face.



The next step in the argument is to show that some fossil skulls possess, to a more or less degree, the features of the Australian skull—the degree of divergence from the normal European type being in direct proportion to their antiquity. As bearing on this important generalisation, let me, in the first



FIGS. 1 and 2. — Front and side views of the skull of a native Austral.an. (After Owen.)

place, refer to the famous calvaria of *Pithecanthropus erectus* (figs. 4 and 5), discovered (1891–2) by Dr Dubois, in the detritus of a Pliocene river in Java, which shows a remarkably low and retreating forehead. In the absence of the facial bones

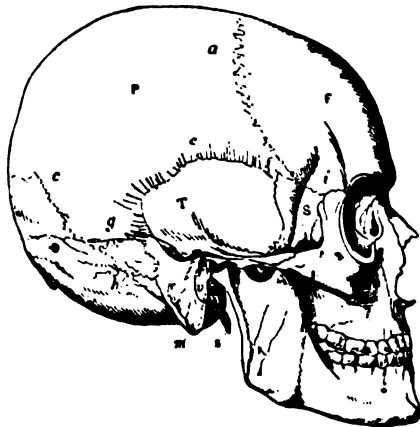


FIG. 3.—Skull of a well-formed European. (After Owen.)

we can only surmise that the individual which originally owned this skull presented a highly prognathic appearance, approaching even to that of *Hylobates*, to which Dr Dubois compares it. (See *Pith. erectus*, Plate I., 1894.)

The femur (fig. 6) discovered by Dr Dubois in the same place has been pronounced by most of the anatomists who had critically examined it to be human; but, as it lay at a distance of 15 mètres from the calvaria, there is no absolute certainty that the two bones belonged to the same individual. There can, however, be no doubt that this femur was that of an animal



FIG. 4.—Side view.

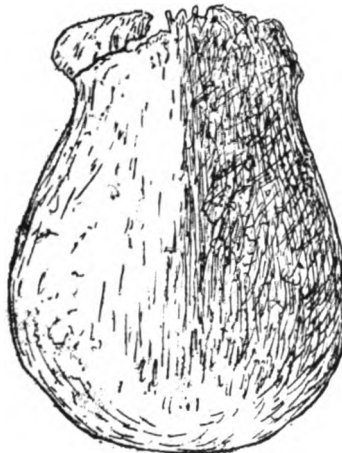


FIG. 5.—Top view.

The skull of *Pithecanthropus erectus*, Java ( $\frac{1}{2}$ ). (After Dr Dubois.)

which, at that early period, had attained the erect attitude— an animal which therefore must have belonged to the genus *Homo*. The logical deduction from these data is thus necessarily limited to probability; but if the hypothesis of organic evolution be correct, the Java skull is precisely in that stage of cranio- logical development which would be expected at that early time in the history of humanity.

The skull of the human skeleton discovered in 1856 in the

cave of Feldhoven, situated at the entrance to the Neanderthal ravine, on the right bank of the Düssel, and since known as the 'Neanderthal skull,' presented such remarkable peculiarities that, when first exhibited at a scientific meeting at Bonn,

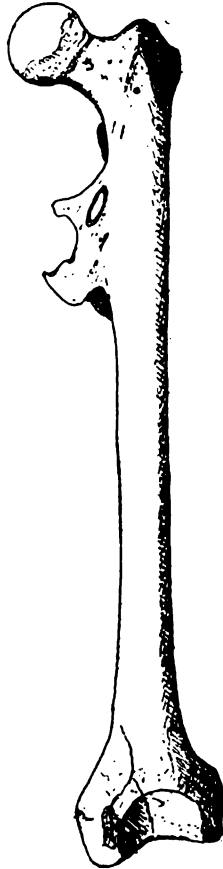


FIG. 6.—Femur of *Pithecanthropus erectus*, found in Java (4).  
(After Dr Dubois.)

doubts were raised by several naturalists as to whether the bones were really human. Figs. 7 and 8 represent two views of this relic, outlined from figures published by Professor Huxley (*Collected Essays*, vol. vii. p. 180), from which its characteristics, especially the low retreating forehead, may be seen at a glance. Writing in 1863, Professor Huxley made the following remarks

on the Neanderthal skull:—"There can be no doubt that, as Professor Schaaffhausen and Mr Busk have stated, this skull is the most brutal of all known human skulls, resembling those of the apes not only in the prodigious development of the superciliary prominences and the forward extension of the orbits, but still more in the depressed form of the brain-case, in the



FIG. 7.—Side view.

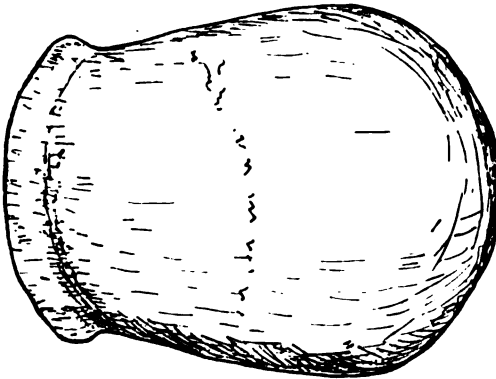


FIG. 8.—Top view.

The Neanderthal skull ( $\frac{1}{3}$ ). (After Huxley.)

straightness of the squamosal suture, and in the complete retreat of the occiput forward and upward, from the superior occipital ridges."—(Lyell's *Antiquity of Man*, p. 84.)

The skull (cephalic index 70) of one of the Spy skeletons (figs. 9, 10 and 11) also shows a low retreating forehead, marked prognathism, a sloping chin, and large third molar teeth. These skeletons were discovered in 1886, buried 12½ feet in fallen *débris* at the entrance of a grotto in the province of Namur,

Belgium. The worked flints found in the cave were of the type known as *Moustérien*, and among the fauna represented were *Rhinoceros tichorhinus*, cave-bear, mammoth, hyæna, etc. No works of art were among the relics, so that the Spy troglodytes

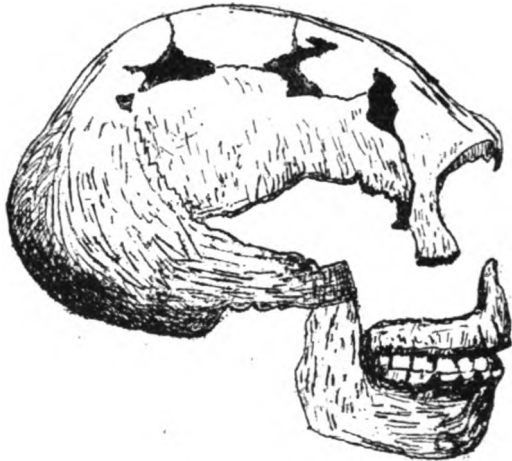


FIG. 9.—Side view.

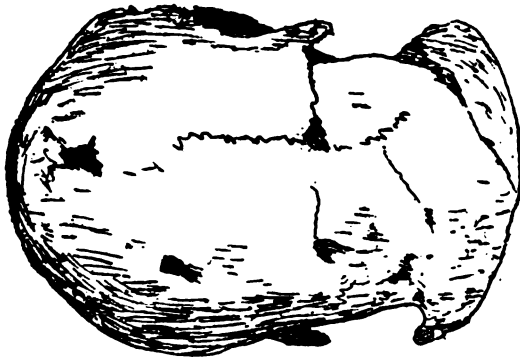


FIG. 10.—Top view.

Skull from the *Grotte de Spy* ( $\frac{1}{3}$ ). (After Fraipont.)

are justly regarded as belonging to an earlier period than that in which the reindeer hunters and artists flourished.

The larger portion of a lower human jaw (figs. 12 and 13) was disinterred in 1865 from the *débris* in the *Trou de la Naulette*, at a depth of 4·50 mètres beneath the last floor of the cave. Above it was a succession of five stalagmitic layers, intercalated with

fluvial deposits from the river Lesse. The fauna represented in



FIG. 11.—Tracing showing size of teeth in the lower jaw of Spy skull (‡).  
(From photograph.)



FIG. 12.—Naulette jaw—side view (‡). (After M. Dupont.)

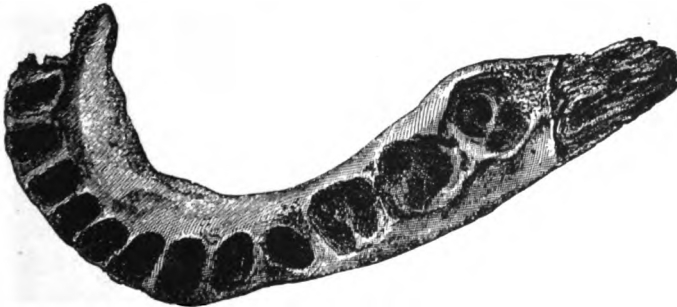


FIG. 13.—Naulette jaw—view from above (‡). (After M. Dupont.)

the same stratum included the mammoth, rhinoceros, horse, and a number of animals common to Neolithic times. The special

features of this jaw are its small height in proportion to its thick-

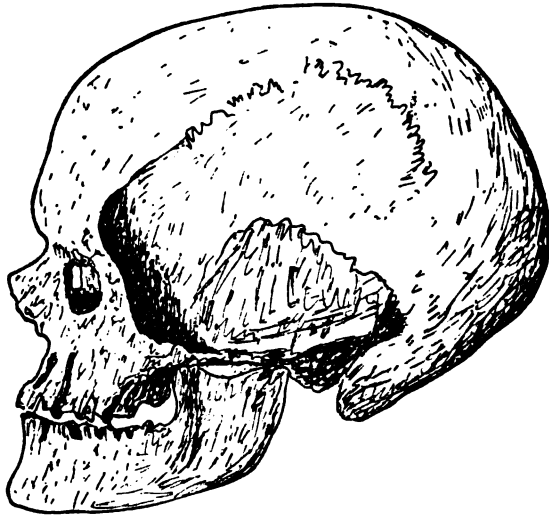


FIG. 14.—Side view.



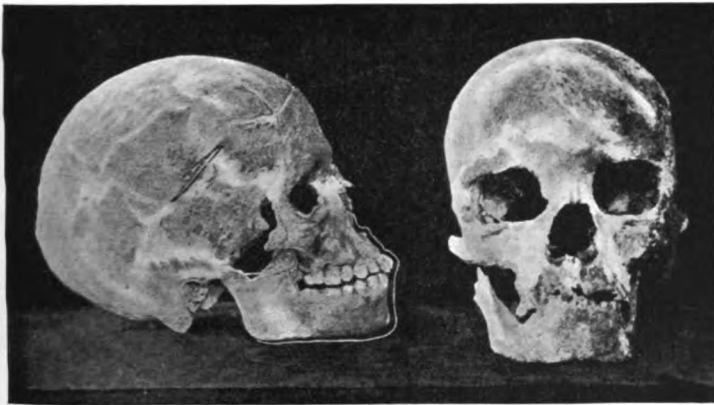
FIG. 15.—Front view.

Skull of the 'Old Man of Cro-Magnon' ( $\frac{1}{2}$ ).

ness, the backward slope of the chin, and the large size of the socket of the third molar.

Figs. 14 and 15 show front and profile views of the skull of

the 'old man of Cro-Magnon,' which discloses a decided approach to the normal type of civilised man. Its cephalic index is 73.6 and its capacity 1590 c.c. The height of this individual was 1.82 mètres (5 feet 11½ inches). The lower jaw has a large ascending ramus, behind which, on both sides, the third molar is partly hidden. These two teeth have also the peculiarity of being smaller than the other molars, being in this respect more allied to the dentition of Neolithic and modern races. For these reasons, as well as the fact that it was found on the surface of the Palæolithic *débris*, some anthropologists maintain that the 'old man of



FIGS. 16 and 17.—Two skulls from the Grotte des Enfants, Mentone.  
(After M. Verneau.)

Cro-Magnon' belonged to the early Neolithic period—a point elsewhere referred to in this address.

Figs. 16 and 17 are reproductions of illustrations by Dr Verneau of two skulls found in the *Grotte des Enfants*, near Mentone. That on the left belonged to a young man, and that on the right to an aged female. They are part of two skeletons which lay close together on a hearth-layer at a depth of 7.75 mètres. The cephalic index of the former is 69.72 and of the latter 68.58. These skeletons were those of small individuals, their respective heights being 1.54 mètres (5 feet 0½ inch) and 1.58 mètres (5 feet 2 inches). About 27½ inches higher up in the *débris* another skeleton, measuring no less than 1.92 mètres in height (6 feet 3¼ inches), was found, which presented all the



characteristics of the Cro-Magnon type (cephalic index 76.26). The *débris* in which these skeletons were discovered contained relics comparable to those of the latest phase of the Palæolithic civilisation (*L'Anthropologie*, vol. xiii. pp. 561-583).

Fig. 18 represents a series of lower jaws illustrating, accord

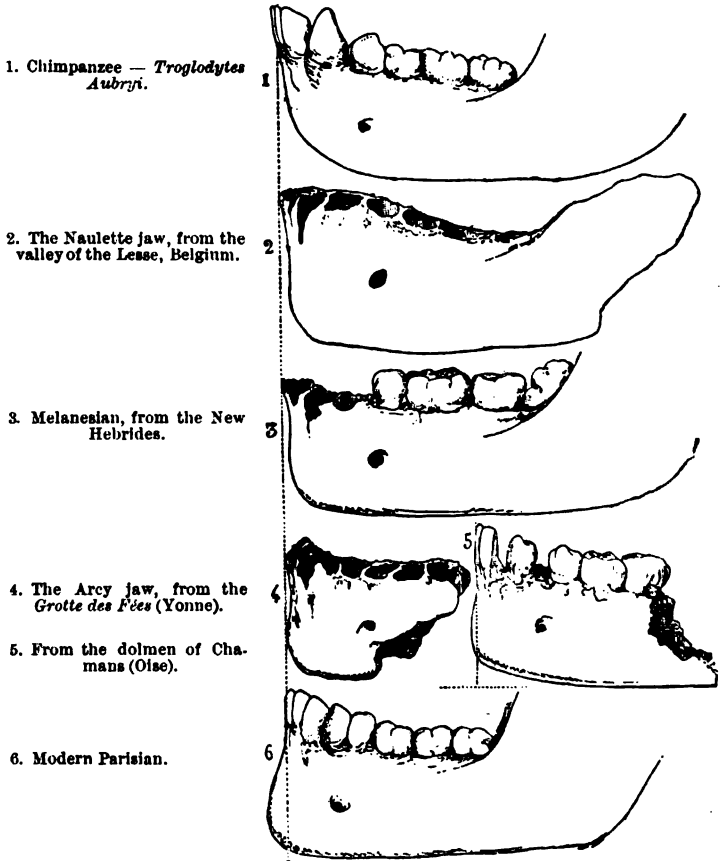


FIG. 18.—Profile of various lower jaws. (After Broca.)

ing to the late Paul Broca, the gradual evolution of the human chin. M. Broca exhibited the drawing in support of his views at the International Congress of Anthropology and Prehistoric Archæology held in Paris in 1867 (*Comptes Rendus*, p. 399). The Spy jaw, which of course was then unknown, would take its place in the series between Nos. 2 and 3.

B.—*Evidence of progressive skill in the handicraft works of Man.*

Plate I. gives a full-sized view of a flint implement found, along with an elephant's tooth, at Gray's Inn Lane, London, about the end of the 17th century, being the first recorded discovery of the

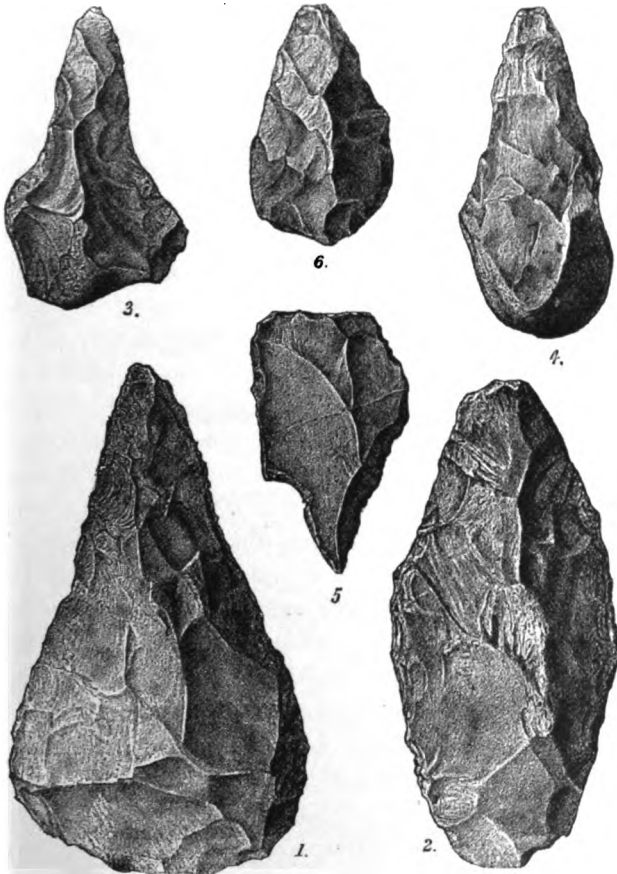


FIG. 19.—Palæolithic flint implements from the Terrace-gravel at Galley Hill ( $\frac{1}{2}$ ).

kind in Britain. It is a typical specimen of what French archaeologists call the 'coup de poing,' probably the first definite type of hand-implement which came to be widely imitated among the earlier races of man. Implements of this kind vary considerably in form and size, the degree of variability being, however, strictly compatible with its function as a hand-tool. Fig. 19 shows a variety

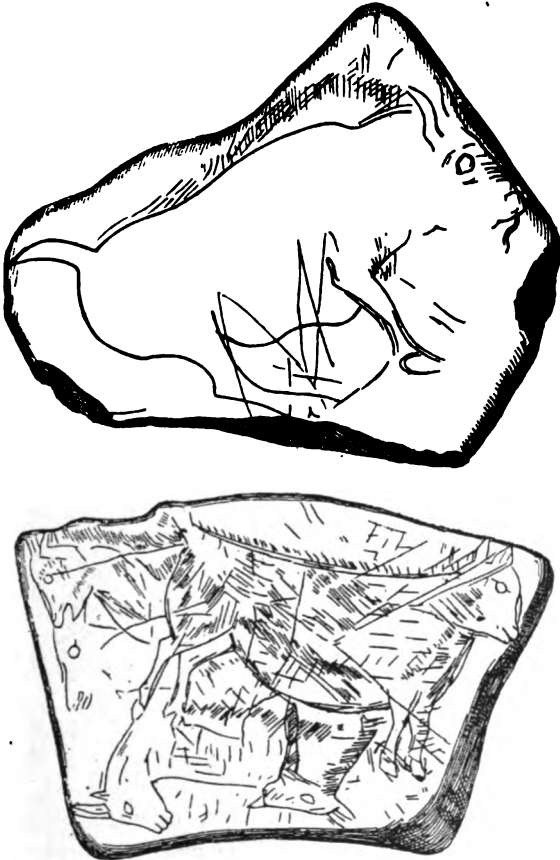
of such implements from the terrace-gravels of Galley Hill, Kent.\* Of course it is not denied that stone implements were used by man long before he invented the 'coup de poing,' but I am unable to classify those earlier forms into any chronological sequence. Nor would I hazard a guess, in the present state of our knowledge, as to whether it is by centuries or millenniums we are to reckon the duration of that earlier stage of man's career.

Worked flints of the 'coup de poing' type are largely collected from the river-drift gravels of England and France, as well as elsewhere, and nearly all have the peculiarity of being made by chipping a nodule so as to convert it into a useful hand-tool—the flakes struck off being apparently of no use. When, however, it was discovered that some of the larger flakes could be utilised as sharp cutting tools, attention began to be directed to the art of producing them for teleological purposes. After some experience a skilled workman could produce a flake of any required size and shape. By subjecting these flakes to secondary chipping, implements of great variety and efficiency were ultimately obtained. This was indeed an important step in flint industry, evidence of which is to be found in the fact that henceforth flakes were the useful products, while the residuary cores were rejected as waste. The worked flints found in the earlier inhabited caves of France and Belgium, such as Moustier and Spy, show that the flaking stage was already in full progress—thus proving that their habitation was later than the formation of the river-drift gravels. Towards the middle of the Palæolithic civilisation (*Solutréen*) the flint industry had attained a state of great perfection, scarcely surpassed in any subsequent period.

That these cavemen did not confine their awakening intelligence to the working of flint objects is amply shown by the array of broken or lost harpoons, lance- and spear-heads, pins, needles, and nondescript articles made of bone or deer-horn which now appear in the *débris* of their inhabited sites. Some idea of their skill in this new industry may be gathered from an inspec-

\* These flint figures are from the *Quarterly Journal of the Geological Society* (vol. li.). The block was kindly lent to me by the Council for use in *Prehistoric Problems*, and it is here reprinted from the *cliché* then made for me.

tion of Plate III. Indeed it would appear as if bone and horn had almost superseded flint in the manufacture of weapons of the chase. This partly accounts for the large number of small flint tools, such as knives, saws, scrapers, borers, etc., found on



FIGS. 20 and 21.—Bovide incised on stone, from the rock-shelter of Bruniquel (‡). (After British Museum Catalogue.)

*Magdalénien* sites (Plate II.). It was, no doubt, by means of these finer flint instruments that the artists were able to bore the eye of a fine needle, to carve hunting scenes, and to sculpture their dagger-handles and *bâtons de commandement* into the conventional forms of familiar animals.

The artistic skill displayed by these primitive hunters has been

one of the most astounding revelations of prehistoric archæology. Typical specimens of their skill in carving and sculpture on bone, deer-horn, and ivory may be studied on Plates III. to X. Figs. 20 and 21 represent two stones from the rock-shelter of Montastruc, Bruniquel, with outlines of bovidæ incised on them, the forms of which might have been intended for the *Bos primigenius*. The originals are now in the British Museum.

C.—*The Carving and Painting of Animals on the Walls of Palæolithic Caves.*

Within later years interest in the art remains of these

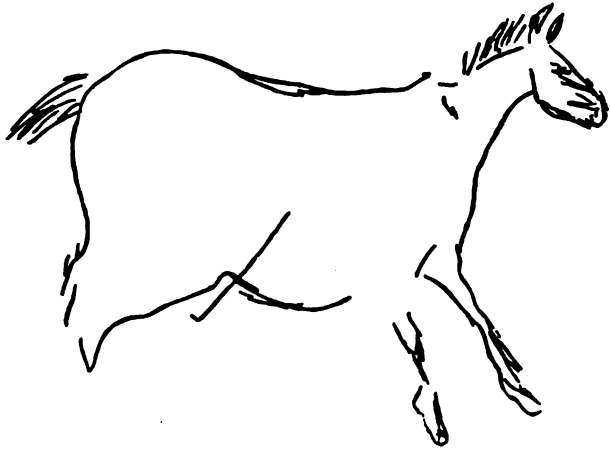


FIG. 22.—Incised figure of horse on the wall of the *Grotte de la Mouthe*.  
(After E. Rivière.)

Palæolithic hunters has been greatly stimulated by the discovery of large engravings, and even coloured paintings, of various animals on the walls of some newly-explored caves in the South of France, more especially those of Combarelles and Font-de-Gaume, both situated in the Commune of Tayac (Dordogne), and within a short distance of the well-known station of Les Eyzies. Obscure indications of this kind of art had been observed as early as 1875 in the cave of Altamira, near Santander, in the north-east of Spain. Subsequently, and at various intervals, more pronounced examples were notified in the caves of Chabot (Gard), La Mouthe (Dordogne), and Pair-non-Pair

(Gironde), in all of which figures of animals regarded as characteristic of the Palæolithic period occurred.

Of the earlier discoveries I reproduce (after M. Rivière) illustrations of two horse figures engraved on the walls of the cave of La Mouthe (*Bull. de la Société d'Anthropologie*, October 19th). These designs were incised on a panel 128 mètres from the entrance. The first (fig. 22) represents an animal with a small head, slender neck, and well-formed fore-quarters; but the posterior part is heavy and altogether out of proportion. The other (fig. 23) has a stout neck, a long head, with a front directed almost vertically, and a heavy chin. Whatever may have been the defects of the artists, the originals of these two



FIG. 23.—Head of horse, *Grotte de la Mouthe*. (Rivière.)

drawings must have been very different animals, if not different species. Among the other animals figured in this cave were bison, bovidæ, reindeer, goat and mammoth.

On the 16th September 1901 MM. Capitan and Breuil submitted a joint note to the Paris Academy of Sciences on "A New Cave with Wall Engravings of the Palæolithic Epoch." This was followed a week later (23rd September) by a second note, by the same explorers, on "A New Cave with Painted Wall Figures of the Palæolithic Epoch." A noteworthy distinction in the art illustrations of these two caves is that one (Combarelles) has its walls adorned almost exclusively with engravings, cut more or less deeply, and the other (Font-de-Gaume) with paintings in ochre and black, or sometimes only

in one colour, forming real silhouettes of the animals thus depicted.

Some of the engravings in the cave of Combarelles have been carefully copied and published by the explorers, from which the following figures are reproduced (*Revue de l'École d'Anthropologie*, January 1902).



FIG. 24.—A group of animals on the wall of the cave of Combarelles.

Fig. 24 shows a group of animals on a portion of the wall. Fig. 25 represents a pony with a large head, shaggy mane, and a bushy tail. It has been suggested by MM. Capitan and Breuil that the animal was domesticated, bridled, and draped with some kind of ornamental covering. Reindeer, wild goat,

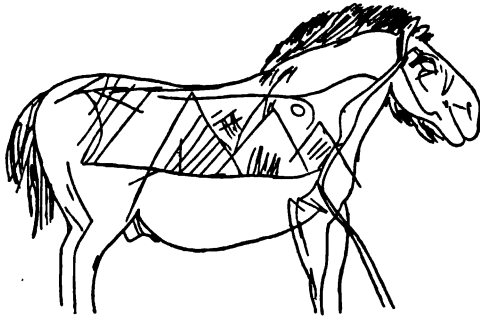


FIG. 25.—Outline of horse supposed to be domesticated. (Combarelles.)

and mammoth will be readily recognised under figs. 26, 27, and 28. It will be of interest to compare with the latter figure that of the skeleton of the mammoth (fig. 29) whose carcass was discovered in 1799 embedded in frozen *tundra* at the mouth of the Lena, Siberia. Seven years later it was purchased by Mr Adams for the museum of St Petersburg, but in the interval dogs and wild animals had eaten the flesh, and only the bones and fragments of the skin with its long hair could be recovered. The carcass of another mammoth was

observed in 1901 near the town of Stredne-Kolymsk, and an expedition under Dr O. Hertz has recently transported the



FIG. 26.—Reindeer incised on wall of Combarelles.

entire animal in sections to Moscow, with the view of mounting it with its skin.

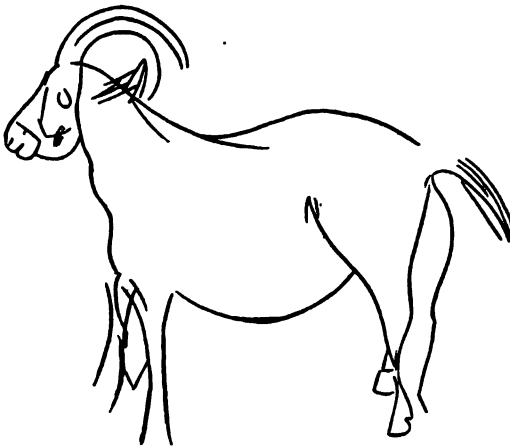


FIG. 27.—Figure of wild goat from the cave of Combarelles.

The total number of engravings in the cave of Combarelles, so far as they could be distinctly made out, is 109:—animals entire but not identified, 19; equidæ, 23; bovidæ, 3; bison, 2;



reindeer, 3; mammoth, 14; heads of goats, 3; heads of antelopes, 4; heads of various animals, chiefly horses, 36; human face, 1 (?); cup-mark, 1. These engravings, in the opinion of the explorers, betray so much artistic skill, precision of details,

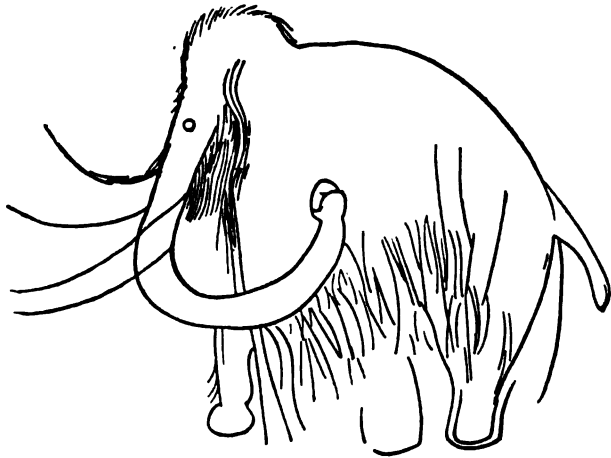


FIG. 28.—Incised figure of mammoth in cave of Combarelles.

Figs. 24 to 28 are reduced from the drawings of MM. Capitan and Breuil.

and knowledge of animal life, that they must be regarded as valuable documents in Palæontology.

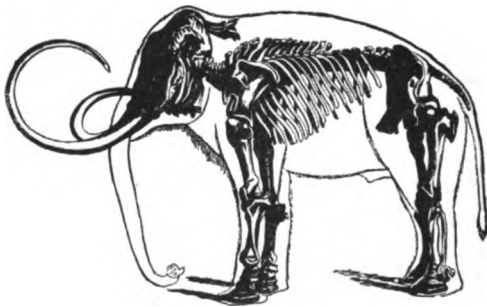


FIG. 29.—Skeleton of the mammoth found in Siberia in 1799, now in St Petersburg.

More recently, MM. Capitan and Breuil published illustrations of some of the painted figures on the walls of the *Grotte de Font-de-Gaume* on two plates, one of which is here reproduced (Pl. XI.) on a smaller scale—(*Revue de l'École d'Anthropologie*, July 1902).

This plate represents an excellent picture of a bison (fig. 1) and a still more striking one of two reindeer (fig. 2). The original drawing of the former is painted in ochre, and measures 1 m. 50 in length and 1 m. 25 in height; that of the latter is 2 m. 10 in length and 1 m. 50 in height, and presents the peculiarity of having portion of the figure on the left executed in incised lines.

The total number of painted figures in this cave is 77:—aurochs, 49; indeterminate animals, 11; reindeer, 4; stag, 1; equidæ, 2; antelopes, 3; mammoth, 2; geometrical ornaments, 3; scalariform signs, 2. The authors suggest that these paintings belong to a later period than the engravings on the walls of Combarelles, founding their opinion on the frequency of the figures of the bison, and the rarity of those of the reindeer and mammoth. Time will not allow me to enlarge on the details of these remarkable rock carvings and paintings, more than to say that MM. Capitan and Breuil have, by their explorations and published reports, greatly added to our knowledge of Palæolithic civilisation.

### III. HUMAN CULTURE AND CIVILISATION IN THE PALEOLITHIC PERIOD.

These illustrations, though only covering a small portion of the available materials, are sufficient to give a general idea of the salient features of the stage of culture to which the inhabitants of Europe had attained towards the close of the Palæolithic period. We have seen that all their works were characterised by a gradual development from simple to more complex forms. Implements, tools and weapons were slowly but surely being made more efficient, thus evincing on the part of their manufacturers a progressive knowledge of mechanical principles. Hence, French anthropologists have arranged these cave-remains in chronological sequence, using the names of the most typical stations to define various stages of culture, as *Moustérien*, *Solutréen*, and *Magdalénien*. The earliest troglodytic station, according to the classification of M. G. de Mortillet, was *le Moustier*, situated on the left bank of the Vézère (Dordogne). During its habitation by man the climate was cold and damp, and among the contemporary

fauna were the mammoth, woolly rhinoceros, cave-bear and musk-ox. The special features of the industrial remains of this period were the scarcity of the *coup de poing*, which is so characteristic of the older river-drift deposits, and the splitting up of flints into smaller implements, such as scrapers, trimmed flakes, etc. The next station in ascending order was the open-air encampment of Solutré (Saone-et-Loire). The stage of civilisation here disclosed was characterised by great perfection in the art of manufacturing flint implements, especially spear and lance-heads, in the form of a laurel leaf (Plate II. No. 12), and by the abundance of horses and reindeer, which were used by the inhabitants as food. The climate was mild and dry, the great glaciers were on the wane, and the rhinoceros seems to have disappeared from the scene. The third and last of the typical stations was the well-known rock-shelter of La Madelaine (Dordogne), characterised by the abundance of objects made of bone and horn, the development of a remarkable artistic talent, the predominance of a northern climate and fauna, and the extinction of the mammoth towards the close of the period.

With regard to the ethnological characteristics of these people little information is to be gained from their artistic productions, as the few engravings and sculptures of the human form hitherto discovered are too rude or fragmentary to be of much value in this respect. That these artist-hunters should have displayed less aptitude in the delineation of their own form and features than of those of the animals hunted, shows how restricted was their conception of human life and of the dignity of man. Evidently the cult of humanity was still in the womb of futurity, and the struggle of life alone was uppermost in their minds. It may be stated, however, that, so far as this line of research leads us, these anthropoid figures represent both sexes as nude and covered with hair, some of them also being, from our point of view, indecent. On the other hand, there can be no doubt, judging from the number of bone needles and pins collected on their inhabited sites, that they wore clothing probably made of skins. Indeed, it would be impossible for human beings who had their origin in a warmer climate to endure with impunity the inclemency of the sub-arctic climate which then obtained in Central Europe without

personal protection of some kind. Our knowledge of their physique and general appearance is, as already mentioned, mainly derived from a comparison of a few of their fossil skeletons with those of modern civilised races. On this phase of the subject we have a considerable amount of evidence to show that since man parted company with the lower animals, there has been a gradual expansion of the cranium, corresponding to an enlargement of certain portions of the organ of thought. All such materials have, however, to be carefully sifted and scrutinised before being admitted as valid assets in a scientific inquiry; and even then, this kind of evidence seldom amounts to more than probability without being corroborated by other discoveries. The subject has grown so much of late that it was impossible in the limits at my disposal to do more than give a few pertinent examples. The race represented by the skulls of Neanderthal and Spy was long anterior to the time of the Palæolithic hunters of the reindeer period, who so greatly distinguished themselves as artists; and as to the Java skull and femur, they are probably the oldest osseous relics of man yet known. The human remains found in the rock-shelter of Cro-Magnon have been for a long time regarded as belonging to, and typical of, the latest Palæolithic people; but as they were merely lying over the *culture-débris*, they are regarded by some archæologists as burials of a more recent date. The fact that the last molars were smaller than the others gives additional support to this view. It does not, however, appear to me that this point is of much consequence, as the amount of superincumbent talus under which the skeletons lay shows that they could not be later than the transition period. Moreover, there are other human remains with regard to which no such doubts have been raised, as, for example, the well-known skulls of Chancelade and Laugerie Basse, both found in the Dordogne district, which show equally advanced cranial characters.

The recent discovery of two skeletons, which Dr Verneau, of Paris, describes as belonging to a new race intermediate between the Neanderthaloid and Cro-Magnon races, marks an important addition to fossil craniology. From the preliminary facts already published, and from what Dr Verneau has told me, anthropologists may look forward with high expectation to the full report of these

and other discoveries in the Mentone caves, which is now being prepared under the direction of the Prince of Monaco. We have already seen that in the same cave, and only 0·70 metre (27½ inches) above the site of the two skeletons just referred to, another skeleton of the Cro-Magnon type has been discovered, thus bringing two different races almost on the same chronological horizon. But this by no means discredits Dr Verneau's theory, as it is not at all unlikely that, while a higher race was being developed, some individuals of lower but vanishing races still survived in Europe. Indeed, the point is no longer a matter of conjecture, as recently two skulls of a distinct negroid type have been found among Neolithic remains in Brittany.\* The skull of the 'old man of Cro-Magnon' is large and well-proportioned, both posteriorly and anteriorly, thus indicating a great stride in the development of mental capacity, but perhaps not more than might be expected of a people who displayed such artistic feeling and mechanical skill as the authors of the art gallery of the reindeer period. But how radically their aims, hopes, aspirations, and manner of life differed from those of their Neolithic successors we shall immediately be in a position to realise.

It would appear from these combined sources of investigation that the earliest Palæolithic people of Europe entered the country from Africa, at a time when there was easy communication between these continents by several land bridges across the present basin of the Mediterranean. At that time man's mental predominance over other animals was not so conspicuous as it now is, as shown by the fact that his mechanical ingenuity was only adequate to the production of one typical implement—the *coup de poing*. Implements of this kind are chiefly found in the stranded gravels of former rivers, and, from their wide distribution in the Old World, they must have been then regarded as the *ne plus ultra* of human craftsmanship. Their original owners are supposed to have inhabited the wooded banks of these rivers, wandering about in isolated family groups till the advent of the glacial period roused their dormant energies. It is difficult to realise how much the severe climatal conditions which then prevailed in Europe contributed to the perfection of human attributes, and consequently

\* *Bull. de la Société d'Anthropologie de Paris*, series v., vol. iv. p. 432.

to the progress of civilisation. The beneficial effect of this uncongenial environment on these early pioneers of humanity was to stimulate their natural capabilities of improvement—for the adage that necessity is the mother of invention was as applicable then as now. Entering Europe as naked, houseless nomads, living on wild fruits and the smaller fauna of a sub-tropical climate, they were ultimately forced by the severity of the climate to take refuge in caves and rock-shelters and to cover their bodies with skins. The natural food productions of a warm climate gradually disappeared, until finally there was little left but fierce animals, such as the mammoth, reindeer, chamois, horse, bison, etc., which came from northern regions into Central Europe. To procure the necessary food and clothing in these circumstances greatly taxed the skill and resources of the inhabitants. But this difficulty they ultimately solved by the manufacture of special weapons of the chase, with which they successfully attacked the larger wild animals which then occupied the country. The *coup de poing*, which for a long time served all the purposes of primitive life, gradually gave place to spear- and lance-heads fixed on long handles, together with a great variety of minor weapons and tools made of stone, bone, horn and wood. When the Palæolithic people finally emerged from this singular contest with the forces of nature, they were physically and mentally better than ever equipped for the exigencies of life. A greater power of physical endurance, improved reasoning faculties, an assortment of tools adapted for all kinds of mechanical work, and some experience of the advantage of housing and clothing, may be mentioned among the trophies which they carried away from that long and uphill struggle.

The civilisation thus developed represents the outcome of a system of human economy founded on the free play of natural laws, and little affected by the principles of religion or ethics—subjects which were as yet in their embryonic stage. The mysteries of the supernatural had not then been formulated into the concrete ideas of gods or demons. The notions of good and evil, right and wrong, were still dominated by the cosmic law that might is right. Neither gloomy forebodings nor qualms of conscience had much influence on the actions

of these people. Their philosophical and sentimental speculations, if they had any, centred exclusively on the habits of the animals they hunted, and on the strategic means by which they could be waylaid and captured. During this time they made great progress in the development of mechanical appliances, as shown by the number of flint implements—saws, borers, scrapers, etc.—with which they manufactured needles, pins, ornaments, weapons and other objects, including the so-called *bâtons de commandement*. Upon the whole, it would appear as if their minds were engrossed with the chase and its exciting scenes and incidents, for their domestic economy indicated little more than the art of broiling the flesh of the captured animals and converting their skins into garments. Possibly some round pebbles abundantly found in the *débris* might have been used as ‘pot-boilers,’ but a few stone mortars (Pl. II. No. 14), which occasionally turned up, would seem to have been used only for mixing colouring matter to paint their bodies, as some modern savages do. Of agriculture, the rearing of domestic animals, the arts of spinning and weaving, and the manufacture of pottery, they appear to have been absolutely ignorant. But yet, in an environment of such primitive resources and limited culture associations, these wild hunters developed a genuine taste for art, and cultivated its principles so effectually that they have bequeathed to us an art gallery of over 400 pieces of sculpture and engraving so true to their models that many of them bear a favourable comparison with analogous works of the present day. They adorned their persons with perforated teeth, shells, coloured pebbles, and pendants of various kinds. They depicted the animals with which they were familiar, especially those they hunted for food, in all their various moods and attitudes, often with startling fidelity. Harpoons, spears and daggers of horn and bone were skilfully engraved, and sometimes the handles of the last were sculptured into the conventional form of one or other of their favourite animals. (See Pls. III. to X.)

They also in some instances adorned the walls of the caverns they frequented with incised outlines of the neighbouring fauna (figs. 22–28), and made actual colour paintings of them in black and ochre, or in one of these colours (Pl. XI.). The discovery

of so many art specimens is of considerable importance among the more notable facts disclosed by these anthropological researches, as it proves that the origin of the artistic faculty was independent of, and prior to, the evolution of religion, ethics, politics, commerce, and other elements of which our modern civilisation is built up.

The other characteristic feature in the lives of these people was, that they lived exclusively on the produce of the chase, for, without agricultural and pastoral avocations, what else could they do but organise daily hunting or fishing expeditions? To capture the big game of the district was a formidable task, requiring not only great strength and agility of person and limb, but also strong and well-made weapons. During the later stages of the Palæolithic civilisation their principal prey consisted of reindeer and horses, both of which animals then roamed in large herds throughout Western Europe, thus rendering themselves more liable to be ambushed, trapped or speared by their wily enemies. It is not likely that they would take the initiative in attacking the hyæna, lion, or cave-bear, except in self-defence. That, however, these formidable creatures were occasionally captured by them is suggested by the fact that their canine teeth were highly prized as personal ornaments, or as a memento of their prowess in the chase. The weapons used by these hunters were harpoons, generally made of reindeer-horn, spear- and lance-heads of flint, and short daggers of bone or horn. Before these weapons were invented it is difficult to imagine that any member of the genus *Homo* would have the courage to attack such a formidable animal as the mammoth armed only with a *coup de poing*, but yet there are facts which suggest that such was the case.

When the physical conditions which called these accomplishments into existence passed away, and the peculiar fauna of the glacial period disappeared from the lowlands of Central Europe—some by extinction, and others by emigration to more northern regions or to the elevated mountains in the neighbourhood—we find the inhabitants of these old hunting grounds in possession of new and altogether different sources of food. Finding the former supplies becoming so limited and precarious that it was



no longer possible to live a roaming life, now gathering fruits and seeds, and now hunting wild animals, they fell somehow into the way of cultivating special plants and cereals, and rearing certain animals in a state of domestication. Whether this new departure was a product of the intelligence of the descendants of the Palæolithic people of Europe, or derived from new immigrants into the country, is a debatable question. At any rate, the expedient was eminently successful. It was in reality the starting-point of Neolithic civilisation, and henceforth there was a rapid increase in the population. They cultivated a variety of fruits, wheat, barley and other cereals; they reared oxen, sheep, goats, pigs, horses and dogs; they became skilled in the ceramic art, and in the manufacture of cloth by spinning and weaving wool and fibrous textures; they ground stone implements so as to give them a sharp cutting edge; in hunting the forest fauna of the period they used, in addition to spears, lances and daggers, the bow and arrow; they built houses, both for the living and the dead—thus showing that religiosity had become an active and governing principle among them. But of the artistic taste and skill of their predecessors they had scarcely a vestige, and whatever they did by way of ornament consisted mainly of a few scratches, arranged in some simple geometrical pattern. The fundamental principles of the two civilisations are really so divergent that the Neolithic can hardly be regarded as a local development of the latest phase of that of the Palæolithic period in Europe. The probability is that, while the isolated colonies of reindeer hunters were still in existence, people of the same stock were elsewhere passing through the evolutionary stages which connected the two civilisations together.

The far-reaching consequence of securing food supplies by means of agriculture and the domestication of animals led to more sedentary and social habits. The existence of large communities concurrent with the development of various trades and professions was but a matter of time, the outcome of which is now a vast system of international commerce. Already the greater portion of the earth capable of being cultivated is converted into gardens and fields, whose choice productions are readily conveyed to all the large cities of the globe. Flesh diet is abundant, but it is no longer

necessary to hunt the animals in primeval forests. Skin-coats, dug-outs and stone weapons are now lineally represented by woven fabrics, Atlantic liners and Long Toms.

Were it possible for one of our Palæolithic ancestors to sit in judgment on the comparative merits of the two civilisations, I fancy his verdict would be something like the following: "You have utilised the forces of nature to a marvellous extent, and thereby greatly increased the means of subsistence to your fellow-creatures; but, at the same time, you have facilitated the physical degeneracy of your race by multiplying the sources of human disease and misery. The invention of money has facilitated the accumulation and transmission of riches to a few; but it has impoverished the many, and supplied incentives to fraud, theft, and all manner of crime. Patriarchal establishments have given place to social organisations, governed by laws founded on moral sentiments and ethics; but their by-products are extreme luxury and extreme poverty. Hence, to support the weak and the unfortunate is no longer a matter of charity, but a legal and moral obligation. Notwithstanding the size of your asylums, hospitals and almshouses, they are always full and always on the increase. Your legislators are selected by the voice of the majority: what if that majority be steeped in superstition, prejudice and ignorance? You have formulated various systems of religion, but whether founded on the principles of fetichism, polytheism or monotheism, they are still more or less permeated with contradictory or controverted creeds and dogmas. Natural sport, as practised with weapons of modern precision, can only be characterised as legalised killing of helpless creatures. To shoot pigeons suddenly liberated from a box at a measured distance, or overfed pheasants, even after they have managed to take wing, or semi-domesticated deer, especially when driven to the muzzle of a rifle—all, of course, within sight of a luncheon basket—is a poor substitute for the excitement and field incidents of the chase in Palæolithic times. With no better weapons than a spear, or lance tipped with a pointed flint, and a small dagger of bone or horn, we had, not infrequently, to encounter in mortal combat the mammoth, rhinoceros, cave-bear, or some other fierce and hungry animal, which, like ourselves, was prowling in quest of a morning meal. Such

scenes had many of the elements of true sport, and being essential to our existence, were of daily occurrence. Moreover, from the standpoint of modern ethics, our method put the combatants on something like a footing of equality, or at least gave our prey a fair chance of escape. We cultivated physical and manly qualities by the natural exercise of the senses, and personal prowess was the distinguishing prerogative of our heroes. Thus we acquired the experience, skill, strength, agility and courage of practised athletes—qualities which left no room for cowardice. With us 'brain power' passed almost directly from the generator to the muscles of the administrator; with you it has to pass through a complicated system of accumulators and distributors, liable to various degrees of leakage, and it is this leakage which often sucks dry the life-blood of your civilisation. Finally, the permanence of your civilisation remains to be tested by the touchstone of time. For civilisations, like the genera and species of the organic world, have their life-histories determined by laws as fixed and definite as those that govern the resultant of the parallelogram of forces. To cosmic evolution, under which our race and civilisation to a large extent flourished, you have superadded altruism, which means the survival of the weak as well as of the strong. But altruism will continue to be a living force among civilised communities only so long as present and prospective food supplies hold out. For, after all, the essential problem of your social existence is to procure food for an ever-increasing population. Whenever these necessities of life become inadequate to meet the demands of the inhabitants of this globe, then your boasted civilisation comes to the end of its tether, and the only solution of the crisis will be to reduce your numbers by a recurrence—*saue qui peut*—to the cosmic law of 'the survival of the fittest.'

---

#### DESCRIPTION OF PLATES.

I. A flint implement in the British Museum found, with a skeleton of an elephant, near Gray's Inn Lane, London, about the close of the seventeenth century. Reproduced from plate i. of Guide to the Antiquities of the Stone Age in the British Museum.

II. Specimens of flint tools illustrating the progressive skill of the Palæolithic cavemen of France, chiefly from the Lartet and Christy Collection, now

in London and Paris. Nos. 1-7, 9-11, 18 and 19 represent saws, borers, scrapers, etc. from the later stations. Nos. 12 and 16 are illustrations of the laurel-leaf-shaped lance-heads commonly described as belonging to the *Solutrén* period. The former was found at Laugerie Basse (Col. Massénat-Girod), and the latter (made of agate) in the Grotte de l'Église (Dordogne). Nos. 8, 15, 17 and 21 are specimens of the earlier implements from Le Moustier, and are all trimmed flakes, with the exception of 17, which is a small *coup de poing*. No. 13 represents a core from Les Eyzies, showing on the left a small portion of the original surface of the flint, and No. 20 a well-made flake from La Madelaine. A small mortar made out of a waterworn pebble from Les Eyzies is shown under figure 14; others like it have been recorded from La Madelaine, Laugerie Basse, Bruniquel, and probably elsewhere.

III. Weapons and ornaments made of bone, teeth, deer-horn, ivory and shells. Nos. 1-14, 15, 17-19 (ivory), 20, 25 (ox), 26 (fox), 27 and 28 are from La Madelaine (Col. L. and C.). Nos. 5-14 are from Laugerie Basse (Col. Massénat-Girod). Nos. 24 and 29, representing a supposed whistle and a sculptured dagger, are from Laugerie Basse (Col. L. and C.). No. 16 is a thin plaque carved of bone, probably an ornamental pendant, found at Bruniquel (British Museum). Nos. 21-23 are from Kent's Cavern. The precise use of the pointed objects figured under Nos. 12-14, 28 and 30 is not known, but it is probable that they were the tips of small lances propelled by means of such an implement as is figured under No. 8, Plate IV. The small harpoon (No. 27) might have been used as an arrow-point, but we have no evidence that bows and arrows were then in use.

IV. On this Plate there is a collection of objects from various stations illustrating the art of the Palæolithic people. No. 1 shows a portion of reindeer-horn with a rude representation of a prone man, apparently in the act of throwing a spear at a male auroch. The hands are imperfectly represented, the body is covered with hair, and a cord, possibly attached to the head of a harpoon, falls behind the legs. This specimen was found at Laugerie Basse (Col. Massénat-Girod). Nos. 2 and 14 represent portions of darts with badly-executed human hands, showing only four fingers. Nos. 3, 4 and 5 are from La Madelaine (Col. L. and C.). One (3) represents a piece of stag's horn (*bâton de commandement*), having a stag with complex antlers incised on it. Another (4) is a plate of the canon bone of a reindeer with incised figures of bovine animals. The third represents a truncated dart ornamented with flowers, and what looks like the outstretched skin of a fox. No. 6 is from Les Eyzies, and shows a ruminant having a spear entering its breast (*ibid.*). A portion of a bevelled dart-head from Laugerie Basse, with a sequence of half-fledged birds, is shown by No. 7 (*ibid.*). No. 8 represents a dart-propeller from Laugerie Basse, ornamented with a horse's head and an elongated forepart of a deer (*ibid.*). Nos. 9, 10 and 15 are also from Laugerie Basse (Col. Massénat-Girod), and represent the well-extended antlers of a reindeer (9), an otter eating a salmon (10), and a hare (15), sculptured in ivory. No. 11, unmistakably showing the hind portion of a pig, is from the Kesslerloch, Switzerland (after Conrad Merk). On the canine of a bear (No. 12) from Duruthy Cave a seal is engraved (*Reliquiæ Aquitanicæ*, p. 223). The palm of the brow antler of a reindeer is incised with the figure of some kind of horned animal (No. 13), probably intended for an ibex.

V. This Plate shows a famous relic in the form of a piece of ivory from the outside layer of the tusk, having incised on it the outline of a hairy elephant (Col. L. and C.). The lofty skull and hollow forehead of the animal here represented are characteristic of the Siberian mammoth, as shown by its skeleton (fig. 29). On comparing it also with the figure of the mammoth incised on the wall of Combarelles (fig. 28), one cannot fail to be struck with the striking resemblance between them.

VI. Portion of a reindeer-horn (*bâton de commandement*), having salmon engraved on one side and eels on the other.

VII. Two *bâtons de commandement* from La Madelaine, one showing a human figure with an upraised club, as if going to strike a horse in front of him, while a serpent (!) seems to be in the act of biting his heel; the other shows four large-headed ponies in sequence (Col. L. and C.).

VIII. Figures of a reindeer, horse, and three ornaments from the Kesaleroch Cave, near Schaffhausen. The two former are among the *chef-d'œuvres* of Palæolithic art. Of the hanging ornaments two are made of shale. All the figures are after Conrad Merk.

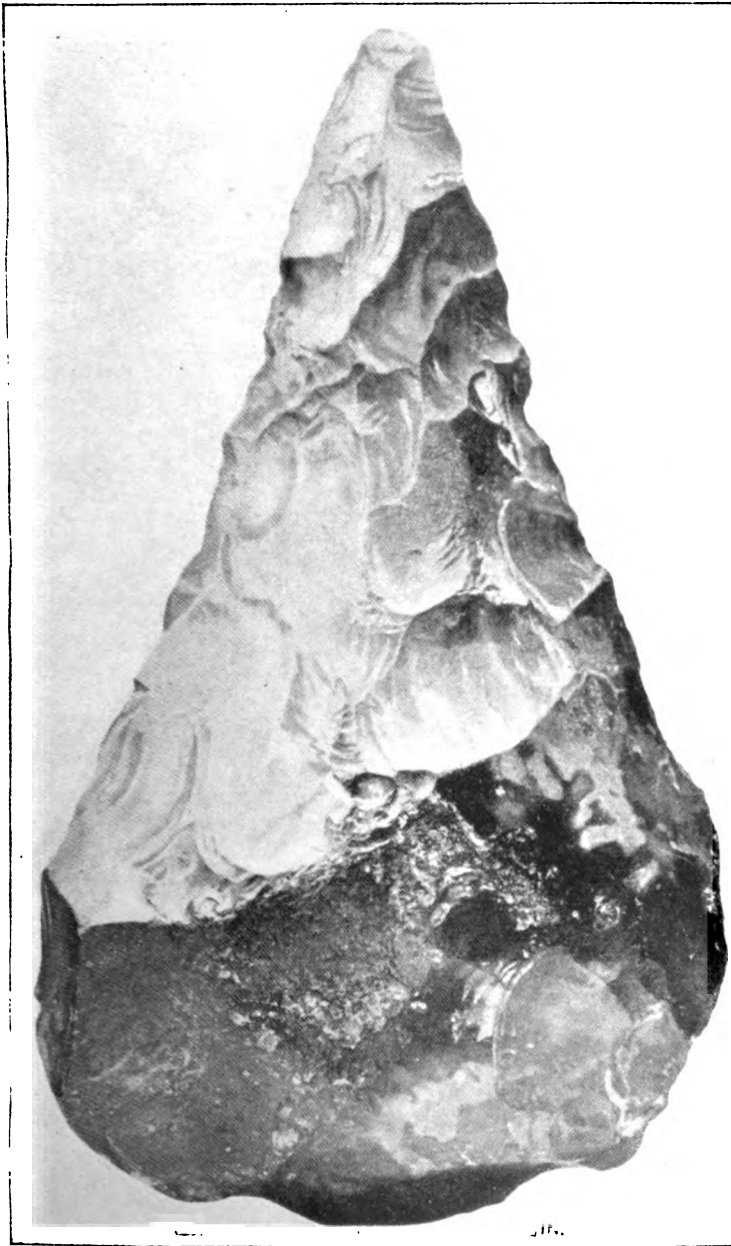
IX. Two carved handles of daggers like the complete specimen from Laugerie Basse figured on Plate III. No. 29. The reindeer is carved in ivory and the mammoth in reindeer-horn. These interesting relics, as well as a third handle of the same kind, are from the rock-shelter of Bruniquel, and are now among the antiquarian treasures of the British Museum. The highly conventional manner in which the artist has adapted horns, tusks and trunk to serve his purpose, shows power of imagination and a facility of execution which even now could only be acquired by long experience. Figure 3 represents some fantastic animal with large mouth and no teeth. It comes from Laugerie Basse (Col. Massénat-Girod).

X. One of the sculptured horse-heads here represented is most remarkable, as the original seems to have been partially skinned. M. Piette, writing in 1889 (*Congrès International, etc.*, Paris, p. 159), makes the following statement:—"L'homme a toujours en l'amour du beau. . . . Pour se perfectionner dans l'art de représenter le vivant, les artistes du Mas d'Azil sculptaient l'écorché et le squelette." Also M. Cartailhac (*La France Préhistorique*, p. 70) thus notices the above piece of sculpture:—"Le relief de la tête en partie décharnée est tout à fait étonnant. Une tête isolée, de la même grotte, est également figurée sans le peau. De tels ouvrages donnent à l'art de l'âge du renne un aspect inattendu. Les découvertes récentes nous ont appris que cet art connaît la fantaisie."

XI. Bison and two reindeer painted in ochre on the walls of the *Grotte de Font-de-Gaume*, reduced from illustrations by MM. Capitan and Breuil (*Revue de l'École d'Anthropologie*, July 1902, pl. ii.).

(Issued separately February 13, 1904.)

PLATE I.

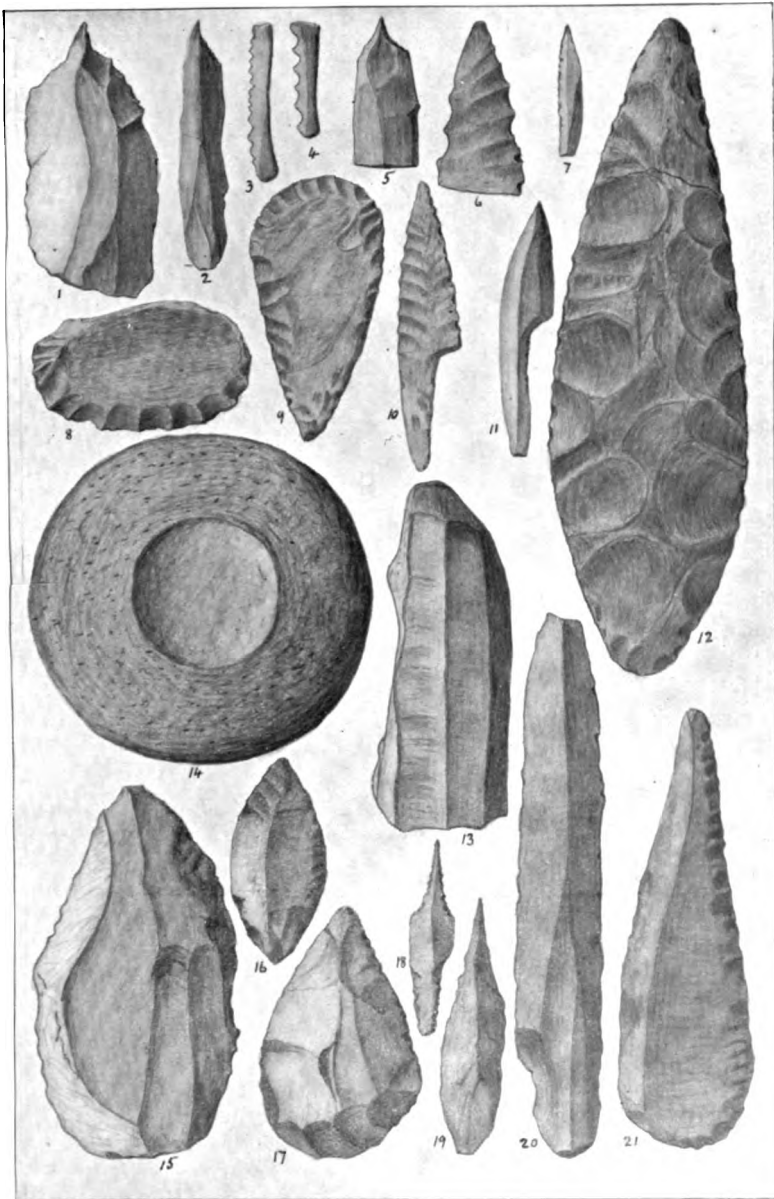


Flint implement—'coup de poing'—from river-drift gravels (†).

DR MUNRO.



PLATE II.



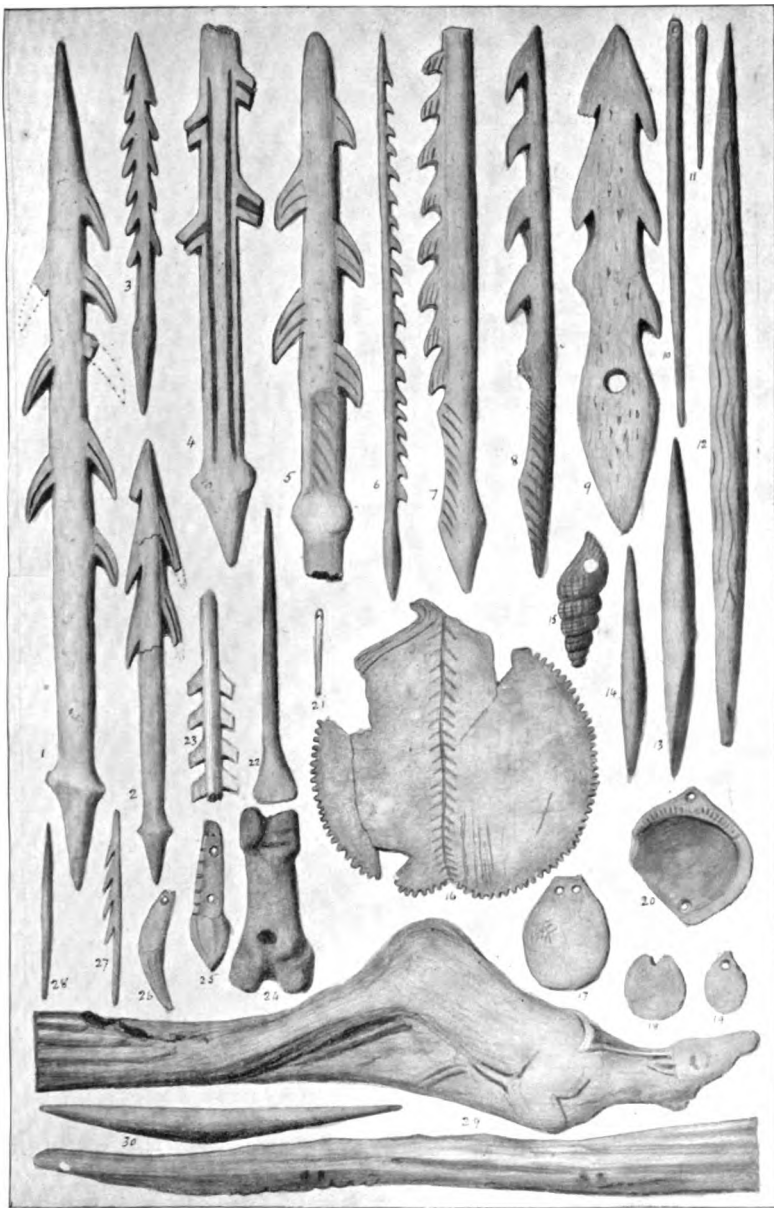
Objects illustrating flint industry among the Cavemen of France ( $\frac{1}{2}$ ).

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PLATE III.

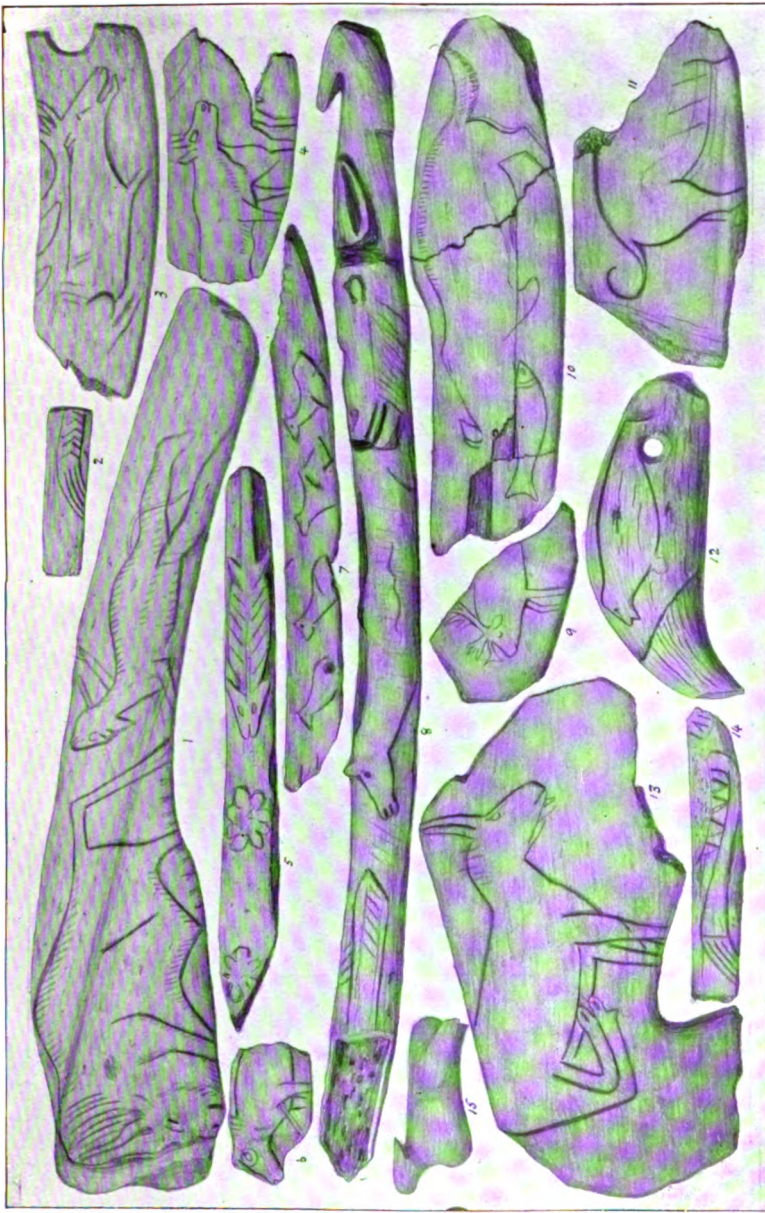


Weapons and ornaments made of bone, teeth, deer-horn, ivory and shells ( $\frac{1}{2}$ ).

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PLATE IV.

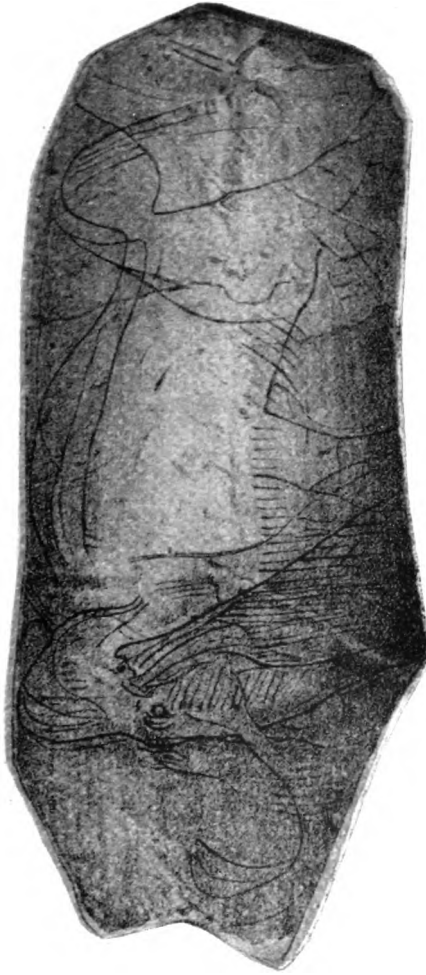


Some illustrations of the art remains of the Paleolithic hunters (4).

Dr. MUNRO.



PLATE V.

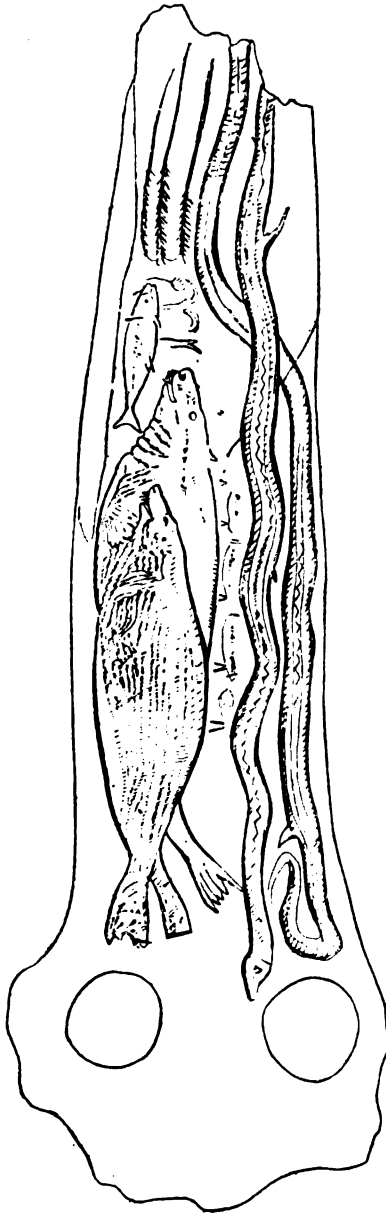
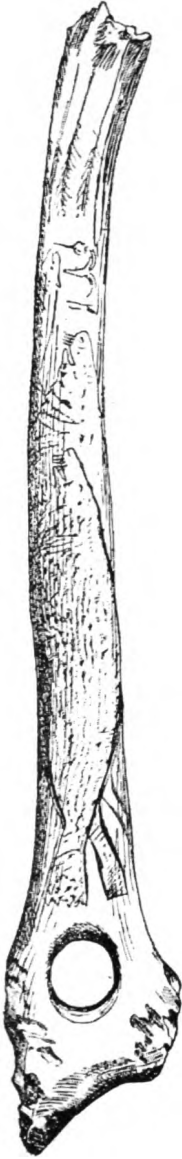


Mammoth engraved on a piece of ivory, La Madeleine ( $\frac{1}{4}$ ). (E. Lartet.)

DR. MUSEO.



PLATE VI.



*Bâton de Montgaudier.* (Collection Pognon.)

DR MUNRO.





PLATE VII.



FIG. 1.—Human figure, horses and serpent (1) (1/2).



FIG. 2.—Horses in sequence (1/2).  
Two bâtons de commandement from La Madelaine.

DR. MURRO.



PLATE VIII.



FIG. 1.—Reindeer on a portion of reindeer-horn (†)



FIG. 2.—Drawing of a horse on portion of reindeer-horn (†).



FIGS. 3, 4, 5.—A perforated shell and hanging ornaments made of coal (†).

Engraved figures of animals and ornaments from the Kesslerloch Cave, near Schaffhausen. (After Conrad Merk.)

DR MUNRO,



PLATE IX.

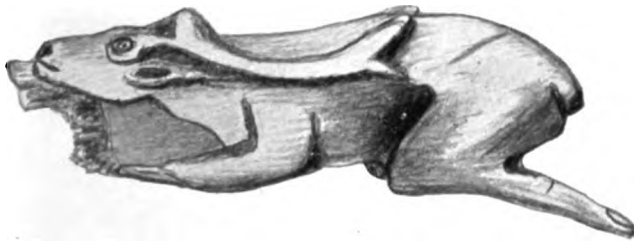


FIG. 1.—Handle of a dagger sculptured into the form of a reindeer.  
Rock-shelter of Bruniquel (§).

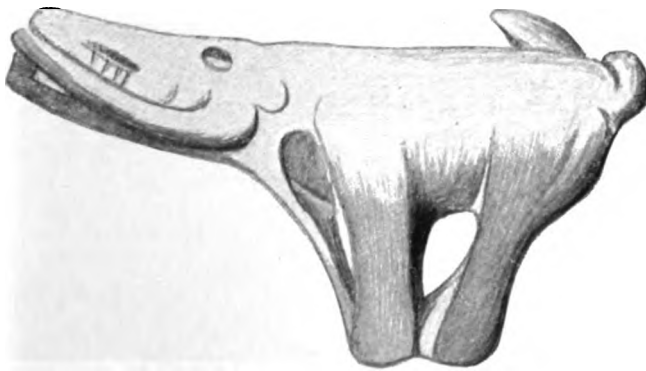


FIG. 2.—Mammoth sculptured in reindeer-horn. Rock-shelter of Bruniquel (§).

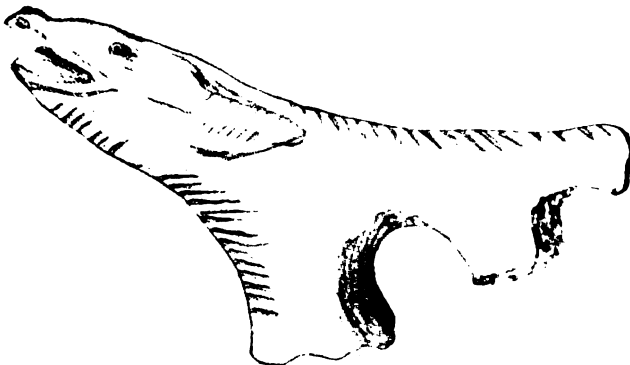


FIG. 3.—Unknown animal sculptured in reindeer-horn. Laugerie Basse (†).

Animals sculptured in ivory and horn.

DR MUSRO.



PLATE X.



Portion of reindeer-horn from Mas d'Azil, sculptured into two horse-heads  
(Col. Piette). After E. Cartailhac—*La France Préhistorique*.

DR MUNRO.





PLATE XI.

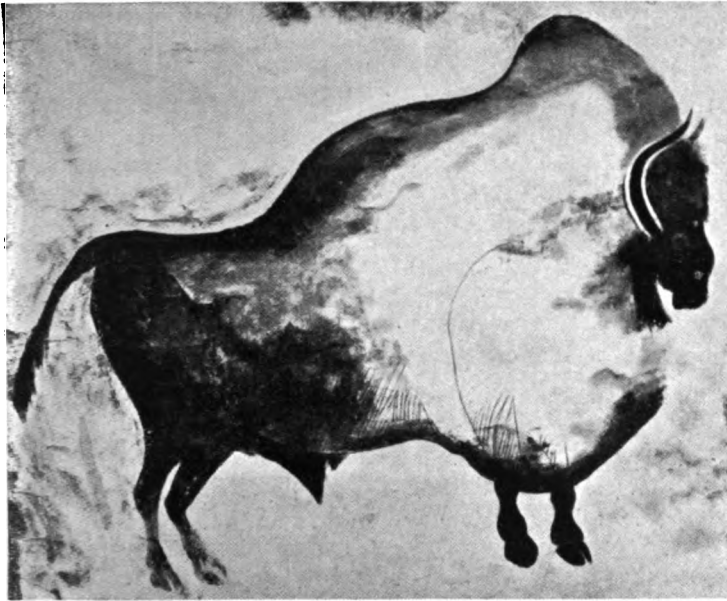


FIG. 1.—Bison painted in ochre.

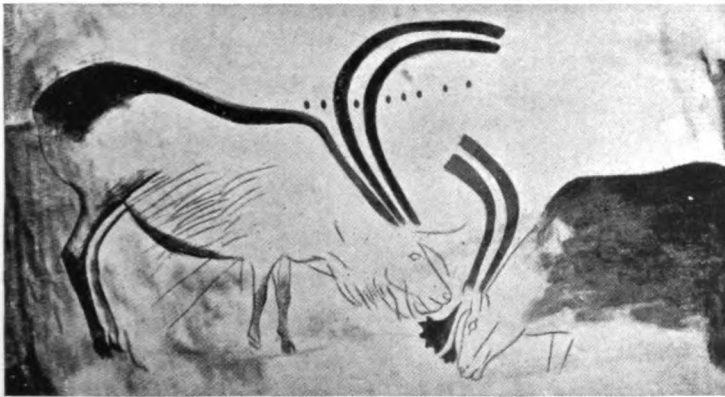


FIG. 2.—Reindeer partly painted and partly incised.

Specimens of painted animals from the Cave of Font-de Gaume, after MM.  
Capitan and Breuil.

DR MCFR0.



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**SESSION 1903-4.**

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*[Continued on page iii of Cover.]*

**The Theory of Continuants in the Historical Order of its Development up to 1870. By Thomas Muir, LL.D.**

(MS. received October 5, 1908. Read November 2, 1908.)

The more or less disguised use of continued fractions has been traced back to the publication of Bombelli's *Algebra* in 1572, eighty-four years, that is to say, before the publication of Wallis' *Arithmetica Infinitorum*, in which Brouncker's discovery was announced and the fractions explicitly expressed.\* The study of the numerators and denominators of the convergents viewed as functions of the partial denominators was first seriously undertaken by Euler in his *Specimen Algorithmi Singularis* of the year 1764, in which denoting by

$$(a), \frac{(a, b)}{(b)}, \frac{(a, b, c)}{(b, c)}, \dots$$

the convergents to

$$a + \frac{1}{b} + \frac{1}{c} + \dots$$

he established a long series of identities, such as

$$\begin{aligned} (a, b, c, d, \dots) &= a(b, c, d, \dots) + (c, d, \dots) \\ (a, b, c, \dots, l) &= (l, \dots, c, b, a), \\ (a, b)(b, c) - (b)(a, b, c) &= 1, \\ (a, b, c)(d, e, f) - (a, b, c, d, e, f) &= -(a, b)(e, f), \\ \dots \end{aligned}$$

The study was pursued by Hindenburg and his followers during the last twenty years of the eighteenth century, but not with any great profit; and, although in the first half of the nineteenth century considerable attention was given to the theory of continued fractions as a whole, little advance was made in elucidating

\* For the early history see Favaro's *Notizie storiche sulle frazioni continue dal secolo decimoterzo al decimosettimo* published in vol. vii. of Boncompagni's *Bollettino*; and as regards Bombelli see a paper by G. Wertheim in the *Abhandl. zur Gesch. d. Math.*, viii. pp. 147-160.



the properties of the functions referred to.\* Their connection with determinants, after the awakening of interest in the latter about 1841, was sure sooner or later to be detected: there is no evidence, however, of the discovery having been made before the year 1853.

SYLVESTER, J. J. (1853, May 13).

[On a remarkable modification of Sturm's theorem. *Philos. Mag.* (4), v. pp. 446-457.]

The mention of Sturm's theorem in the title of a paper renders not improbable the occurrence therein of matter connected with continued fractions. Especially likely is this in the case of a writer like Sylvester when in a characteristic mood; and, assuredly, the present communication is in structure, style, and originality redolent of its author. It must have been written in the white heat of discovery. The main part of it consists of six pages: this is followed by a "Remark" a page and a quarter long; then comes a "Postscript" of three and a half pages; and finally a small-page footnote as long as the "Remark."

It is the postscript which particularly concerns us. It begins thus:—

"Suppose that we have any series of terms,  $u_1, u_2, u_3, \dots, u_n$ , where

$$u_1 = A_1, \quad u_2 = A_1 A_2 - 1, \quad u_3 = A_1 A_2 A_3 - A_1 - A_3, \quad \dots$$

and in general

$$u_i = A_i u_{i-1} - u_{i-2},$$

then  $u_1, u_2, u_3, \dots, u_n$  will be the successive principal coaxial determinants of a symmetrical matrix. Thus suppose  $n = 5$ ; if we write down the matrix

$$\begin{array}{cccccc} A_1 & 1 & 0 & 0 & 0 \\ 1 & A_2 & 1 & 0 & 0 \\ 0 & 1 & A_3 & 1 & 0 \\ 0 & 0 & 1 & A_4 & 1 \\ 0 & 0 & 0 & 1 & A_5 \end{array}$$

\* The state of the theory in 1833 can best be gathered from Stern's monograph published in vol. x. of *Crelle's Journal*.

(the mode of formation of which is self-apparent), these successive coaxal determinants will be

$$1, A_1, \begin{vmatrix} A_1 & 1 \\ 1 & A_2 \end{vmatrix}, \begin{vmatrix} A_1 & 1 & 0 \\ 1 & A_2 & 1 \\ 0 & 1 & A_3 \end{vmatrix}, \begin{vmatrix} A_1 & 1 & 0 & 0 \\ 1 & A_2 & 1 & 0 \\ 0 & 1 & A_3 & 1 \\ 0 & 0 & 1 & A_4 \end{vmatrix}, \text{ etc.,}$$

i.e.

$$\begin{aligned} &1, A_1, A_1A_2 - 1, A_1A_2A_3 - A_1 - A_3, \\ &A_1A_2A_3A_4 - A_1A_2 - A_1A_4 - A_3A_4 + 1, \\ &A_1A_2A_3A_4A_5 - A_1A_2A_5 - A_1A_4A_5 - A_3A_4A_5 - A_1A_2A_3 \\ &\qquad\qquad\qquad + A_5 + A_3 + A_1. \end{aligned}$$

It is proper to introduce the unit because it is, in fact, the value of a determinant of zero places, as I have observed elsewhere."

After using this as an aid to prove his proposition regarding Sturm's theorem, he returns to his new determinant in the following words:—

"I may conclude with noticing that the determinative [determinantal?] form of exhibiting the successive convergents to an improper continued fraction affords an instantaneous demonstration of the equation which connects any two consecutive such convergents as

$$\frac{N_{i-1}}{D_{i-1}} \quad \text{and} \quad \frac{N_i}{D_i} \quad \text{viz.} \quad N_i D_{i-1} - N_{i-1} D_i = 1.$$

For if we construct the matrix which for greater simplicity I limit to five lines and columns,

A	1	0	0	0
1	B	1	0	0
0	1	C	1	0
0	0	1	D	1
0	0	0	1	E

and represent umbrally as

$$\begin{matrix} a_1 & a_2 & a_3 & a_4 & a_5 \\ b_1 & b_2 & b_3 & b_4 & b_5; \end{matrix}$$

and if, by way of example, we take the fourth and fifth convergents, these will be in the umbral notation represented by

$$\frac{\begin{matrix} a_2 & a_3 & a_4 \\ b_2 & b_3 & b_4 \end{matrix}}{\begin{matrix} a_1 & a_2 & a_3 & a_4 \\ b_1 & b_2 & b_3 & b_4 \end{matrix}} \quad \text{and} \quad \frac{\begin{matrix} a_2 & a_3 & a_4 & a_5 \\ h_2 & b_3 & b_4 & b_5 \end{matrix}}{\begin{matrix} a_1 & a_2 & a_3 & a_4 & a_5 \\ b_1 & h_2 & b_3 & b_4 & b_5 \end{matrix}}$$

respectively. Hence

$$\begin{aligned} & N_3 D_4 - N_4 D_5 \\ = & \frac{a_2 a_3 a_4 a_5}{b_2 b_3 b_4 b_5} \times \frac{a_2 a_3 a_4 a_1}{b_2 b_3 b_4 b_1} - \frac{a_2 a_3 a_4}{b_2 b_3 b_4} \times \frac{a_2 a_3 a_4 a_5 a_1}{b_2 b_3 b_4 b_5 b_1}, \\ = & \frac{a_2 a_3 a_4 a_5}{b_2 b_3 b_4 b_5} \times \frac{a_2 a_3 a_4 a_1}{b_2 b_3 b_4 b_1} - \overline{\frac{a_2 a_3 a_4 a_5}{b_2 b_3 b_4 b_5}} \overline{\frac{a_2 a_3 a_4 a_1}{b_2 b_3 b_4 b_1}}, \\ = & \frac{a_2 a_3 a_4 a_5}{b_2 b_3 b_4 b_1} \times \frac{a_2 a_3 a_4 a_1}{b_2 b_3 b_4 b_5}, \\ = & \frac{a_2 a_3 a_4 a_5}{b_1 b_2 b_3 b_4} \times \frac{a_1 a_2 a_3 a_4}{b_2 b_3 b_4 b_5}, \\ = & \begin{matrix} 1 & B & 1 & 0 & 1 & 0 & 0 & 0 \\ 0 & 1 & C & 1 & B & 1 & 0 & 0 \\ 0 & 0 & 1 & D & 1 & C & 1 & 0 \\ 0 & 0 & 0 & 1 & 0 & 1 & D & 1, \end{matrix} \\ = & 1 \times 1 = 1, \end{aligned}$$

as was to be proved. And the demonstration is evidently general in its nature."

In regard to this there has to be noted, first, the use of

$$\frac{\begin{matrix} a_2 & a_3 & a_4 \\ b_2 & b_3 & b_4 \end{matrix}}$$

when it would have been equally effective to use

$$\begin{matrix} 2 & 3 & 4 \\ 2 & 3 & 4; \end{matrix}$$

and, second, the use of a theorem for expressing the product of a five-line determinant and one of its secondary minors as an aggregate of products of pairs of four-line determinants.

Following on this comes the assertion that

"We may treat a proper continued fraction [*i.e.* with positive unit numerators] in precisely the same manner, substituting throughout  $\sqrt{-1}$  in place of 1 in the generating matrix,

and we shall thus, by the same process as has been applied to improper continued fractions, obtain

$$N_{i+1}D_i - N_iD_{i+1} = (\sqrt{-1})^i \times (\sqrt{-1})^i = (-1)^i.$$

This would seem to imply that as yet Sylvester had not observed that an alternative mode of representation was obtainable by merely changing the sign of the units on one side of the diagonal.

The footnote contains two additional observations, the first being to the effect that the new mode of representation

“gives an immediate and visible proof of the simple and elegant rule for forming any such numerators or denominators by means of the principal terms [term ?] in each ; the rule, I mean, according to which the  $i^{\text{th}}$  denominator may be formed from

$$q_1q_2q_3q_4 \dots q_i$$

( $q_1, q_2, \dots, q_i$  being the successive quotients) and the  $i^{\text{th}}$  numerator from

$$q_2q_3q_4 \dots q_i$$

by leaving out from the above products respectively any pair or any number of pairs of consecutive quotients as  $q_pq_{p+1}$ .

For instance, from  $q_1q_2q_3q_4q_5$  by leaving out  $q_1q_2, q_2q_3, q_3q_4$  and  $q_4q_5$  we obtain

$$q_3q_4q_5 + q_1q_4q_5 + q_1q_2q_5 + q_1q_2q_3;$$

and by leaving out  $q_1q_2q_3q_4, q_1q_2q_4q_5, q_2q_3q_4q_5$  we obtain

$$q_5 + q_3 + q_1;$$

so that the total denominator becomes

$$q_1q_2q_3q_4q_5 + q_3q_4q_5 + q_1q_4q_5 + q_1q_2q_5 + q_1q_2q_3 + q_5 + q_3 + q_1;$$

and in like manner the numerator of the same convergent is

$$q_2q_3q_4q_5 \left\{ 1 + \frac{1}{q_2q_3} + \frac{1}{q_3q_4} + \frac{1}{q_4q_5} + \frac{1}{q_2q_3q_4q_5} \right\}.$$

i.e.

$$q_2q_3q_4q_5 + q_4q_5 + q_2q_5 + q_2q_3 + 1."$$

The “rule” here spoken of is that enunciated for the more general case of

$$a_1 + \frac{b_1}{a_2} + \frac{b_2}{a_3} + \dots$$

in Stern's *Theorie der Kettenbrüche*, the fourth section of which is given up to the consideration of such rules (*Crelle's Journ.*, x. pp. 4-7).

The other observation is to the effect that

"every progression of terms constructed in conformity with the equation

$$u_n = a_n u_{n-1} - b_n u_{n-2} + c_n u_{n-3} \pm \dots$$

may be represented as an ascending series of principal coaxial determinants to a common matrix. Thus if each term in such progression is to be made a linear function of the three preceding terms, it will be representable by means of the matrix

$$\begin{array}{cccccc} A & B & C'' & 0 & 0 & \\ 1 & A' & B'' & C''' & 0 & \\ 0 & 1 & A'' & B''' & C'''' & \\ 0 & 0 & 1 & A''' & B'''' & \\ 0 & 0 & 0 & 1 & A'''' & \end{array}$$

indefinitely continued, which gives the terms

$$1, A, AA' - B, AA'A'' - BA'' - AB'' + C'', \dots$$

This exhausts the paper so far as determinants are concerned: the results announced in it, one can readily own, were such as fairly to entitle the enthusiastic author to express his belief that "the introduction of the method of determinants into the algorithm of continued fractions cannot fail to have an important bearing upon the future treatment and development of the theory of numbers."

SPORTISWOODÆ, W. (1853, August).\*

[Elementary theorems relating to determinants. Second edition, rewritten and much enlarged by the author. *Crelle's Journ.*, li. (1856) pp. 209-271, 328-381.]

Save the utilisation of the fact that the denominator of any convergent of the continued fraction

$$a_1 + \frac{b_1}{a_2 + \frac{b_2}{a_3 + \dots}}$$

\* This is the author's date at the end of the paper (p. 381). The first two parts of the volume, however, are dated 1855, and the remaining two 1856.

is the differential-quotient of the numerator, Spottiswoode did nothing but report the fundamental result reached by Sylvester. The full passage (p. 374) is as follows:—

“The improper continued fraction

$$\frac{1}{A - \frac{1}{B - \frac{1}{C - \dots}}} = \frac{d}{dA} \log_e \nabla$$

where

$$\nabla = \begin{vmatrix} A & 1 & 0 & \dots & 0 & 0 \\ 1 & B & 1 & \dots & 0 & 0 \\ 0 & 1 & C & \dots & 0 & 0 \\ \dots & \dots & \dots & \dots & \dots & \dots \\ 0 & 0 & 0 & \dots & M & 1 \\ 0 & 0 & 0 & \dots & 1 & N \end{vmatrix}$$

in which any number of rows may be taken at pleasure, and the formula will give the corresponding convergent fraction.

The same holds good for the continued fraction

$$\frac{1}{A} + \frac{1}{B} + \dots$$

if we write

$$\nabla = \begin{vmatrix} A & 1 & 0 & \dots \\ 1 & B & 1 & \dots \\ 0 & 1 & C & \dots \\ \dots & \dots & \dots & \dots \end{vmatrix}$$

SYLVESTER, J. J. (1853, Sept.).

[On a fundamental rule in the algorithm of continued fractions. *Philos. Mag.* (4), vi. pp. 297-299.]

Without any reference to his previous paper on the subject Sylvester here announces that if

$$(a_1, a_2, \dots, a_i)$$

be the denominator of the  $i^{\text{th}}$  convergent to

$$\frac{1}{a_1} + \frac{1}{a_2} + \frac{1}{a_3} + \dots$$

then

$$(a_1, \dots, a_m, a_{m+1}, \dots, a_{m+n}) = (a_1, \dots, a_m)(a_{m+1}, \dots, a_{m+n}) + (a_1, \dots, a_{m-1})(a_{m+2}, \dots, a_{m+n}),$$

—a possibly new result which he considers “the fundamental theorem in the theory of continued fractions.” This, he says, is an immediate consequence of the fact that  $(a_1, \dots, a_{m+n})$  can be expressed as a determinant, all that is further necessary being the application of the “well-known simple rule for the decomposition of determinants.” Thus, *e.g.*, the determinant

$$\begin{array}{ccccccc} a & 1 & & & & & \\ -1 & b & 1 & & & & \\ & -1 & c & 1 & & & \\ & & -1 & d & 1 & & \\ & & & -1 & e & 1 & \\ & & & & -1 & f & \end{array}$$

is obviously decomposable into

$$\begin{array}{ccc} a & 1 & \\ -1 & b & 1 \\ -1 & c & \end{array} \times \begin{array}{ccc} d & 1 & \\ -1 & e & 1 \\ -1 & f & \end{array} + \begin{array}{ccc} a & 1 & \\ -1 & b & \end{array} \times \begin{array}{ccc} e & 1 & \\ -1 & f & \end{array},$$

or into

$$\begin{array}{ccc} a & 1 & \\ -1 & b & \end{array} \times \begin{array}{ccc} e & 1 & \\ -1 & d & 1 \\ -1 & e & 1 \\ -1 & f & \end{array} + \begin{array}{ccc} a & & \\ & & d & 1 \\ & & -1 & e & 1 \\ & & & -1 & f \end{array}$$

or into

$$\begin{array}{ccc} a & & \\ & & b & 1 \\ & & -1 & c & 1 \\ & & & -1 & d & 1 \\ & & & & -1 & e & 1 \\ & & & & & -1 & f \end{array} + \begin{array}{ccc} c & 1 & \\ -1 & d & 1 \\ -1 & e & 1 \\ -1 & f & \end{array}$$

Following this is what is called “Corollary I.” viz.,

$$(a_1, a_2, \dots, a_m) \cdot (a_2, a_3, \dots, a_{m+i}) - (a_2, a_3, \dots, a_m) \cdot (a_1, a_2, \dots, a_{m+i}) = (-)^m (a_{m+i} a_{m+i-1} \dots \text{to } i-1 \text{ terms}),$$

its connection with the expression for the difference of two convergents being illustrated by the instances  $i = 1, 2, 3, 4, \dots$

The next "corollary," viz.,

$$\begin{aligned} & (a_1, \dots, a_p, a_{p+1}, \dots, a_{p+r})(a_1, \dots, a_p, a_{p+1}, \dots, a_{p+k}) \\ & - (a_1, \dots, a_p, a_{p+1}, \dots, a_{p+g})(a_1, \dots, a_p, a_{p+1}, \dots, a_{p+h}) \\ & = (-)^r \{ (a_{p+1}, \dots, a_{p+r})(a_{p+1}, \dots, a_{p+k}) - (a_{p+1}, \dots, a_{p+g})(a_{p+1}, \dots, a_{p+h}) \} \end{aligned}$$

is clearly incorrect, it being impossible for the value of the left-hand side to be independent of the elements  $a_1, a_2, \dots, a_p$ . Further, as the author gives no accompanying word of comment, the difficulty of suggesting the true theorem is increased. A "sub-corollary" is appended dealing with the case where all the  $a$ 's are equal, and leading up, not without some misprints or inaccuracies, to a theorem of Euler's quoted from the *Nouvelles Annales de Math.*, v. (Sept. 1851) pp. 357-358, to the effect that if  $T_{n+1} = aT_n - bT_{n-1}$  be the generating equation of a recurrent series, then

$$\frac{T_{n+1}^2 - aT_nT_{n+1} + bT_n^2}{b^n}$$

is a constant with respect to  $n$ . Of course the more natural form of this expression is

$$\frac{T_{n+1}^2 - T_nT_{n+2}}{b^n},$$

the numerator of which being

$$\begin{vmatrix} T_{n+1} & T_{n+2} \\ T_n & T_{n+1} \end{vmatrix} \quad -$$

is successively transformable by means of the recursion-formula into

$$b \begin{vmatrix} T_n & T_{n+1} \\ T_{n-1} & T_n \end{vmatrix}, \quad b^2 \begin{vmatrix} T_{n-1} & T_n \\ T_{n-2} & T_{n-1} \end{vmatrix}, \quad b^3 \begin{vmatrix} T_{n-2} & T_{n-1} \\ T_{n-3} & T_{n-2} \end{vmatrix}, \dots$$

so that the constant in question is

$$\begin{vmatrix} T_1 & T_2 \\ -T_0 & T_1 \end{vmatrix}.$$

This, however, Sylvester does not show.\*

\* An interesting extension of this is given by Brioschi in the *Nouvelles Annales de Math.*, xiv. (Jan. 1854) p. 20.



Finally, and to more purpose, it is noted that if we pass from  $(a_1, a_2, \dots, a_i)$  to the readily-suggested extension

$$\begin{array}{ccccccc} m_1 & l_1 & & & & & \\ n_1 & m_2 & l_2 & & & & \\ & n_2 & m_3 & l_3 & & & \\ & & & \dots & \dots & \dots & \\ & & & & n_{i-1} & m_i & l_i \\ & & & & & n_i & m_{i+1} \end{array}$$

the corresponding fundamental theorem is

$$\begin{pmatrix} l_1 \dots l_{i+j} \\ m_1, m_2, \dots, m_{i+j+1} \\ n_1 \dots n_{i+j} \end{pmatrix} = \begin{pmatrix} l_1 \dots l_{i-1} \\ m_1, m_2, \dots, m_i \\ n_1 \dots n_{i-1} \end{pmatrix} \begin{pmatrix} l_{i+1} \dots l_{i+j} \\ m_{i+1}, m_{i+2}, \dots, m_{i+j+1} \\ n_{i+1} \dots n_{i+j} \end{pmatrix} \\ - l_i \begin{pmatrix} l_1 \dots l_{i-2} \\ m_1, m_2, \dots, m_{i-1} \\ n_1 \dots n_{i-2} \end{pmatrix} \begin{pmatrix} l_{i+2} \dots l_{i+j} \\ m_{i+2}, m_{i+3}, \dots, m_{i+j+1} \\ n_{i+2} \dots n_{i+j} \end{pmatrix}.$$

SYLVESTER, J. J. (1853, Oct., Nov.).

[On a theory of the syzygetic relations of two rational integral functions, comprising an application to the theory of Sturm's functions, and that of the greatest algebraical common measure. *Phil. Trans. Roy. Soc. London*, cxliii. pp. 407-548.]

Although this lengthy memoir in its original form bears date "16th June 1853," it is the equally lengthy "supplements" added later while passing through the press that claim attention in the present connection. In the first of these (§ i, p. 474) the denominator of the fraction

$$\frac{1}{q_1} - \frac{1}{q_2} - \dots - \frac{1}{q_n}$$

is denoted by  $[q_1, q_2, \dots, q_n]$ , and termed a "cumulant," and throughout the later portion of the paper this name constantly recurs. It is not, however, until we come to the second

“supplement” that anything apparently new in substance is met with. There in § a (p. 497) the following lemma occurs:—

“The roots of the cumulant  $[q_1, q_2, \dots, q_i]$  in which each element is a linear function of  $x$ , and wherein the coefficient of  $x$  for each element has the like sign, are all real: and between every two of such roots is contained a root of the cumulant  $[q_1, q_2, \dots, q_{i-1}]$  and *ex converso* a root of the cumulant  $[q_2, q_3, \dots, q_i]$ : and (as an evident corollary) for all values of  $\zeta$  and  $\zeta'$  intermediate between 1 and  $i$  the greatest root of  $[q_1, q_2, \dots, q_i]$  will be greater, and the least root of the same will be less than the greatest and least roots respectively of  $[q_\rho, q_{\rho+1}, \dots, q_{\rho-1}, q_\rho]$ .”

Even this, however, may be placed under the well-known theorem regarding the roots of the equation

$$\begin{vmatrix} a_{11} - x & a_{12} & a_{13} & \dots \\ a_{12} & a_{22} - x & a_{23} & \dots \\ a_{13} & a_{23} & a_{33} - x & \dots \\ \dots & \dots & \dots & \dots \end{vmatrix} = 0$$

which had been enunciated by Cauchy in 1829.\*

The next noteworthy result occupies § i. (p. 502). As a preparation for it the theorem

$$[a_1, a_2, \dots, a_m, b_1, b_2, \dots, b_n] = [a_1, a_2, \dots, a_m][b_1, b_2, \dots, b_n] - [a_1, a_2, \dots, a_{m-1}][b_2, b_3, \dots, b_n]$$

may be recalled, the group of elements on the left being now viewed as consisting of two sub-groups. This theorem Sylvester writes in the form

$$[\Omega_1 \Omega_2] = [\Omega_1][\Omega_2] - [\Omega'_1][\Omega_2]$$

and he succeeds in including in it a general theorem, not explicitly formulated, in which the number of groups is  $i$ , the next two cases being

$$[\Omega_1 \Omega_2 \Omega_3] = [\Omega_1][\Omega_2][\Omega_3] - [\Omega'_1][\Omega_2][\Omega_3] - [\Omega_1][\Omega'_2][\Omega_3] + [\Omega'_1][\Omega'_2][\Omega_3],$$

\* v. The theory of orthogonants . . . in *Proc. Roy. Soc. Edinburgh*, xxiv. p. 261.

and

$$\begin{aligned}
 [\Omega_1\Omega_2\Omega_3\Omega_4] &= [\Omega_1][\Omega_2][\Omega_3][\Omega_4] \\
 &\quad - [\Omega'_1][\Omega_2][\Omega_3][\Omega_4] - [\Omega_1][\Omega'_2][\Omega_3][\Omega_4] - [\Omega_1][\Omega_2][\Omega'_3][\Omega_4] \\
 &\quad + [\Omega'_1][\Omega'_2][\Omega_3][\Omega_4] + [\Omega'_1][\Omega_2][\Omega'_3][\Omega_4] + [\Omega_1][\Omega'_2][\Omega'_3][\Omega_4] \\
 &\quad - [\Omega'_1][\Omega'_2][\Omega'_3][\Omega_4].
 \end{aligned}$$

The general theorem is described as giving an expression for  $[\Omega_1\Omega_2 \dots \Omega_i]$  in terms of

$$\begin{aligned}
 &[\Omega_1], [\Omega_2], \dots, [\Omega_{i-1}], [\Omega_i] \\
 &[\Omega'_1], [\Omega'_2], \dots, [\Omega'_{i-1}] \\
 &[\Omega_2], \dots, [\Omega_{i-1}], [\Omega_i] \\
 &[\Omega'_2], \dots, [\Omega'_{i-1}];
 \end{aligned}$$

that is to say, in terms of all the unaltered  $\Omega$ 's, all the curtailed  $\Omega$ 's except the last, all the beheaded  $\Omega$ 's except the first, and all the "doubly-apocopated"  $\Omega$ 's except the first and the last; and it is pointed out that the number of products (or terms) in the expansion is  $2^{i-1}$  "separable into  $i$  alternately positive and negative groups containing respectively 1,  $(i-1)$ ,  $\frac{1}{2}(i-1)(i-2)$ , . . . ,  $i-1$ , 1 products." Further, it is noted that "in every one of the above groups forming a product the accents enter in pairs and between contiguous factors, it being a condition that if any  $\Omega$  have an accent on the right the next  $\Omega$  must have one on the left, and if it have one on the left the preceding  $\Omega$  must have an accent on the right, and the number of pairs of accents goes on increasing in each group from 0 to  $i-1$ ."

In a footnote the case where each  $\Omega$  has only one element, and where, therefore, each singly-accented  $\Omega$  becomes 1, and each doubly-accented  $\Omega$  vanishes, is stated to be identical with the "rule"

$$\begin{aligned}
 [a_1, a_2, \dots, a_i] &= a_1 a_2 \dots a_i - \sum \frac{1}{a_r a_{r+1}} \cdot a_1 a_2 \dots a_i \\
 &\quad + \sum \frac{1}{a_r a_{r+1} a_s a_{s+1}} \cdot a_1 a_2 \dots a_i - \dots
 \end{aligned}$$

SYLVESTER, J. J. (1854, August).

[Théorème sur les déterminants de M. Sylvester. *Nouv. Annales de Math.*, xiii. p. 305.]

This communication in its entirety is as follows:—

“Soient les déterminants

$$\begin{array}{ccccccc}
 \lambda, & \lambda & 1 & & \lambda & 1 & 0 & 0 \\
 & & 1 & \lambda, & & 2 & \lambda & 2 & 0 \\
 & & & & 0 & 1 & \lambda, & & 0 & 2 & \lambda & 3 \\
 & & & & & & & & 0 & 0 & 1 & \lambda, \\
 & & & & & & & & & & & & \lambda & 1 & 0 & 0 & 0 \\
 & & & & & & & & & & & & & 4 & \lambda & 2 & 0 & 0 \\
 & & & & & & & & & & & & & & 0 & 3 & \lambda & 3 & 0 \\
 & & & & & & & & & & & & & & & 0 & 0 & 2 & \lambda & 4 \\
 & & & & & & & & & & & & & & & & 0 & 0 & 0 & 1 & \lambda, & \dots\dots
 \end{array}$$

la loi de formation est évidente; effectuant, on trouve

$$\lambda, \lambda^2 - 1, \lambda(\lambda^2 - 2^2), (\lambda^2 - 1^2)(\lambda^2 - 3^2), \lambda(\lambda^2 - 2^2)(\lambda^2 - 4^2), \\
 (\lambda^2 - 1^2)(\lambda^2 - 3^2)(\lambda^2 - 5^2), \lambda(\lambda^2 - 2^2)(\lambda^2 - 4^2)(\lambda^2 - 6^2),$$

et ainsi de suite.”

That Sylvester was the author of the implied theorem may be considered proved by an entry in the index of the volume (v. p. 478), and by a statement of Cayley’s in the *Quarterly Journal of Mathematics*, ii. p. 163. Probably the title of the communication was prefixed by the editors, who, knowing of Sylvester’s papers in the *Philosophical Magazine*, felt themselves justified in applying the name “Sylvester’s determinants.”

SCHLÄFLI, L. (Nov. 1855).

[Réduction d’une intégrale multiple qui comprend l’arc de cercle et l’aire du triangle sphérique comme cas particuliers. *Journ. de Liouville*, xx. pp. 359-394.]

Here there appears the equation

$$\frac{\Delta(\alpha, \beta, \dots, \zeta, \eta)}{\Delta(\beta, \dots, \zeta, \eta)} = 1 - \frac{\cos^2 \alpha}{1} - \frac{\cos^2 \beta}{1} - \dots - \frac{\cos^2 \zeta}{1 - \cos^2 \eta}$$

where, in view of the contents of a subsequent paper (see under year 1858), it would seem that  $\Delta(a, \beta, \dots, \zeta, \eta)$  was used for

$$\begin{vmatrix} 1 & \cos a & & & & & \\ -\cos a & 1 & \cos \beta & & & & \\ & -\cos \beta & 1 & & & & \\ & & & \dots & & & \\ & & & & -\cos \zeta & 1 & \cos \eta \\ & & & & & -\cos \eta & 1 \end{vmatrix}.$$

No properties, however, of this determinant are given.

### RAMUS (1856, March).

[Determinanternes Anvendelse til at bestemme hoven for de convergerende Brøker. *Oversigt . . . danske Vidensk. Selsk. Forhandlinger* . . . (Kjøbenhavn), pp. 106–119.]

Ramus' introduction consists in recalling the result of the application of determinants to the solution of a set of linear equations, his mode of stating the result being that given by Jacobi in the *De formatione* . . . of the year 1841,—that is to say, he takes for his set of equations

$$\left. \begin{array}{l} a_0^0 y_0 + a_1^0 y_1 + a_2^0 y_2 + \dots + a_n^0 y_n = u_0 \\ a_0^1 y_0 + a_1^1 y_1 + a_2^1 y_2 + \dots + a_n^1 y_n = u_1 \\ \dots \\ a_0^n y_0 + a_1^n y_1 + a_2^n y_2 + \dots + a_n^n y_n = u_n \end{array} \right\}$$

and puts the solution in the form

$$R_n y_r = A_r^0 u_0 + A_r^1 u_1 + A_r^2 u_2 + \dots + A_r^n u_n \quad (\omega)$$

where

$$\begin{aligned} R_n &= \sum \pm a_0^0 a_1^1 a_2^2 \dots a_n^n, \\ A_i^i &= \sum \pm a_0^0 a_1^1 \dots a_{i-1}^{i-1} a_{i+1}^{i+1} \dots a_n^n, \\ A_i^k &= - \sum \pm a_0^0 a_1^1 \dots a_{i-1}^{i-1} a_i^k a_{i+1}^{i+1} \dots a_{k-1}^{k-1} a_{k+1}^{k+1} \dots a_n^n. * \end{aligned}$$

\* It is in this mode of writing  $A_i^k$ , viz., with the negative sign, that Jacobi's peculiarity consists. Not content with removing from  $R_n$  the row and column in which  $a_i^k$  occurs and prefixing to the minor thus obtained the sign-

He then recalls the further fact that if  $y_0, y_1, y_2, \dots, y_n$  be the numerators of the convergents of the continued fraction

$$a_0 + \frac{b_1}{a_1 + \frac{b_2}{a_2 + \dots + \frac{b_n}{a_n}}}$$

there exists the set of equations

$$\left. \begin{aligned} y_0 &= a_0 \\ - a_1 y_0 + y_1 &= b_1 \\ - b_2 y_0 - a_2 y_1 + y_2 &= 0 \\ \quad - b_3 y_1 - a_3 y_2 + y_3 &= 0 \\ \dots &\dots \dots \dots \dots \dots \dots \dots \dots \dots \dots \\ \quad \quad \quad - b_n y_{n-2} - a_n y_{n-1} + y_n &= 0 \end{aligned} \right\}$$

and he thereupon draws the natural conclusion that the previous result can be applied to the determination of  $y_0, y_1, y_2, \dots, y_n$ .

Making the necessary substitution for the  $u$ 's and for  $R_n$  he of course obtains

$$y_n = a_0 A_n^0 + b_1 A_n^1,$$

$A_n^0, A_n^1$  being now determinants which for want of Cayley's notation he cannot accurately specify, but which he persists in writing in the form

$$- \sum \pm a_0^n a_1^1 a_2^2 \dots a_{n-1}^{n-1}, \quad - \sum \pm a_0^0 a_1^n a_2^2 \dots a_{n-1}^{n-1}.$$

From this result he calculates in succession the values of  $y_1, y_2, y_3, y_4$ ; but it will readily be understood that the process is neither elegant nor short.

In the remainder of the paper (§§ 4-9) no further use of the properties of determinants is made, the contents of the last ten pages being such as might appear in any ordinary exposition of continued fractions. First there is established the old "rule" for writing out the value of  $y_n$ , above referred to as being given by Stern. This is followed by the results

factor  $(-1)^{i+\kappa}$ , he takes the further step of moving the row with the index  $\kappa$  over  $\kappa - i + 1$  rows, thus arriving at

$$A_\kappa^i = - \sum \pm a_0^0 a_1^1 \dots a_{i-1}^{i-1} a_{i+1}^{i+1} \dots a_{\kappa-1}^{\kappa-1} a_\kappa^\kappa a_{\kappa+1}^{\kappa+1} \dots a_n^n.$$

Of course there is at the second step the option of moving the column with the index  $i$  over  $\kappa - i + 1$  columns, and this Ramus does.

$$a + \frac{b}{a} + \frac{b}{a^2} + \dots (n \text{ } b's) = \frac{1}{\sqrt{a^2 + 4b}} \left\{ \left( \frac{a + \sqrt{a^2 + 4b}}{2} \right)^{n+2} - \left( \frac{a - \sqrt{a^2 + 4b}}{2} \right)^{n+2} \right\},$$

$$= a^{n+1} + C_{n,1} a^{n-1} b + C_{n-1,2} a^{n-2} b^2 + \dots$$

which by putting  $a = 1 = b$  give the number of terms in  $Y_n$ ,—a number also obtained in the form

$$\frac{1}{2^{n+1}} \left\{ C_{n+2,1} + C_{n+2,3} \cdot 5 + C_{n+2,5} \cdot 5^2 + \dots \right\}.$$

Anything else is of small moment.

CAYLEY, A. (1857, April).

[On the determination of the value of a certain determinant. *Quart. Journ. of Math.*, ii. pp. 163–166 ; or *Collected Math. Papers*, iii. pp. 120–123.]

The determinant in question is rather more general than Sylvester's of the year 1854 (*Nouv. Annales de Math.*, xiii. p. 305), being

$$\begin{vmatrix} \theta & 1 & . & . & \dots & . & . \\ x & \theta & 2 & . & \dots & . & . \\ . & x-1 & \theta & 3 & \dots & . & . \\ . & . & x-2 & \theta & \dots & . & . \\ \dots & \dots & \dots & \dots & \dots & \dots & \dots \\ . & . & . & . & \dots & \theta & n-1 \\ . & . & . & . & \dots & x-n+2 & \theta \end{vmatrix},$$

while the other is obtained from this by putting  $x = n - 1$ . Denoting his own form by  $U_n$ , Cayley, with Sylvester's results before him, found

$$U_2 = (\theta^2 - 1) - (x - 1),$$

$$U_3 = \theta(\theta^2 - 4) - 3(x - 2)\theta,$$

$$U_4 = (\theta^2 - 1)(\theta^2 - 9) - 6(x - 3)(\theta^2 - 1) + 3(x - 3)(x - 1);$$

so that, if he put  $H_n$  for the value of  $U_n$  in Sylvester's case (viz., when  $x = n - 1$ ), he could write

$$U_2 = H_2 - (x - 1)H_0$$

$$U_3 = H_3 - 3(x - 2)H_1$$

$$U_4 = H_4 - 6(x - 3)H_2 + 3(x - 3)(x - 1)H_0,$$

.....

and thence, doubtless, divined the generalisation

$$U_n = H_n - B_{n,1} \cdot (x - n + 1) \cdot H_{n-2} + B_{n,2} \cdot (x - n + 1)(x - n + 3) \cdot H_{n-4} - \dots$$

where

$$H_n = (\theta + n - 1)(\theta + n - 3)(\theta + n - 5) \dots \text{to } n \text{ factors}$$

and

$$B_{n,s} = \frac{n(n-1)(n-2) \dots (n-2s+1)}{2^s \cdot 1 \cdot 2 \cdot 3 \dots s}$$

The establishment of the truth of this is all that the paper is occupied with, the procedure being to expand  $U_n$  in terms of the elements of its last row and their complementary minors, thus obtaining

$$U_n = \theta U_{n-1} - (n-1)(x-n+2)U_{n-2}$$

and thence

$$U_n + \left\{ (n-1)(x-n+2) + (n-2)(x-n+3) - \theta^2 \right\} U_{n-2} + (n-2)(n-3)(x-n+3)(x-n+4)U_{n-4} = 0,$$

and showing that the above conjectural expression for  $U_n$  satisfies the latter equation. The process of verification is troublesome, and was not viewed with satisfaction by Cayley himself.

As a preliminary the coefficients of the  $H$ 's in the value of  $U_n$  are for shortness' sake denoted by  $A_{n,0}, -A_{n,1}, \dots$ , and for the same and an additional reason the coefficient of  $U_{n-2}$  in the difference-equation is denoted by

$$M_{n,s} = \left\{ \theta^2 - (n-2s-1)^2 \right\},$$

which is equivalent to putting

$$M_{n,s} \equiv (n-1)(x-n+2) + (n-2)(x-n+3) - (n-2s-1)^2.$$

The operation to be performed being thus the substitution of

$$A_{n,0}H_n - A_{n,1}H_{n-2} + \dots + (-)^s A_{n,s}H_{n-2s} + \dots$$

for  $U_n$  in the expression

$$U_n + \left[ M_{n,s} - \left\{ \theta^2 - (n-2s-1)^2 \right\} \right] U_{n-2} + (n-2)(n-3)(x-n+3)(x-n+4)U_{n-4},$$



it is readily seen that the result will be an aggregate of expressions like

$$A_{n,s}H_{n-2s} + \left[ M_{n,s} - \left\{ \theta^2 - (n-2s-1)^2 \right\} \right] A_{n-2,s}H_{n-2-2s} \\ + (n-2)(n-3)(x-n+3)(x-n+4)A_{n-4,s}H_{n-4-2s}.$$

Now if we bear in mind that by definition

$$\left\{ \theta^2 - (n-2s-1)^2 \right\} H_{n-2-2s} = H_{n-2s},$$

the second of the three terms of this

$$= M_{n,s}A_{n-2,s}H_{n-2-2s} - A_{n-2,s}H_{n-2s},$$

or, if we write  $s-1$  for  $s$  in one case,

$$= -M_{n,s-1}A_{n-2,s-1}H_{n-2s} - A_{n-2,s}H_{n-2s} \\ = -H_{n-2s} \left\{ M_{n,s-1}A_{n-2,s-1} + A_{n-2,s} \right\};$$

and the third, by writing  $s-2$  for  $s$ ,

$$= (n-2)(n-3)(x-n+3)(x-n+4)A_{n-4,s-2}H_{n-2s}.$$

Consequently the sum of the three will vanish if

$$A_{n,s} - (M_{n,s-1}A_{n-2,s-1} + A_{n-2,s}) + (n-2)(n-3)(x-n+3)(x-n+4)A_{n-4,s-2} = 0,$$

and therefore if

$$B_{n,s}(x-n+1) - B_{n-2,s}(x-n+2s+1) \\ - B_{n-2,s-1}M_{n,s-1} + B_{n-4,s-2}(n-2)(n-3)(x-n+4) = 0,$$

that is, if

$$(x-n) \left[ B_{n,s} - B_{n-2,s} - (2n-3)B_{n-2,s-1} + (n-2)(n-3)B_{n-4,s-2} \right] \\ + \left[ B_{n,s} - (2s+1)B_{n-2,s} - \left\{ 5n-8 - (n-2s+1)^2 \right\} B_{n-2,s-1} + 4(n-2)(n-3)B_{n-4,s-2} \right] = 0.$$

But this is the case; for, as Cayley shows, both the cofactor of  $x-n$  and the other similar expression following it vanish identically. The verification aimed at is thus attained.

PAINVIN (1858, February).

[Sur un certain système d'équations linéaires. *Journ. de Liouville* (2), iii. pp. 41-46.]

The system of equations referred to in the title of Painvin's paper had presented themselves to Liouville in the course of the research which led to his "Mémoire sur les transcendentes elliptiques . . ." (*Journ. de Liouville* (1), v. pp. 441-464). Painvin's reason for taking up the subject was his belief that one of Liouville's results could be more simply arrived at by the use of determinants; and in a few lines of introduction he succeeds in showing that the result in question can be viewed as merely the resolution of the determinant

$$\begin{vmatrix} r & a & . & . & \dots & . & . \\ n(a-1) & r-1 & 2a & . & \dots & . & . \\ . & (n-1)(a-1) & r-2 & 3a & \dots & . & . \\ . & . & (n-2)(a-1) & r-3 & \dots & . & . \\ \dots & \dots & \dots & \dots & \dots & \dots & \dots \\ . & . & . & . & \dots & r-n+1 & na \\ . & . & . & . & \dots & a-1 & r-n \end{vmatrix}$$

into factors.

In explanation of the process followed the case of the fourth order

$$\begin{vmatrix} r & a & . & . \\ 3(a-1) & r-1 & 2a & . \\ . & 2(a-1) & r-2 & 3a \\ . & . & a-1 & r-3 \end{vmatrix}$$

will suffice. Increasing each element of the first row by the corresponding elements of the other rows,—an operation which we may for the nonce symbolise by

$$\text{row}_1 + \text{row}_2 + \text{row}_3 + \dots \dots \dots,$$

—he removes the factor  $r + 3a - 3$  and finds left the cofactor

$$\begin{vmatrix} 1 & 1 & 1 & 1 \\ 3(a-1) & r-1 & 2a & . \\ . & 2(a-1) & r-2 & 3a \\ . & . & a-1 & r-3 \end{vmatrix}.$$

On this are performed the operations

$$\text{col}_1 - \text{col}_2, \text{col}_2 - \text{col}_3, \text{col}_3 - \text{col}_4, \dots$$

the result being a determinant of the next lower order

$$- \begin{vmatrix} 3a - r - 2 & r - 2a - 1 & 2a \\ 2 - 2a & 2a - r & r - 3a - 2 \\ \cdot & 1 - a & a - r + 2 \end{vmatrix}.$$

Finally, after changing the signs of all the elements here, the operations

$\text{row}_1 + \text{row}_2 + \text{row}_3 + \dots, \text{row}_2 + \text{row}_3 + \dots, \text{row}_3 + \dots, \dots$  are performed, the result

$$\begin{vmatrix} r - a & a & \cdot \\ 2(a - 1) & r - a - 1 & 2a \\ \cdot & a - 1 & r - a - 2 \end{vmatrix}$$

being a determinant exactly similar in form to the original but with  $r - a$  instead of  $r$ . This, therefore, in turn may be transformed into

$$(r + a - 2) \begin{vmatrix} r - 2a & a \\ a - 1 & r - 2a - 1 \end{vmatrix},$$

and so on.

The value thus obtained for the above-written determinant of the  $(n + 1)$ <sup>th</sup> order is

$(r + na - n)(r + na - n - 2a + 1)(r + na - n - 4a + 2) \dots (r - na)$  each factor being less than the preceding by  $2a - 1$ , and the whole a function of  $a(a - 1)$ .

The special case is noted where  $a = \frac{1}{2}$ , and where therefore all the  $n + 1$  resulting factors are alike. This Painvin writes in the form

$$\begin{vmatrix} r & \frac{1}{2} & \cdot & \cdot & \cdot & \cdot & \cdot & \cdot \\ -\frac{n}{2} & r - 1 & \frac{2}{2} & \cdot & \cdot & \cdot & \cdot & \cdot \\ \cdot & -\frac{n-1}{2} & r - 2 & \frac{3}{2} & \cdot & \cdot & \cdot & \cdot \\ \cdot & \cdot & -\frac{n-2}{2} & r - 3 & \cdot & \cdot & \cdot & \cdot \\ \cdot & \cdot & \cdot & \cdot & \cdot & \cdot & \cdot & \cdot \\ \cdot & \cdot & \cdot & \cdot & \cdot & r - n + 1 & \frac{n}{2} & \cdot \\ \cdot & \cdot & \cdot & \cdot & \cdot & \cdot & \cdot & r - n \end{vmatrix} = \left(r - \frac{n}{2}\right)^{n+1};$$

but a preferable is, evidently,

$$\begin{vmatrix} \rho & 1 & . & . & \dots & . & . \\ -n & \rho-2 & 2 & . & \dots & . & . \\ . & -n+1 & \rho-4 & 3 & \dots & . & . \\ . & . & -n+2 & \rho-6 & \dots & . & . \\ . & . & . & . & \dots & . & . \\ . & . & . & . & \dots & \rho-2n+2 & n \\ . & . & . & . & \dots & -1 & \rho-2n \end{vmatrix} = (\rho-n)^{n+1}.$$

HEINE, E. (1858, Sept.).

[Auszug eines Schreibens über die Laméschen Functionen an den Herausgeber. Einige Eigenschaften der Laméschen Functionen. *Crelle's Journ.*, lvi. pp. 79-86, 87-99.]

In the case of Heine the functions afterwards known as "continuants" made their appearance under totally different circumstances, viz., while he was engaged in transforming a special homogeneous function of the second degree by means of an orthogonal transformation. It will be remembered that if the quadric

$$\begin{aligned} a_{11}x_1^2 &+ 2a_{12}x_1x_2 + 2a_{13}x_1x_3 + \dots \\ &+ a_{22}x_2^2 + 2a_{23}x_2x_3 + \dots \\ &+ a_{33}x_3^2 + \dots \\ &\dots \end{aligned}$$

be transformed by an orthogonal transformation into

$$A_{11}\xi_1^2 + A_{22}\xi_2^2 + A_{33}\xi_3^2 + \dots$$

the coefficients of the latter expression are the roots of the equation

$$\begin{vmatrix} \alpha_{11} - A & a_{12} & a_{13} & \dots \\ a_{12} & \alpha_{22} - A & a_{23} & \dots \\ a_{13} & a_{23} & \alpha_{33} - A & \dots \\ \dots & \dots & \dots & \dots \end{vmatrix} = 0.$$

Now Heine's peculiar quadric was

$$\begin{aligned} c_0^2x_0^2 - 2\kappa c_0c_1x_0x_1 \\ + (c_1^2 + c_2^2)x_1^2 - 2\kappa c_2c_3x_1x_2 \\ + (c_3^2 + c_4^2)x_2^2 - \dots \\ + (c_{2\sigma}^2 - 1 + c_{2\sigma}^2)x_{\sigma}^2 \end{aligned}$$

where in every case the coefficient of the product of two  $z$ 's vanishes if their suffixes differ by more than 1, and where

$$\begin{aligned}
 c_0^2 &= \frac{1}{2}(n)(n+1), \\
 c_1^2 &= \frac{1}{4}(n-1)(n+2), \\
 &\dots \dots \dots \\
 c_r^2 &= \frac{1}{4}(n-r)(n+r+1), \quad (r > 0) \\
 &\dots \dots \dots \\
 c_{n-1}^2 &= \frac{1}{2}n, \\
 \text{and } \kappa &= \frac{c^2 - b^2}{c^2 + b^2}.
 \end{aligned}$$

He was thus naturally led to the equation in  $z$

$$\begin{vmatrix}
 z - c_0^2 & \kappa c_0 c_1 & \cdot & \dots & \cdot \\
 \kappa c_0 c_1 & z - c_1^2 - c_2^2 & \kappa c_2 c_3 & \dots & \cdot \\
 \cdot & \kappa c_2 c_3 & z - c_3^2 - c_4^2 & \dots & \cdot \\
 \dots & \dots & \dots & \dots & \dots \\
 \cdot & \cdot & \cdot & \dots & \kappa c_{2\sigma-2} c_{2\sigma-1} \\
 \cdot & \cdot & \cdot & \dots & z - c_{2\sigma-1}^2 - c_{2\sigma}^2
 \end{vmatrix} = 0,$$

where either  $c_{2\sigma}^2$  is  $c_{n-1}^2$ , or  $c_{2\sigma-1}^2$  is  $c_{n-1}^2$  and, if the latter,  $c_{2\sigma}^2 = 0$ . From a knowledge of Painvin's paper he recognised the left-hand side of the equation as being the numerator of the continued fraction

$$z - c_0^2 - \frac{\kappa c_0^2 c_1^2}{z - c_1^2 - c_2^2} - \frac{\kappa c_2^2 c_3^2}{z - c_3^2 - c_4^2} - \dots$$

but he ventured nothing in elucidation of it. Even the special case where  $b=0$  and where therefore  $\kappa=1$  appears to have proved at the time too troublesome, although he knew otherwise that in this case the continued fraction

$$= \frac{z(z-2^2)(z-4^2) \dots (z-n^2)}{(z-1^2)(z-3^2) \dots (z-n-1^2)} \text{ if } n \text{ be even}$$

and

$$= \frac{(z-1^2)(z-3^2)(z-5^2) \dots (z-n^2)}{(z-2^2)(z-4^2) \dots (z-n-1^2)} \text{ if } n \text{ be odd};$$

for his words are—"Einen directen Beweis für diese Summirung des Kettenbruchs habe ich noch nicht aufgefunden."

SCHLÄFLI, L. (1858).

[On the multiple integral  $\int^n dx dy \dots dz$  whose limits are

$$p_1 = a_1x + b_1y + \dots + h_1z > 0, p_2 > 0, \dots, p_n > 0, \\ \text{and } x^2 + y^2 + \dots + z^2 > 1. \text{ Quart. Journ. of Math., ii.} \\ \text{pp. 269-301, iii. pp. 54-68, 97-108.}]$$

The determinant which makes its appearance in the course of Schläfli's research is

$$\begin{vmatrix} 1 & -\cos \alpha & . & . & . & . & . & . \\ -\cos \alpha & 1 & -\cos \beta & . & . & . & . & . \\ . & -\cos \beta & 1 & . & . & . & . & . \\ . & . & . & . & . & . & . & . \\ . & . & . & . & . & . & . & . \\ . & . & . & . & . & 1 & -\cos \eta & . \\ . & . & . & . & . & -\cos \eta & 1 & -\cos \theta \\ . & . & . & . & . & . & -\cos \theta & 1 \end{vmatrix}$$

which for shortness' sake he denotes by

$$\Delta(\alpha, \beta, \gamma, \dots, \eta, \theta)$$

and whose connection with continued fractions he therefore specifies by the equation

$$\frac{\Delta(\alpha, \beta, \gamma, \dots, \eta, \theta)}{\Delta(\beta, \gamma, \dots, \eta, \theta)} = 1 - \frac{\cos^2 \alpha}{1} - \frac{\cos^2 \beta}{1} - \dots - \frac{\cos^2 \eta}{1 - \cos^2 \theta}$$

The first property noticed is, naturally,

$$\Delta(\alpha, \beta, \gamma, \dots, \theta) = \Delta(\beta, \gamma, \dots, \theta) - \cos^2 \alpha \cdot \Delta(\gamma, \dots, \theta).$$

Later there is given what may be viewed as an extension of this, viz.,

$$\Delta(\alpha, \dots, \delta, \epsilon, \zeta, \eta, \theta, \dots, \lambda) = \Delta(\alpha, \dots, \delta, \epsilon) \cdot \Delta(\eta, \theta, \dots, \lambda) \\ - \cos^2 \zeta \cdot \Delta(\alpha, \dots, \delta) \cdot \Delta(\theta, \dots, \lambda),$$

the proof being said to present no difficulty. The third is a little more complicated, and is logically led up to by taking four instances of the first property, viz.,

$$\Delta(\alpha, \beta, \gamma, \dots, \zeta) = \Delta(\beta, \gamma, \dots, \zeta) - \cos^2 \alpha \cdot \Delta(\gamma, \delta, \dots, \zeta), \\ \Delta(\beta, \gamma, \delta, \dots, \zeta, \eta) = \Delta(\gamma, \delta, \dots, \eta) - \cos^2 \beta \cdot \Delta(\delta, \dots, \zeta, \eta), \\ \Delta(\gamma, \delta, \dots, \zeta, \eta, \theta) = \Delta(\gamma, \delta, \dots, \eta) - \cos^2 \theta \cdot \Delta(\gamma, \delta, \dots, \zeta), \\ \Delta(\delta, \dots, \zeta, \eta, \theta, \alpha) = \Delta(\delta, \dots, \zeta, \eta, \theta) - \cos^2 \alpha \cdot \Delta(\delta, \dots, \zeta, \eta),$$

using in connection with these the multipliers

$$\Delta(\delta, \dots, \zeta, \eta), \quad -\Delta(\delta, \dots, \zeta), \quad \Delta(\delta, \dots, \zeta), \quad -\Delta(\gamma, \delta, \dots, \zeta),$$

respectively, performing addition, and then showing that the right-hand sum vanishes, the result thus being

$$\begin{aligned} &\Delta(\alpha, \beta, \gamma, \delta, \dots, \zeta) \cdot \Delta(\delta, \dots, \zeta, \eta) - \Delta(\delta, \dots, \zeta, \eta, \theta, \alpha) \cdot \Delta(\gamma, \delta, \dots, \zeta) \\ &= \{ \Delta(\beta, \gamma, \delta, \dots, \zeta, \eta) - \Delta(\gamma, \delta, \dots, \zeta, \eta, \theta) \} \cdot \Delta(\delta, \dots, \zeta). \end{aligned}$$

The fourth property concerns the determinant

$$\begin{vmatrix} \Delta(\beta, \gamma, \dots, \eta, \theta) & \Delta(\alpha, \beta, \gamma, \dots, \eta, \theta) \\ \Delta(\beta, \gamma, \dots, \eta) & \Delta(\alpha, \beta, \gamma, \dots, \eta) \end{vmatrix}$$

which by reason of the first property can be shown equal to

$$\begin{vmatrix} \Delta(\beta, \gamma, \dots, \eta, \theta) & -\Delta(\gamma, \dots, \eta, \theta) \\ \Delta(\beta, \gamma, \dots, \eta) & -\Delta(\gamma, \dots, \eta) \end{vmatrix} \cos^2 \alpha,$$

or

$$\begin{vmatrix} \Delta(\gamma, \dots, \eta, \theta) & \Delta(\beta, \gamma, \dots, \eta, \theta) \\ \Delta(\gamma, \dots, \eta) & \Delta(\beta, \gamma, \dots, \eta) \end{vmatrix} \cos^2 \alpha,$$

and ultimately, "by repeating this sort of transformation," equal to

$$\cos^2 \alpha \cos^2 \beta \cos^2 \gamma \dots \cos^2 \theta.$$

If we use for a moment the present-day notation for continuants, viz., where

$$\alpha_1 + \frac{b_1}{\alpha_2} + \frac{b_2}{\alpha_3} + \dots = \frac{K \left( \begin{matrix} b_1 & b_2 & & \\ \alpha_1 & \alpha_2 & \alpha_3 & \dots \end{matrix} \right)}{K \left( \begin{matrix} b_2 & & & \\ \alpha_2 & \alpha_3 & & \end{matrix} \right)}$$

Schläfli's results are seen to be

$$\begin{aligned} K \left( \begin{matrix} \beta_1 & \beta_2 & \beta_3 & \\ 1 & 1 & 1 & \dots \end{matrix} \right) &= K \left( \begin{matrix} \beta_2 & \beta_3 & \\ 1 & 1 & \dots \end{matrix} \right) + \beta_1 K \left( \begin{matrix} \beta_3 & \beta_4 & \\ 1 & 1 & \dots \end{matrix} \right), \\ K \left( \begin{matrix} \beta_1 & \beta_2 & \dots & \beta_{\kappa} & \beta_{\kappa} \\ 1 & 1 & \dots & 1 & \dots & 1 \end{matrix} \right) &= K \left( \begin{matrix} \beta_1 & \beta_2 & \dots & \beta_{\kappa-1} & \\ 1 & 1 & \dots & 1 & \end{matrix} \right) \cdot K \left( \begin{matrix} \beta_{\kappa+1} & \beta_{\kappa} \\ 1 & 1 & \dots & 1 \end{matrix} \right) \\ &\quad - \beta_{\kappa} K \left( \begin{matrix} \beta_1 & \dots & \beta_{\kappa-2} & \\ 1 & 1 & \dots & 1 \end{matrix} \right) \cdot K \left( \begin{matrix} \beta_{\kappa+2} & \dots & \beta_{\kappa} \\ 1 & 1 & \dots & 1 \end{matrix} \right), \end{aligned}$$

$$\left. \begin{aligned} & \mathbf{K} \left( \begin{smallmatrix} \beta_1 & & & \beta_{n-2} \\ 1 & 1 & \dots & 1 \end{smallmatrix} \right) \cdot \mathbf{K} \left( \begin{smallmatrix} \beta_4 & & & \beta_{n+1} \\ 1 & 1 & \dots & 1 \end{smallmatrix} \right) \\ - \mathbf{K} \left( \begin{smallmatrix} \beta_4 & & & \beta_n \beta_1 \\ 1 & 1 & \dots & 1 \end{smallmatrix} \right) \cdot \mathbf{K} \left( \begin{smallmatrix} \beta_3 & & & \beta_{n-2} \\ 1 & 1 & \dots & 1 \end{smallmatrix} \right) \end{aligned} \right\} = \left. \begin{aligned} & \left( \mathbf{K} \left( \begin{smallmatrix} \beta_2 & & & \beta^{n-1} \\ 1 & 1 & \dots & 1 \end{smallmatrix} \right) \right) \mathbf{K} \left( \begin{smallmatrix} \beta_4 & & & \beta_{n-2} \\ 1 & 1 & \dots & 1 \end{smallmatrix} \right), \\ & \left( - \mathbf{K} \left( \begin{smallmatrix} \beta_3 & & & \beta_n \\ 1 & 1 & \dots & 1 \end{smallmatrix} \right) \right) \end{aligned} \right\}$$

$$\left. \begin{aligned} & \mathbf{K} \left( \begin{smallmatrix} \beta_2 & & & \beta_n \\ 1 & 1 & \dots & 1 \end{smallmatrix} \right) \mathbf{K} \left( \begin{smallmatrix} \beta_1 & & & \beta_n \\ 1 & 1 & \dots & 1 \end{smallmatrix} \right) \\ & \mathbf{K} \left( \begin{smallmatrix} \beta_2 & & & \beta_{n-1} \\ 1 & 1 & \dots & 1 \end{smallmatrix} \right) \mathbf{K} \left( \begin{smallmatrix} \beta_1 & & & \beta_{n-1} \\ 1 & 1 & \dots & 1 \end{smallmatrix} \right) \end{aligned} \right| = (-1)^n \beta_1 \beta_2 \beta_3 \dots \beta_n,$$

the only change being the writing of  $\beta_1, \beta_2, \dots$  for  $-\cos^2 \alpha, -\cos^2 \beta, \dots$

WORPITZKY (1865, April).

[Untersuchungen über die Entwicklung der monodromen und monogenen Functionen durch Kettenbrüche. (Sch. Progr.) 39 pp., Berlin.]

Of the six sections into which the paper giving the results of Worpitzky's painstaking investigation is divided it is only the first headed "Fundamentalrelationen" which concerns us, these relations being nothing else than what we should now call "properties of continuants."

He takes his continued fraction in the same form as Schläfli, viz.,

$$1 + \frac{a_1}{1} + \frac{a_2}{1} + \dots + \frac{a_n}{1},$$

showing of course that it equals

$$\frac{N_{1,n}}{N_{2,n}},$$

where

$$N_{\kappa,n} = \begin{vmatrix} 1 & 1 & \dots & \dots & \dots & \dots & \dots & \dots \\ -a_\kappa & 1 & 1 & \dots & \dots & \dots & \dots & \dots \\ \dots & -a_{\kappa-1} & 1 & \dots & \dots & \dots & \dots & \dots \\ \dots & \dots & \dots & \dots & \dots & \dots & \dots & \dots \\ \dots & \dots & \dots & \dots & \dots & -a_{n-1} & 1 & 1 \\ \dots & \dots & \dots & \dots & \dots & \dots & -a_n & 1 \end{vmatrix}.$$



The first matter of interest is the expansion of  $N_{\kappa,n}$  as a sum of products of  $a_{\kappa}, a_{\kappa-1}, \dots, a_n$ , *e.g.*,

$$N_{1,3} = 1 + (a_1 + a_2 + a_3) + a_1 a_3.$$

This is written in the form

$$1 + a_{\kappa,n}^1 + a_{\kappa,n}^2 = \dots$$

where, he says, " $a_{\kappa,n}^r$  die Summe aller möglichen (als Producte aufgefassten) Combinationscomplexionen ohne Wiederholung bedeutet, welche sich aus  $a_{\kappa}, a_{\kappa+1}, \dots, a_n$  so zu je  $r$  Elementen bilden lassen, dass nicht zwei neben einander stehende Elemente  $a_s, a_{s+1}$  dieser Reihe in den einzelnen Producten zugleich vorkommen." By way of proof it is pointed out (1) that the term independent of all the  $a$ 's is

$$\begin{vmatrix} 1 & 1 & & & & \\ 0 & 1 & 1 & & & \\ & & \dots & \dots & & \\ & & & 0 & 1 & 1 \\ & & & & 0 & 1 \end{vmatrix} \quad \text{i.e. } + 1;$$

(2) that the cofactor\* of  $(-a_r)(-a_s)(-a_t) \dots$  when two of the  $a$ 's are consecutive is

$$\begin{vmatrix} 1 & 1 & & & & & & & & & \\ 0 & 1 & 1 & & & & & & & & \\ & & \dots & \dots & & & & & & & \\ & & & 0 & 1 & 1 & & & & & \\ & & & & 1 & 0 & 0 & & & & \\ & & & & & 1 & 0 & 0 & & & \\ & & & & & & 0 & 1 & 1 & & \\ & & & & & & & \dots & \dots & & \\ & & & & & & & & 0 & 1 & 1 \\ & & & & & & & & & 0 & 1 \end{vmatrix} \quad \text{i.e. } 0;$$

\* To obtain the cofactor of the product of a number of a set of elements in a determinant Worpitzky puts a 1 in the determinant in place of each element occurring in the said product, 0's in all the other places of the rows to which these elements belong, and 0's for all the other elements of the set.

and (3) that the cofactor of  $(-a_r)(-a_s)(-a_i) \dots$  when no two of the  $a$ 's are consecutive and their number is  $p$ , is

$$\begin{vmatrix} 1 & 1 & & & & & & & & & \\ 0 & 1 & 1 & & & & & & & & \\ & & \dots & \dots & & & & & & & \\ & & & 0 & 1 & 1 & & & & & \\ & & & & 1 & 0 & 0 & & & & \\ & & & & & 0 & 1 & 1 & & & \\ & & & & & & \dots & \dots & & & \\ & & & & & & & 0 & 1 & 1 & \\ & & & & & & & & 1 & 0 & 0 \\ & & & & & & & & & 0 & 1 & 1 \\ & & & & & & & & & \dots & \dots & \\ & & & & & & & & & & 0 & 1 & 1 \\ & & & & & & & & & & & 0 & 1 \end{vmatrix}$$

i.e.  $\begin{vmatrix} 1 & 1 \\ 1 & 0 \end{vmatrix}^p$  i.e.  $(-1)^p$ ,

and that, therefore, the cofactor of  $a_r a_s a_i \dots$  in this case is  $+1$ .

In exactly similar fashion by partitioning  $N_{k,n}$  into terms which contain  $-a_s$ , and terms which do not, he finds

$$N_{k,n} = D_0 - a_s D_s,$$

where

$$D_0 = \begin{vmatrix} 1 & 1 & & & & & & & & & \\ -a_k & 1 & 1 & & & & & & & & \\ & & \dots & \dots & & & & & & & \\ & & & -a_{s-1} & 1 & 1 & & & & & \\ & & & & & 0 & 1 & 1 & & & \\ & & & & & & -a_{s+1} & 1 & 1 & & \\ & & & & & & & \dots & \dots & & \\ & & & & & & & & -a_{n-1} & 1 & 1 \\ & & & & & & & & & -a_n & 1 \end{vmatrix},$$

$$= \begin{vmatrix} 1 & 1 & & & & & & & & & \\ -a_k & 1 & 1 & & & & & & & & \\ & & \dots & \dots & & & & & & & \\ & & & -a_{s-2} & 1 & 1 & & & & & \\ & & & & & -a_{s-1} & 1 \end{vmatrix} \cdot \begin{vmatrix} 1 & 1 & & & & & & & & & \\ -a_{s+1} & 1 & 1 & & & & & & & & \\ & & \dots & \dots & & & & & & & \\ & & & -a_{n-1} & 1 & 1 & & & & & \\ & & & & & -a_n & 1 \end{vmatrix} = N_{k,s-1} \cdot N_{s+1,n},$$



from the expression on the left merely in having  $k+1$  in place of  $k$ ; so that there results

$$\begin{aligned} N_{k,n}N_{k+1,s} - N_{k,s}N_{k+1,n} &= (-1)^2 a_k a_{k+1} (N_{k+2,n}N_{k+3,s} - N_{k+2,s}N_{k+3,n}) \\ &= \dots\dots\dots \\ &= (-1)^{s-k+1} a_k a_{k+1} \dots a_{s+1} N_{s+3,n}. \end{aligned}$$

This also, it will be seen, is connected with a result of Schläfli's; for putting  $s = n - 1$  we have\*

$$\begin{vmatrix} N_{k+1,n-1} & N_{k,n-1} \\ N_{k+1,n} & N_{k,n} \end{vmatrix} = (-1)^{n-k} a_k a_{k+1} \dots a_n,$$

which becomes identical with Schläfli's last proposition on transposing the two rows of the determinant and (what is equally immaterial) putting  $k = 1$ .

THIELE, T. N. (1869, 1870).

[Bemærkninger om Kjædebrøker. *Tidsskrift for Math.* (2), v. pp. 144-146.

Den endelige Kjædebrøksfunctions Theori. *Tidsskrift for Math.* (2), vi. pp. 145-170.]

The first of the two notes comprising Thiele's first paper contains only one result, viz.,

$$a_1 + \frac{b_1}{a_2} + \frac{b_2}{a_3} + \dots + \frac{b_n}{a_n} = \frac{(a_1, a_2, \dots, a_n)}{(a_2, \dots, a_n)},$$

where  $(a_1, a_2, \dots, a_n)$  is used to stand for

$$\begin{vmatrix} a_1 & b_1 & . & \dots & . & . \\ -1 & a_2 & b_2 & \dots & . & . \\ \dots\dots\dots & \dots & \dots & \dots & \dots & \dots \\ . & . & . & \dots & a_{n-1} & b_{n-1} \\ . & . & . & \dots & -1 & a_n \end{vmatrix}$$

\* In giving to  $N_{s+1,s}$ ,  $N_{s+2,s}$ ,  $N_{s+3,s}$  the values 1, 1, 0 which are necessitated by assuming the generality of the recursion-formula

$$N_{k,n} = N_{k+1,n} + a_k N_{k+2,n}$$

Worpitzky forgets to note that in these cases the proposition  $N_{k,n} = N_{n,k}$ , used by him in the demonstration, does not hold.

There is nothing to indicate that this is not viewed as a fresh discovery, notwithstanding the fact that Ramus' paper of 1856 containing virtually the same identity was published in the same city.

The other paper may be described as a careful study of finite continued fractions with the help of determinants. Instead of  $b_1, b_2, \dots$  are used  $a_{12}, a_{23}, \dots$ ; and

$$\begin{vmatrix} a_p & a_{p,p+1} & \cdot & \cdot & \cdot & \cdot & \cdot & \cdot \\ 1 & a_{p+1} & a_{p+1,p+2} & \cdot & \cdot & \cdot & \cdot & \cdot \\ \cdot & \cdot & \cdot & \cdot & \cdot & \cdot & \cdot & \cdot \\ \cdot & \cdot & \cdot & \cdot & \cdot & \cdot & a_{q-1} & a_{q-1,q} \\ \cdot & \cdot & \cdot & \cdot & \cdot & \cdot & 1 & a_q \end{vmatrix}$$

is denoted by  $K(p, q)$ .

Further, this determinant is spoken of as a "Kjædebrøksdeterminant," or, shortly, a "K-Determinant"; and a section (§ 3, pp. 149-152) is devoted to a statement of its properties.

There is no need to rehearse all of these, the last portion (D) of the section being alone that which contains fresh matter. Opening with the double use of a previous property, viz.,

$$\begin{aligned} K(h, m) &= K(h, k-1) \cdot K(k, m) - a_{k-1,k} K(h, k-2) \cdot K(k+1, m), \\ K(h, n) &= K(h, k-1) \cdot K(k, n) - a_{k-1,k} K(h, k-2) \cdot K(k+1, n), \end{aligned}$$

where  $h, k, m, n$  are in ascending order of magnitude, the author eliminates  $K(h, k-1)$  and obtains

$$\begin{vmatrix} K(h, m) & K(k, m) \\ K(h, n) & K(k, n) \end{vmatrix} = a_{k-1,k} K(h, k-2) \cdot \begin{vmatrix} K(k, m) & K(k+1, m) \\ K(k, n) & K(k+1, n) \end{vmatrix} \quad (a)$$

Then by taking the particular case of this where  $k$  appears in place of  $h$  and  $k+1$  in place of  $k$  there results

$$\begin{vmatrix} K(k, m) & K(k+1, m) \\ K(k, n) & K(k+1, n) \end{vmatrix} = a_{k,k+1} \begin{vmatrix} K(k+1, m) & K(k+2, m) \\ K(k+1, n) & K(k+2, n) \end{vmatrix},$$

which when applied to one of the determinants occurring in itself gives

$$\begin{vmatrix} K(k, m) & K(k+1, m) \\ K(k, n) & K(k+1, n) \end{vmatrix} = a_{k,k+1} a_{k+1,k+2} \begin{vmatrix} K(k+2, m) & K(k+3, m) \\ K(k+2, n) & K(k+3, n) \end{vmatrix},$$

and finally

$$\begin{aligned}
 &= a_{k,k+1} a_{k+1,k+2} \dots a_{m,m+1} \cdot \begin{vmatrix} K(m+1,m) & K(m+2,m) \\ K(m+1,n) & K(m+2,n) \end{vmatrix}, \\
 &= a_{k,k+1} a_{k+1,k+2} \dots a_{m,m+1} \cdot K(m+2,n). \quad (\beta)
 \end{aligned}$$

Further, by using this to make a substitution in the previous result (a) there is obtained

$$\begin{vmatrix} K(h,m) & K(k,m) \\ K(h,n) & K(k,n) \end{vmatrix} = a_{k-1,k} a_{k,k+1} \dots a_{m,m+1} \cdot K(h,k-2) K(m+2,n), \quad (\gamma)$$

which on putting  $k = h + 1$  and  $m = n - 1$  becomes

$$\begin{vmatrix} K(h,n-1) & K(h+1,n-1) \\ K(h,n) & K(h+1,n) \end{vmatrix} = a_{h,h+1} a_{h+1,h+2} \dots a_{n-1,n},$$

—a result which may be compared with one of Schläfli's and Worpitzky's, but which is more general in that the main diagonal of each "K-Determinant" does not consist of units.

LIST OF AUTHORS

whose writings are herein dealt with.

	PAGE		PAGE
1853. SYLVESTER . . . . .	130	1857. CAYLEY. . . . .	144
1853. SPOTTISWOODE . . . . .	134	1858. PAINVIN . . . . .	147
1853. SYLVESTER . . . . .	135	1858. HEINE . . . . .	149
1853. SYLVESTER . . . . .	138	1858. SCHLÄFLI . . . . .	151
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(Issued separately, February 26, 1904.)

**On the Origin of the Epiphysis Cerebri as a Bilateral Structure in the Chick.** By **John Cameron, M.B.** (Edin.), M.R.C.S. (Eng.), Carnegie Fellow, Demonstrator of Anatomy, United College, University of St Andrews. *Communicated by Dr W. G. AITCHISON ROBERTSON.*

(MS. received January 4, 1904. Read same date.)

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(1) RESULTS OF THE PRESENT RESEARCH.

Till within recent years the epiphysis cerebri has been generally regarded as a mesial outgrowth from the roof of the thalamencephalon in Vertebrates. The researches of Béranek (5), Dendy (11), Hill (17), and Locy (19), however, tend to demonstrate the fact that this structure arises in the form of two bilateral outgrowths; while Gaskell (12) has drawn attention to its bilateral nature in *Ammocoetes*. Some observations which the author made on the development of the epiphysis in Amphibia (8 and 9) were found to agree in the main with those of the above-mentioned workers. The present research was therefore undertaken with the view of corroborating the results which had been obtained in the Amphibia, and it was found that these received support in the case of the chick.

A number of early chick-embryos (chiefly between the 50th and 60th hours of incubation) were examined; and although it was difficult in every instance to obtain distinct evidence of the bilateral nature of the epiphysis, still in the majority of cases this condition was distinctly marked. The reason for the difficulty of demonstrating in all cases the presence of the bilateral epiphysial condition will be explained later.

Fig. 1 is drawn from a chick-embryo at the 50th hour of incubation, and represents a transverse section of the thalamencephalon in the pineal region. The larger of the two evaginations lies distinctly to the left of the mesial plane (which is represented by the dotted line in the figure), while on the right side a much

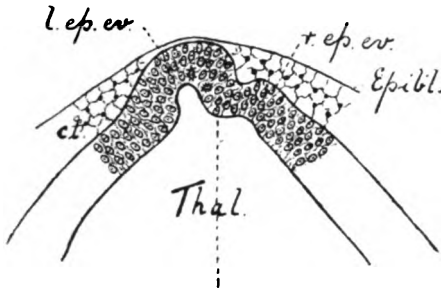


FIG. 1.

smaller evagination exists. The latter was found to be evident in the whole series of sections of the pineal region in this embryo, but it was in every instance much smaller than the left evagination.

Fig. 2 is from a chick-embryo at the 60th hour of incubation,

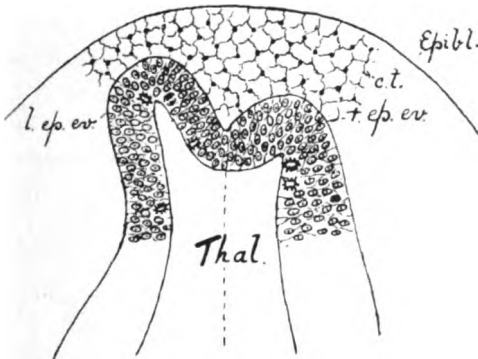


FIG. 2.

and represents a transverse section of the roof of the thalamencephalon in the pineal region as in the previous instance. The resemblance between this fig. and the fig. No. 5 which illustrates Dendy's paper (11) is most striking, as will be at once recognised on comparing the two. Fig. 2 shows with marked clearness the simi-



taneous presence of both the right and left primary epiphysial outgrowths. Here, again, the left is by far the more marked of the two; but the right outgrowth is also well developed—more so than in the previous instance (fig. 1). This section seems to the author to afford distinct proof of the fact that the epiphysis in the chick arises in the form of two distinct evaginations. Many other figs. of this early stage could have been represented; but those already given amply demonstrate the presence of the right and left epiphysial outgrowths. In all the many sections showing paired outgrowths the left was better developed than the right.

A study of the later stages of development of the epiphysis in the chick shows that the duration of the bilateral condition is very brief—the right and left primary outgrowths blending with one another to form the single unpaired epiphysial evagination. This is found to take place towards the end of the 3rd day—after the 60th hour of incubation. The bilateral condition is thus best observed between the 50th and 60th hours of incubation, so that it has a very transient existence (just as in Amphibia); and this probably explains why the bilateral origin has not been previously recognised. But it should also be noted that in some instances the right or smaller evagination was present, but only faintly distinguishable, so that it was quite possible either to overlook its presence altogether (more especially if a single embryo was being examined instead of a series), or to consider it was as a small fold of the cerebral wall due to faulty fixation, or, lastly, to look upon it as an anomalous condition. All the eggs which were examined in this research were incubated under a 'broody' hen, so that the occurrence of those anomalies which ensue from the use of an artificial incubator was avoided. All the embryos were carefully fixed in Bles' fluid, which is an excellent fixative for embryonic tissues, and all risks of shrinkage were thus entirely obviated.

As has been already stated, the bilateral condition of the epiphysis ceases to exist about the end of the 3rd day of incubation; but one cannot draw a hard-and-fast line of demarcation regarding the duration of the bilateral condition, as it is a well-recognised fact that chick-embryos vary considerably in their rate of growth. In some cases, therefore, the presence of the bilateral condition was observed previous to the 50th hour of incubation,

while in the other cases this condition was distinctly evident after the 60th hour of incubation.

Fig. 3 is, like the others, a transverse section of the thalamencephalon in the pineal region, and is from a chick-embryo at the end of the 3rd day of incubation. This figure represents what might be termed the unpaired condition of the epiphysis. On close examination, however, the presence of two small angular recesses within the evagination will be noted, and it may be suggested that these are probably lingering evidences of the previously existing bilateral outgrowths—the process of coalescence having apparently just taken place.



FIG. 3.

*It therefore appears that what in its earlier stages of development used to be looked upon as a mesially placed epiphysial evagination is really situated to the left of the mesial plune, while a more feebly formed evagination exists on the right side. This bilateral condition is, however, very transitory, and soon gives rise to the unpaired condition of the epiphysis by a coalescence of the primary elements.*

(2) COMPARISON OF RESULTS.

The results of this research are of interest in so far as they support the observations previously made by the author in the Amphibia (8 and 9). They also agree in the main with the results obtained by various observers in reference to other classes of the Vertebrata. In Amphibia the author has described the presence in the early stages of right and left recesses from the roof of the thalamencephalon, of which the left is the better developed of the two; and has shown that these very soon coalesce to form a single epiphysial structure. It will be at once observed that these conclusions are corroborated in the case of the chick.

It is also interesting to compare the results of the present research with those of Dendy (11) on *Hatteria*. This observer has

demonstrated in embryos of this reptile the presence of right and left epiphysial outgrowths, which remain distinct and separate from each other. Of these, the left is the more important, and gives rise to the pineal eye, while the right never becomes transformed into anything resembling a pineal eye, but retains its attachment to the roof of the thalamencephalon, and constitutes the epiphysial stalk. So also in the chick the left evagination is the more important of the two. It is, however, unable to remain separate from the right evagination, and thus fails to retain its individuality.\*

Hill (17) has described right and left epiphysial evaginations in Teleosteans and in *Amia*; but in the specimens examined by him the right outgrowth was somewhat more vigorous than the left, while they showed no tendency to blend with one another.

Locy (19) has been another worker in this field of research. He describes the epiphysis of Elasmobranchs as developing from a pair of united accessory optic vesicles. In this group of Fishes, therefore, the paired elements tend to blend with one another as in the case of the chick and the Amphibia.

This research was conducted in the Anatomy Department of the United College, University of St Andrews, under the terms of my appointment both as a Carnegie Fellow and as a Research Fellow of St Andrews University. I wish here to express my best thanks to Professor Musgrove for many valuable facilities which were afforded to me during the progress of the work. I intend to study the early stages of development of the epiphysis in Mammalia in order to ascertain whether any evidence of the bilateral condition of the epiphysis can be found in this class of Vertebrates.

### (3) SUMMARY AND CONCLUSIONS.

(1) The epiphysis cerebri in the chick-embryo first appears in the form of right and left outgrowths or evaginations. Of these, the left is the better marked of the two.

\* My attention has been directed to a statement in Bateson's *Materials for the Study of Variation*, to the effect that the functional eyes of Vertebrates, like other structures near the mesial plane, tend in certain rare instances to coalesce. This cyclopic condition has been described in the chick (see page 458 of the above work), while on page 461 there is illustrated a specimen of the worker-bee (*Apis mellifica*) with the two compound eyes fused together in the mesial plane.

(2) The right primary evagination blends with the left at an early stage of development to form a unified structure.

(3) These observations correspond for the most part with those already made by the author in the case of the Amphibia. They also agree in many ways with those of Béraneck, Dendy, Gaskell, Hill and Locy in other classes of the Vertebrata. As a result of this, it is evident that in the four lower Vertebrate classes the epiphysis cerebri arises as a bilateral, and not as a mesial structure.

(4) It is probable that the ancestors of Vertebrates possessed a pair of parietal eyes, and not a single unpaired structure.

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(5) EXPLANATION OF FIGURES.

[The figures were drawn with the aid of Zeiss's camera lucida apparatus. Zeiss's objective A and ocular No. 3 were employed.]

*c.t.*, subcutaneous connective tissue; *ep.*, epiphysis; *epib.*, epiblast; *l. ep. ev.*, left epiphysial evagination; *r. ep. ev.*, right epiphysial evagination; *thal.*, cavity of thalamencephalon.

Fig. 1. Transverse section of the roof of the thalamencephalon in the pineal region. Embryo-chick at the 50th hour of incubation. The right and left primary epiphysial evaginations are seen. Two germinal nuclei in a condition of karyokinesis are observable. The dotted line represents the mesial plane.

Fig. 2. Transverse section of the roof of the thalamencephalon in the pineal region. Embryo-chick at the 60th hour of incubation. The right and left primary epiphysial evaginations are especially well marked. Several germinal nuclei are seen. The mesial plane is represented by the dotted line.

Fig. 3. Transverse section of the roof of the thalamencephalon in the pineal region. Embryo-chick at the end of the 3rd day of incubation. The unpaired condition of the epiphysis is shown. The presence of two small angular recesses, however, within the epiphysial evagination may denote traces of the previously existing bilateral condition.

*Issued separately March 17, 1904.*)

**Theorem regarding the Orthogonal Transformation  
of a Quadric. By Thomas Muir, LL.D.**

(MS. received July 27, 1903. Read November 2, 1903.)

(1) The theorem in question arises out of a consideration of several passages in Jacobi's important memoir of 1833\* on orthogonal transformation. Having determined the substitution which simultaneously changes

$$x_1^2 + x_2^2 + \dots + x_n^2 \text{ into } y_1^2 + y_2^2 + \dots + y_n^2$$

and

$$\sum_{\kappa\lambda} a_{\kappa\lambda} x_\kappa x_\lambda \text{ into } G_1 y_1^2 + G_2 y_2^2 + \dots + G_n y_n^2,$$

Jacobi proceeds to show (p. 12) that, by the same substitution,

$$G_1^p y_1^2 + G_2^p y_2^2 + \dots + G_n^p y_n^2,$$

where  $p$  is any positive integer, can be expressed in terms of  $x_1, x_2, \dots, x_n$  ("expressionen per ipsas  $x_1, x_2, \dots, x_n$  exhibere licet"). The actual result, however, is not sought for. Later on (p. 14) he reaches a theorem which would enable him to remove the restriction on  $p$  so as to admit *negative* integral values as well, but the opportunity is not used. The reason for the seeming neglect probably is that he has in view a second return to the subject when prepared to deal more effectively with it. However this may be, certain it is that he does return to it, and gives a hypothetical form of the desired expression in  $x_1, x_2, \dots, x_n$ . His words (p. 20) are:—

$$\text{"Statuamus } G_1^p y_1^2 + G_2^p y_2^2 + \dots + G_n^p y_n^2 = \sum_{\kappa\lambda} p_{\kappa\lambda} x_\kappa x_\lambda, \text{ ubi}$$

$$G_1^p a_{1\kappa} a_{1\kappa} + G_2^p a_{2\kappa} a_{2\kappa} + \dots + G_n^p a_{n\kappa} a_{n\kappa} = p_{\kappa\lambda}, \text{"}$$

and where, we may add, the  $a$ 's are the coefficients of the substitution. Regarding the validity of this nothing is said, but proof is

\* JACOBI, C. G. J., De binis quibuslibet functionibus homogeneis secundi ordinis per substitutiones lineares in alias binas transformandis . . . . . *Crelle's Journ.*, xii. pp. 1-69. (Aug. 1833.)

adduced to show that whether  $p$  be a positive or negative integer the coefficient of  $x_\mu x_\lambda$  is a rational function of the coefficients of the original quadric.

With this general statement of the case before us, let us take up the individual results in order, and see what is obtainable therefrom in the light of later work.

(2) The primary result is the transformation implied in the equation

$$\sum_{\kappa\lambda} a_{\kappa\lambda} x_\kappa x_\lambda = G_1 y_1^2 + G_2 y_2^2 + \dots + G_n y_n^2.$$

This, for our purpose, it is essential to write in a form which brings into evidence the matrix  $M$  of the discriminant of the quadric, viz., in the form

$$\begin{array}{ccc|c} x_1 & x_2 & x_3 & \\ \hline a_{11} & a_{12} & a_{13} & x_1 \\ a_{21} & a_{22} & a_{23} & x_2 \\ a_{31} & a_{32} & a_{33} & x_3 \end{array} = G_1 y_1^2 + G_2 y_2^2 + G_3 y_3^2,$$

where, merely for shortness' sake, only three variables are taken. Now, as Jacobi himself showed, any equation which holds between the  $x$ 's and  $y$ 's will still hold if we put

$$\begin{pmatrix} a_{11} & a_{12} & a_{13} \\ a_{21} & a_{22} & a_{23} \\ a_{31} & a_{32} & a_{33} \end{pmatrix} (x_1, x_2, x_3) \text{ for } x_1, x_2, x_3$$

and  $G_1 y_1, G_2 y_2, G_3 y_3$  for  $y_1, y_2, y_3$ .

This substitution, however, in the bipartite function on the left results simply in the matrix of the discriminant being twice multiplied by itself,\* so that we have

$$\begin{array}{ccc|c} x_1 & x_2 & x_3 & \\ \hline M^2 & & & x_1 \\ & & & x_2 \\ & & & x_3 \end{array} = G_1^2 y_1^2 + G_2^2 y_2^2 + G_3^2 y_3^2.$$

\* *Trans. R. S. Edinb.*, xxxii. p. 480.



The continuation of the process, and the same treatment applied to the equation

$$\begin{array}{ccc|c} x_1 & x_2 & x_3 & \\ \hline 1 & . & . & x_1 \\ . & 1 & . & x_2 \\ . & . & 1 & x_3 \end{array} = y_1^2 + y_2^2 + y_3^2$$

thus lead us to the result that, for any positive integer  $p$ , we have—

$$\begin{array}{ccc|c} x_1 & x_2 & x_3 & \\ \hline M^p & & & x_1 \\ & & & x_2 \\ & & & x_3 \end{array} = G_1^p y_1^2 + G_2^p y_2^2 + G_3^p y_3^2.$$

Not only therefore do we know that  $\sum_{\lambda} G_{\lambda}^p y_{\lambda}^2$  can be expressed in terms of the  $x$ 's, but the actual form of the expression—and a beautifully simple form—is obtained.

(3) If this result is to hold for negative values of  $p$ , some convention must be established as to negative powers of a matrix. Now according to Cayley the first negative power,  $M^{-1}$ , is defined by the equation

$$\begin{pmatrix} a_{11} & a_{12} & a_{13} \\ a_{21} & a_{22} & a_{23} \\ a_{31} & a_{32} & a_{33} \end{pmatrix}^{-1} = \begin{pmatrix} \frac{A_{11}}{\Delta} & \frac{A_{21}}{\Delta} & \frac{A_{31}}{\Delta} \\ \frac{A_{12}}{\Delta} & \frac{A_{22}}{\Delta} & \frac{A_{32}}{\Delta} \\ \frac{A_{13}}{\Delta} & \frac{A_{23}}{\Delta} & \frac{A_{33}}{\Delta} \end{pmatrix},$$

where  $\Delta = |a_{11} a_{22} a_{33}|$  and  $A_{11}, A_{12}, \dots$  are the cofactors of  $a_{11}, a_{12}, \dots$  in  $\Delta$ ; consequently the  $p^{\text{th}}$  negative power,  $M^{-p}$ , may be viewed either as

$$\left\{ \begin{pmatrix} a_{11} & a_{12} & a_{13} \\ a_{21} & a_{22} & a_{23} \\ a_{31} & a_{32} & a_{33} \end{pmatrix}^p \right\}^{-1} \quad \text{or} \quad \begin{pmatrix} \frac{A_{11}}{\Delta} & \frac{A_{21}}{\Delta} & \frac{A_{31}}{\Delta} \\ \frac{A_{12}}{\Delta} & \frac{A_{22}}{\Delta} & \frac{A_{32}}{\Delta} \\ \frac{A_{13}}{\Delta} & \frac{A_{23}}{\Delta} & \frac{A_{33}}{\Delta} \end{pmatrix}^p.$$

With this before us let us return to the primary result

$$\begin{array}{ccc|c} x_1 & x_2 & x_3 & \\ a_{11} & a_{12} & a_{13} & x_1 \\ a_{21} & a_{22} & a_{23} & x_2 \\ a_{31} & a_{32} & a_{33} & x_3 \end{array} = G_1 y_1^2 + G_2 y_2^2 + G_3 y_3^2,$$

and make use of the theorem \* that any equation which holds between the  $x$ 's and  $y$ 's will still hold if we put

$$\left( \begin{array}{ccc} \frac{A_{11}}{\Delta} & \frac{A_{21}}{\Delta} & \frac{A_{31}}{\Delta} \\ \frac{A_{12}}{\Delta} & \frac{A_{22}}{\Delta} & \frac{A_{32}}{\Delta} \\ \frac{A_{13}}{\Delta} & \frac{A_{23}}{\Delta} & \frac{A_{33}}{\Delta} \end{array} \right) (x_1, x_2, x_3) \text{ for } x_1, x_2, x_3$$

and  $\frac{y_1}{G_1}, \frac{y_2}{G_2}, \frac{y_3}{G_3}$  for  $y_1, y_2, y_3$ .

The performance of the substitution on the left-hand side changes the matrix  $M$  into  $M^{-1} M M^{-1}$ , that is,  $M^{-1}$ , and we have

$$\begin{array}{ccc|c} x_1 & x_2 & x_3 & \\ M^{-1} & & & x_1 \\ & & & x_2 \\ & & & x_3 \end{array} = \frac{y_1^2}{G_1} + \frac{y_2^2}{G_2} + \frac{y_3^2}{G_3}.$$

The repetition of the substitution upon this equation, and the application of the same process to the equation

$$\begin{array}{ccc|c} x_1 & x_2 & x_3 & \\ 1 & . & . & x_1 \\ . & 1 & . & x_2 \\ . & . & 1 & x_3 \end{array} = y_1^2 + y_2^2 + y_3^2.$$

\* Jacobi's enunciation of this is "In relationibus omnibus, quae inter variables  $x_1, x_2, \dots, x_n$  et variables  $y_1, y_2, \dots, y_n$  locum habent, simul loco  $y_m$  poni posse  $\frac{y_m}{G_m}$ , atque loco  $x_\lambda$

$$\frac{b_{1\lambda}x_1 + b_{2\lambda}x_2 + \dots + b_{n\lambda}x_n}{G_1 G_2 \dots G_n} = \frac{b_{1\lambda}x_1 + b_{2\lambda}x_2 + \dots + b_{n\lambda}x_n}{\sum \pm a_{11} a_{22} \dots a_{nn}};$$

where the  $b$ 's correspond to the modern  $A$ 's, and the sign of equality is used for 'or.'

lead to the result

$$\begin{array}{c} x_1 \ x_2 \ x_3 \\ M^{-p} \left\{ \begin{array}{l} x_1 \\ x_2 \\ x_3 \end{array} \right. = \frac{y_1^2}{G_1^p} + \frac{y_2^2}{G_2^p} + \frac{y_3^2}{G_3^p}. \end{array}$$

(4) Combining this with the result of § 2, we have the general theorem:—

*The orthogonal substitution which changes*

$$(x_1, x_2, x_3)(M)(x_1, x_2, x_3) \text{ into } G_1y_1^2 + G_2y_2^2 + G_3y_3^2$$

*will change*

$$(x_1, x_2, x_3)(M^p)(x_1, x_2, x_3) \text{ into } G_1^py_1^2 + G_2^py_2^2 + G_3^py_3^2$$

*where p is any integer, positive or negative.*

(5) Since  $G_1, G_2, G_3$ , are the roots of the equation

$$\begin{vmatrix} a_{11} - x & a_{12} & a_{13} \\ a_{21} & a_{22} - x & a_{23} \\ a_{31} & a_{32} & a_{33} - x \end{vmatrix} = 0,$$

it is at once suggested from § 4 that the equation whose roots are the  $p^{\text{th}}$  powers of the roots of this equation is got by substituting for  $a_{11}, a_{12}, \dots$ , the corresponding elements of the matrix which is the  $p^{\text{th}}$  power of

$$\begin{pmatrix} a_{11} & a_{12} & a_{13} \\ a_{21} & a_{22} & a_{23} \\ a_{31} & a_{32} & a_{33} \end{pmatrix},$$

a theorem first formulated by Sylvester in 1852 (v. *Nouv. Annales de Math.*, xi. p. 438).

(Issued separately March 17, 1904.)

**Ocean Temperatures and Solar Radiation.**  
By **Professor C. G. Knott.**

(Read February 15, 1904.)

Two years ago I communicated a short paper on Solar Radiation and Earth Temperatures (*Proc.*, vol. xxiii., pp. 296-311). This paper had its origin in a critical discussion of certain results deduced by Dr Buchan from observations of Mediterranean temperatures which had been made by the staff of the Austrian war-ship *Pola*. The mathematical method by which I discussed the relation between the solar energy incident on the surface of earth or sea, and the corresponding fluctuations of temperature in the rock of the Calton Hill and the surface waters of the Mediterranean, has attracted some attention in America; and correspondence with Professor Cleveland Abbe has drawn my attention again to the subject. In this paper I propose to consider more carefully the significance of the observations made and published by the Austrians. These are contained in four quarto volumes, which Dr Buchan has kindly placed in my hands for the purposes of a thorough investigation from the point of view of solar radiation. Dr Buchan clearly saw that something might be made out of these; and the results he gave two and a half years ago before the Society indicated a penetration of solar heat every day to a depth of more than 100 feet. The results were based upon means of temperature at different depths grouped according to the time of day at which they were taken. As I showed in my former paper, the results so deduced indicated a daily penetration into the waters of the Mediterranean of an amount of heat greater than the sun could supply.

From the point of view of the present inquiry, the method adopted by the Austrian observers is not altogether satisfactory. Their immediate object seemed to have been to accumulate a sufficient number of temperature and salinity observations at various depths and at various stations, so as to enable them to draw isotherms and lines of equal salinity at different depths in

the eastern half of the Mediterranean Sea. This they have accomplished, and no doubt their results in this respect are fairly accurate. With this object in view they took complete sets of observations at as many different stations as possible, and at stations in as many different situations as possible. After finishing a set of observations at one station at early morning, they

TABLE A.—*List of Selected Stations, with Latitude, Longitude and Time of Observations.*

Station.	Long. E.	Lat. N.	Date.	Time of Observation.
188	30° 14'·1	32° 5'·8	Sept. 5	6.15 to 7 a.m.
191	31 12	31 58·2	5	4.40 ,, 5.30 p.m.
210	32 14·9	32 41·4	9	5.30 ,, 6.15 p.m.
212	33 19·9	32 39·5	10	6.10 ,, 7.30 a.m.
213	34 7·7	32 45·8	10	5.35 ,, 6.30 p.m.
219	34 28·9	33 20·9	12	6.30 ,, 7.10 a.m.
220	33 38·9	33 15·8	12	3.10 ,, 4.15 p.m.
222	32 54·1	33 14·5	13	6.10 ,, 7.15 a.m.
223	33 19·5	33 38	13	6 ,, 6.45 p.m.
225	34 7·8	33 47·3	14	6.15 ,, 7.30 a.m.
226	34 52·6	33 47·6	14	6 ,, 6.45 p.m.
228	33 21·5	34	15	6.10 ,, 7.30 a.m.
229	34 28·5	34 6·7	15	3.15 ,, 4.20 p.m.
231	33 57·7	34 10·5	16	6.5 ,, 6.50 a.m.
232	33 46·1	34 35·7	16	1.5 ,, 2 p.m.
235	34 8·5	34 43	21	5.55 ,, 6.15 a.m.
248	33 17	35 29·6	26	6.45 ,, 7.20 a.m.
250	33 2·6	35 51	26	2.5 ,, 2.30 p.m.
252	32 50·2	35 57·2	27	7.15 ,, 9.45 a.m.
253	32 7·4	35 40	27	4.2 ,, 6.5 p.m.
257	31 29·1	34 32·1	28	2.10 ,, 6 p.m.
259	31 6·5	35 27·1	29	6.10 ,, 6.55 a.m.
260	31 21·7	36 3·9	29	2.10 ,, 6 p.m.
262	30 40·9	36 10·4	30	6.30 ,, 7.5 a.m.
264	30 19·3	36 5·2	30	1.17 ,, 2.15 p.m.

would, for example, steam off to another station twenty or thirty miles distant, and make similar observations at the new station at a later hour the same day. They never made two sets of observations in the morning and afternoon of the same day at the same place. For our present purpose a few days' steady observations at the *same* station would have given more useful results than can be derived from the observations as made. Still, by comparing the temperatures at different depths at contiguous stations, for which the times of observation did not differ by more than ten or twelve hours, we may hope to get some data available for our

purpose. It should be said that the Austrian observers deserve great credit for the manner in which they carried out the work.

A little consideration showed that only a selection of the numerous stations were available for the present inquiry. Dr Buchan had already pointed out the necessity for confining the

TABLE B.—*Temperatures at Various Depths.*

Depth.	0	2	5	10	20	30	50	70	100
Station.									
188	26.0	26.1	25.9	25.2	24.3	24.0	20.0	...	...
191	27.0	26.8	26.7	26.6	25.3	24.8	21.8'	...	...
212	27.8	27.6	27.5'	27.4	27.3	26.4	22.4	19.9'	17.5
213	28.3	27.9	27.8'	27.8	27.3	26.5	22.7	20.2'	18.0
219	27.7	27.5	27.4'	27.6	27.0	25.6	20.4	18.4	17.4
220	28.1	27.8	27.7'	27.8	27.2	25.8	20.6	18.5	17.6
222	27.4	27.2	27.0'	27.0	26.7	25.5	20.5	18.8	17.8
223	28.3	27.9	27.5'	27.1	26.9	25.6	20.5	18.7'	17.4
225	27.8	27.5	27.4'	27.3	26.8	25.8	21.1	18.6	17.3
226	28.1	27.6	27.5'	27.5	26.9	26.3	21.5'	19.2'	17.9
228	27.7	27.8	27.7'	27.5	27.0	26.5	21.3	19.2'	17.9
229	27.9	27.8	27.7'	27.7	27.2	24.8'	19.6	18.0	17.3
231	26.7	26.7	26.9'	27.0'	26.8	24.4	19.2	18.1'	17.4
232	27.7	27.8	27.8'	27.6	25.6	22.0'	19.0	17.9'	16.8
235	27.0	26.9	26.8'	27.0	25.7	21.3	19.0	17.8'	16.7'
238	27.4	27.4	27.0'	26.2	23.4	20.4	18.3	17.3	16.3
248	26.7	26.6	26.4'	26.2	25.4'	22.4	19.8'	18.2'	16.8'
250	26.9	26.7	26.6'	26.4	26.2	23.8	19.5	17.6'	16.4'
252	27.0	26.9	26.9'	27.0	26.1	24.4	20.3	18.4'	16.9
253	27.1	26.9	26.7'	26.5	25.4	22.4	19.1	17.7'	16.5
257	26.3	26.2	26.0'	25.8	25.1	22.8	18.1	17.0'	16.3
259	26.1	25.7	25.5'	25.3	24.4	20.8	17.8	16.6	16.1
260	27.0	26.6	26.4'	26.4	25.8	21.3	18.5	17.4'	16.5
262	27.0	26.9	26.7'	26.6	25.5	22.2	19.5	18.3'	16.7
264	27.4	27.3	27.0'	26.7	25.5'	23.2'	20.0	18.1	16.6
Means	27.3	27.12	26.98	26.85	26.03	23.96	20.02	18.26	17.05

stations chosen to those of deep water. Thus all the stations near land, however important their temperatures and salinities from the point of view of a general survey, must obviously be discounted when the question was one of the direct penetration of solar radiation. Dr Buchan accordingly picked out the stations characterised by great depths of water. I think, however, that his method of selection is not altogether sound. He seems to have aimed at getting as many stations as possible without paying

sufficient heed to the necessity for having them in contiguous pairs, so as to have for every morning set of observations a corresponding afternoon set not more than twelve hours apart. Guided by this and other considerations, I found myself compelled to take a very limited selection of stations, all situated in the Levant. These selected stations are given in Table A, along with their

TABLE C.—*Temperature Differences at Various Depths.*

D ths.	0	2	5	10	20	30	50	70	100
Station.									
191-188	1	.7	.8	1.4	1	.8	1.8	...	...
213-212	.5	.8	.8	.4	0	.1	.3	.3	.5
220-219	.4	.8	.8	.2	.2	.2	.2	.1	.2
220-222	.7	.6	.7	.8	.5	.3	.1	.8	.2
223-222	.9	.7	.5	.1	.2	.1	0	.1	.4
223-225	.5	.4	.1	-.2	.1	-.2	-.6	.1	.1
226-225	.3	.1	.1	.2	.1	.5	.4	.6	.6
226-228	.4	-.2	-.2	0	-.1	-.2	.2	0	0
229-228	.2	0	0	.2	.2	-1.7	-1.7	-1.2	-.6
229-231	1.2	1.1	.8	.7	.4	.4	.4	-.1	-.1
232-231	1	1.1	.9	.6	-1.2	-2.4	-.2	-.2	-.6
238-235	.4	.5	.2	-.8	-2.3	-.9	-.7	-.5	-.4
250-248	.2	.1	.2	.2	.8	1.4	-.3	-.6	-.4
250-252	-.1	-.2	-.3	-.6	.1	-.6	-.8	-.8	-.5
253-252	.1	0	-.2	-.5	-.7	-2.0	-1.2	-.7	-.4
257-259	.2	.5	.5	.5	.7	2.0	.3	.4	.2
260-259	.9	.9	.9	1.1	1.4	.5	.7	.8	.4
260-262	0	-.3	-.3	-.2	.3	-.9	-.1	-.9	-.2
264-262	.4	.4	.3	.1	0	1.0	.5	-.2	-.1
Means	0°.48	0°.37	0°.29	0°.22	0°.09	-0°.08	-0°.09	-0°.18	-0°.11
Probable Error	±.057	±.065	±.062	±.088	±.12	...	...	...	...

latitudes and longitudes, and the date and hour at which the observations were made. The number of the station is the number in the *Pola* reports. All the observations here discussed were made in September of 1892.

Table B contains the corrected observations of temperature for all these stations at the depths 0, 2, 5, 10, 20, 30, 50, 70, 100 metres. Most of the observations at the depth 5 are interpolated, and are so entered in the Report. The interpolation can be effected with considerable accuracy since the law of diminution of

temperature with increase of depth is very steadily maintained throughout the whole series of observations, and is best given by the means of all (see Table B, and the figure on page 181).

Table C contains the differences of temperatures at corresponding depths at pairs of stations, at which the times of observations differed by approximately half a day. The precise difference in time in any case can be found from Table A. In all it will be seen that there are just nineteen pairs of stations available for the inquiry. If the waters to a depth of 100 metres were heated up during the day by direct solar radiation, and cooled off again during the night, these differences should all be positive. A glance shows that out of the nineteen there is one negative value at the surface, three at a depth of 2 metres, four at 5 metres depth, five at 10, four at 20, eight at 30, eight at 50, eleven at 70, and eleven at 100. At depths greater than 20 metres there is no evidence of penetration of solar radiation. Even at 20 metres it is doubtful if we can find any evidence of direct daily heating. We may, however, take the means of the differences at each depth, and then test the sufficiency of the observations by calculating the probable errors in the usual way. The result is as follows:—

Depth in Metres.	Mean Daily Difference of Temperature (C.).	Probable Error.
0	0·48	±0·057
2	0·37	±0·065
5	0·29	±0·062
10	0·22	±0·088
20	0·09	±0·12
30	-0·08	..
50	-0·09	...
70	-0·18	...
100	-0·11	...

The thermometers read to tenths of degrees, so that little value can be attached to the second decimal place.

It would obviously be wasted labour to calculate the probable errors for the last four depths. At depth 20 metres the probable error is numerically greater than the mean; so that we can say nothing definite as to the effect of solar radiation at this depth.

The errors are so great that we may, without running any risk of introducing greater errors, combine these numbers by a linear formula, assuming that the difference of temperature  $t$  between morning and afternoon in the waters of the Mediterranean during



the month of September is connected with the depth by the formula

$$t = a + bx.$$

Combining the first four temperature differences down to a depth of 10 metres by the method of least squares we find

$$t = 0.44 - 0.025x.$$

If we include the difference for the 20-metre depth we find

$$t = 0.42 - 0.018x.$$

Another result obtained by using twenty-seven selected pairs of stations instead of nineteen is

$$t = 0.47 - 0.02x.$$

For this last case the mean differences at the four smaller depths were 0.49, 0.42, 0.33, 0.28.

If we compare the values of the mean differences of temperature here calculated with the values given in the former paper, we see that the present values derived from a carefully-selected number of stations are distinctly smaller, and that no confidence can be placed upon the means for depths greater than 10 metres.

We may now complete the investigation by calculating how much heat accumulation and loss of heat day by day this fluctuation of temperature in the Mediterranean means. This is at once done by integrating the expression  $t dx$  from  $x=0$  to  $x$  equal to the value for which  $t$  vanishes. These values are for the three formulæ given above—17.6, 23.3, and 23.5 respectively. Integrating for these cases and using the corresponding superior limit for  $x$  we find

$$0.44x - 0.0125x^2 = 3.9$$

$$0.42x - 0.009x^2 = 4.9$$

$$0.47x - 0.01x^2 = 5.5$$

Changing the unit from the metre to the centimetre we find 390, 490, 550 calories as estimated values for the amount of solar radiation which heats the Mediterranean waters daily during the month of September. The probable errors in each of these determinations are large, so that only the first significant figure is of any real value. Let us consider  $450 \pm 50$  as a fair average, and compare this with the amount of solar energy available as cal-

culated in the previous paper. On page 299 in that paper a table is given from which we may estimate the amount of solar energy available in one day in the middle of September for localities in the latitude of  $33^{\circ}$  N. Taking the average declination of the sun during September at about  $3^{\circ}$ , we find for the solar energy supplied in one day the value  $6 \times 117 = 700$ . According to the present calculation we conclude that about two-thirds of the solar energy incident on the surface of the Mediterranean Sea heats the surface waters through a depth of nearly 20 metres. This, perhaps, is not an unreasonable result, and is an important correction upon the earlier result, as showing that the Austrian observations are from this point of view in sufficient accordance with Langley's valuable investigations into the value of the solar constant.

Dr Buchan has drawn attention to the importance of the observations in relation to the manner in which the ocean waters (first) gain their heat in the day, and (second) lose it again at night. But here again their value would have been greatly increased if the observers had had this particular problem present to their mind when the observations were being made. Had the *Pola*, on one particularly quiet sunny day, in the centre of the Levant, far from land, made throughout a complete day of twenty-four hours a succession of complete sets of temperature readings at the various depths, at intervals, say, of two or three hours, a great deal of valuable information bearing on this question would have been obtained. The conditions of the survey undertaken quite precluded this. Fortunately, however, observations of the temperature of the surface waters at midnight were frequently, though not regularly, taken. By comparing these with the preceding afternoon temperatures and the succeeding morning temperatures, and taking into consideration the air temperatures at the same times, we gain distinct evidence of convection in the surface layers. The data are given in Table D, sixteen different cases in all. In only two cases was the early morning temperature lower than the immediately preceding midnight temperature; in two cases it was the same; in all other cases it was higher, sometimes markedly so. In thirteen out of the sixteen cases the air temperature was lower than that of the water at early morning; and in eleven of these it was lower even than the contiguous midnight temperature. We

may therefore safely conclude that the warming of the water between midnight and early morning was not due to atmospheric influence. The simple reason is, in fact, not far to seek. By whatever processes the daily heating of the waters is produced, it

TABLE D.—*Table Showing Convection During Cooling.*

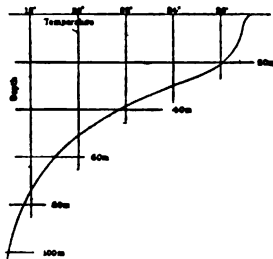
Station.	Hour.	Surface Temp.	Air Temp.	Station.	Hour.	Surface Temp.	Air Temp.
193	7.40p.	26.9	27.6	233	12.30p.	26.9	30.2
194	11.45p.	26.5	...	234	1a.	26.6	...
195	6a.	27.1	26.0	235	5.55a.	27.0	26.5
210	5.30p.	26.6	28.0	238	6.5p.	27.4	28.1
211	12.20a.	26.9	...	239	12.1a.	27.2	...
212	6.10a.	27.8	25.6	240	6.45a.	27.6	26.6
213	5.35p.	28.3	26.9	243	12.1p.	27.8	27.6
214	12.30a.	27.5	...	244	1a.	27.2	...
215	6.8a.	28.1	26.7	245	6.14a.	27.2	26.5
217	2.10p.	28.3	28.5	253	4.2p.	27.1	27.8
218	12.15a.	27.6	...	254	12.20a.	25.9	...
219	6.30a.	27.7	26.0	255	6.10a.	25.9	24.6
220	3.10p.	28.1	27.5	257	2.10p.	26.3	26.5
221	12.30a.	27.2	...	258	12.30a.	25.8	...
222	6.10a.	27.4	27.3	259	6.10a.	26.1	24.2
223	6p.	28.3	27.9	260	2.10p.	27.0	27.6
224	12.30a.	26.9	...	261	12.30a.	26.5	...
225	6.15a.	27.8	27.1	262	6.30a.	27.0	27.3
226	6p.	28.1	28.3	268	1.45p.	24.1	24.5
227	12.30a.	27.3	...	269	12.10a.	23.4	...
228	6.10a.	27.7	28.5	270	6.10a.	23.7	23.2
229	3.15p.	27.9	30.0	272	2.45p.	26.3	27.6
230	12.20a.	26.8	...	273	12.10a.	25.6	...
231	6.5a.	26.7	27.5	274	6.20a.	24.6	23.9

is evident that as the sun sinks the surface layer of the water will begin to cool by radiation. Suppose it to cool by half a degree centigrade: it will then become denser than the slightly warmer

water beneath; and if it could sink without loss of heat it would find its position of equilibrium at a depth of about 5 metres. This, of course, is a very crude description of what really occurs; but it is sufficient to indicate the general nature of the convective process. The steady cooling by radiation of the surface waters must be accompanied by a steady vertical convection determined by the average temperature gradient and the viscosity of the liquid. This will go on steadily until an approximate equilibrium is reached, probably towards early morning; and it is evident that by this process a considerable depth of surface waters will be cooled.\*

Of no small importance with respect to the question of the penetration of solar heat through the surface waters of an ocean or lake is the manner in which the temperature falls off as the depth increases. The curve shown in the figure represents the means given in Table B, and may be taken as typical of all cases in which the body of water is above the temperature of maximum density.

It will be seen at a glance that the vertical distribution of temperature follows a somewhat complex law. As the depth increases the temperature falls off, first fairly rapidly, then more slowly until a depth of 20 metres is reached. Thereafter a rapid rate of diminution sets in, which attains its



maximum at a depth of about 30 metres. The rate of decrease of temperature with increase of depth then begins to diminish, and continues falling off till the greatest depths are reached. It is evident that this fairly permanent vertical distribution of temperature cannot be explained by conduction alone. Probably for depths greater than 40 metres the main factor is conduction of heat from the upper warmer layers to the cooler lower layers. But it is quite clear that some other factor powerfully affects the distribution of tem-

\* For an interesting discussion of similar phenomena in the fresh-water lakes of the Austrian Alps, see "Seestudien," by Professor E. Richter (*Geographische Abhandlungen*, edited by Professor Penck, Vienna, Band VI., Heft 2, 1897)—an important memoir.

perature in the surface layer above 20 metres depth. This factor can only be convection, or, let us say, diffusion of liquid. As already shown, this convection will set in as the sun sinks and the day cools towards night, and will continue till early morning. No doubt also surface waves and ripples due to wind will aid this convection; nor can we leave out of account the vertical migration of fish and other denizens of the deep. Convective movements may also occur during the day in bodies of salt water, the surface layer of which, in virtue of evaporation and consequent increase of salinity, may become denser than the slightly cooler water immediately below it. This last-named factor we should not expect to find in the case of fresh-water lakes. That the main causes are, however, the same in fresh-water lakes as in salt-water seas is proved by the general resemblance in the law of variation of temperature with depth in the two types of cases. From the data furnished by Professor Richter in the memoir already referred to, and from similar data supplied by W. F. Ganong, who studied the vertical distribution of temperature in certain American lakes, we notice, however, one striking difference between the fresh-water lakes and the Mediterranean Sea. In the Mediterranean Sea the most rapid vertical variation of temperature occurs at a depth of 30 metres; in the fresh-water lakes, on the other hand, the corresponding maximum gradient occurs at much less depths—namely, from 6 to 12 metres. The reason for this difference may probably be found in the following considerations. In the first place, the somewhat higher temperature of the Mediterranean Sea will no doubt mean a greater depth of the layer of quickest variation; but this can hardly explain the magnitude of the difference. It must be remembered, however, that in the case of the fresh-water lakes the vertical distribution of temperature experiences a complete change during the winter months when the mass of water is at or below the temperature of maximum density. Hence the summer distribution of temperature, which resembles in type the distribution throughout the whole year in the waters of the Mediterranean, has just time to establish itself before the autumn and winter conditions set in again, and finally overturn the whole type of distribution. On the other hand, in the Mediterranean the waters are never cooled sufficiently so as to come within sight of the

temperature of the maximum density even of fresh water, and consequently the same type of vertical temperature distribution remains permanent throughout the year. In the Mediterranean we are therefore dealing with a permanent average distribution of temperature which is the steady resultant effect of ages of solar radiation, convective cooling, and heat conduction, down from the warmer surface waters and up from the slightly warmer earth below the cold bottom waters.

Superposed upon this steady average distribution we have the daily see-saw of temperature due to direct solar radiation and to the complex indirect effects which accompany it. As the sun rises the surface waters become heated, and to some extent evaporate. This may cause increased salinity in the surface waters, and give rise to gravitation convection currents. Ripples, waves, migration of fish aid the mixing of the waters, so that down to a depth of perhaps 5 or 10 metres the temperature distribution is largely affected by these causes, the pure conduction effect being comparatively unimportant. The direct heating effect of solar radiation at depths greater than 15 metres may be regarded as negligible, because of the great absorption of solar energy in the water near the surface. From the *Pola* records we know that luminosity can penetrate to considerable depths, for white objects at depths of 50 metres were frequently visible. But these rays must be robbed of by far the greater part of their original energy, which, indeed, has gone to heat the surface waters. As evening comes on evaporation will largely cease, the surface waters will cool off by radiation, and convection will be set up which will last well through the night, warmer water continually welling up to replace the cooler heavier water which sinks. By this means the temperature throughout the upper layers becomes steadily reduced, and the heat gained in the day is lost at night. During the day the process of heating is mainly due to the radiant energy of the sun, being absorbed by the water near the surface, aided by mechanical mixing of the layers of water. At night the process of convection tends to bring to the surface all the water comprised within a layer whose depth will depend upon the temperature reached during the day, the rate of cooling of the surface during the night, and the viscosity of the water. The depth to which

solar radiation penetrates in the waters of the Mediterranean does not exceed 20 metres, and the accumulation of heat within this layer during the sunshine of a September day may be estimated at 450 calories per square centimetre of surface, or about two-thirds of the available radiant energy incident on the surface.

These, broadly speaking, are the conclusions to which a study of the *Pola* observations seems to lead. But it is obvious that a much more valuable set of data would be obtained by the use of several platinum thermometers permanently fixed in mid-ocean at convenient depths, and read at fairly frequent intervals throughout the day and night, under different atmospheric conditions as regards cloudiness and wind.

(*Issued separately April 4, 1904.*)

**On Deep-water Two-dimensional Waves produced by any given Initiating Disturbance. By Lord Kelvin.**

(Read February 1, 1904. MS. received February 13, 1904.)

§ 1. Consider frictionless water in a straight canal, infinitely long and infinitely deep, with vertical sides. Let it be disturbed from rest by any change of pressure on the surface, uniform in every line perpendicular to the plane sides, and left to itself under constant air pressure. It is required to find the displacement and velocity of every particle of the water at any future time. Our initial condition will be fully specified by a given normal component velocity, and a normal component displacement, at every part of the surface.

§ 2. Taking O, any point at a distance  $h$  above the undisturbed water level, draw OX parallel to the length of the canal, and OZ vertically downwards. Let  $\xi$ ,  $\zeta$  be the displacement-components of any particle of the water whose undisturbed position is  $(x, z)$ . We suppose the disturbance infinitesimal; by which we mean that the change of distance between any two particles of water is infinitely small in comparison with their undisturbed distance; and the line joining them experiences changes of direction which are infinitely small in comparison with the radian. Water being assumed frictionless, its motion, started primarily from rest by pressure applied to the free surface, is essentially irrotational. Hence we have

$$\xi = \frac{d}{dx} \phi(x, z, t); \quad \zeta = \frac{d}{dz} \phi(x, z, t); \quad \dot{\xi} = \frac{d}{dx} \dot{\phi}; \quad \dot{\zeta} = \frac{d}{dz} \dot{\phi} \quad (1);$$

where  $\phi(x, z, t)$ , or  $\phi$ , as we may write it for brevity when convenient, is a function of the variables which may be called the displacement-potential; and  $\dot{\phi}(x, z, t)$  is what is commonly called the velocity-potential. Thus a knowledge of the function  $\phi$ , for all values of  $x, z, t$ , completely defines the displacement and the velocity of the fluid. And, by the fundamentals of hydrokinetics, a knowledge of  $\phi$  for every point of the free



surface suffices to determine its value throughout the water; in virtue of the equation

$$\frac{d^2\phi}{dx^2} + \frac{d^2\phi}{dz^2} = 0 \quad \dots \dots \dots (2).$$

The motion being infinitesimal, and the density being taken as unity, another application of the fundamental hydrokinetics shows that, as found by Cauchy and Poisson,

$$p - \Pi = g(z - h + \zeta) - \frac{d^2\phi}{dt^2} = g(z - h) + g\frac{d\phi}{dz} - \frac{d^2\phi}{dt^2} \quad \dots (3);$$

where  $g$  denotes gravity;  $\Pi$  the uniform atmospheric pressure on the free surface; and  $p$  the pressure at the point  $(x, z + \zeta)$  within the fluid.

§ 3. To apply (3) to the wave-surface, put in it,  $z = h$ ; it gives

$$g\left(\frac{d\phi}{dz}\right)_{z=h} = \left(\frac{d^2\phi}{dt^2}\right)_{z=h} \quad \dots \dots \dots (4);$$

and therefore if we could find a solution of this equation for all values of  $z$ , with (2) satisfied, we should have a solution of our present problem. Now we can find such a solution; by a curiously altered application of Fourier's celebrated solution

$$\left[ (t+c)^{-1} e^{\frac{-x^2}{4k(t+c)}}, \text{ for } \frac{dv}{dt} = k \frac{d^2v}{dx^2}, \right]$$

his equation for the linear conduction of heat. Change  $t+c, x, k$ , into  $z+\omega, t, g^{-1}$  respectively:—we have (4), and we see that a solution of it is

$$\frac{1}{\sqrt{(z+\omega)}} e^{\frac{-g^2x^2}{4(z+\omega)}} \quad \dots \dots \dots (5);$$

which also satisfies (2) because any function of  $z+\omega$  satisfies (2) if  $\iota$  denotes  $\sqrt{-1}$ . Hence if {RS} denotes a realisation\* by taking half sum of what is written after it with  $\pm \iota$ , we have, as a real solution of (4) for our problem

$${}_1\phi(x, z, t) = \{RS\} \frac{1}{\sqrt{(z+\omega)}} e^{\frac{-g^2x^2}{4(z+\omega)}} \quad \dots \dots \dots (6),$$

\* A very easy way of effecting the realisations in (6) and (9) is by aid of De Moivre's theorem with, for one angle concerned in it,  $\chi = \tan^{-1}x/s$ ; and another angle  $= g^2x/4(z^2+x^2)$ .

$$= \frac{1}{\sqrt{2} \cdot \rho} \left[ \sqrt{(\rho+z)} \cos \frac{gt^2x}{4\rho^2} + \sqrt{(\rho-z)} \sin \frac{gt^2x}{4\rho^2} \right] \epsilon^{\frac{-gt^2x}{4\rho^2}} \left. \vphantom{\frac{1}{\sqrt{2} \cdot \rho}} \right\} \quad (7),$$

where  $\rho^2 = z^2 + x^2$

$$= \sqrt{\frac{1}{\rho}} \sin \left( \frac{gt^2x}{4\rho^2} + \theta \right) \epsilon^{\frac{-gt^2x}{\rho^2}} \left. \vphantom{\sqrt{\frac{1}{\rho}}} \right\} \quad (8).$$

where  $\theta = \tan^{-1} \sqrt{\frac{\rho+z}{\rho-z}}$

The sign of  $\sqrt{(\rho-z)}$  changes when  $x$  passes through zero.

Going back now to (5), and denoting by {RD} the difference of its values for  $\pm x$  divided by  $2x$ , we have another solution of our problem essentially different from (6), as follows

$${}_2\phi(x, z, t) = \{RD\} \frac{1}{\sqrt{(z+ix)}} \epsilon^{\frac{-gt^2}{4(z+ix)}} \quad (9),$$

$$= \frac{1}{\sqrt{2} \cdot \rho} \left[ \sqrt{(\rho+z)} \sin \frac{gt^2x}{4\rho^2} - \sqrt{(\rho-z)} \cos \frac{gt^2x}{4\rho^2} \right] \epsilon^{\frac{-gt^2x}{4\rho^2}} \quad (10),$$

$$= \sqrt{\frac{1}{\rho}} \sin \left( \frac{gt^2x}{4\rho^2} + \theta - \frac{\pi}{2} \right) \epsilon^{\frac{-gt^2x}{4\rho^2}} \quad (11).$$

§ 4. The annexed diagram, fig. 1, represents for  $t=0$  the solutions  ${}_2\phi$  and  ${}_1\phi$  as functions of  $x$ , with  $z=1$  for convenience in the drawing. The formulas which we find by taking  $t=0$  in (7)  $\times \sqrt{2}$  and (10)  $\times \sqrt{2}$  are

$${}_1\phi = \frac{\sqrt{[\sqrt{(x^2+z^2)}+z]}}{\sqrt{(x^2+z^2)}}; \quad {}_2\phi = \frac{\sqrt{[\sqrt{(x^2+z^2)}-z]}}{\sqrt{(x^2+z^2)}} \quad (12).$$

Before passing to the practical interpretation of our solutions, remark first that (12) contain full specifications of two distinct initiating disturbances; in each of which  $\phi$  may be taken as a displacement-potential, or as a velocity-potential, or as a horizontal displacement-component or velocity, or as a vertical displacement-component or velocity. Thus we have really preparation for six different cases of motion, of which we shall choose one,  $-\zeta = \sqrt{2} \times (7)$ , for detailed examination.

§ 5. Taking  $z=h=1$ , for the water surface, let the two curves of figure 1 represent initial *displacements*, (12), of the water surface, left to itself with the water everywhere at rest. The displacements at any subsequent time  $t$  are expressed in real symbols by (7) (10) without the divisor  $\sqrt{2}$ , and by (8) (11) with a factor  $\sqrt{2}$  introduced; either of which may be chosen according to convenience in calculation. One set has thus been calculated from (8), with

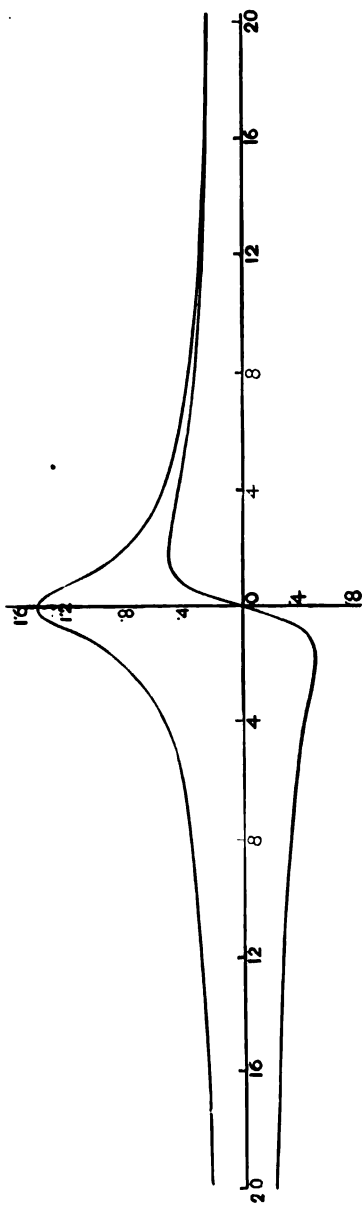


FIG. 1.  $t=0$ ,  $z=1$ . Abcissas represent time; ordinates  $\frac{\sqrt{(\rho+z)}}{\rho}$ ,  $\frac{\sqrt{(\rho-z)}}{\rho}$ .

$g = 4$ , and  $z = 1$ , for six values of  $t$ ;  $\cdot 5$ ,  $1\cdot 5$ ,  $2$ ,  $2\cdot 5$ , and  $5$ ; and for a sufficiently large number of values of  $x$  to represent the results by the curves shown in figs. 2 and 3. Except for the time  $t = 5$ , each curve shows sufficiently all the most interesting characteristics of the figure of the water at the corresponding time. The curve for  $t = 5$  does not perceptibly leave the zero line at distances  $x < 1\cdot 8$ ; but if we could see it, it would show us two and a half wavelets possessing very interesting characteristics; shown in the table of numbers, § 7 below, by which we see that several different curves with scales of ordinates magnified from one to one thousand, and to one million, and to ten thousand million, would be needed to exhibit them graphically.

§ 6. Looking to the curves for  $t = 0$  and  $t = \frac{1}{2}$ ; we see that at first the water rises at all distances from the middle of the disturbance greater than  $x = 1\cdot 9$ , and falls at less distances. And we see that the middle ( $x = 0$ ) remains a crest (or positive maximum) till a very short time before  $t = \frac{1}{2}$ , when it begins to be a hollow. A crest then comes into existence beside it and begins to travel outwards. On the third curve,  $t = 1$ , we see this crest, travelled to a distance  $x = 1\cdot 7$ , from the middle where it came into being; and on the fourth, fifth, sixth, seventh curves (figs. 1, 2) we see it got to distances  $2\cdot 9$ ,  $4\cdot 8$ ,  $6\cdot 5$ ,  $22$ , at the times  $1\frac{1}{2}$ ,  $2$ ,  $2\frac{1}{2}$ ,  $5$ . This crest travelling rightwards on our diagrams has its anterior slope very gradual down to the undisturbed level at  $x = \infty$ . Its posterior slope is much steeper; and ends at the bottom of the hollow in the middle of the disturbance, at times from  $t = \frac{1}{2}$  to  $t = 1\frac{1}{2}$ . At some time, which must be very soon after  $t = 1\frac{1}{2}$ , this hollow begins to travel rightwards from the middle, followed by a fresh crest shed off from the middle. At  $t = 2$ , the hollow has got as far as  $x = \cdot 9$ ; at  $t = 2\frac{1}{2}$ , and  $5$ , respectively, it has reached  $x = 1\cdot 75$ , and  $x = 6\cdot 7$ . Looking in imagination to the extension of our curves leftwards from the middle of the diagram, we find an exact counterpart of what we have been examining on the right. Thus we see an initial elevation, symmetrical on the two sides of a convex crest, of height  $1\cdot 41$  above the undisturbed level, sinking in the middle and rising on the two flanks. The crest becomes less and less convex till it gets down to height  $1\cdot 1$ , when it becomes concave; and two equal and similar wave-crests

are shed off on the two sides, travelling away from it rightwards and leftwards with accelerated velocities, each remaining for ever convex. Thus we see the beginnings of two endless processions of waves travelling outwards in the two directions; originating as infinitesimal wavelets shed off on the two sides of the middle line. Each crest and each hollow travels with increasing velocity. Each wave-length, from crest to crest, or from hollow to hollow, becomes longer and longer as it advances outwards; all this according to law fully expressed in (8) of § 3 above.

§ 7. Here is now the table of numbers promised in § 5 above; it practically defines the forms and magnitudes of the two and a half wavelets, between  $x=0$  and  $x=2$ , which the space-curve for  $t=5$  (figs. 2 and 3) fails to show.

$$\rho^2 = x^2 + h^2; h = 1; g = 4; t = 5; \quad -\zeta = \sqrt{\frac{2}{\rho}} \sin\left(\frac{x^2}{\rho^2} + \theta\right) \epsilon^{\frac{-t^2}{\rho^2}}$$

Col. 1.	Col. 2.	Col. 3.	Col. 4.	Col. 5.	Col. 6.	Col. 7.
$x$ .	$\sqrt{\frac{2}{\rho}}$	$\sqrt{\frac{\rho+1}{2\rho}} = \sin \theta$ .	Initial Elevation of Water-surface at Distance $x$ . $\sin \theta \cdot \sqrt{\frac{2}{\rho}} = \zeta_0$ .	$\sin\left(\frac{x^2}{\rho^2} + \theta\right)$ .	$\epsilon^{\frac{-t^2}{\rho^2}}$ .	Elevation of Water-surface at Time $t$ and Distance $x$ . $-\zeta$ .
0	1·4142	1·0000	1·4142	1 0000	10 <sup>-10</sup> ·1357	+10 <sup>-10</sup> ·1963
·05	1·4140	·9997	1·4140	·8434	„ ·1478	„ „ ·0717
·064	...	...	...	0	...	0
·10	1·410	·9987	1·409	- ·7541	„ ·1778	-10 <sup>-10</sup> ·1891
·15	1·407	·9972	1·403	- ·8997	„ ·3066	„ „ ·3882
·20	1·401	·9952	1·393	- ·0032	„ ·3632	„ „ ·0016
·202	...	...	...	0	...	0
·30	1·384	·9894	1·370	·8997	„ 1·094	+10 <sup>-10</sup> ·1362
·363	...	...	...	0	...	0
·40	1·362	·9820	1·338	- ·5451	„ 4·366	-10 <sup>-10</sup> ·243
·60	1·309	·9638	1·262	- ·2341	„ 103·9	„ „ 31·84
·632	...	...	...	0	...	0
·80	1·249	·9437	1·179	·7593	10 <sup>-5</sup> ·02396	+10 <sup>-5</sup> ·0227
1·00	1·190	·9239	1·099	·8962	„ ·2958	„ „ ·3152
1·25	1·118	·9015	1·007	·6831	„ 5·793	„ „ 4·424
1·50	1·053	·8817	·9287	·4923	„ 45·63	„ „ 23·67
1·517	...	...	...	0	...	0
1·75	·9961	·8651	·8616	- ·6832	„ 212·5	-10 <sup>-5</sup> ·144·6
2·00	·9456	·8506	·8043	- ·9997	„ 848·2	„ „ 801·9
2·50	·8612	·8243	·7142	- ·1633	„ ·03180	„ „ 447·3
2·54	...	...	...	0	...	0
3·00	·7952	·8113	·6450	·8296	„ 08210	„ „ ·0542
3·50	·7411	·7980	·5917	·9473	„ 1516	„ „ ·1064

*Continued on p. 193.*

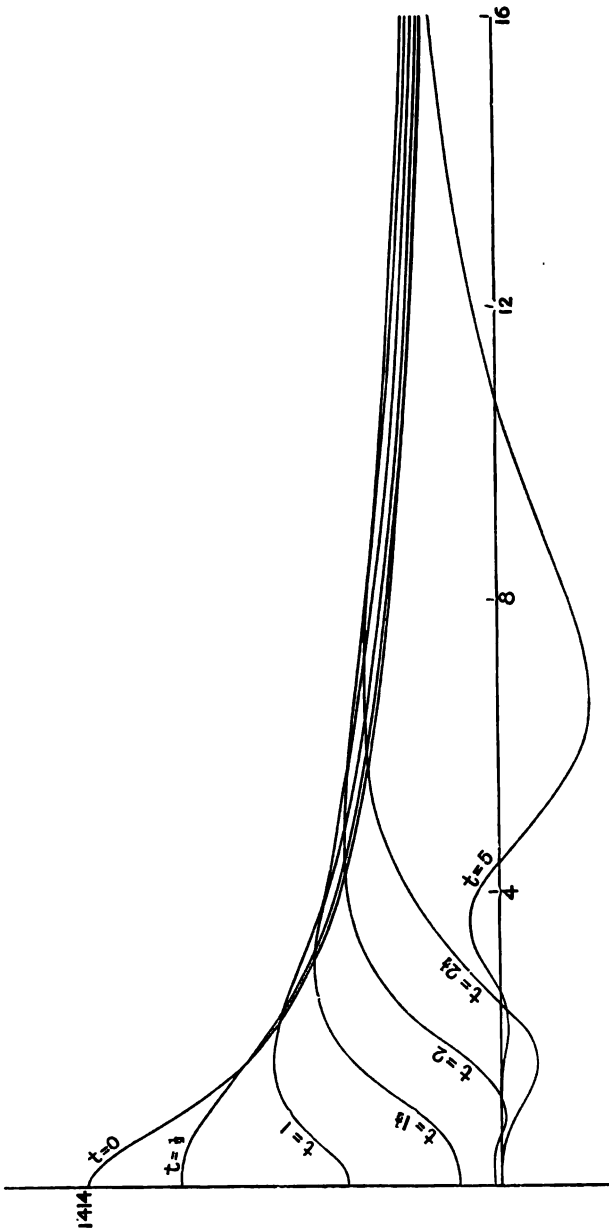


FIG. 2.

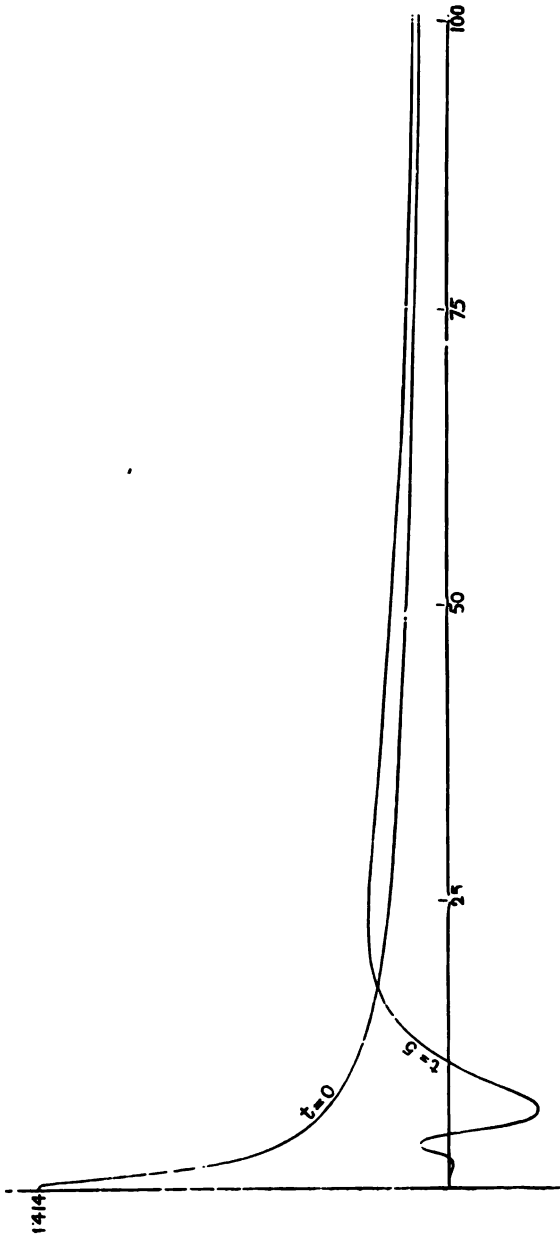


FIG. 8.

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[Continued on page iii of Cover.]

$$h = 1; g = 4; t = 5; -\zeta = \sqrt{\frac{2}{\rho}} \sin\left(\frac{xt^2}{\rho^2} + \theta\right) \epsilon^{\frac{-x^2}{\rho^2}}.$$

Col. 1.	Col. 2.	Col. 3.	Col. 4.	Col. 5.	Col. 6.	Col. 7.
$x$ .	$\sqrt{\frac{2}{\rho}}$ .	$\sqrt{\frac{2}{\rho}} + 1 = \sin \theta$ .	Initial Elevation of Water-surface at Distance $x$ . $\sqrt{\frac{2}{\rho}} = \zeta_0$ . $\sin \theta$ . $\sqrt{\frac{2}{\rho}}$ .	$\sin\left(\frac{xt^2}{\rho^2} + \theta\right)$ .	$\epsilon^{\frac{-x^2}{\rho^2}}$ .	Elevation of Water-surface at Time $t$ , and Distance $x$ . $-\zeta$ .
4.0	.6965	.7882	.5490	.4856	.2298	.07771
4.41	...	...	...	0	...	0
4.5	.6588	.7798	.5139	-.0944	.3083	-.01917
5.0	.6262	.7733	.4843	-.5684	.3823	-.1386
5.5	.5981	.7678	.4592	-.8457	.4493	-.2273
6.0	.5733	.7629	.4375	-.9781	.5122	-.2872
6.5	.5513	.7587	.4185	-.9956	.5641	-.3096
7.0	.5318	.7555	.4018	-.9374	.6066	-.3024
7.5	.5150	.7522	.3868	-.8333	.6462	-.2773
8.0	.4980	.7494	.3734	-.7053	.6808	-.2392
9.0	.4699	.7451	.3501	-.4289	.7372	-.1486
10	.4461	.7416	.3308	-.1679	.6346	-.04753
10.62	...	...	...	0	...	0
11	.4255	.7385	.3142	.05698	.8147	.01975
12	.4076	.7359	.2999	.2428	.8416	.08375
13	.3916	.7339	.2874	.3940	.8644	.1334
14	.3775	.7318	.2762	.5175	.8808	.1721
15	.3648	.7302	.2663	.6163	.8954	.2013
16	.3533	.7286	.2574	.6953	.9082	.2231
18	.3331	.7266	.2419	.8098	.9260	.2498
20	.3160	.7256	.2290	.8831	.9396	.2622
22	.3014	.7230	.2179	.9313	.9497	.2666
24	.2885	.7216	.2082	.9627	.9579	.2661
26	.2772	.7206	.1997	.9815	.9638	.2622
28	.2672	.7193	.1923	.9915	.9685	.2565
30	.2581	.7187	.1856	.9979	.9727	.2505
32	.2500	.7181	.1795	.9999	.9759	.2439
34	.2425	.7173	.1740	.9993	.9786	.2371
38	.2294	.7163	.1643	.9933	.9828	.2240
42	.2182	.7155	.1561	.9840	.9847	.2188
46	.2084	.7147	.1490	.9734	.9883	.2005
50	.2000	.7141	.1429	.9623	.9902	.1905
55	.1906	.7135	.1360	.9486	.9917	.1794
60	.1826	.7129	.1302	.9361	.9931	.1697
70	.1690	.7120	.1203	.9125	.9949	.1535
80	.1581	.7114	.1125	.8931	.9961	.1407
100	.1415	.7108	.1005	.8628	.9977	.1217
$\infty$	0	.7071	0	.7071	1.0000	0

§ 8. Look at the values shown in the previous table for the three factors which constitute  $\zeta$ ;—we see that the first factor (col. 2) decreases slowly from  $x=0$  to  $x=\infty$ ; the second factor (col. 5) alternates between  $+1$  and  $-1$  with increasing distances (semi-wave-lengths) from zero to zero as  $x$  increases. The third

factor (col. 6) increases gradually from  $\epsilon^{-\rho^2/h^2}$  at  $x=0$ , to 1 at  $x=\infty$ . At  $x=50h$ , the third factor is .99, which is so nearly unity that the diminution of amplitude is, for all greater values of  $x$ , practically given by the first factor alone, which diminishes from .2 at  $x=50h$ , to 0 at  $x=\infty$ .

§ 9. The diagrams hitherto given, figs. 1, 2, 3, may be called space-curves, as on each of them abscissas represent distance from the centre of the disturbance. Fig. 4 is a time-curve (abscissas representing time) for  $x=2h$ . It represents a very gradual rise, from  $t=0$  to  $t=.6$ , followed by a fall to a minimum at  $t=2.8$ , and a succession of alternations, with smaller and smaller maximum elevations and depressions, and shorter and shorter times from zero to zero, on to  $t=\infty$ . The same words with altered figures describe the changes of water level at any fixed position farther from the centre of disturbance than  $x=2$ . The following table shows, for the case  $x=100h$ , all the times of zero less than  $71h$ , and the elevations and depressions at the intermediate times when the second factor (col. 5 of § 7) has its maximum and minimum values ( $\pm 1$ ). These elevations and depressions are very approximately the greatest in the intervals between the zeros, because the third factor (col. 6, § 7) varies but slowly, as shown in the first column of the present table.

$$h = 1 ; x = 100 ; \rho = 100.005.h ; \theta = \tan^{-1} \sqrt{\frac{101}{99}} = 45^\circ 18'.$$

$\frac{-t^2}{\epsilon \rho^2}$	Times of Zero and of Approximate Maximum Elevation and Depression.	Approximate Maximum Elevations and Depressions.	$\frac{-t^2}{\epsilon \rho^2}$	Times of Zero and of Approximate Maximum Elevation and Depression.	Approximate Maximum Elevations and Depressions.
.9922	8.333	+ .1403	.7718	50.90	+ .1091
...	15.33	0	...	52.42	0
.9616	19.80	- .1360	.7478	53.90	- .1058
...	23.43	0	...	55.34	0
.9317	26.57	+ .1317	.7247	56.74	+ .1025
...	29.38	0	...	58.10	0
.9031	31.94	- .1277	.7023	59.45	- .0993
...	34.31	0	...	60.75	0
.8750	36.54	+ .1237	.6806	62.03	+ .0962
...	38.62	0	...	63.29	0
.8480	40.61	- .1199	.6595	64.51	- .0933
...	42.50	0	...	65.72	0
.8219	44.31	+ .1162	.6392	66.90	+ .0904
...	46.04	0	...	68.07	0
.7964	47.72	- .1157	.6195	69.21	- .0876
...	49.34	0	...	70.34	0

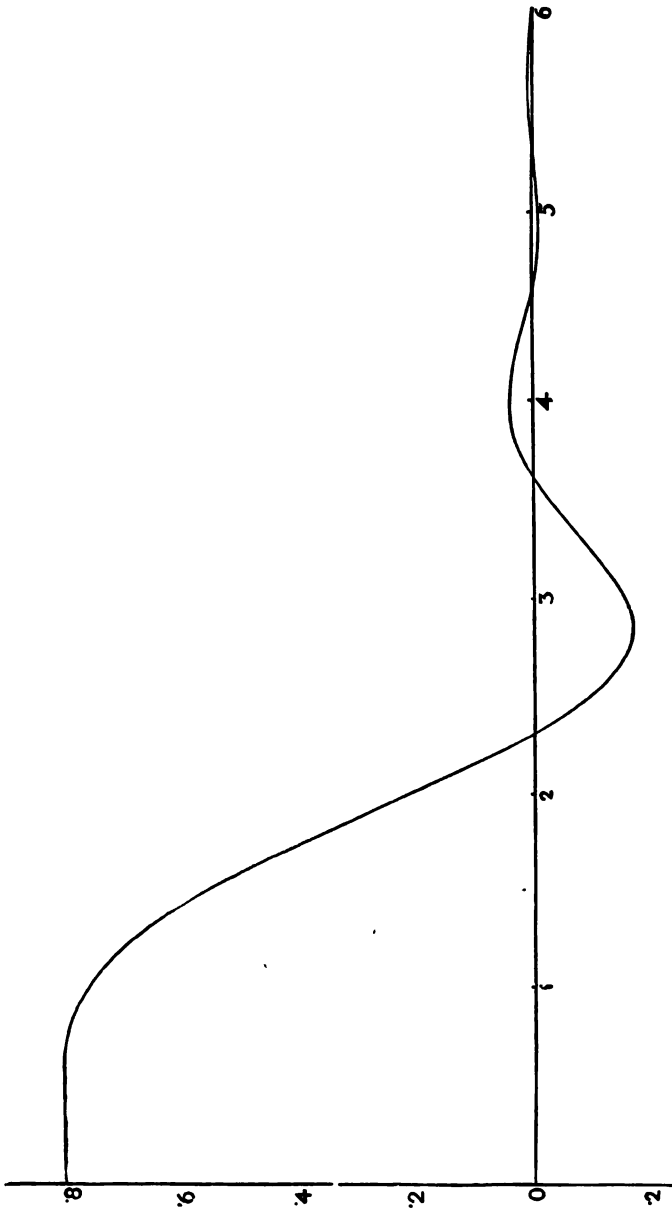


FIG. 4.  $x = 2t$ ,  $g = 4$ . Abcissas represent time.  $-\zeta = \sqrt{\frac{2}{\sqrt{5}}} \sin \left( \frac{2t^2}{5} + \sin^{-1} \frac{\sqrt{5} + 1}{2\sqrt{5}} \right) \epsilon^{-\frac{t^2}{5}}$ .

§ 10. Our assumption  $h=1$  for the free surface involves no restriction of our solution to a particular case of the general formula (7). Our assumption  $g=4$  merely means that our unit of abscissas is half the space fallen through in our unit of time. The fundamental formulas of § 3 may be geometrically explained by, as in § 2, taking O, our origin of co-ordinates, at a height  $h$  above the water level, and defining  $\rho$  as the distance of any particle of the fluid from it. When, as in §§ 5-9, we are only concerned with particles in the free surface (that is to say when  $z=h$ ), we see that if  $x$  is a large multiple of  $z$ ,  $\rho \doteq x$ . See for example the heading of the table of § 9. And if we are concerned with particles below the surface, we still have  $\rho \doteq x$ , if  $x$  is a large multiple of  $z$ . Thus we have the following approximation for (7) of § 3 :—

$${}_1\phi(x, z, t) \doteq \frac{1}{\sqrt{2}x} \left[ \sqrt{(x+z)} \cos \frac{gt^2}{4x} + \sqrt{(x-z)} \sin \frac{gt^2}{4x} \right] e^{\frac{-gtz}{4x^2}} \quad (13).$$

Suppose now  $d\phi/dt$  to represent  $\zeta$  (instead of  $\phi$ , as in §§ 5-9); we have

$$\zeta = \frac{d}{dt} {}_1\phi(x, z, t) \quad \dots \dots \dots (14),$$

which is easily found from (13) without farther restrictive suppositions. But if we suppose that  $z$  is negligibly small in comparison with  $x$ ; and farther that

$$\frac{gt^2z}{4x^2} \doteq 0 \quad \dots \dots \dots (15),$$

we find by (14)

$$\zeta \doteq \frac{gt}{2\sqrt{2}x^{3/2}} \left( \cos \frac{gt^2}{4x} - \sin \frac{gt^2}{4x} \right) \quad \dots \dots \dots (16).$$

This, except the sign - instead of +, is Cauchy's solution;\* of which he says that when the time has advanced so much as to violate a condition equivalent to (15), "le mouvement change avec la méthode d'approximation." The remainder of his Note XVI. (about 100 pages) is chiefly devoted to very elaborate efforts to obtain definite results for the larger values of  $t$ . This object is thoroughly attained by the exponential factor in (8) of § 3 above, without the crippling restriction  $z/x \doteq 0$  which vitiates (16) for small values of  $x$ .

\* *Œuvres*, vol. i. note xvi. p. 193.

(Issued separately April 4, 1904.)

**Some Field Evidence Relating to the Modes of Occurrence of Intrusive Rocks, with some Remarks upon the Origin of Eruptive Rocks in General** By J. G. Goodchild, of the Geological Survey, F.G.S., F.Z.S., Curator of the Collection of Scottish Mineralogy in the Edinburgh Museum of Science and Art. *Communicated by* R. H. TRAQUAIR, LL.D., M.D., F.R.S.

(Read Dec. 5, 1903 ; MS. received Jan. 6, 1904.)

SYNOPSIS.

1. Introduction, pp. 197-199. History of previous opinion, pp. 199-202. Evidence bearing upon the question whether intrusive rocks displace or replace the rocks they invade, pp. 202-218. Basic sills in sandstones, pp. 202-204 ; in shales, pp. 205-207 ; in limestones, p. 208 ; in coal seams, pp. 208-210. Basic dykes in the same connection, pp. 211-212. Acid intrusions, pp. 212-213. Anomalies in the mode of occurrence of dykes discussed, pp. 213-217. Relation between dykes and sills, p. 217. Evidence cited from other sources, pp. 217-218. Summary of the author's conclusions, pp. 218-226.

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It is commonly believed by geologists, as well as by coal miners, that the inner faces of the rocks which enclose intrusive masses were at one time in contact, and that each of these surfaces is the counterpart in form to the other, from which it has been severed by the forces to which the injection of the intrusive mass was due. In the case of a sill, for example, this belief implies that the rock floor below the sill and the roof above it were in unbroken contact at some time before the sill was intruded, and that the floor and the roof have been forced apart to a distance equal to the thickness of the intrusive mass. In like manner, so it is believed, the walls right and left of a dyke are supposed to have been thrust apart from their original position. In other words, it is evidently the common belief that these intrusive rocks, whatever their volume may be, have added that volume to the rocks they invade. To put this statement into yet another form, it is evidently believed that two seams of coal, or beds of black-band, or of oil shale, which occur under normal conditions at ten



fathoms apart, are thrust to twenty fathoms apart if there happens to be ten fathoms of intrusive rock between them. A reference to almost any treatise on geology in which this relationship between intrusive masses and the "country rock" is discussed will at once prove that the view referred to has evidently been the one that the author had in mind.

Amongst colliery people, who have to deal with these questions in a practical way, there has long been some difference of opinion upon this point; some believing that trap rocks cut out the measures. But as they are "only practical men," their opinion upon a geological matter is apt to be ignored. Furthermore, as will be evident from the sequel, many field geologists are now of opinion that intrusive masses USUALLY REPLACE the rocks they invade.

It is obviously a matter of considerable commercial importance to test by field evidence whether the current view referred to above is or is not the correct one. This is especially the case in connection with the Scottish coal-fields, which are in many cases "much troubled with whin," as the increasing demand for coal is leading to the prospecting of parts of coal-fields which have hitherto been left untouched, because the areas referred to have been known to be affected by intrusive masses. A little consideration will suffice to show that the question is one of at least equal interest to geologists, as it is one of wide-reaching importance, and as, moreover, it raises many questions in both chemistry and physics which are much more easily asked than answered. One may indeed go farther than even that, for if it can be shown that the current view is not in accordance with the facts, it is obvious that our views on the origin of eruptive rocks in general will have to be reconsidered, and we may even have to modify our opinions on some matters relating to the succession of events which took place in the earlier geological, or later astronomical, periods of the Earth's history.

Fully realising, therefore, the importance of the issues about to be raised, I shall endeavour, in the first part of this paper, to keep rigidly to a statement of the facts which bear upon this question, and then, after summarising the evidence, I shall go on to point out the conclusions to which the study of these facts

appears to lead. In the latter part of the paper, while passing additional facts in review, I shall venture to submit for the consideration of field geologists\* an hypothesis which appears to me to be in full harmony with the facts.

The question whether intrusive rocks displace or replace the rocks they invade has often been raised before. A brief notice of two or three of the more important papers dealing with the subject cannot be out of place, and accordingly they are given here.

In 1852 or 1853 the late Prof. J. Beete Jukes wrote in the *Geological Survey Memoir*, "On the Geology of the South Staffordshire Coal-field," pp. 246-7, as follows: "I was assured also by almost every one engaged in the works of this neighbourhood that, notwithstanding the variation in thickness of 'The Green Rock' [a basic sill], there was no change in the total thickness of the measures; that, for instance, the thickness between the New Mine Coal and the Blue Flats Ironstone remained the same, whatever might be the variation in the thickness of 'The Green Rock.' In other words, it was affirmed almost universally that 'The Green Rock' not only intruded between the measures, but *obliterated* [the italics are the author's] a mass of beds equal to its own thickness." Jukes then goes on to express a doubt about the miner's conclusions; nevertheless, on the next page (247) he adds: "At Union Colliery, north of [Walsall], the Bottom Coal is cut out entirely by 'green rock.'" I do not give the evidence cited by Jukes in support of his own view, as the fact that he was informed of evidence of the trap cutting out the coal is all that need be referred to here now.

There may have been other evidence published before that, or since, of which I have at present no information. But, in 1867, Mr Hughes (now the Woodwardian Professor of Geology at Cambridge, wrote as follows in a review of Nicholson's "Essay on the Geology of Cumberland and Westmorland," *Geol. Mag.*, dec. i., vol. v., pp. 466-7 (1868):—

"One point seems often to come out from a careful examination of a granite mass. The granite seems to *replace* a certain portion

\* The questions raised are of a *petrographical* as distinguished from *lithological* character.

of the sedimentary strata, and not to *displace* them, leaving them pushed out on all sides. If we suppose the intruded rock to eat its way into the sedimentary strata, assimilating portions of it, we allow a good deal of what is asked by those who hold the metamorphic origin of granite rocks, *i.e.*, the possibility of changing a sedimentary into a granitoid rock. The advocates of that theory may take their stand upon the assimilated portion, and ask is it the heat of the intruded mass, or the new conditions under which the minerals have been brought into contact with the sedimentary rocks, which has produced the change, and then point out that both the one and the other may be obtained by a sufficient depression of the sedimentary rocks" [the above italics are the author's].

In a later reference, made in the *Geological Survey Memoir* on 98 S.E., pp. 41-42, the same author repeats the statement chiefly with reference, on this occasion, to the dykes of minette, porphyrite, and quartz felsite which occur in the region described. He adds the remark: "It may be worth consideration whether in some cases it might not be possible that the action of gases or of hot water holding minerals in solution, communicating along lines of fissure with the joints, might produce the phenomena observed."

As I happened to be working with the author at the time when both of these remarks were penned, and had abundant opportunities, then and on later occasions, of observing the facts upon which his conclusions were based, I can confirm them in every particular. Attention may be directed to the fact that no mention was made of any lithological passage from that of the dyke to the country rock. Nevertheless, in the discussions which followed the publication of the above passages, only side issues were raised, mainly on the ground that no evidence of a lithological passage could be made out; and the statements of fact, thus apparently discredited, were allowed to drop out of sight.

In 1879 Mr Clough of the Geological Survey took up the matter again, in connection with the Whin Sill of Teesdale, and read a paper before the Geological Society of London, in which similar views were advanced, and supported by an excellent array of facts and arguments. Again a side issue was raised, and the

paper was not allowed to appear in the *Quarterly Journal*. But in the *Geological Magazine*, decade ii., vol. xii., pp. 434-447 (October 1880), the substance of that communication appeared under the title of "The Whin Sill of Teesdale as an Assimilator of the Surrounding Beds." Besides the materials collected in the field by himself, Mr Clough was able to get corroborative evidence in support of his views from Dr James Geikie, Dr Peach, myself, and other of his then colleagues. Mr Clough was quite as fully aware of the fact as any of his predecessors in the field that though the dolerite in question replaces beds of very diverse chemical composition, its own mineral constitution remained uniform, and he was equally well aware that there is no trace of any lithological passage from the country rock to the intruder, or *vice versa*. To meet this very formidable chemical difficulty, which still looms very large indeed in the eyes of cabinet geologists, he wrote (p. 442), referring to objections likely to be raised on these grounds: "But any force which this objection possesses depends upon the assumption, that if sedimentary beds were taken up by the Whin, they would remain in it close at hand in their original situation, whereas there may have been a very general circulation, both on a large scale and molecule by molecule, reducing all the parts of the mixture to a general uniformity of composition. The very possibility of forming alloys and of modifying the properties of metals by adding to them small portions of other substances depends upon this principle of circulation or diffusion, so that it cannot be said that we are without warrant for it."

I may add that the paper has always appeared to me to be a very valuable one, and that I can adduce abundant corroborative evidence in support of the author's statements of fact, partly from a knowledge of the areas adjacent to Teesdale, where similar phenomena are seen, and partly from an examination of the part of Teesdale referred to, after the Geological Survey map of the district was published.

Again, in Mr Clough's case, were the facts ignored or explained away, apparently on no other ground than that it appeared very unlikely that an extensive sheet of dolerite could, by any means, eat up large volumes of sandstone without showing a higher silica

percentage than usual, or that it could assimilate thick beds of limestone without the development of any additional lime silicates, or that it could eat up shales without any perceptible increase in alumina-silicates being evident in any part of the invading rock.

It must occur to any reasoning person, however, that the **FACTS**, at least, either do exist as stated, or they do not. If they do, then it is very illogical to close our eyes to them. It would be much better to face those facts at once, and either to accept them as such without attempting to explain how they came about, or else to re-examine the evidence and endeavour to frame some hypothesis which would harmonise what is known about them ; or, at least, to think out some explanation which would serve for the time being as a working hypothesis until a better one could be suggested.

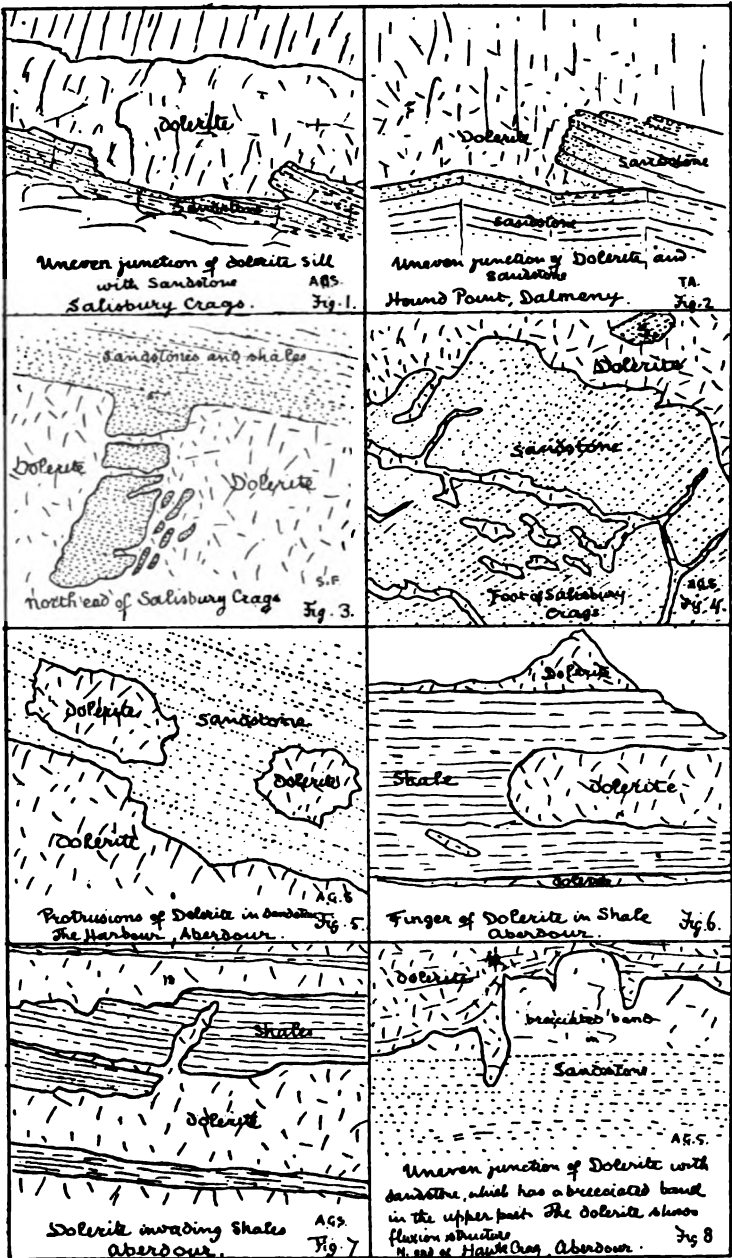
Bearing these considerations in mind, I have collected much additional evidence which bears upon this controverted question. Most of the facts have been obtained in the Lowlands of Scotland, and I have aimed, as much as possible, at citing instances which are either to be seen without difficulty in such easily-visited localities as the Queen's Park, or else at other places within a short distance of Edinburgh. The behaviour of basic intrusive rocks will be considered first, taking sills in the first place and dykes next.

In view of the fact that many geologists think that mechanical disturbance always accompanies the intrusion of eruptive masses, I have thought it well to give first an outline drawing (fig. 1) taken from a photograph by Mr A. G. Stenhouse, F.G.S., of the well-known example in the quarry at the south end of the foot of Salisbury Crags, which is the example illustrated in Hay Cunningham's Fig. 3, Plate III., *Mem. Wern. Soc.*, vol. vii. In this case a wedge of dolerite has been, so to speak, arrested while in the act of *forcing off* a fragment of one of the beds of Cornstone there. The section to the left of the wedge follows the method of attack usual in such cases. Fig. 2, traced from a photograph taken at Hound Point, Dalmeny, by the well-known vulcanologist Dr Tempest Anderson, shows a similar *wedging off* of the country rock by the intrusive mass, which in this case is also a dolerite. It may be remarked that within six feet of this

wedge the dolerite is seen, as in the last case cited, to have eaten its way into the rock, across joints and faults as well, without any signs of disruption.

Fig. 3 shows the top of the dolerite in the old quarry at the north end of Salisbury Crags. The dolerite in this case has made its way upwards into the Cornstones there in a very irregular manner, and has consequently left a downward extension or tongue of sandstone (now altered into a quartzite) with the intrusive rocks on either side of it. The figure, traced from a photograph by Mr Fingland, of the Glasgow University, shows irregular tongues of the dolerite in the sandstone, which have evidently made their way there without causing the least mechanical disturbance. Two or three cases are seen in this example in which the dolerite has tunnelled into the sandstone, and has left an unbroken ring of the sandstone around. At the bottom right-hand side are included fragments\* of the country rock still remaining undissolved within the dolerite. The section at the foot of Salisbury Crags described by Hay Cunningham (*op. cit.*), and figured on Plate IV. of his *Geology of the Lothians*, is one of very considerable interest in the present connection. One aspect of it is represented on fig. 4, traced from a photograph by Mr Stenhouse. It shows several tongues of dolerite ending off against unbroken country rock (Cornstones). With these finger-like processes there are several protrusions of dolerite completely surrounded by the unbroken sandstone. One example of this has been detached, and is now exhibited in the Gallery of Scottish Geology and Mineralogy in the Edinburgh Museum of Science and Art, along with other examples to be referred to in detail presently. On the south side of the Queen's Park alone nineteen cases of dolerite, either ending off against unbroken rock, or else completely surrounded by it, have already been noted, and there are probably many others there, as well as more in other parts of the Park. In Hay Cunningham's treatise (*op. cit.*, Pl. III. fig. 1) is an example of the same kind occurring at the base of the intrusive basalt of St Leonard's Hill. Again, there are masses of sandstone caught up in the curious dyke-like mass of dolerite which rises into the rock of Salisbury Crags from below the Radical Road, near its western-

\* Why should these be called Xenoliths?



most extremity, and which has so often been likened to the stem of the mushroom of which the Crag forms the cap. In this sandstone there are several examples of the same nature. In connection with the dolerite sills which give rise to the beautiful scenery around Hawk Crag, Aberdour, there are many remarkable and most instructive examples of the same kind. Some are to be seen at the foot of the crag N.N.E. of the outer end of the stone pier; but the best occur just above high-water mark on either side of the base of the pier. The sedimentary rocks consist of carbonaceous shales and sandstones belonging to some part of the Oil Shale subdivision of the Lower Carboniferous Rocks. The rocks on the north side of the pier base are chiefly sandstones. The dolerite has tunnelled its way into these rocks in several places, so that it now occurs in apparently isolated masses entirely enclosed within sandstone. These are shown in fig. 5, which is from a photograph taken by Mr Stenhouse. One of these was got out, and is now exhibited in the Collection above referred to. At the roadside facing the south edge of the pier occurs a bank of shale which is traversed by at least nine small sheets and wedges of dolerite. In a generalised way this also was figured by Hay Cunningham (*op. cit.*, Pl. XIV., and here, drawn from a photograph, in fig. 7). It is an excellent example of the manner in which bands of dolerite interdigitate amongst the strata near where rapid variations in the thickness of the intruder are taking place, or near where it is dying out. Amongst these tongues or fingers are several which end off abruptly against unbroken shale. One of these, which is in the Edinburgh Museum, is shown in fig. 6; while the irregular junction of the larger mass in the east side of the harbour with the sandstone beneath, taken from one of Mr Stenhouse's photographs, is shown in fig. 8. Fine examples of this lateral passage by interdigitation of an intrusive mass into the country rock may be observed also at the west face of The Dasses, in the Queen's Park, about midway between The Washing Green and The Piper's Road. It is quite a common occurrence for sheets of dolerite (and also sills of other kinds) to end off by interdigitation in this way. A good example is that presented by the dolerite sill which forms Fair Head, on the coast of Antrim. In Geikie's *Ancient Volcanoes*, vol. ii., p. 304, fig. 317, is given a



section at Farragandoo Cliff, at the west end of Fair Head, which shows this indigitation of dolerite with the country rock in a manner which is thoroughly typical of the behaviour of sills in that respect. It will be observed that there is little evidence, if any, of mechanical disturbance. On the contrary the whole mass of field evidence seems to point to the intrusive rock having taken the place of the shale, without causing any uplift of the rock surfaces which are supposed by some writers to have been thus laccolitised. The relationship of the one rock to the other is certainly not of the kind that might be illustrated by thrusting one's fingers between the leaves of an otherwise closed book lying upon its side. The separation of the leaves forming the upper half of the book from these forming the other would in such a case bear an exact proportion to the size of the fingers thrust in; and there must in all such cases be a certain amount of curvature of the upper part, which occasions some lateral movement of the ends of the separated parts relative to their position before the "intrusion." This shortening, supposing the uplift to take place on one side only, would be proportionate at either end of the uplifted part to half the difference between the length of the arc formed by the lifted portion and half the length of the portion undisturbed. In a rock thus acted upon the adjustment to the changed lateral dimensions must occasion some mechanical disturbance. Traces of such I have never met with. In actual examples the case is rather of that kind which might happen if part of the leaves, corresponding in shape and in volume to those of the fingers thrust in, had been cut out. In the former case, supposing we are dealing with a closed book lying on its side, the outer cover would be lifted; if the case were of the latter kind, the cover might remain quite undisturbed while the fingers were pushed in. I shall adduce some further evidence in support of the view that the case last illustrated is the usual one; though there may well be some occasional exceptions to it. Fig. 9, on page 207, shows a well-known case of intrusion at Dodhead Quarry, near Burntisland Golf Course. Fig. 10, from the same quarry, is traced from a photograph taken by Professor Reynolds of Bristol, in which yet another example occurs of an intrusive rock eating its way into shales, which remain undisturbed above and below. Its position is shown by a B

on fig. 9. In this, as in the other cases cited, it is perfectly evident that the intrusive mass has not added its volume to that of the

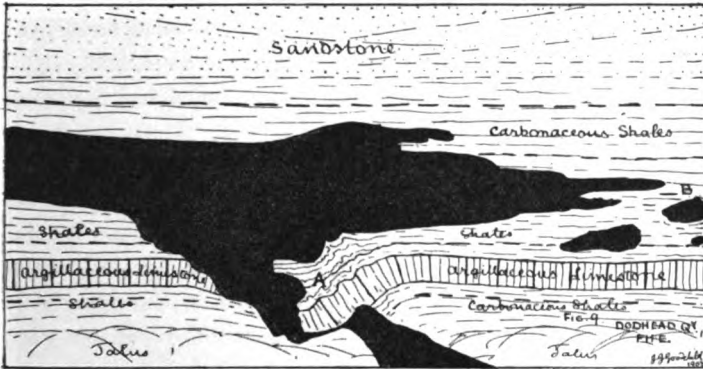


FIG. 9.—Eastern face of Dodhead Quarry, within the Golf Links, Burntisland, Fife.

The sedimentary rocks here are mostly sandstones and shales, more or less carbonaceous in character. They belong to some part of either the Oil Shale Series or to the subdivision of the Lower Carboniferous Rocks yet below that. In the lower part of the quarry occurs a thin band of a more calcareous type, which might be regarded as a finely-laminated shaly limestone. It is shown in the section by vertical ruling. Two or three sills of basic rock have been intruded into the sedimentary rocks hereabouts, and one of these, altered by the carbonaceous matter into "White Trap," traverses the quarry from the present section northwards, maintaining throughout nearly the same thickness, and keeping to nearly one horizon. In Dodhead Quarry the "trap" begins to thicken, thin, die out, and reappear, in a very irregular manner, as shown by the figure, which has been carefully drawn in the quarry from a series of photographs taken with the express object of showing the phenomena in every possible aspect, and checked on the ground by actual measurement.

It will be noticed that the distance between the limestone and the base of the sandstone remains the same at either end of the section, alike where the trap is present and where it is wanting. The evidence of replacement of the country rock by the "trap" is quite clear. There is also quite clear evidence of local displacement *below* the trap. This phenomenon sometimes occurs in the cases in which there are two sills present (as there are in the present case, the second occurring a little below).

It is presumed that the forcible injection of the magma forming the sill displaced part of the magma forming the dyke-like extension of the mass and ruptured the sediments in the manner shown. The patch marked B is separately represented in fig. 10.

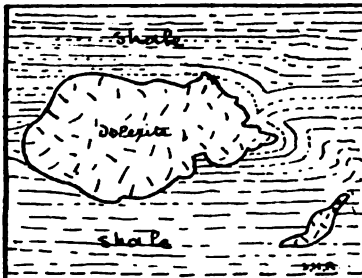
The section embodies examples of nearly all of the phenomena which usually accompany the intrusion of eruptive masses, and hence it has been selected as a TYPICAL SECTION.

country rock around it. The volume remains just the same, whether the intruder is present or not; just as the Staffordshire coal miners told Jukes was the case in their district. Evidently the older rock has been gradually removed by some means, and the newer one just as gradually introduced into its place.

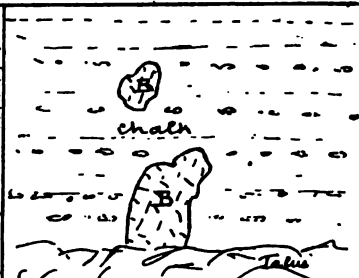
Mr Clough cited some cases in which limestone had been eaten out when the Whin Sill was being intruded. I can corroborate his statements from my own observations along the Cross Fell Escarpment, which I mapped in connection with the Geological Survey of that district. Quite recently the Berwickshire Naturalists' Club paid a visit to Dunstanburgh Castle, on the coast of Northumberland, where the Whin Sill occurs in the upper third of the Yoredale Rocks. In Queen Margaret's Cove, at that place, a mass of sandstone, capped by limestone, has been caught up in the lower part of the dolerite, and in the caught-up portion several protrusions of the Whin Sill into the limestone are clearly shown, some of which are surrounded by limestone in an unbroken condition, just as occurs in the sandstones and shales already mentioned.

Turning for the occasion to the evidence afforded by an intrusive mass of dolerite from a foreign locality, it may be mentioned that Mr Walcot Gibson of the Geological Survey of Great Britain has a photograph which shows the very uneven upper surface of a bed of dolerite which has been intruded into sandstones. This photograph has been traced, and is reproduced in outline in fig. 12. It will be observed that in this instance again there is absolutely no evidence of the beds above the dolerite being lifted, or "laccolitised," so that their dip conforms to the surface of the sandstone. On the contrary it is quite evident that one of two things has happened in this case: either the sandstone has been deposited after the dolerite, or else the latter has eaten its way into the sandstone. As there is abundant evidence of contact metamorphism in the rock in the marginal zone next the dolerite, the alternative explanation may be at once dismissed from further consideration.

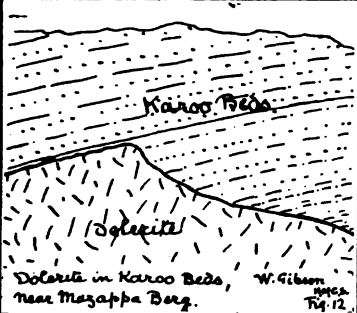
Passing now to notice cases in which the basic intrusive mass comes into contact with coal seams, beds of oil shale, of blackband ironstone, or other carbonaceous rocks, it may be men-



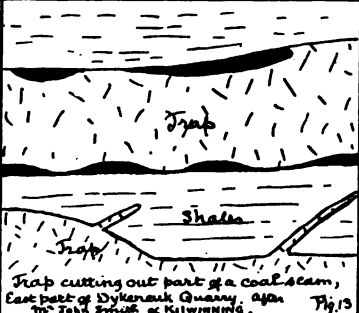
Dodhead Q<sup>d</sup>.  
From B on figure 9. Fig 10



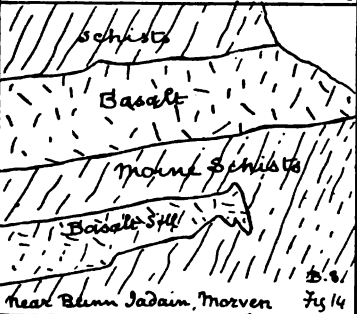
Basalt dyke in Chalk A.W.P.  
White Rocks, Portrush. Fig. 11



Dolerite in Karoo Beds, W. Gibson  
near Masappa Berg. Fig. 12.



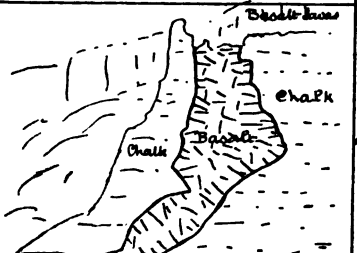
Trap cutting out part of a coal seam,  
East part of Dykerank Quarry, also  
the Tord Street of Kilmarnock. Fig. 13



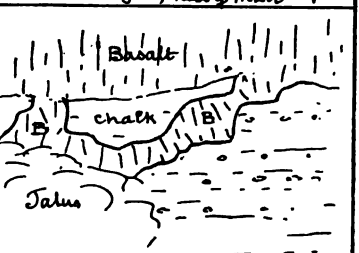
B.S.  
Near Blinn Jadam, Morven Fig 14



Schists  
For na Sealga, Ross of Mull Fig 15



M.K.A.  
Basalt traversing Chalk and Shales.  
Whitwell Quarry, Belfast Fig 16.



M.K.A.  
Basalt traversing Chalk below lamina  
Whitwell Quarry, Belfast. Fig. 17

tioned that, when this paper was read, Mr Cadell cited a case in his own collieries at Bo'ness. A bed of dolerite one foot in thickness had been intruded into a three-foot coal seam, and it left one foot of coal above and another foot below: one foot of coal had disappeared and one foot of dolerite had taken its place; the upper surface of the seam remaining three feet above the lower, just as if no dolerite were present. Mr John Smith of Kilwinning, amongst other practical men, has furnished me with a similar instance which occurs in a quarry 350 yards N.E. of Dykeneuk farmhouse. Fig. 13 is an outline taken from Mr Smith's sketch sent to me. It may be added that my colleagues Mr Grant Wilson, Dr Peach, and others have assured me that these are typical cases. Mr Dron, the author of an important work on the Scottish Coal-fields, has mentioned other cases. I would specially mention the cases illustrated by figs. 24 and 25 in the Survey Memoir on the Geology of Central and Western Fife.

Lastly, a reference may be made to two of many cases that might be cited in which a dolerite sill invades schistose rocks. Fig. 14 is traced from a photograph by Dr Bernard Stracey, F.G.S., and is from near Beinn Iadain, Morven. It shows well the abrupt termination of the sill against quite unbroken schist. The other, fig. 15, is from Torr na Sealga, Ross of Mull, from a photograph by Mr David Russell of Markinch, and a drawing made on the spot by myself.

We may now consider a few cases in which the relationship of DYKES to the country rock can be made out. The current belief in regard to these certainly is clearly enough expressed in nearly all treatises on the subject. The relationship implied in these statements may be well illustrated by taking a row of books, placed on edge and side by side, to represent the country rock, and then by intercalating other books here and there between them. This illustration makes it clear that there must be a lateral shift corresponding in amount to the aggregate width of the volumes intercalated. If a small book happens to be thrust between the leaves of a large one in the row the pages are separated from each other to an extent determined by the size of the smaller book in question, just as was illustrated by the "intrusion"

of one's fingers into a book, referred to above in connection with sills. References to the *letterpress* of almost any text-books on Geology will suffice to show that this relationship is what the authors had in mind when they wrote. Strangely enough the *figures* of dykes in these books are usually drawn in accordance with the facts, just as figures are which relate to sills or to other forms of intrusive rocks.

Out of a large number of cases a few will suffice to show that dykes generally replace their own volume of the rocks they invade. This is the case, just as it is with sills, quite irrespective of either the lithological character or the structure of either the intruder or the country rock. Fig. 18 is traced from a photograph showing the upward termination of a Tertiary basalt dyke in New Red Sandstone, near the Borough Cemetery at Belfast, and figs. 16 and 17 other dykes traversing Chalk at Whitewell Quarry, Belfast. These show an entire want of correspondence between the opposite walls of the country rock, such as could not have occurred had the dykes filled simple rents. For both of these I am indebted to Miss Andrews. Fig. 11 is taken from a photograph by Mr Vöge, showing the upward termination of a similar dyke in Chalk at the White Rocks, near Portrush. The rounded patch seen above the end of the dyke is probably the continuation of the same dyke, which has bent in its upward course, so that it passes behind the face of the cliff for a short distance. Fig. 19 shows a tertiary basalt dyke, which ends off abruptly in a remarkable melange of (Devonian) granite and Highland Schist at Torr na Sealga, in the Ross of Mull, already referred to. This locality will be referred to presently in another connection. Again, in the cliffs formed by the basalt lavas of Skye and Mull, many fine examples of the same kind are clearly laid open to view. This is especially the case in the grand range of precipices forming the cliff below Beinn an Aonidh, on the south shore of Mull, west of Carsaig. There may be seen dykes and sills of basic rocks which zigzag their way up the face of the cliff through the various beds of lava without producing the least disturbance of these volcanic rocks, and without adding their own thickness to that of the pile in which they occur. Fig. 20 shows some intrusions at Carsaig Arches, sketched from the sea.

Fig. 21 is traced from a photograph by Miss M. K. Andrews of Belfast at a quarry in the Upper New Red Sandstone of Scrabo Hill, County Down, in which some dolerite sills of Tertiary age traverse the sandstone without the intrusion being accompanied by the slightest evidence of any mechanical disturbance, or of any "laccolitisation" of the overlying strata. The sills are traversed by a later dyke, as shown.

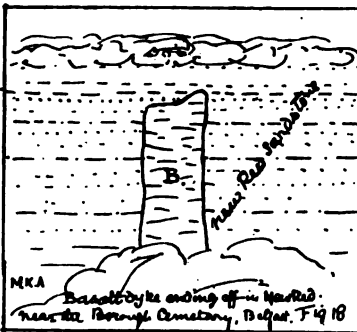
Basic dykes and sills have been considered first in relation to the country rock because they are of more common occurrence. But it can easily be shown that precisely the same inter-relation exists also in the cases in which rocks of a more acid type are concerned. There is only one acid intrusion of any size near Edinburgh, which is that of the microgranite of Black Hill in the Pentland area. This, geologically, is an intrusive mass of Devonian age, which appears to represent a subterranean mass of the more acid type of rock whose lavas form the trachytes of the Caledonian Old Red Volcanic Series of the Pentlands. It has evidently been formed at a late period in the history of the Pentland volcanoes, and has been intruded into, amongst other rocks, the conglomerate which lies at the base of the volcanic series. Close to Logan Lee Waterfall its relation to the conglomerate can be easily examined. At several places its upper surface has welded itself to the old gravel which forms the conglomerate referred to, and the union has been so firm that many patches of the conglomerate may be observed still adhering to the face of the granitic rock. At the foot of Logan Lee Waterfall the conglomerate is much hardened, and veins and protrusions of the microgranite traverse it in exactly the same manner as in the cases of the basic intrusions already described. The veins are not easily photographed, though they are readily seen on the ground. But the relationship between the one rock and the other may be seen to be of exactly the same kind as that so well illustrated by Mr Griffith Williams' beautiful photograph in the Brit. Assoc. Series (G. J. W. 603), of the case which occurs at Tan y Grisiau, in North Wales. Mr Williams kindly outlined the granite protrusions upon a print of the photograph and sent it to me, and a tracing made over these lines is given here on fig. 23. Field geologists must be fully aware that the

case cited is a perfectly typical one so far as the relation of veins of granite to the country rock is concerned. There is not the slightest evidence of any disruption of the rock invaded by the granite; but, on the contrary, it is perfectly clear that there has been, in these cases also, a concurrent removal of the country rock going on while the introduction of the material that afterwards consolidated as granite was in progress. But before passing on to consider in more detail the mode of attack followed by these acid intrusive rocks, I may perhaps be permitted to repeat the statement that the acid and subacid dykes (of Devonian age) which traverse the Ordovician and Silurian Rocks of the Kendal and Sedbergh districts, referred to at the commencement of this paper, behave in precisely the same manner as the granite veins just cited. The lamprophyre occurring at Swindale Beck, Knock, near Appleby, which was figured in Teall's *British Petrography* as a typical minette, certainly eats its way into the country rock in the manner already described in so many other cases. I have figured it in plan in the Geological Survey Memoir on Sheet 102 S.W., to which the reader may be referred.

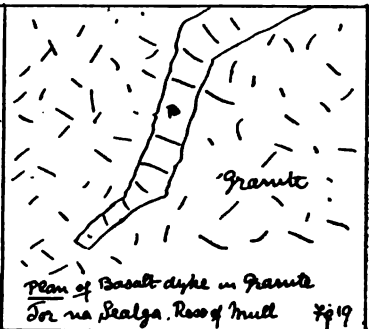
Lastly, so far as the mode of occurrence of dykes is concerned, the well-known pitchstone of Corriegills Shore, on the east coast of Arran, sends finger-like ramifications into the enclosing rock, some of which are clearly seen to terminate against the Bunter Sandstone around it in the manner already described in connection with the dykes of basalt. One specimen showing this mode of occurrence of the pitchstone is exhibited in the Scottish Collection already referred to.

Leaving this part of the subject for the present, it may be remarked here that there are some singular features about basic dykes in general which may be noticed in the present connection. These are (1) the very small proportion which their width bears to their length (and usually to their depth); (2) their wonderful uniformity of composition as a whole, which they maintain throughout the whole of their extent; (3) the remarkable parallelism of their enclosing walls as a rule; (4) the fact that the dykes most extensive in their range are those in which lime-soda feldspars predominate. Furthermore, the mode of occurrence of a basic dyke suggests that the attacking surface formed by its magma was limited to its

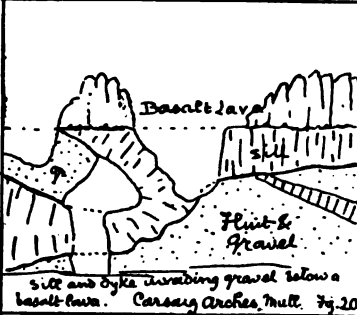




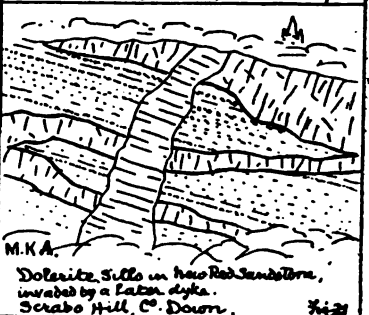
M.K.A.  
Basalt dyke ending off in Wastad  
near the Borough Cemetery, Belfast. Fig 18



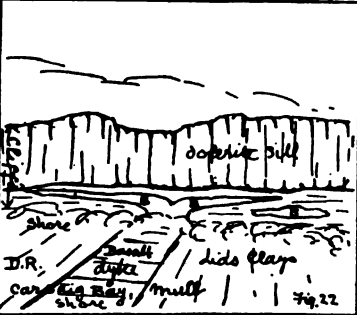
Plan of Basalt dyke in Granite  
For na Sealga, Ross of Mull. Fig 19



Skill and dyke invading gravel below a  
basalt lava. Carsraig Achea, Mull. Fig 20



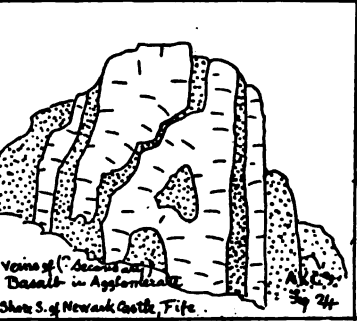
M.K.A.  
Dolerite Sills in New Red Sandstone,  
invaded by a later dyke.  
Scrabo Hill, Co. Down. Fig 21



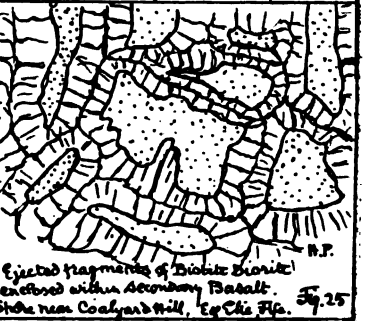
D.R.  
Small dyke  
Shale clay  
Carraig Achea, Mull. Fig 22



G.N. & M.M.  
Granite in Sandstone Strata  
San y Gwladus, North Wales. Fig 23



Veins of Secondary  
Basalt in Agglomerate  
Shore S. of Newark Castle, Fife. A.R.C. Fig 24



R.P.  
Ejected fragments of Diorite  
enclosed within Secondary Basalt.  
Shore near Coalyard Mill, Eg Chis Flc. Fig 25

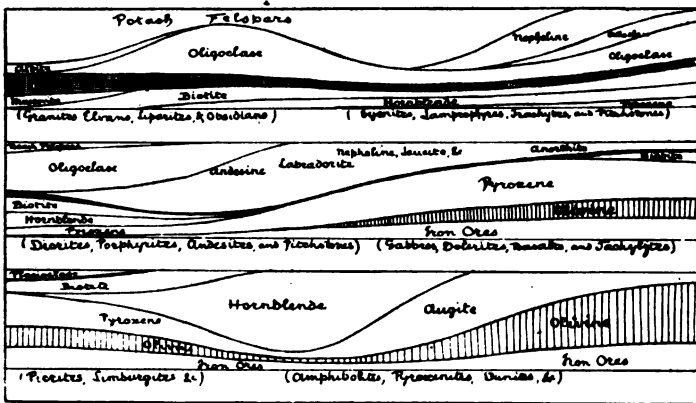


FIG. 26.

Fig. 26 has been drawn up so as to afford a conspectus of the proportions in which the Essential Minerals of the Eruptive Rocks occur in any one of the sections into which the whole lithological series can be divided. For example, taking the second band, the proportions in which the plagioclase felspars occur relatively to the ferro-magnesian silicates in any one of either the sub-basic or the basic eruptive rocks, can be estimated by comparing the distance above the thick curved line traversing the middle with that below, measured at any point along a line perpendicular to the base of the diagram. The same method can be employed in the case of any other of the subdivisions of the series.

The principle of arrangement followed is based, primarily, upon the percentage of silica present—the rocks containing highest percentage being represented at the top left-hand, and those with the lowest at the bottom right; and, secondarily, with reference to the nature of the dominant alkali, or alkaline earth, which characterises each of the compounds.

The classes of rocks formed of these components may be grouped under three primary categories, to each of which one subdivision of the diagram is devoted. At the top are represented the Mineral Combinations arising from the action of a Potash Magma upon other rocks in which the dominant alkali is Soda. The middle of the diagram includes those which are here regarded as due to the action of a Soda-Lime Magma upon sedimentary rocks. The lowest subdivision is intended to represent the products of consolidation of a Ferro-magnesian Magma. Further subdivisions, which are sometimes convenient for use, are made in accordance with the dominant substance, and are as follows: rocks characterised by minerals containing Potash, Potash-Soda, Soda, Soda-Lime, Lime-Soda, Lime, Lime-Magnesia, Magnesia.

The graphical method here employed can be used also to illustrate the proportions of each of the mineral constituents present in the Aplites (or more acid segregations of each group), as well as those of the Pegmatites and Gneisses whose composition allies them to that of their massive prototypes.

extremities, *i.e.*, to the ends and the upper side of the intrusive mass. Wedge-shaped intrusions are much less common in the case of the dykes composed of basic, or of sub-basic, materials than in those which contain potash felspars. Why this is the case is not clear.

Occasionally basic dykes are clearly seen to terminate *downwards*. Sir Archibald Geikie has lately figured some examples from Fife which are seen to do this. But all those which do so belong, I think, to a different category from the one which is here specially under consideration, and they will be considered in that connection in another paper.

It seems to be generally assumed that dykes often coincide with lines of fault. In the course of an extensive field experience I have but rarely met with cases in which it was quite clear that this was so: but as geologists of good repute say that such cases are of common occurrence, I will not press my own convictions too far. It seems to me that in many cases where a dyke has risen in contiguity to a fault of older date that the dyke is not in the least influenced by the old plane of weakness. Quite commonly, however, older dykes may deflect the course of a newer one which has cut obliquely across them, in a manner analogous to that which happens where a newer fault is "trailed" by an older one—a phenomenon quite different in its nature from the "heave" produced when an older fault and its enclosing rock are bodily shifted by a later thrust. This is only referred to here because there seems to have been some misunderstanding regarding the relative ages of two dykes of which one has gone off on one side of another dyke in a different plane from that at which the two met on the other. I have previously discussed this matter at some length in a paper on "Faults" in the *Trans. Edin. Geol. Soc.* for 1889, pp. 71–74.

There is a fine example of the influence of an older sill upon the upward course of a dyke on the west shore of Carsaig Bay in Mull. The dyke rises through Lias Shales, and on coming near to the base of the sill the dyke suddenly spreads out laterally, so as to pass on both sides into a sill, which it does, however, without coalescing with the older one, or even quite reaching it. On either side the lateral extension of the dyke thins out within a

short distance. It may well have been the case that a difference of relative temperature of the country rocks and the magma at the part where the dyke passes into the form of a sill may have had something to do with the change of direction. (See fig. 22.)

The fact just referred to suggests the question, why should the same magma eat its way in a horizontal plane at one part and at another within the same type of country rock make its way upwards in a nearly vertical plane? Possibly the answer to the question may be that the magma was injected from below obliquely upward and outward from the focus, and that its course, as a whole, has really followed the oblique direction; but as it traversed strata of very varying degrees of resistance to the thrust, the magma eats its way upwards in a zigzag manner, forming a sill on one platform, then going off as a dyke, again as a sill, and so on (see fig. 27, p. 226). The phenomenon may be illustrated by attempting to scarp a fluted surface by drawing the end of a walking-stick in an oblique direction across the flutings. The stick will run along one of the flutings, make a jump to the next, along that again in a line nearly parallel to the first one, and so on. This is what is above referred to as "trailing," which is a phenomenon of common occurrence wherever a newer set of faults crosses an older set in an oblique direction.

On the view just set forth, the abundant Tertiary dykes of North Britain may be represented by sills at no great depth below the surface, and need not be supposed to extend downwards to anything like the depth with which they are credited.

A few additional examples, out of a great many that might be selected from amongst Scottish writers on Geology, will now be referred to, in which those writers have figured the relationship which actually exists between an intrusive rock and the rocks it invades. For this purpose I give a list selected from Sir Archibald Geikie's *Ancient Volcanoes*, and his two recently-issued memoirs on the Geology of Fife; the references preceded by an asterisk are particularly noteworthy:

*Ancient Volcanoes of Great Britain*, vol. ii., Figs. 238, 241-245, 248, 249, 251, \*255, 304, \*322, 323, 329, 349, 351, 353-5, 361, 371, 380, \*381. "Geology of Central and Western Fife"

(*Mems. Geol. Survey*), Figs. 18, \*20, \*23-25. "Geology of Eastern Fife" (*Mems. Geol. Survey*), Figs. 32, 60, 62. To these reference may be made to Mr David Burns' diagrams relating to the Whin Sill which illustrates his paper in the *Proceedings N. of England Institute of Mining and Mechanical Engineers*, vol. xxvii., Plate V.

The illustrations cited relate to a considerable variety of petrographical types, of both the intruding masses and rocks invaded. They include several figures of sections in which eruptive rocks are clearly seen to cut out coal seams—not merely by altering their quality, so that they have been rendered unfit for ordinary uses, but by actually replacing the coal seams, in the same manner as many intrusive rocks occupy the place of other materials which have been removed, concurrently with the act of intrusion. As before remarked, this feature is one of considerable importance both from a commercial point of view and on account of its bearing upon the questions here under consideration.

I commend the facts above stated to the careful consideration of all unprejudiced geologists. It must be quite evident to such workers, after a study of the foregoing considerations, that the views commonly held with regard to intrusive rocks will have to be modified to a very considerable extent. That must be done, whatever view one may entertain with regard to how these facts have been brought about. It may be well to remark here that I do not wish the readers to understand that any other signs of mechanical rupture than those specially referred to do not exist; but I certainly do intend to convey the idea that such evidence is of very much less common occurrence than most people seem to believe. Furthermore, I state emphatically that even in the cases where there undoubtedly is evidence of a certain amount of displacement, the extent of that displacement is, as a rule, by no means commensurate with the volume of the rock intruded. It appears likely that the degree of viscosity of the magma on the one hand, and the resistance presented to the intrusive force on the other, are the chief factors which determine the mode of occurrence of intrusive masses. Where a viscous, or a half-consolidated, mass is being forced between imperfectly consolidated materials, and under relatively small superincumbent pressure, it is most likely that the overlying

rocks would actually lift and thus conform to the upper boundary of the intrusion. But where the magma is more fluid, and the pressure to be overcome surpasses some, as yet undetermined, amount, solution ensues, and the process becomes a physico-chemical one instead of a purely mechanical act.

At any rate, and by whatever means the process may have been carried out, I can confidently assure my fellow-workers that the replacive mode of occurrence of intrusive masses is the rule and not the exception. The belief founded upon these facts is by no means what it has lately been described—a superstitious belief entertained by ignorant miners, but is one that geologists in general will have sooner or later to accept, whether that belief is in accordance with preconceived ideas or not.

Taking it for granted that the evidence of replacement is admitted, there next arises the question as to how the missing rock has been removed. Evidence bearing upon this, and helping to furnish some kind of answer to that question, is certainly not entirely wanting. It will be found in many cases that Nature has not always finished the work of removing the rock so neatly that no trace of the mode of attack can be found. Various stages may be seen when a large number of junctions come to be examined, and by patient investigation it is quite possible to arrive at a tolerably good idea regarding the method that has been followed. A brief description of a few cases observed by myself may be given first, and to these may be added some observations made by other geologists, selected from the writings of those whose claim to be regarded as careful observers probably no one will question. Choice will be made of the phenomena at first on a large scale, and I shall choose the mode of attack followed by granite as being the most suitable for the purpose in view. One of the best examples is that presented by the marginal zone of the Ross of Mull granite. That granite rises through some ancient rocks of sedimentary origin, which pertain, I think, to the lower part of the Highland Schists. They are chiefly greywackes and flaggy quartzites which had been much affected by dynamic metamorphism long prior to the intrusion of the granite. The marginal zone is one of considerable width, and is by no means a mere line, as one is apt to suppose is usually the case. For quite

a quarter of a mile, in some parts, it is difficult to say whether the rocks should be described as schists traversed by veins of granite, or granite enveloping blocks of schist. I do not, however, mean to convey the idea by this that there is any lithological passage of the one type into the other; for that there certainly is not. On the contrary, the line between the granite and the schist is clearly seen in hand specimens to be quite sharp and well-defined, and, under the microscope, the presence of crystalline felspar on one side of the boundary line and its absence on the other can also readily be made out. The field relations of these rocks, as seen at Torr na Sealga, is shown in fig. 15 already referred to.\*

It may be remarked, in passing, that having regard to the fact that a zone consisting of closely interwoven, or spliced, granite and schist extends for a considerable distance around the granite proper, one is led to speculate what the result would be were the whole area subjected to extensive dynamic metamorphism. The granite would deform into muscovite-biotite gneiss, the plexus of granite veins and fragments of hornfelsed greywacke, quartzite, and mica schist, would form a gneissoid complex of a second kind, while the schists themselves would form a third group, the only feature common to the whole being a general parallelism of the planes of schistosity. There cannot be much doubt that many older complex areas of this kind occurring in the Highlands and elsewhere have been affected in this manner, and it may well be the case that some of the anomalous groups of gneisses and gneissoid rocks of the Central Highlands of Scotland owe much of their present character to the fact that the parent rocks were of the type seen in the marginal zones of the Ross of Mull granite.

But, to return to the consideration of the mode of attack followed by the granite in this area: what has really happened can easily be made out. The granite sends forward into the schist thin wedges of its own material, which thicken as they advance along the joints or other divisional planes, and do so at the expense of the schist. The impression one gathers from a study of numerous examples of this nature is that the whole periphery of the granitic magma exercised a corrosive effect wherever it came into con-

\* See a paper by the present author, "On a Granite Junction in the Isle of Mull," *Geol. Mag.*, dec. iii., vol. ix. pp. 447-451 (1898).

tact with the rock invaded. Hence the magma was enabled to advance along the joints and other divisional planes of the country rock. Every stage of the process can be traced, from the first insinuation of a thread, or a knife edge, of granite, through the later stages of development, where the advancing mass has widened out, and has begun to form a thick wedge, up to the point where it has eaten its way so far into the adjoining rock that the portion attacked has become surrounded by the fluid magma, and thus ready to float away as an isolated mass into what one may term the trunk stream. (Here, perhaps, it may be as well to repeat the remark that I do not entertain the belief that the fluid granite is simply so much quartzite or greywacke in a different state from what it was at first. Granite cannot be made simply out of greywacke, much less out of quartzites, for there are several important constituents present in the eruptive rock which are absent from the other.) But the advance of the veins of granite into the schists, the enlargement, ramification, and coalescence of contiguous veins, carried on until the two are closely spliced into one, can be seen in every stage of progress. Whatever may have been the particular solvent, its mode of operation is sufficiently evident from a study of the various intermediate stages in the process of, what may be termed, the mastication and assimilation of which records have been left. The process has clearly been of a physico-chemical nature, and one in which the continual subdivision of the rock undergoing attack has been effected by the erosive action of the peripheral parts of the magma. Each stage in the process of comminution has led to an increase of the area being exposed to attack, and has led, finally, to the complete solution of the fragments. I have long regarded the basic inclusions so often found in plutonic masses as incompletely assimilated portions of the country rock. This view, I am glad to notice, is now being adopted by many of the rising generation of field geologists.

Some reference has already been made to the different mode of attack followed by the more basic as compared with the more acid magmas which, by the way, I should like to refer to henceforth under the respective terms SODA MAGMA and POTASH MAGMA. The evidence appears to suggest that the soda magmas in general acted with more corrosive effects at the extremities of their masses,



while the potash magmas often appear to have possessed equal corrosive power over the whole of their surface in contact with the rock undergoing attack. A thin dyke, or a thin sill, of a basic rock, has made its way underground as a nearly parallel sheet, in some cases over an area which may be hundreds of square miles in extent, and, what is still more remarkable, it has done so notwithstanding the fact that the rock invaded was at a lower temperature than the soda magma. Had the corrosive effect been equal over the entire surface in contact with the country rock, it must be obvious that the part first invaded, that is to say, the part nearest the conduit which gave emission to the fluid magma from below—would be the parts where the intruded rock would be very much thicker than at the points near the extremities. But many intrusive sheets appear to retain nearly the same thickness for a distance of many miles. The Whin Sill, for example, varies but little from the mean thickness throughout the greater part of the extensive area it occupies. The potash magmas, on the other hand, usually give rise to short and thick lenticular masses, and it is very rarely indeed that they appear as sheets with parallel boundaries. One is, of course, reminded by these facts of the similar behaviour of basic lavas, which may flow with comparatively little variation in thickness for thirty, forty, or even fifty, miles, while a lava stream of acid composition but rarely extends more than a very few miles from its point of emission, and in many cases does not get more than a few hundred yards away from that point before it comes to a standstill. Of course the temperature of the country rock must be an important factor in this connection in the case of all intrusive masses, even in those of trappean, as distinguished from plutonic, origin. Still, the fact remains, that potash magmas erode over their entire surface, so that they tend to eat their way outward in the form of gradually-enlarging wedges. It follows that the rock surfaces on either side of one of these wedges may retain much similarity of form, and that the shapes of the opposite sides of a wedge may nearly or quite match, even though a considerable quantity of the intervening rock may have been removed.

For the information of those who may wish to examine the evidence, it may be mentioned here that the best sections where the relations of the Ross of Mull granite to the country rock can

be studied are all within easy distances of Bunessan, where the *Dunara Castle* calls twice weekly from Glasgow. There are large quarries at Camas Tuadh, Ardalanish, and other places near, and there are exceptionally fine coast sections at Carraig Mhòr and Torr na Sealga, which can easily be examined from Bunessan. Even in passing by steamer from Iona to Oban the broader features can easily be made out with the aid of a good field-glass.

On referring to the older literature of the subject I find that some of the statements here put forth regarding the granite margins had, to some extent, been anticipated by previous writers. Thus M'Culloch gives a most interesting account of the relationship between the granite of Cruachan and the schists around, which tallies in almost every respect with what I observed in the Ross of Mull (see *Trans. Geol. Soc. Lond.*, vol. iv., pp. 126 *et seq.*). Jameson noticed the same features in connection with the granite of Braemar (*Annals of Philosophy*, vol. iv., p. 419). Mr Carne has recorded similar facts around the granite of Cornwall (*Geol. Trans. of Cornwall*, vol. i., p. 22). So did Dr Davy; also Dr Boase, De la Beche, and others. But as these observers were not well acquainted with modern petrographical methods, it may be as well to add to their testimony the evidence lately put forth by one of our ablest workers in that department of science, which is accordingly subjoined.

Since my paper "On a Granite Junction in the Ross of Mull" was published, my colleague, Mr Kynaston, has mapped the area around the granite mass of Ben Cruachan, which is probably of the same age as the granite of the Ross of Mull, and, like that mass, it rises through the Highland Metamorphic Series. In the *Summary of Progress of the Geological Survey of the United Kingdom* for 1900 an outline is given of Mr Kynaston's conclusions. These are so pertinent to the subject at present under consideration that no apology is needed for quoting them nearly in full. The quotation, pp. 73-74, is as follows:

"Great difficulty was experienced in mapping out the boundary line between the granite and the schists owing to the complicated nature of the marginal area. Indeed, in some places the granite and the schistose rocks are so intermixed that no sharply-marked boundary-line can be drawn between them. . . . The contact zone

consists of a network of sills, veins, bands, and tongue-like protrusions of granite, covering a belt of mountainous ground sometimes more than a mile broad. The vein-like offshoots do not, as a rule, anastomose with one another, but tend to run in a roughly-parallel direction, coinciding with the original planes of foliation of the schists, although irregular intrusions of granite, having no apparent relation to any planes of weakness, are not uncommon. The complication is such that a line can only with difficulty be drawn between schists crowded with granite veins and sill-like bands and granite crowded with strips and inclusions of schist of every size up to a mile or more in length. . . . As we approach the main mass of the granite the schists are frequently seen to be so impregnated with granitic material that it is impossible in a hand-specimen to distinguish the igneous portion from the material of sedimentary origin. . . . In many places the schists have been broken up under the process of injection and a breccia has been formed . . . consisting of a confused mingling of altered schistose fragments in a granitic matrix . . . [Some of the] fragments are usually crowded with flakes of secondary biotite in more or less parallel layers, and are somewhat suggestive of the origin of certain ill-defined patches rich in biotite, occasionally seen in the granite." [My own remarks about these inclusions, which form a most conspicuous feature in the granites of Ballachulish, were written, but not published, before I knew that Mr Kynaston had published the note. J. G. G.]

As contact or thermo-metamorphism of the country rock must play an important part in the subsequent processes of conversion, especially in the cases where the preliminary changes have taken place under plutonic conditions, a few remarks here upon that subject may well be given. In the case of certain schists, and of some of the older greywackes, both of which may have contained mineral matter of eruptive origin before they were affected by thermo-metamorphism, there is usually some advance towards the conversion of the rock into hornfels, knotted schist, andalusite rock and the like. Radiolarian cherts have been altered into granular quartz, almost into quartzite, around the Galloway granites, and graptolitic mudstones into graphitic schist. In Mull, in Glenco, and in the Lake District, the Green Earths

which formerly occupied the vapour-cavities of the lavas have been converted by subsequent thermo-metamorphism into various forms of Epidote and the associated zeolites into Albite or other felspars. These are common effects in the areas that have been affected by thermo-metamorphic action.

But some of the most striking cases of the development of minerals by the causes which have given rise in adjacent areas to eruptive masses of deep-seated origin are to be found in the case of the metamorphic marbles which occur in various parts of the Highlands of Scotland and elsewhere. Referring to the specimens in the Scottish Mineral Collection, I find the following species occurring within the substance of these altered limestones:—Quartz, Andesine, Anorthite, Tremolite, Diopside, Forsterite, Biotite, Phlogopite, Sphene and Apatite, besides Graphite, Idocrase, Garnet, Zoisite, Wollastonite, and a variety of other minerals with which at present we are not concerned. The feature of special interest in these cases is the development within the limestone by the same causes to which the formation of eruptive rocks is due (whatever that may be), of an assemblage of rock-forming minerals which are either identical with those which characterise rocks of eruptive origin, or else are allied to them. Amongst these are Quartz, two felspars (or more than two); Tremolite, as a representative of the Amphiboles; Diopside and Wollastonite as representatives of the group to which Pyroxene belongs; Forsterite, which is closely allied to Olivine; two micas (perhaps three), and other rock-forming minerals. Yet no one seems to doubt that these minerals have been developed by metamorphic changes out of impurities which occurred within the marble. But it does not matter in the present connection whether the limestone was impure to begin with, and contained in those impurities the substances required for making the silicates referred to, or whether part of these requisites may have been introduced into the rock through the agency of the thermal waters which have been concerned in bringing about the final result. Any way, the fact is one of great importance in the present connection, and must on no account be allowed to drop out of sight.

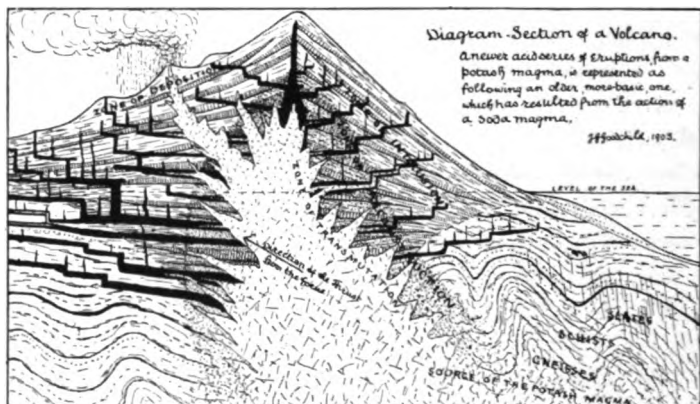


FIG. 27.

(NOTE added April 22, 1904.)

The remainder of the paper dealt with theoretical considerations, which may be summarised as follows :—

In explanation of the facts, it is suggested that four chief factors are concerned, which are as follows :—(1) Earth movements, which generate the heat required for volcanic action, and also furnish the motive power by which the magma is forced outwards from the focus. (2) The presence, at the focus of a volcano, of saline waters, whose dissolved salts become concentrated by prolonged boiling, and the consequent escape of steam at the surface. These saline solutions, operating at high pressures and temperatures, dissolve the rock in various directions around the volcanic focus, and add their own alkalis to the magma so formed. (3) An excess of alkalis (especially of soda) in the magma, whereby it is enabled to gradually extend its ramifications into the rock around its focus. (4) Circulatory movements from the extremities of the system to the volcanic focus and back, analogous to the movements of the hot water in the pipes of a heating apparatus. This circulation behaved in a manner analogous to that of the circulatory system in a tree, in which the leaves generate one set of products, and the roots carry in another, in the shape of water and alkalis. These commingle, and then travel outwards from below, to be finally left in the solid form, and thus contribute to the extension of the whole.

An ordinary sedimentary aggregate, to which the dissolved constituents of sea-water had been added, operating under high temperatures and pressures, might furnish the materials of the basic and sub-basic eruptive rocks; while the granitic materials constituting the floor of the Earth's crust could supply the additional potash and silica required for the formation of acid and sub-acid series of rocks.

It was further suggested that many basalts, and most gabbros, were of secondary origin, and that their present structure is due to changes which have originated within the core of a volcano. Some basaltic tuffs had thus been softened and reconsolidated as pseudo-massive rocks; while many basalt lavas, dykes and sills, occurring within the same zone of reconstruction, appear, in like manner, to have been softened and then recrystallised into gabbro. Most granophyric granites associated with gabbros may represent such changes carried further still, and may be due to the solvent action of a granite magma upon an older set of basic rocks (see fig. 27 above).

The bearing of these considerations upon various other metamorphic processes connected with the origin of gneisses and rocks allied thereto, was discussed in some detail.

(Issued separately, May 20, 1904.)

**Note on the Standard of Relative Viscosity, and on  
"Negative Viscosity."** By W. W. Taylor, M.A., D.Sc.  
*Communicated by Professor CRUM BROWN.*

(Read March 21, 1904.)

THE UNIT OF RELATIVE VISCOSITY.

The absolute viscosity calculated from the formula

$$\rho = \frac{\pi p r^4 t}{8 v l}$$

(where  $p$  = the pressure,  $t$  the time,  $r$  the radius,  $l$  the length of capillary, and  $v$  the volume of liquid), which connects the viscosity of a liquid with the rate of flow through a long capillary tube, is not often made use of, mainly on account of the difficulty of accurately determining some of the constants ( $r$  in particular). Further, a correction has to be made if the velocity of outflow is not sufficiently slow.\* For most purposes the viscosity is referred to that of a given liquid as standard, and is calculated from the formula

$$\eta = \eta_0 \frac{s t}{s_0 t_0}$$

where  $\eta_0$ ,  $s_0$ ,  $t_0$ , are the viscosity, density, and time of flow through a tube of a given volume of the standard liquid, and  $\eta$ ,  $s$ ,  $t$  are the corresponding data for the other liquid. Of  $\eta_0$ , Ostwald-Luther (*Phys. Chem. Messungen*, p. 260) say, "the viscosity of water at 0° C. (or at the temperature of experiment) is put = 1."

It is the general practice to take the viscosity of the solvent (whether water or other liquid) at the temperature of experiment as  $\eta_0 = 1$ . In place of this, it would be an advantage if the viscosity of water at 0° C. were taken as standard, and the relative viscosity of liquids and solutions referred to this alone.

For certain purposes, *e.g.* demonstration of the additive character of the viscosity of salt solutions, the relation between viscosity and atomic weight, or between viscosity and concentra-

\* Cf. Ostwald-Luther, *Phys. Chem. Messungen*, p. 359.

tion, where all the experiments are made at one temperature, the general practice is not inconvenient, but it has several disadvantages:—

1. It is possible, and may be desirable, to determine the viscosity of a solution at temperatures below the freezing-point or above the boiling-point of the solvent; in this case  $\epsilon_0$ ,  $t_0$  cannot be determined.

2. It affords no good way of graphically representing the relation between viscosity and temperature.

3. It may lead to misunderstanding. Most of the experiments on solutions have been made at 17° or 25° C., and a comparison of the relative viscosity of, *e.g.*, 1 n KCl is as follows:—

	Temp. 15°	Temp. 25°	
Water	1	1	
1 n KCl	0.972	1.001	
Water	0.640	0.501	} (water at 0° = 1);
1 n KCl	0.622	0.502	

from which it appears that the relative viscosity of the solution increases with increase of temperature. In this connection it may be remarked that Euler,\* referring to the influence of temperature, says,—“whilst the specific viscosity of all solutions of non-electrolytes decreases with rise of temperature, the solutions of strongly-dissociated electrolytes are affected in the opposite direction.” Without a definition of “specific” viscosity this statement might be misunderstood.

If the viscosities are referred to water at 0° as unit, it is seen that they do not increase with rise of temperature, but that they do not diminish so rapidly as the solvent; in other words, the temperature coefficient of the solution is smaller than that of the solvent, but is of the same sign. Of course, there may still be a fundamental difference between the two classes of solutions.

As to the unit, no maximum of viscosity for water is known (as there is of density at +4° C.), and there is not much to choose between water at 0° and +4°; in either case,  $S_0$  can be put = 1 without appreciable error in  $\eta$ , which is ordinarily not more accurate than one in 500 or 600.

\* *Zeit. f. Phys. Chem.*, 25, p. 536 (1898).

There is no need to determine  $t_0$  directly; the simplest way is to determine  $t$  for the solvent at the temperature of experiment, and to calculate  $t_0$  from it by means of the table of viscosity of water at various temperatures.

“NEGATIVE VISCOSITY.”

The bearing of this on “negative viscosity” (a term frequently used to denote that the viscosity of the solution is less than that of the solvent at the same temperature) is indicated below.

In general, the temperature coefficient of the solution will be (a) less or (b) greater than that of the solvent.

(a) If at a given temperature the viscosity of the solution is greater than that of the solvent, and its temperature coefficient is smaller than that of the solvent, at higher temperatures the viscosity-temperature curves will diverge, but at lower temperatures they will approach, and finally intersect at some temperature, below which “transition temperature” the solution will exhibit “negative viscosity.”

(b) If, on the other hand, the temperature coefficient of the solution be greater than that of the solvent, the curves will diverge on lowering the temperature, whilst they will approach and intersect on raising the temperature. In this case the solution will exhibit “negative viscosity” at higher temperatures.

The particular case where the solution and solvent have the same temperature coefficient needs no discussion.

Aqueous solutions of electrolytes appear to belong to group (a), and in some cases, at any rate, a solution has “positive viscosity” at one temperature and “negative viscosity” at lower temperatures, e.g. KCl,  $\text{KNO}_3$ ,\* etc.

Until quite recently no solutions other than aqueous solutions of electrolytes were known to exhibit “negative viscosity,” and on this Euler† based his explanation,—“the electric charge of the ion causes a compression (electro-striction) of the water, on account of which the viscosity is diminished.” But Mühlenbein,‡

\* Sprung, *Pogg. Ann.*, 159, p. 20 (1876).

† *Loc. cit.*, p. 541.

‡ Dissertation, Leipzig, 1901. Also Wagner, *Zeit. f. Phys. Chem.*, 46, p. 872 (1903).



a pupil of Wagner, has found that some organic substances in organic solvents do also exhibit it, *e.g.* cyanobenzol in ethyl alcohol.

In the known cases of group (a), increase of concentration raises the transition temperature: there is very little to show in what way concentration affects the transition temperature of solutions in class (b), whether decrease of concentration will lower it or not, but measurements by Rudolf\* on aqueous solutions of carbamide indicate that at 25° C. the relative viscosity decreases with dilution, and even becomes "negative," *e.g.*—

Concentration.	$\eta$ (Water at 25° = 1).
0·937 <i>n</i>	1·010
·469	1·002
·234	0·996
·117	·993
·058	·995

—but the viscosity is so nearly the same as that of water that it is not safe to base any conclusions on these data.

Increase of molecular weight, in the known cases of class (a), raises the transition temperature, and this affords another means of bringing it within the range of experiment.

The general case, where the viscosity curves of solution and solvent intersect twice, is of some interest. According as the one curve or the other represents the solution, there will be a transition from "positive" to "negative" viscosity, or *vice versa*, at both high and at low temperatures. It may not be possible to realise this case, except perhaps with a very soluble substance, and a solvent which permits of a wide range of temperature, but there should not be much difficulty in realising the particular case of it where at one extreme of temperature and concentration the one part of the curve is obtained, and the other part at the other extreme.

I hope to commence experiments, in the near future, with a view to verifying these conclusions.

\* *Zeit. f. Phys. Chem.*, 43, p. 257 (1903).

(*Issued separately June 16, 1904.*)

**The Viscosity of Aqueous Solutions of Chlorides, Bromides, and Iodides.** By **W. W. Taylor, M.A., D.Sc.,** and **Clerk Ranken, B.Sc.** *Communicated by Professor CRUM BROWN.*

(Read March 21, 1904.)

In a recent investigation on the aluminium anode, by one of us, in conjunction with Inglis,\* a striking difference was found between chloride and bromide during some preliminary experiments on the rate of solution of aluminium in sulphuric acid:—addition of a small quantity of potassium *chloride* to the sulphuric acid greatly increased the rate of evolution of hydrogen, but addition of an equivalent quantity of potassium *bromide*, under the same conditions, appeared to have no effect at all. Subsequent investigation, not yet completed, has shown that, under similar conditions and with solutions of pure hydrochloric acid and hydrobromic acid which are isohydric (have the same concentration of H<sup>+</sup>), the rate of evolution of hydrogen from hydrochloric acid is about *thirty* times as great as from hydrobromic acid. No experiments have yet been made with hydriodic acid.

Such marked differences between chloride and bromide are by no means common; so far as we are aware, the only one previously recorded is by Ostwald,† that chloride, bromide, and iodide have very different effect on the periodic dissolution of chromium in acids. Another interesting instance has since been found by Elbs and Nübling ‡—that with a lead anode and hydrochloric acid as electrolyte, a compound of quadrivalent lead is formed; but that when hydrobromic acid or hydriodic acid is the electrolyte, no similar compound is formed. It is a curious circumstance that in each of these cases the reaction is one which takes place at the

\* *Phil. Mag.* (6), 5, p. 312 (1903).

† *Zeit. für Phys. Chem.*, 35, pp. 33, 204 (1900); 38, p. 441 (1901).

‡ *Zeit. für Elektrochemie*, ix, p. 776 (1903).

surface of a metal in contact with a solution. In the paper on the Aluminium Anode (*loc. cit.*) it is suggested that the permeability of the surface film of aluminium hydroxide by Cl' and impermeability by SO<sub>4</sub>" is the cause of the differences observed between hydrochloric acid and sulphuric acid; and if this be so, differences of permeability by Cl', Br', and I' are to be expected.

As it seemed probable that similar differences might manifest themselves in other physical properties, we decided to determine the relative viscosity of solutions of chloride, bromide, and iodide under various conditions of temperature and concentration. The viscosity of solutions of potassium chloride has been determined many times at one temperature (17° or 25° C.) and one concentration (usually 1 n). Sprung\* determined the viscosity of potassium chloride, bromide, and iodide over a considerable range of temperature (5° C. to 50° C.), but at only two concentrations of chloride, and the other solutions were not at comparable concentrations. Wagner† also made determinations of viscosity of hydrochloric acid at various concentrations and temperatures. Their results are referred to later on.

#### EXPERIMENTAL.

The potassium chloride and bromide were purified by repeated precipitation from hot aqueous solution by addition of ethyl alcohol; the iodide was twice recrystallised from water. The hydrobromic acid was made by the direct union of hydrogen and bromine in contact with hot platinised tile, the gas absorbed in water, and the solution redistilled; no rubber or cork joints were used in the apparatus, so that the bromine and acid never came in contact with organic matter. The most concentrated solutions of the salts were made up by weight, and the others were prepared from them by dilution; the concentration of each solution was further checked by titration with silver nitrate. The concentrations of the acid solutions were ascertained by titration with barium hydroxide solution.

The densities were determined by means of an Ostwald-Sprengel pyknometer. The viscosity apparatus used is the form figured

\* *Pogg. Ann.*, 159, p. 1 (1876).

† *Wied. Ann.*, 18, p. 259 (1883).

and described in Ostwald-Luther (*Phys. Chem. Messungen*, p. 260). In every experiment the time of flow was observed six or seven times and the mean of all the readings taken; also, in many cases duplicate determinations were made, but no difference in the mean result was obtained except at 0° C., where a difference of 0.1-0.2 sec. in 150 sec. were obtained; the times were measured by means of a stop-watch, giving 0.2 sec.

In every case the viscosity of the solution is referred to the viscosity of water at 0° C. as unit = 1; for convenience of comparison, the viscosity of water at the temperature of experiment is added. The temperature at 15° and 25° did not vary 0.1°, but the low temperature varied between 0.1° and 0.15°, and the data are corrected to 0° C. We made determinations of the relative viscosity of water with each of the three tubes used in the other experiments, and the results given below are the means of all the five values obtained at each temperature :—

0° C.	1.000	15°	0.6395	25°	0.501
	1.000		0.638		0.501 ( <i>Thorpe and Rodger</i> ).*
	1.000		0.637		0.500 ( <i>Hosking</i> ).†

TABLE I.—*Potassium Chloride.*

Temp.	Mol. per litre.	Density.	Viscosity.	Viscosity of Water.
0°	1	1.0480	0.931	1.000
	2	1.0935	.886	
	3	1.1371	.880	
15°	1	1.0455	.622	0.640
	2	1.0901	.615	
	3	1.1333	.625	
25°	1	1.0433	.502	0.501
	2	1.0877	.507	
	3	1.1295	.517	

\* *Phil. Trans.*, 185, p. 397 (1894).† *Phil. Mag.* (5), 49, p. 274 (1900).

TABLE II.—*Potassium Bromide.*

Temp.	Mol. per litre.	Density.	Viscosity.	Viscosity of Water.
0°	1	1·0858	0·911	1·000
	2	1·1692	·837	
	3	1·2521	·815	
15°	1	1·0831	·601	0·640
	2	1·1662	·585	
	3	1·2453	·582	
25°	1	1·0804	·483	0·501
	2	1·1623	·477	
	3	1·2413	·486	

TABLE III.—*Potassium Iodide.*

Temp.	Mol. per litre.	Density.	Viscosity.	Viscosity of Water.
0°	1	1·1212	0·854	1·000
	2	1·2415	·778	
	3	1·3621	·748	
15°	1	1·1188	·583	0·640
	2	1·2365	·552	
	3	1·3552	·544	
25°	1	1·1159	·467	0·501
	2	1·2323	·458	
	3	1·3499	·459	

TABLE IV.—*Hydrochloric Acid.*

Temp.	Mol. per litre.	Density.	Viscosity.	Viscosity of Water.
0°	1	1·0160	1·020	1·000
	2	1·0327	1·041	
	3	1·0489	1·059	
15	1	1·0144	0·667	0·640
	2	1·0303	·695	
	3	1·0454	·725	
25°	1	1·0123	·529	0·501
	2	1·0278	·557	
	3	1·0426	·585	

TABLE V.—*Hydrobromic Acid.*

Temp.	Mol. per litre.	Density.	Viscosity.	Viscosity of Water.
0°	1	1·0530	0·987	1·000
	2	1·1052	·970	
	3	1·1540	·962	
15°	1	1·0512	·650	0·640
	2	1·1020	·657	
	3	1·1495	·671	
25°	1	1·0489	·514	0·501
	2	1·0990	·529	
	3	1·1460	·544	

## RESULTS.

In the first place, it may be pointed out that the value we have obtained for 1 n KCl solution at 25° is slightly greater than the viscosity of water at that temperature, whereas it is generally stated to be less than water; the value for 17·6° C. (interpolated between 15° and 25°) agrees extremely well with that given by Arrhenius. A certain amount of confusion has arisen regarding

various determinations of these data: *e.g.* Rudorf\* gives a table comparing the data for various salt solutions by Abegg,† Arrhenius,‡ and Reyher,§ stated to be for 25°; whereas Reyher's alone are for that temperature, those of Arrhenius were for 17·6° C., and those of Abegg apparently for 15° or 16°. It is not surprising that the data do not show good agreement.

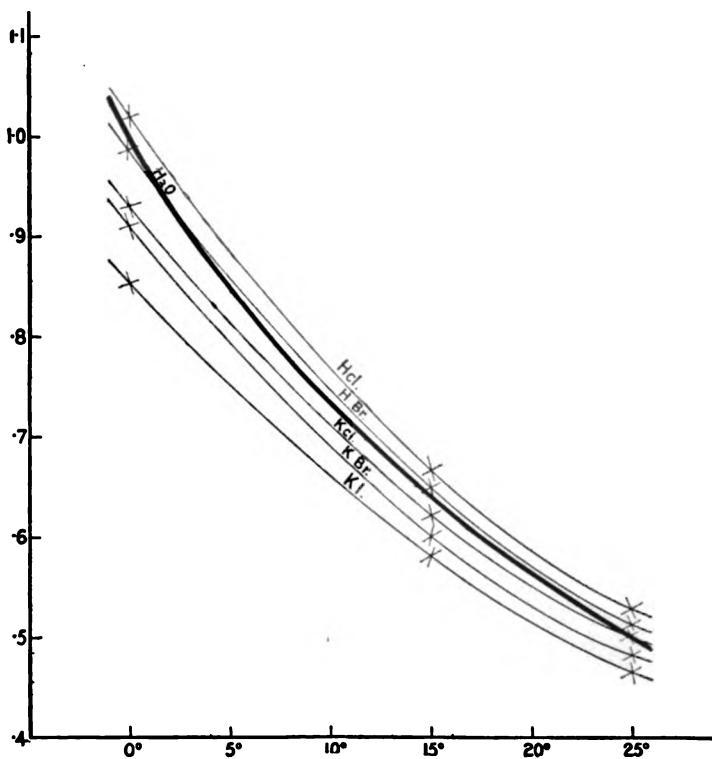


FIG. 1.—Concentration of solutions 1 mol. per litre.

The results contained in the above tables show that there is a considerable difference between chloride, bromide, and iodide, not only at any one temperature and concentration, but especially in the effect of variation of temperature and concentration on the viscosity. The experiments have, unfortunately, not been extended over a sufficient range of temperature and concentration

\* *Zeit. f. Phys. Chem.*, 43, p. 257 (1903).

+ *Ibid.*, 11, p. 248 (1893).

‡ *Ibid.*, 1, p. 296 (1887).

§ *Ibid.*, 2, p. 744 (1868).

to warrant general conclusions, but some points worthy of notice may be referred to.

*The Effect of Temperature.*—In every case the viscosity decreases with increase of temperature, but at different rates for the three salts, the rate for chloride being greatest and iodide the smallest.

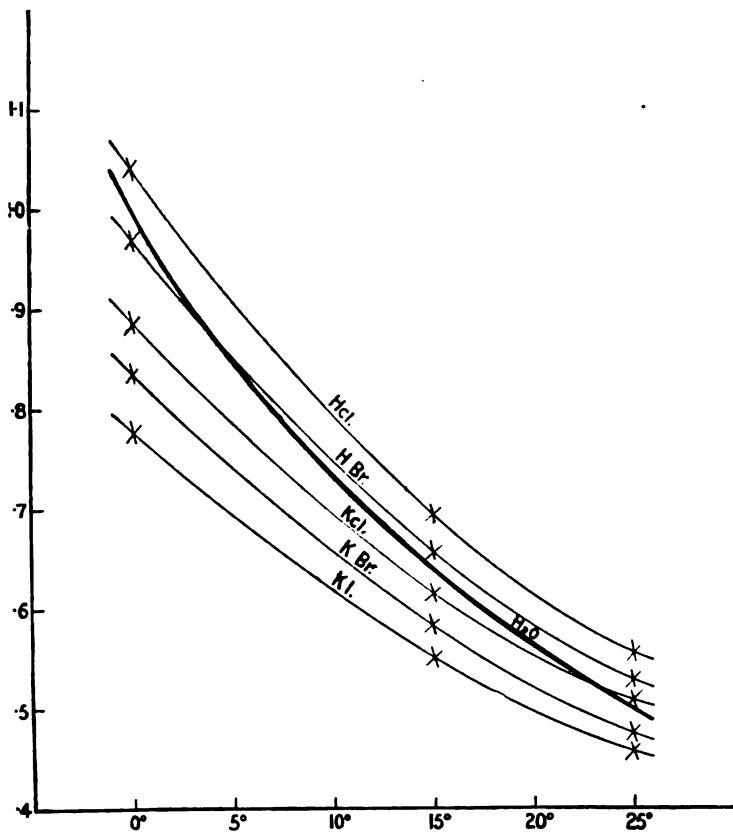


FIG. 2.—Concentration of solutions 2 mola. per litre.

It will be noticed, too, that a solution can at one temperature exhibit “negative viscosity,”\* and “positive” viscosity at another; *e.g.* potassium chloride at each of the three concentrations is “positive” at 25° C. and “negative” at 15° C., while all the

\* The term “negative viscosity” has been frequently employed to express the fact that the viscosity of the solution is less than that of the pure solvent at the same temperature.



solutions of hydrobromic acid are "positive" at 15° C. and "negative" at 0° C. (cf. figs. 1, 2, 3).

Another effect of temperature is well seen in fig. 4, in which, for the purpose of comparison, the viscosity of water at each temperature is shown by a thick black line. At 0° hydrochloric acid alone has viscosity greater than that of water at all concentrations, at 15° the viscosity of hydrochloric acid and hydro-

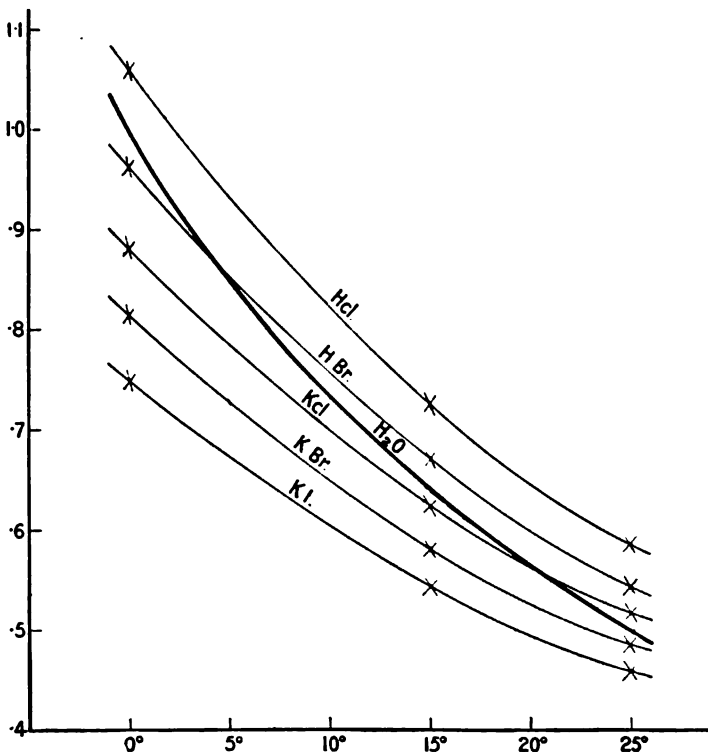


FIG. 3.—Concentration of solutions 3 mols. per litre.

bromic acid is greater than that of water, while at 25° potassium bromide and iodide still have viscosity smaller than that of water, but the one normal solution of potassium chloride has practically the same viscosity as water, though at all three concentrations it is greater than that of water. Sprung (*loc. cit.*) has shown that at higher temperatures the viscosity of the concentrated solutions becomes greater than that of water.

*Effect of Concentration.*—The effect of concentration on the viscosity depends very much on the temperature, as is seen in fig 4. The viscosity of hydrochloric acid increases with increase of concentration at all three temperatures; this is in accord with Wagner's results (*loc. cit.*).

Increase of concentration increases the viscosity of hydrobromic acid at 25° and 15°, but decreases it at 0° C. In the case of the

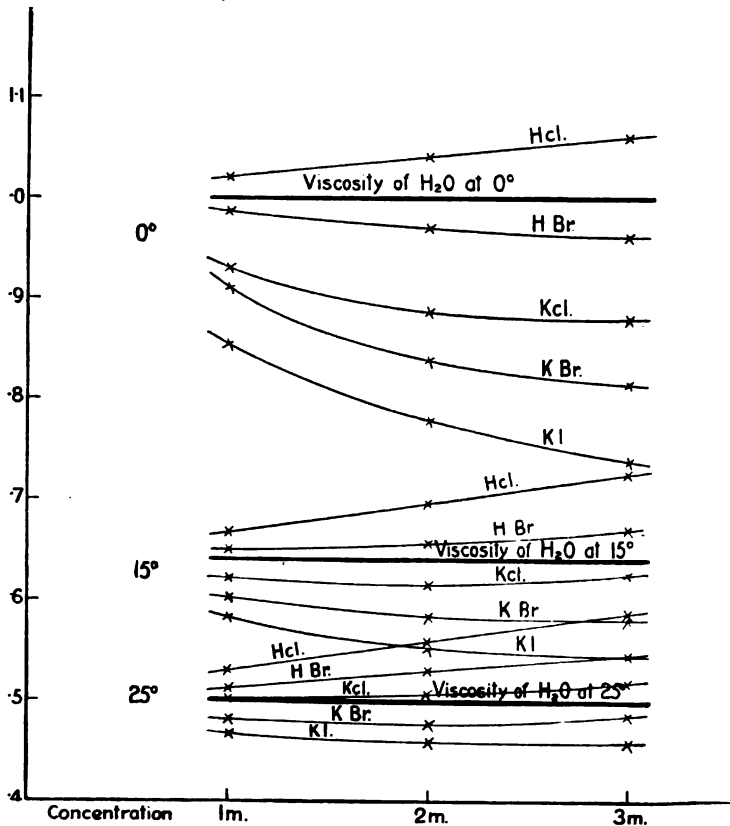


FIG. 4.—Effect of concentration at different temperatures.

salts the viscosity decreases at 0° with increase of concentration, at 15° bromide and iodide still decrease, while chloride passes through a minimum; and at 25° chloride increases, while bromide and iodide pass through a minimum. This is in agreement with Sprung's\* conclusions, qualitatively at least, as will be seen by com-

\* *Loc. cit.*

parison of his curves ; \* it is plain, however, that experiments over a much wider range of concentration are required before any satisfactory conclusions can be reached.

#### ELECTRIC CONDUCTIVITY AT 0° C.

The equivalent conductivities of dilute solutions of chloride, bromide, iodide of a metal are practically the same at 18° C., though concentrated solutions do show small differences. In order to see if greater differences exist at a lower temperature, we have determined the conductivity of all the solutions employed in the viscosity experiments at 0° C. The method was the usual Kohlrausch alternating current method, with bridge and telephone. The results are corrected for the slight variations in temperature, and the cell constant was determined by means of the value at 0° C. of 1 n KCl, as given in Kohlrausch (*Leitvermögen*, p. 204).

Whetham † has recently determined the conductivity of a number of solutions at 0° C., potassium chloride being one of them : for 1 n KCl (1·2 n was the most concentrated solution employed) he found  $\Delta = 69\cdot0$ . There is also in Kohlrausch (*Leitvermögen*, p. 199) a table of temperature coefficients of conductivity for *dilute* solutions of HCl, KCl, KI, as determined by Deguisne.

TABLE VI.

	Mol. per litre.			Equivalent conductivity.
KCl	1	...	...	65·4
	2	...	...	63·1
	3	...	...	62·4
KBr	1	...	...	68·3
	2	...	...	67·6
	3	...	...	65·8
KI	1	...	...	70·0
	2	...	...	69·5
	3	...	...	68·0
HCl	1	...	...	187·0
	2	...	...	165·7
	3	...	...	143·5
HBr	1	...	...	203·0
	2	...	...	175·0
	3	...	...	148·3

\* These are viscosity-percentage concentration curves, and are not comparable, as are viscosity-molecular concentration curves.

† *Proc. Roy. Soc.*, 71, p. 332 (1903).

The differences between the conductivity at 0° C. of equivalent solutions of KCl, KBr, and KI are very similar to the differences at 18° C. (*cf.* Kohlrausch, —*Leitvermögen*).

An explanation of the equal mobility of Cl', Br', and I' has been suggested—that molecules of the solvent may be associated with an ion; that the number of molecules associated depends on the electro-affinity of the ions; and in this case the difference in number of molecules associated with Cl', Br', and I' causes the mobilities to be the same. The formation of complexes is also referred to the electro-affinity of the element.\* Some influence of this might be expected in the viscosities of the solutions; but whether the differences observed, especially with variation of temperature, are to be connected with this, it is not possible to say; we have worked with concentrated solutions only, and possibly dilute solutions would be better for this purpose.

#### SUMMARY.

1. We have determined the relative viscosity of aqueous solutions of KCl, KBr, KI, HCl, HBr at 0°, 15°, and 25° C.; and at concentrations of 1 mol., 2 mol., and 3 mol. per litre. Also the equivalent conductivity of the same solutions at 0° C.

2. The change of viscosity with change of temperature diminishes from Cl—Br—I.

3. The effect of concentration on the viscosity depends on the temperature: it may affect the viscosity in opposite directions at different temperatures.

4. There are considerable differences in viscosity of chloride, bromide, and iodide, and especially in the effect of changes in concentration and temperature.

\* *Cf.* Abegg and Bodländer, *Zeit. f. anorg. chem.*, 20, p. 453 (1899), and Baur, *Ahren's Sammlung chem. u. chem. tech.*, Vorträge viii. No 12 (1903).

(Issued separately June 16, 1904.)

**On the Date of the Upheaval which caused the 25-foot Raised Beaches in Central Scotland. By Robert Munro, M.A., M.D., LL.D.**

(MS. received March 28, 1904. Read May 2, 1904.)

About forty-two years ago Mr Archibald Geikie (now Sir Archibald), then an energetic member on the staff of the Geological Survey, propounded and advocated the doctrine that the change in the relative level of sea and land, indicated by the 25-foot raised beaches which have been long known to geologists as fringing the winding shores of the firths of Central Scotland, took place subsequent to the occupation of the district by the Romans. Further researches, together with a more careful examination of the archæological phenomena on which Sir Archibald mainly relied as evidence, convinced later observers that the facts did not justify this conclusion. Hence for some years I have been under the impression that the post-Roman theory was abandoned, not only by the general body of geologists and archæologists, but, as I understood, by the author himself. The following statement of opinion on the subject,\* recently urged in the interests of the Trustees of the British Museum by a distinguished Professor of Geology, and one who has had exceptional opportunities of making himself conversant with all the factors of the problem, will, however, show how wide of the truth that impression must have been. Professor Edward Hull, F.R.S., said "that he was formerly Director of the Geological Survey of Ireland. The spot where the articles were found was part of what was known to geologists as a raised beach. The raised beach extended all along the north coast of Ireland, and down the east coast as far as Wicklow. In the north it was about 15 feet high, but towards the south its height was only about 4 ft. Its general character was, that it was a nearly

\* Evidence given in the recent case of the Attorney-General v. The Trustees of the British Museum with regard to the remarkable hoard of gold ornaments found near Lough Foyle, Ireland. (*Times Law Reports*, June 13, 1903.)

flat terrace, of varying width, with the old coast-line on the inland side, and a slope down to the sea on the other side. A similar formation was found in Scotland, but there the height was generally greater—about 25 feet. The Carse of Gowrie was an example. In the raised beach in the north of Ireland were found not only shells of the present period, but flint arrow-heads and other articles made of flint. In Scotland there was stronger evidence of the date of formation. There had been found skeletons of whales, and canoes, some hollowed out of single trunks, but others clinker-built of sawn planks, with holes for riveting. Iron anchors and boat-hooks had also been found in the raised beach in Scotland. The raised beaches in Ireland and Scotland were a simultaneous formation, in his opinion. The iron implements were important in fixing the date. He should say that the beaches began to be formed about the fourth century A.D. His opinion was founded upon all the sources of information available. . . . It was a disputed question when the sea retired from these beaches. The flint implements dated from the Celtic era, which might be from the second century B.C. to the second century A.D.”

Differing from Professor Hull with regard to some of the items in the above statement, more especially that the finding of cetaceous remains, canoes, iron anchors, etc., entitles him to fix the date of the upheaval to so recent an epoch as the fourth century A.D., I propose in this paper to reopen the former discussion on the subject, though to many it may seem to be slaying a dead animal. For this purpose it is necessary to go back to the early sixties of last century, when the post-Roman theory was first promulgated by Sir Archibald Geikie, whose researches were evidently the *fons et origo mali* of the Professor's statements.

In his first published essay on the subject (*Edinburgh New Phil. Journal*, vol. xiv., 1861) Sir Archibald restricted the field of his researches to the Firth of Forth. The principal evidence then adduced was the discovery by himself and Dr Young of small pieces of two kinds of pottery “in a regularly stratified deposit” in the lower reaches of the Water of Leith, which they considered to be of Roman origin. In support of the validity of this argument he writes: “Since the examination of the sand-pit at Leith I have visited all the localities along the shore where

Roman remains are known to have existed, and I have found no authentic evidence that in any way militates against the recent elevation of the land, but, on the contrary, several facts that tend to confirm it." (*Ibid.*, p. 107.) At Inveresk and Cramond all the Roman remains were, so far as he could discover, 60 or 70 feet above present high-water mark. He ridicules the tradition that some old carving to be seen on the Eagle Rock, near Cramond, and situated only a little above present high-water mark, was Roman workmanship.

"Antiquaries," he writes, "have grown eloquent at the sight of this relic of the creative genius of the old legionaries, but the carving has really about as much claim to be considered Roman as the famous prætorium of Jonathan Oldbuck. In a niche of the soft sandstone crag stands a rude figure, as like that of a human being as of an eagle, with a very short stump by way of legs, surmounted by a long and not very symmetrical body, on one side of which an appendage that may be an arm hangs stiffly down, while the corresponding one shoots away up at an uncomfortable angle on the other side. Like other carvings on the shores of the Forth (as the figure near Dysart and Queen Margaret's footstep at South Queensferry), it must take rank among the handiworks of idle peasants or truant schoolboys." (*Ibid.*, p. 110.)

By way of strengthening his theory, he further observed that the Roman wall commenced at the Hill of Carriden; that, according to the author of *Caledonia Romana*, the remains of the Roman *Portus ad Vallum* existed (near Camelon) down to the last century, and that an iron anchor was dug up in the same locality. These statements will be dealt with later on.

In restricting his observations to the valley of the Forth, the author did not then think it necessary to the truth of the conclusions of his paper "that the west coast of Scotland—as, for instance, at the termination of the Wall of Antonine—should be proved to have experienced any elevatory movements at all." However, in the following year he recurred to the subject in a more comprehensive communication to the Geological Society of London (*Journal*, March 19, 1862), entitled, "On the Date of the Last Elevation in Central Scotland," from which it will be seen that he no longer confined himself to the east of Scotland, as he included in his purview the Firth of Clyde, and, indeed, "the greater part of the British Isles."

Before proceeding to discuss the scientific value of the evidence

advanced in support of these views, it is desirable to start with a clear idea of what is meant by a 'raised beach.' In reality, the elevated portion includes not only the former sea-margin, or beach proper, but also wide patches of sea-bottom which, in course of the terrestrial process of upheaval, came to the surface, and have remained dry land since. As an authoritative description of the composition and general appearance of these beaches, I know nothing better than that which Sir Archibald has himself put on record—for in geological matters he is to be implicitly trusted. It is only when weighing archæological facts in the balance of probability that he becomes vulnerable. In the following extract he brings both parties in perfect agreement to the very core of the controversy, and admirably places before us the materials on which our keenest deductive faculties are henceforth to be exercised :—

"The Firths of Clyde, Forth, and Tay are each bordered with a strip of flat land, varying in breadth from a few yards to several miles, and having a pretty uniform height of 20 or 25 feet above high-water mark. This level terrace is the latest and, on the whole, the most marked of the raised beaches. It must have been formed when the land was from 20 to 30 feet lower than at present, and evinces an upheaval which was nearly uniform over the whole of the central valley of Scotland. What, then, was the date of this upheaval? The discovery of human remains in the sands and clays of the raised beach affords the only ground for an answer to this question. From these strata canoes, stone hatchets, boat-hooks, anchors, pottery, and other works of art have been exhumed on both sides of the island."

Sir Archibald first deals with the Clyde Canoes, and, at the outset, makes some judicious observations on the nature of the evidence to be derived from their study. "It must be borne in mind," he writes, "that the occurrence of these canoes in the same upraised silt by no means proves them to be synchronous, nor even to have belonged to the same geological period." After discussing the various degrees of technical skill displayed in their construction, he concludes that "the only evidence that remains is that which may be afforded by the character of the antiquities." But yet, in face of this well-selected and, indeed, unassailable position, he deliberately pens the following remarks as his final opinion on the evidential value of the Clyde canoes on the upheaval problem :—



"It is plain that the islanders who built this primitive fleet were not only acquainted with the use of metal, but that before they could have cut out the more highly-finished canoes they must have been long familiar with its use. They must have had serviceable metal tools wherewith they could saw an oak through cleanly and sharply at its thicker part, make thin oaken boards and planks, and plane down a large tree into a smoothly cut and polished canoe. They had advanced, too, to a high degree of mechanical ingenuity." . . . "Two of the canoes were built, not out of a single oak stem, but of planks. That of Bankton, already described, had its deals fastened to strong ribs like a modern boat; its prow was turned up 'like the beak of an antique galley,' and its whole build suggests that the islander who constructed it may have taken his model, not from the vessels of his countrymen, but from some real galley that had come from a foreign country to his secluded shores. Nor is this the sole ground for inferring that, at least at the time indicated by some of these canoes, the natives of the west of Scotland had some communication with a more southern and civilised race. How otherwise are we to account for the plug of cork? \* It could only have come from the latitudes of Spain, Southern France, or Italy. By whom, then, was it brought? Shall I venture to suggest that the old Briton who used it was not so ignorant of Roman customs as antiquaries have represented him, and that the prototype of the galley-like war-boat may have come from the Tiber to the Clyde? But whether such a suggestion be accepted or not, it is abundantly evident that the elevation of the bed of the estuary, by which the canoes have attained an altitude of sometimes 22 feet above high-water mark, cannot be assigned to the rude ages of the Stone period, but must have taken place long after the islanders had become expert in the use of metal tools." (*Journal*, p. 224.)

The above sweeping deduction, with which he brings the Clyde canoe-controversy to an end in conformity with his own views, is the weakest link in the whole chain of his arguments, as there is really no logical connection between the premises and the conclusion. Nor does it require much critical acumen to expose where the fallacy comes in. Some of these Clyde canoes have been found above, at, and below present high-water mark. In discussing the chronological problems suggested by their respective positions, it must be borne in mind that, as boats may be submerged in any depth and afterwards become silted up, their final positions afford no reliable criterion for determining the

\* One of the Springfield group had a hole in its bottom said to contain a cork plug. The Clyde canoes were found at an average depth of 19 feet beneath the surface of the ground, and about 100 yards back from the original edge of the Clyde, chiefly in a thick bed of finely-laminated sand. (*Smith's Newer Pliocene Geology*, p. 163.)

relative level of sea and land at that time. It is only when they are found in marine stratified beds above high-water mark that their final positions can have any bearing on this point. Mr Robert Chambers (*Ancient Sea Margins*, p. 206) describes the situation of the boats found under the Tontine and Trades' Lands as twenty-one or twenty-two feet above high-water in the river, but this is the only instance in which such a height has been recorded. The canoe containing a stone celt, found under St Enoch's Church, lay at a depth of 25 feet from the surface, but of course that does not indicate the height of the site above high-water level. Since the publication of Mr John Buchanan's paper describing the discovery of eighteen canoes in the bed of the Clyde, and from which Sir Archibald derived his data, seven additional canoes have been recorded from the same place, five of them being prior to the 2nd February 1869.

On that date Mr Buchanan, in an address to the Glasgow Archæological Society, made the following statement:—“The last of the five canoes was found also last summer, a little below Milton Island, near Douglas. It is 22 feet in length and about 2 feet 10 inches in breadth. The interior is well scooped out. Some interesting relics were got inside. These consist of six stone celts, an oaken war-club, and a considerable piece of deer's horn.” To what age would Sir Archibald assign this canoe? Judged by the character of the antiquities, which, according to his own dictum, is the only chronological criterion admissible, the Stone Age is undoubtedly here indicated.

It must not, however, be forgotten that canoes do not necessarily carry us back to prehistoric times, as they are frequently, if not invariably, associated with crannogs and other mediæval structures. It is therefore extremely probable that some of the Clyde fleet may have been comparatively modern. A few years ago a fine specimen of the dugout was discovered close to the site of the so-called crannog of Dumbuck, in a kind of dock of artificial construction, and just barely covered with mud. At low-water its site was exposed for several hours, but at high tide it was submerged to a depth of 8 to 12 feet. Again, some years ago four canoes were discovered in the Loch of Kilbirnie, one of which contained a lion-shaped ewer and a three-legged

pot, both made of brass or bronze—relics which, of course, relegate the canoe to late mediæval times (*Ancient Scottish Lake Dwellings*, p. 66). The canoe exposed during the excavation of the Buston crannog had been mended by boards fastened to its sides by wooden pins. A gold coin of the sixth or seventh century found in the *débris* gives some clue to the date of this crannog. (*Ibid.*, p. 206.)

As to the difficulty about the cork boat-plug, if the material really was cork, there is no valid reason why it would not have been brought to the Clyde by trading vessels in Roman or post-Roman times. Had the clinker-built boat been deposited in stratified marine sands anywhere within the substance of the 25-foot raised beach above present high-water mark, Sir Archibald's deduction would have some foundation in fact. But the record is silent on this crucial point, and only states that the boat lay keel uppermost, as if swamped in finely-laminated sands, about 250 feet back from the ancient river-margin. Its position relative to sea-level may, however, be approximately inferred from the fact that it was found near Mr Thomson's new shipbuilding yard. Allowing its depth below the surface to have been 19 feet (see footnote, p. 246), it is manifest, from the lowness of the locality, that its site could not have been much above, but possibly greatly below, the level of present high-water mark.

It is therefore quite evident that canoes were used on the Clyde, without any break of continuity, from the Stone Age down to mediæval times. But no specimen, to my knowledge, showing evidence of having been made in the Iron Age, or in post-Roman times, has been recovered in circumstances which would suggest that it was abandoned while the level of the Clyde estuary stood 25 feet higher than at present. While, therefore, the opinion that some of the Clyde canoes foundered in the Stone Age prior to the formation of the raised beach, has some foundation in fact, the inference that this change had taken place "long after the islanders had become expert in the use of metal tools" can only be regarded as a mere gratuitous assertion, unsupported by any kind of evidence.

Sir Archibald Geikie next deals with the archæological phe-

nomena of the Forth valley. He begins by giving an excellent account of the composition of the Carse lands, with a description of the whale skeletons, and the deer-horn implements found along with them. It may be mentioned that since then another deer-horn implement associated with a whale skeleton has been found, and, having fortunately come into the possession of Sir William Turner, is now carefully preserved in the Anatomical Museum of the University of Edinburgh (fig. 1). It is the only one of its kind now available for study, all those previously recorded having been lost. By Sir William's kind permission I have had the privilege of publishing an illustration of this unique object (*Prehistoric Scotland*, p. 58), from which it will be seen that it is not a harpoon, but a veritable hammer-axe, made of a portion of the beam of a stag's antler, and perforated



FIG. 1.—Hammer-axe head of stag's horn, found with a whale's skeleton at Meiklewood; near Stirling. (½.)

for a handle. Judging from their descriptive records, the other horn implements (some two or three in number), which were found associated with cetaceous remains, were evidently of the same kind, and had been used by the natives to cut the blubber from the stranded whales. "The circumstances under which these remains were found," writes Sir Archibald (p. 226), "leave no possibility of doubt that the land here has been upraised at least 24 feet, and that this upheaval has been witnessed by man. The horn weapons do not, indeed, indicate an advanced state of civilisation; yet they unquestionably prove the presence of a human population, perhaps contemporary with that which built the ruder canoes of the primitive fleet of Glasgow."

While cordially agreeing with the inferential statements in the above extract, let us note the admission that some of the Clyde canoes might have been contemporary with the whale

catastrophe in the Forth, *i.e.* when the Carse lands were still submerged—for it is not admissible to suppose that the date of elevation was different in the two localities. The fact of the matter is, that neither the whale skeletons nor horn implements have any bearing on the date of the raised beach, beyond proving that primitive races inhabited the Forth valley when the school of whales were stranded in the shallow sea which then occupied its upper reaches. Had the horn axe-head been made of iron or had worked objects of undoubted Roman origin been found along with any of the cetaceous remains, the date of upheaval would unquestionably have been brought down to post-Roman times.

The evidential materials of the Forth valley, by which the upheaval is brought within the domain of positive chronology, are thus set forth:—

“In the elevated alluvial plains of the Forth, canoes similar to some of those of the Clyde have also been found. One was dug up on the Carse, not far from Falkirk, from a depth of 30 feet. Early in the last century, too, a flood in the river Carron, which flows through the Carse, undermined a part of the alluvial plain, and laid bare what was pronounced at the time to be an antediluvian boat. It lay 15 feet below the surface, and was covered over with layers of clay, moss, shells, sand and gravel. Its dimensions were greater than those of any other canoe yet found in Scotland, for it reached a length of 36 feet with a breadth of 4 feet. ‘It was described by a contemporary newspaper as finely polished and perfectly smooth, both inside and outside, formed from a single oak-tree, with the usual pointed stem and square stern.’

“These features,” he goes on to say, “seem to harmonise well with those of the more perfect of the Clyde canoes, and to justify the inference that they were produced by the employment, not of stone, but of metal tools.

“But on the Carse of the Forth an implement of metal has actually been found, and one formed not of bronze, but of iron. It was an iron anchor, dug up a little to the south-east of the place from whence the Dunmore whale was obtained. The exact depth at which it lay is not given; it was probably about 20 feet above high-water. . . . Pieces of broken anchors have also been found below Larbert Bridge, near Camelon.

“Putting together, therefore, the archæological evidence to be gathered from the contents of the elevated silt of the Forth, the inference, I think, can hardly be avoided that not only was the upheaval effected subsequent to the first human immigration, but that it did not take place until the natives along the banks of the Forth had learned to work in metals, and

until vessels sailing over that broad estuary had come to be moored with anchors of iron." (*Ibid.*, p. 216.)

The *non sequitur* of the latter half of the above conclusion is too transparent to mislead any cautious reader, but yet, so as to leave no loophole for escape, we will consider *seriatim* the various items on which it is founded.

(1) In the absence of precise details of the relative positions of the Carron and Falkirk canoes to present high-water level, and of the general circumstances in which they were found, it would be sheer folly to draw any inference as to whether they were swamped or abandoned before or after the upheaval. If depth or thickness of the superincumbent materials be a valid criterion of age, then both these canoes must have been far older than the whale skeletons, which lay only a few feet beneath the surface of the clay. Then again, the well-known shiftings of river and estuary detritus during floods are the effects of powerful natural agencies, which at one time unearths the works of antiquity, and at another buries those of modernity under fathoms of gravel and mud.

(2) The story of the iron anchor said to have been discovered near the site of the Dunmore whale skeleton is thus recorded by Mr Keddoch in a letter to Professor Jameson (*Edin. Phil. Jour.*, vol. xi. p. 416):—

"Many years ago an iron anchor was dug up a little to the south-east of it (the whale skeleton). The fleuks (*sic*) were much decayed, but the beam, which was of a rude square form with an iron ring, was tolerably perfect. It hung many years in the old tower near Dunmore, but was at length stolen. Dunmore Moss extends a great way to the south-west, and in it, at about 300 yards from the skirts of the wood, are found the roots of large oaks."

From this record we have no certainty that the writer had ever seen this anchor, or examined the conditions under which it had been found, so that he is merely repeating hearsay evidence. We are informed that the skeleton of the Dunmore whale was 200 yards from the then bed of the Forth, so that "a little to the south-east of it" would be in the direction of the river; but it would be useless to speculate on the precise distance. From the constant shifting of the windings of the Forth, there is nothing very improbable in the discovery of a small anchor belonging to a comparatively modern boat in this raised beach. Such anchors are

not usually thrown in deep water, like those of large vessels, but on the shore, and one might have been easily lost and buried in the mud during a storm. At any rate it would be a violation of the rules of scientific archæology to admit such vague statements as evidence that the raised beach was formed after iron anchors came to be used in the Forth, or that this particular one had any chronological relationship with the "Dunmore whale."

(3) The chronological value of the pieces of anchors found below Larbert Bridge may be estimated by the perusal of the following extract from Nimmo's *History of Stirlingshire*, one of the authorities quoted for the statement :—

"After the river hath left the village and bridge of Larbert, it soon comes up to another small valley, through the midst of which it hath now worn to itself a straight channel, whereas, in former ages, it had taken a considerable compass southwards, as appears by the track of the old bed, which is still visible. The high and circling banks upon the south side give to this valley the appearance of a spacious bay; and, as tradition goes, there was once an harbour here. Nor does the tradition appear altogether groundless; pieces of broken anchors have been found here in the memory of people yet alive, and the stream-tides would still flow near the place, if they were not kept back by the great damhead built across the river at Stonehouse. There is reason, too, to believe that the forth flowed considerably higher in former ages than it does at present; so that there is no improbability in supposing that at least small craft might have advanced thus far. In the near neighbourhood of this valley stands the ruins of ancient Camelon, which, though we have no ground to believe that it ever had possessed that degree of extent and splendour which some credulous authors mention, yet might be inhabited by the natives of the country for several ages after it was abandoned by the Romans." (Page 73, 2nd ed.)

Of all the explanations that might have been offered as to how small anchors came to be dropt in a locality to which even now the tides reach, the hypothesis that the level of the sea was then 25 feet higher than at present is surely the least satisfactory. Would it not be more rational to suppose that in earlier times the embouchure of the river Carron was more inland, and that consequently the tides flowed further up? \* Is there no allowance to be made for the accumulation of the detritus brought down by

\* On referring to the Ordnance map, I find the highest point to which the ordinary spring tides now flow is at a sluice in the Carron Ironworks, from which Camelon is less than a mile distant.

its floods during so many centuries? Besides, the flowing of the tides 25 feet higher would by no means help to explain the position of the anchors, as it is more likely that they would be lost on the shallow margin of a tidal river than in a depth of 25 feet of water.

As a preliminary to the discussion of the more important archæological phenomena of the Firth of Tay, Sir Archibald points out, in the words of Mr Robert Chambers, that "along the Carse of Gowrie many of the hillocks and eminences which rise above the general level of the plain bear names in which the Celtic word *inch* (island) occurs; such as Inchyra, Megginch, Inchmichael, Inchmartin, Inchsture—as if a primitive people had originally recognised these as islets in the midst of the shallow firth." (*Ancient Sea Margins*, p. 18.) To this is added the evidence of tradition to the effect that the Flaw Craig and the rock on which Castle Huntly stands bore iron rings, to which ships were fastened when the sea covered the surrounding carse lands. Finally, we have the following statement of the discovery of specific objects of iron, to which the author seems to attach great importance:—"Between 60 and 70 years ago a small anchor was dug up, not many feet beneath the surface, on a piece of low ground near Megginch (*N. St. Act.*, "Perthshire," p. 378). Mr Chambers refers to another anchor as having been met with in casting a drain below the Flaw Craig (*Ancient Sea Margins*, p. 19). But the most important and the most carefully investigated relic yet discovered in the district was an iron boat-hook (fig. 2), found in 1837 by some workmen on the farm of Inchmichael." (*Ibid.*, p. 19; and *N. Phil. Journal*, 1850, p. 233.)

It is not surprising that the discovery of such an array of relics associated with early navigation, especially when brought before us by so skilled a writer, should carry some weight with general readers. It is therefore all the more necessary to inquire what their chronological value may be.

With regard to the philological argument that the Gaelic word *inis* (an island) appears in the composition of several place-names in the Carse of Gowrie, it will be sufficient to observe that its English equivalent, *inch*, has often been applied to low-lying meadows near water, such as the North and South Inches in the



town of Perth, which never were islands. The story of the existence of iron rings in the adjacent rocks for the mooring of boats wants the essential link of an eye-witness to make it admissible as an argument in this inquiry. There remain, therefore, to be seriously considered the circumstances under which the two anchors and boat-hook were discovered.

The Megginch anchor is thus referred to by the author of the article on "Perthshire" in the *N. St. Act. of Scotland* (p. 378):—

"The writer has conversed with a man who told him that he recollects distinctly of hearing his father state that, at a period of

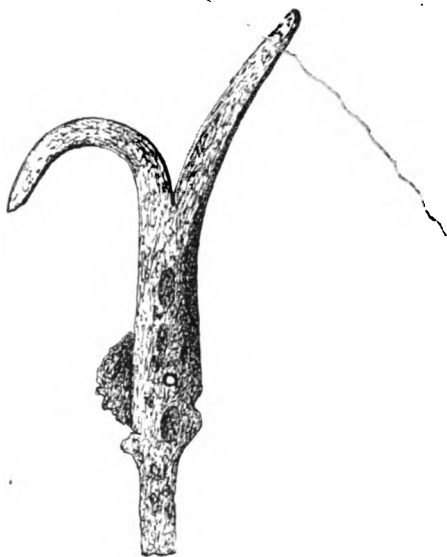


FIG. 2.—Boat-hook of iron, found in Carse of Gowrie. (4.)

about forty years ago, the latter was engaged in digging in a piece of very low ground on the estate of Megginch, not many feet beneath the surface, when he and his fellow labourer found a small anchor, the figure of which was tolerably preserved, but which mouldered down or went to pieces when lifted."

The discovery of the other anchor and the boat-hook is recorded by Mr Robert Chambers (*Ancient Sea Margins*, p. 20):—

"In the same district, which is fully a mile from the margin of the firth, a boat-hook was discovered 8 feet below the surface,

sticking among the gravel, as if left by the tide on the sea-shore. This relic has been preserved by the farmer who found it.\*

"I am also assured that what was considered as the remains of an anchor were found some years ago in casting a drain below Flaw Craig, a cliff which overlooks the Carse, between Kiinnaird and Fingask."

Mr Chambers takes the precaution to state that for these remarks, and others which followed, he quotes from "a letter from a lady, the daughter of one of the chief proprietors of the Carse." Subsequently, however, owing to the importance of the subject, he recurs to it (*Edin. Phil. Journal*, vol. 49, p. 233, 1850), and informs us that he "took some trouble to ascertain the precise local and geological circumstances of the relic, as observed at the time of the discovery.

It is unnecessary to epitomise the result of this inquiry, the upshot of which was that the spot where the boat-hook lay was 8 feet below the surface, 20 feet above the level of present high tides, and about a mile distant from the estuary of the Tay. It is advisable, however, to quote the following incidental remarks, which seem to contain the germ of a more natural explanation of its presence in the locality than that of Sir Archibald Geikie.

"One important feature of the Carse in this district is now to be adverted to, namely, a trench or ditch in which a little rill crosses the plain obliquely to join the estuary in one of those creeks locally called *pows*. The distance of this rill is not more than 150 yards from the spot where the boat-hook was discovered. It is, in these days of high cultivation, a narrow ditch of well-defined sides, but no one can doubt that in other times it would comprehend a wider space. Now, the bottom of the ditch at this place is so little above the level of the sea that an abnormal tide might reach it."

After describing several instances of great floods Mr Chambers writes:—"With such events as those on record, within the period

\* This object (fig. 2) is now in the National Museum of Antiquities, Edinburgh, and consists of a socketed spike, 11 inches in length, from the middle of which the hook curves backwards. The socket is formed by the backward folding of the iron, the edges only partially meeting, and in it the handle was fixed by a rivet. From its appearance, it might belong to comparatively recent times.

during which iron implements have been in use, it does not appear very difficult to account for the loss and embedding of the Inch-michael boat-hook, without calling any greater geological forces into operation in the case."

Mr Chambers' idea, that a flood might account for the stranding of the boat-hook, was opposed by Sir Archibald Geikie, on the ground that the effects of a storm would not adequately explain the geological phenomena. "We can hardly conceive," he writes, "the sea rising upwards of 28 feet above high-water mark, and flowing for more than a mile inland; still less can we believe that, if it did so rise, it could deposit 8 feet of sediment over the surface of the Carse." But, waiving the intervention of a flood, is there anything very improbable in the supposition that the *pow*, described by Mr Chambers as little above present sea-level, was formerly sufficiently deep, either by natural or artificial means, to admit of a boat being rowed to the spot? Before the days of railways, harbours, and piers, trading vessels were beached on convenient places for the purpose of loading or unloading their cargoes. But surely it is unnecessary to discuss the possible ways in which such a portable object as a small boat-hook might have got strayed. The suggestion that it was lost by a sporting sailor in a wild-boar hunt is as feasible an explanation as that it was dropt from a sailing-vessel while the Carse lands were still submerged. But whatever the true explanation may be, there can be no doubt that this boat-hook is a relic of post-Roman times, and probably much nearer the present day than the Roman period.

Sir Archibald's next and final argument in support of his thesis is the relative positions of the ends of the Wall of Antoninus to the high-water marks in the adjacent estuaries. It is thus presented to us:—

"Mr Smith of Jordan Hill was the first to assert that since the Antonine Wall was built (about A.D. 140) there could have been no change in the relative position of sea and land, inasmuch as the ends of the wall were evidently constructed with reference to the existing level (*Mem. Wern. Soc.*, viii. p. 58, and *Edin. New Phil. Journal*, vol. xxv., for 1838, p. 385). This statement has been the foundation of all the subsequent geological arguments as to the long period at which the British Isles have been stationary. If it be true, then we must allow that the upheaval, of which the evidence has been adduced in the present communication, is referable

to a period certainly previous to the Roman invasion. If the statement be erroneous, the other alternative remains, that the upward movement may have been wholly or in part effected after the Roman invasion.

“After carefully examining both extremities of the wall, and reading the narratives of the various antiquaries who have treated of the Roman remains in Scotland, I have no hesitation in affirming that not only is there no evidence that the wall was constructed with a regard to the present level of the land, but there is every ground for believing that it was built when the land was at least 20 feet lower than it is at present. To begin with the east end: from the Avon, west of Borrowstounness, eastward to Carriden, the ground rises from the old coast line as a steep bank, the summit of which is from 50 to 100 feet above the sea; between the bottom of this abrupt declivity and the present margin of the Firth there is a narrow strip of flat ground, about 200 yards broad, on which Borrowstounness is built, and which nowhere rises more than 20 feet above high-water. It is a mere prolongation of the Falkirk *caes*, already described, and beyond doubt formed the beach where the sea broke against the base of the steep bank. Now the Roman Wall was carried, not along this low land bordering the sea, but along the high ground that rose above it. The extremity at Carriden, therefore, instead of having any reference to the present limit of the tides, actually stood on the summit of a steep bank overhanging the sea, above which it was elevated fully 100 feet. If the land here were depressed 25 feet, no part of the wall would be submerged. The only change on the coast-line would be in the advance of the sea across the narrow flat terrace of Borrowstounness and Grange, as far as the bottom of the abrupt declivity.

“The western termination of the Antonine Wall stood on the little eminence called Chapel Hill, near West Kilpatrick, on the north bank of the Clyde. Between this rising ground and the margin of the river lies the Forth and Clyde Canal, the surface of which is 20 feet above high-water mark, and the base of the hill at least 5 or 6 feet higher. Hence the wall terminated upon a hill, the base of which is not less than 25 feet above the present level of the sea. In making the canal, a number of Roman antiquities were found at various depths in the alluvium: these seem to have been part of the ruins from the fort above. If we admit that the wall was constructed previous to the last elevation of the land, we see a peculiar fitness in the site of its western termination. The Chapel Hill must, in that case, have been a promontory jutting out into the stream, and at high-water the river must have washed the base of the Kilpatrick Hills—a range of heights that rise steeply from lower grounds, and sweep away to the north-east. Hence, apart altogether from considerations dependent upon the strategic position of the hills, which were infested by the barbarians, we obtain an obvious reason why Lollius Urbicus ended his vallum at Old Kilpatrick.”—(*Ibid.*, p. 228.)

For the purpose of homologating these views, he quotes passages from the writings of various antiquaries, the most pertinent of which are the following:—

“If the Falkirk carses were not entirely overflowed in the time of the Romans, it is probable at least that they were then salt-marshes, subject in some degree to temporary inundations in high spring tides.” (Roy, *Military Antiquities*, book iv. c. iii. sect. 2.)

Mr Stuart, author of *Caledonia Romana* (p. 177), declares his belief that “the whole of this lower district (towards the mouth of the Carron) had in all likelihood been covered by the sea when the Roman forces occupied the Wall of Antonine. It is likewise probable that the entire plain between Inneravon and Grahams-town (that is, the whole of the Falkirk carse) was at the same period subject to the influx of the tide, which may even have penetrated the deeper hollows of the Carron as far up as Dunipace.”

In a footnote at the end of his long communication, Sir Archibald writes as follows:—

“I have not deemed it necessary to increase the length of this communication by controverting the alleged Roman origin of certain roadways and other traces of art, found along the present coast-line at a height of less than 20 feet above high-water mark. The causeway of logs, for instance, which crossed a part of the Kincardine Moss, in the Carse of Stirling, is commonly spoken of as Roman, but this is mere conjecture. The bronze vessel found in the same moss, and cited by some writers as a Roman camp-kettle, is most certainly of ancient British workmanship.”

The final conclusions drawn from these elaborate investigations are thus stated:—

“Putting together all the evidence which the antiquities yet discovered along the Scottish coast-line afford as to the date of the last upheaval of the country, we are led to infer that this upheaval must have taken place long after the first human population settled in the island—long after metal implements had come into use, after even the introduction of iron; and reviewing the position and nature of the relics of the Roman occupation, we see no ground why the movement may not have been effected since the first century of our era; nay, there appear to be several cogent arguments to make that date the limit of its antiquity” (p. 232).

The publication of Sir Archibald's essay naturally attracted attention. His theory as to the date of the 25-foot raised beach was accepted by some of the leading geologists and archæologists of the day, among whom were Sir Charles Lyell (*Antiquity of*

*Man*, 3rd ed., p. 50 *et seq.*)\* Sir Daniel Wilson (*Prehistoric Annals*, vol. i. p. 38), and Professor Ramsay (*Geology and Geography of Great Britain*, p. 251). On the other hand, several local geologists raised objections on various grounds to the validity of some of his arguments. Mr Alexander Bryson, F.R.S.E., contended that the so-called Roman pottery from the Leith sand-pit were merely fragments of dishes made, within the memory of living persons, at a Portobello manufactory, and of glazed flower-pots which skippers were in the habit of bringing from Holland to adorn their parlour windows (*Proc. Roy. Phys. Soc.*, vol. iii. p. 284). In 1873 David Milne Home, Esq., successfully controverted his deductions from the height of the ends of the Antonine Wall above present sea-level (*Trans. Roy. Soc. Edin.*, vol. xxvii.)—a result mainly due to the discovery in 1868 of a Roman sculptured tablet which definitely fixed the eastern termination of the wall to be at Bridgeness, and not at Carriden, as was generally supposed when Sir Archibald wrote his paper.

Mr Home's chief argument was that the position of the tablet at Bridgeness proved that the Antonine Wall terminated so close to the sea as to preclude the idea that, when that wall and tablet were inserted, the land could have been 25 feet lower than now. The spot where the tablet was found was exactly 19 feet above ordinary spring tides, and at the place where it lay there was a quantity of squared stones in a confused heap, some of which bore the marks of masons' tools, evidently forming part of the wall in which the tablet had been fixed. At the point, and only one or two feet above present high-water mark, a portion of a building was discovered, a few yards in length, consisting chiefly of large whinstone boulders. The line of this building pointed towards the place where the tablet was found, "so that if the building had continued on the same line, it would have passed through or near the site of the tablet." The effect of these discoveries on the post-Roman theory of the upheaval is thus stated:—

\* It appears that Sir Charles Lyell, in consequence of the articles of Mr Milne Home, abandoned the post-Roman theory, and accordingly his remarks on the subject were deleted from the fourth edition of his *Antiquity of Man*. *Trans. of the Roy. Soc. Edin.*, vol. xxvii. pp. 39-41.

"If the land was then twenty-five feet lower than now, then the tablet, and the wall in which it was fixed, must have been six feet under the sea at every tide, and must also have been so exposed to the beating of the waves that neither tablet nor wall could have stood many weeks. It is impossible to suppose that the tablet, with elaborate sculpturing, and bearing a dedication to the emperor, could have been set up in such a position. Moreover, the neck of land which joins the ness or knoll to the mainland being only twenty-three feet above high-water, must have been submerged and exposed, so that any wall or rampart on that neck would soon also have succumbed to the waves. Then there is the old building at the point of the ness, which, if Roman (as it appears to be), must have been *at all times* under water, even at the lowest tide, were Professor Geikie's theory correct." (*Trans. Roy. Soc. Ed.*, vol. xxvii. p. 45.)

In criticising Sir Archibald Geikie's speculative deductions, founded on the geological and archæological phenomena connected with the western termination of the Antonine Wall on the top of Chapel Hill, Mr Home thus expresses himself:—

"If the Roman antiquities here mentioned (see page 257) be the same as those described in the *Statistical Account*, their position is not correctly stated by Professor Geikie. They can in no sense be represented as having fallen from the fort above. The relics were found, not (as he says) at various depths in the alluvium, but *in a subterranean recess—i.e.* in a cavity which contained them. As there were vases as well as coins, the probability is that it was a grave. Now, as this recess, when formed, must have been several feet below the surface of the ground, and as the surface of the ground is admitted to have been only twenty feet above the present high-water mark, the 'recess' must have been at least seven or eight feet under the sea if, during the Roman occupation, the land was twenty-five feet lower than now." (*Ibid.*, p. 48.)

Hitherto my chief rôle in this controversy has been to meet the statements and logic of the advocates of the post-Roman theory with a *non sequitur* on all the points raised—of course utilising for this purpose the arguments advanced against it by previous writers on the subject. Henceforth, however, I become a direct supporter of a theory about these beaches which I have elsewhere formulated, and which for distinction may be called the pre-Roman theory, viz., that the upheaval took place "subsequent to the appearance of man in the district, but *prior* to its occupation by the Romans." This was the conclusion come to in an address which, as president of the Antiquarian Section of the Archæological Institute, I gave at Lancaster in 1898 (*Journal*, vol. 55, pp. 259-285).

In looking about for positive evidence in support of the pre-Roman theory, we shall first of all deal with the wooden roadway and the so-called Roman camp-kettle, which Sir Archibald Geikie did not think of sufficient archæological value to be discussed among the evidential materials from the Forth valley.

Nothing can be more certain than that the chronological sequence in the physical phenomena of the Forth valley was sea, forest, peat, and modern cultivation—the last stage being due to the removal of the peat by the hand of man. Now, objects of human workmanship which happened to be lost or abandoned in these woods became ultimately covered over with peat, and so were less liable to the ordinary processes of decay. Hence such relics, when recovered in these circumstances, are often in an excellent state of preservation. Of the condition of the peat mosses of Kincardine and Flanders towards the end of the eighteenth century, we have a good account by the Rev. Christopher Tait, minister of the parish of Kincardine (*Trans. Roy. Soc. Edin.*, vol. iii.), from which the following is an interesting extract:—

“The trees are oak, birch, hazel, alder, willow, and in one place there are a few firs. Among these the oak abounds most, especially on the west side of the moss, where forty large trees of this species were lately found lying by their roots, and as close to one another as they can be supposed to have grown. One of these oaks measures 50 feet in length and more than 3 feet in diameter, and 314 circles or years' growths were counted in one of the roots.” (*Ibid.*, p. 272.)

He further observes that the trees were not blown down, but cut about 2 feet from the ground. “The marks of an axe, not exceeding 2½ inches in breadth, are sometimes discernible on the lower ends of these trees.”

The Roman roadway is thus described:—

“That a people more civilised than the ancient Caledonians must have been in this country before the moss of Kincardine existed is completely established by the discovery of a road on the surface of the clay at the bottom of that moss, after the peat, to the depth of 8 feet, had been removed. The part of this road already discovered is about 70 yards long; the breadth of it is 4 yards, and it is constructed of trees measuring from 9 to 12 inches in diameter, laid in the direction of the road. Across these have been laid other trees about half their size, and the whole has been covered with brushwood. The depths of the materials varies in conformity to the nature of the soil; the trees, which are laid



lengthwise, being generally on the surface of the clay, but in the lowest and wettest parts they are sunk about 2 feet under the surface.

"This road lies across a piece of ground lower than the adjacent grounds, and its direction is from the Forth across the moss, where it is narrowest, towards a road, supposed to be Roman, that passes between the moss and the river Teith. The vestiges of this last road have been traced, from about four miles north-west of the Bridge of Drip, where formerly there was a ford across the river, south-east of Torwood and Larbert, to Camelon on the wall." (*Ibid.*, 276.)

The significance and bearing of this road on the upheaval question is concisely stated by Mr Milne Home as follows:—

"The tide now comes up to Craigforth, which is about half a mile below Drip, and with a fall of only 4 feet between the two points. If, therefore, the land was during the time of the Romans 25 feet lower than now, neither the Drip Ford nor any river could then have existed, for the whole country west of Stirling must have been covered by the sea, even at the lowest spring tides." (*Ibid.*, vol. xxvii. p. 49.)

The finding of portions of similar roadways in Flanders Moss is noticed by several writers of the period. One such structure, described as having logs lying across each other like a raft, with a general direction from south-east to north-west, is supposed to have been a branch of the Roman way from Camelon.

The general evidence, over and above tradition, which associates these roads with the incursion of the Romans into the valley, has, in my opinion, considerable weight, certainly more than can be expressed by the words "mere conjecture." Historians are almost unanimously of the opinion that the march of the soldiers of Agricola to the estuary of the Tay was from Camelon, *via* Stirling, Dunblane, Ardoch, and Stratherne; in which case the most convenient place to cross the river Forth would be a few miles to the west of Stirling (as shown on the map in Gordon's *Itinerarium Septentrionale*), and just in line with the wooden causeway in the Kincardine Moss. In support of this view the following fact is worth mentioning. It will be recollected that the Rev. Mr Tait, in noticing the cutting marks on the felled trees found in the Kincardine Moss, describes the axe cuts as not exceeding  $2\frac{1}{2}$  inches in breadth. Now it is very significant that the only iron axe-head found in the Ardoch camp, during its recent exploration by the Society of Antiquaries, measured  $5\frac{3}{4}$  inches in

length by  $2\frac{1}{2}$  inches across its cutting face (*Proc.*, vol. xxxiii., fig. 14, p. 463).

If it be true, then, that when the Romans invaded Scotland, towards the close of the first century A.D., the areas subsequently covered by peat within the 25-feet raised beach were then occupied by great forests, it is but natural to suppose that objects lost in these forests would be recovered, in modern times, in course of the operation of removing the peat, so as to convert the rich clays underneath into arable land. On this point Mr Milne Home writes:—"Stone hatchets and other stone implements of a very primitive people have been found also on Blair-Drummond estate, lying on the surface of the carse clay, after the peat moss lying above it was removed. These implements were, as I understand, in localities below or within the line of the old sea-cliff, and not very far from where the Blair-Drummond whale was found. I have seen three of these implements: one was in the Macfarlane Museum, Stirling; the other two in the possession of the late Mr Home Drummond, who showed them to me at Blair-Drummond in September 1863." (*The Estuary of the Forth*, p. 116.) This would seem to show that the elevation made some progress in the Stone Age.

Among other relics thus brought to light, there is one which has a special chronological value, viz., a large bronze caldron (fig. 3), now preserved in the National Museum of Antiquities, Edinburgh. It is recorded as having been found in 1768, "upon the surface of the clay, buried under the moss." It is made of thin plates of beaten bronze riveted together, the rounded bottom portion being fashioned out of one piece, and measures 25 inches in diameter and 16 inches in depth. The everted rim is formed of a couple of bands of sheet bronze fastened to the upper edge of the vessel, and bears marks of the rivets by means of which a pair of ring-handles had been attached. Sir Daniel Wilson informs us that two rings (presumably its detached handles), each measuring  $4\frac{3}{4}$  inches in diameter, were found along with it. "No question," writes Sir Daniel, "can exist of its native workmanship. The rings and staples are neatly designed, but rudely and imperfectly cast and finished, and are decorated exactly as those of the

Farney caldron. The circles embossed on the side of the vessel are, in like manner, such as have been frequently noted on objects of the Bronze period, both in Britain and on the Continent. Nevertheless, in accordance with the classical system of designation, which is even yet only partially exploded, this remarkable native relic figures in the printed list of donations in the *Archæologia Scotica* as a Roman camp-kettle." (*Ibid.*, p. 409.)

The acceptance of Sir Daniel's opinion as final carries with it strong presumptive evidence to show that the surface of the



FIG. 3.—Bronze Caldron found in the Moss of Kincardine (25 inches diameter).

clay beneath the peat was already dry land in the latter part of the Bronze Age—an admission which would at once give the *coup de grâce* to the post-Roman theory of the raised beaches. But as this opinion may be controverted on the ground that the caldron might be regarded as a survival from a former to a later age, it is desirable to determine as accurately as possible the chronological range of the class of objects to which it belongs.

Spheroidal bronze caldrons, similar in type and make to the

Kincardine caldron, have been discovered elsewhere in Scotland, as well as in various localities in England and Ireland. Of the Scottish finds, some consist of merely ring-handles or other fragments, such as were among the bronze hoards found in Duddingstone Loch and at Kilkerran (*Prehistoric Annals*, vol. i. p. 349). Entire specimens were, however, among the Bronze Age relics at Dowris, King's Co., Ireland, and at Heathery Burn Cave, Durham (*Ancient Bronze Implements*, pp. 361 and 412; *Proc. Soc. Antiq.*, 2nd series, vol. ii. p. 132). On the other hand, analogous caldrons, but perhaps not so artistically finished, have been discovered at Cockburnspath, Berwickshire, and in Carlingwark Loch, Kirkcudbrightshire, associated with iron tools and other objects undoubtedly of post-Roman date. The former of these Iron Age finds are thus described:—

“They included two large vessels of extremely thin sheet bronze, apparently with traces of gilding externally, and measuring, the one about 21 inches in diameter and 10 inches in depth, and the other 13 inches in diameter and  $7\frac{1}{2}$  inches in depth. When found these vessels were entire, and the one appeared to have been inverted on the other, with the articles within them. The large one has obviously been much exposed to the fire, and repeatedly repaired; the smaller one has had handles fastened to it on opposite sides by three rivets, the holes for which remain, and it has probably also been strengthened by a rim of iron, without which it would collapse, from the extreme thinness of the metal, if lifted full of water. It is probable that the whole were contained in a large wooden pail, as there were two large rings with staples and nails, the latter of which are bent in, indicating the thickness of the staves to have been about  $\frac{3}{4}$  of an inch. The rings measure  $4\frac{1}{2}$  inches in diameter. There are also a number of iron hoops, broken and crushed together, but which there can be little doubt encircled the wooden pail.

“The objects enclosed included a bronze Roman patella of the usual form,  $6\frac{3}{4}$  inches in diameter, and with the bottom composed of concentric rings in bold relief, but wanting the handle; the large iron chain figured above, measuring 27 inches in length; a circular bronze ornament, apparently the shield to which the handle of some object has been attached, measuring nearly 3 inches in diameter; an iron lamp-stand, similar to examples frequently found on Roman sites; two iron knives, one of them with a wooden handle; an iron gouge; two iron hammers; an iron tankard or jug, crushed flat; two ornamental ends of pipes, like the mouth-piece of a trumpet, of bright yellow bronze, and a mass of the same metal weighing nearly  $1\frac{1}{2}$  lb.” (*Proc. S.A., Scot.*, vol. i. pp. 43, 44.)

The Carlingwark caldron, though of the spheroidal type, is

slightly different in shape. It measures 26 inches in diameter across the mouth, the sides being straight, but bulging out to the extent of 1 inch above the rounded and somewhat flattened bottom. When dredged up it contained a number of iron tools and other objects — axes, hammers, staples, rings, a file, a saw, a bridle-bit, a tripod, portions of chain mail, a bronze vessel, green glass, etc. (*Ibid.*, vol. vii. pp. 7, 10.) One or two other spheroidal caldrons have been found in Scotland, but not being associated with objects which furnish any chronological data bearing on the problem at issue, they need not be discussed here.

We now come to another series of caldrons which, though made of plates of thin beaten bronze and riveted together in the same way as that found in the Kincardine Moss, differ from it in having a bucket-like shape and a flat bottom. A caldron of this description (fig. 4) was discovered, some two generations ago, in the north-west corner of Flanders Moss, on the Cardross estate, "in what had always been considered to be a Roman camp." This vessel, hitherto unique among Scottish antiquities, was exhibited at a meeting of the Society of Antiquaries of Scotland on 9th January 1888 by H. D. Erskine, Esq. of Cardross, and a full description of it by Dr Joseph Anderson is inscribed in their *Proceedings* for that year. It measures 19 inches in height, 10 inches in diameter at the base, and 14 inches at the mouth, widening to 16 inches at the shoulder. Two large rings for suspension, passing through ornamental loops, are attached to the inside of the lip. Although this is the only specimen known to have been found north of the Tweed, several have been met with in different parts of the British Isles, especially in Ireland. The conjunction of both types of caldrons — the spheroidal and bucket-shaped — in the Dowris and Heathery Burn Cave bronze hoards shows that they were contemporary in Britain at the close of the Bronze Age.

The foreign models, from which both these types of British and Irish caldrons are derivatives, became first recognised among the grave goods of an early Iron Age cemetery at Hallstatt (Austria), which dates from about the eighth to the second century B.C. These Hallstatt relics showed that the people of the dis-

trict had acquired the art of making thin plates of beaten bronze, as vessels of that material analogous to the British caldrons just described were among them. They differed, however, from the British types, inasmuch as the spheroidal forms on the



FIG. 4.—Bronze Caldron (19 inches in height) found at Cardross.

Continent had no suspension rings, but only handles riveted to their sides, while the buckets had generally bow handles like those of our common water-pails. As this Hallstatt civilisation spread westwards in Europe, it gathered so many new ideas in France and Switzerland that it became necessary to distinguish

its art and industrial products in these countries under the designation of La Tène civilisation—a name derived from the shallow outlet of Lake Neuchâtel, where stood the Helvetian *oppidum* which yielded its most characteristic relics. That both these culture streams had reached our shores is proved by the discovery in Britain and Ireland of a number of objects whose origin can be clearly traced to prototypes in Hallstatt and La Tène. But our insular artists, in the process of imitation, so handled their materials as to give their works a sufficiently distinctive character to differentiate them from their original models, and hence originated the style of art known as 'Late Celtic.' When the Romans took possession of Britain in the first century A.D., this native art was in a highly flourishing condition, but its further development in the southern portion of the island was cut short by the introduction of the civilisation of the conquerors. How long it was in existence previous to this event it is difficult to say, but it is safe to assume that some of its foreign prototypes reached the British Isles some three or four centuries before the Christian era—a period which, however, may be equated with the early Iron Age of Central Europe. The presence of both the spheroidal and conical caldrons in Britain and Ireland during the late Bronze Age shows that their importation into or development in these countries was altogether independent of Roman influence. I am unable to agree with the general opinion that all these caldrons are of native origin, although undoubtedly such vessels were made at home. We are told in the Tripartite Life of St Partick that the saint, when a boy in slavery in Ireland, was sold to some mariners at the mouth of the Boyne for two caldrons of bronze; also that Daire gave him an *aeneum mirabilem transmarinum*, i.e. "a wonderful brazen caldron from over the sea" (Joice, *Social History of Ireland*, vol. ii. p. 124). At any rate the most artistic specimens—in which category that found in the Kincardine Moss must be reckoned—were not only prior to the Roman occupation, but probably earlier than the most flourishing period of Late Celtic art.

In corroboration of these views it may be observed that among the antiquities found in *Oppidum* La Tène were about a dozen

caldrons, including both the spheroidal and conical types. The former were always constructed on a uniform plan, the special feature of which was a lower rounded portion made of thin bronze, and an upper band of iron to which the lower was riveted, and to which also were fastened two large suspension rings. (See Gross, *Oppidum Helvétie*, p. 45 and pl. xiii.)

It will be remembered that one of the Cockburnspath caldrons was supposed to have had its mouth strengthened by an iron band. Similar caldrons made of iron have been found in Ireland, two being among the collection of relics from the Lisnacrogghera crannog, which also contained a number of Late Celtic objects (*Lake Dwellings of Europe*, p. 386). It would thus appear that there was an evolutionary sequence in the manufacture of these caldrons in the British Isles: first, those made of bronze; second, those made of bronze and iron; and third, those made exclusively of iron. On the Continent, caldrons were generally found associated with sepulchral remains, except those from *Oppidum La Tène*, but in the British Isles they were undoubtedly used for culinary purposes. In protohistoric times in Ireland they were so highly prized that they are often referred to as heirlooms in families, and as forming part of the special property of kings. Tradition tells us that among the treasures brought to that country by the Tuatha De Danóan was the *Coire an Daghdha*, or Magic Caldron. On these grounds I see no reason why the Kincardine caldron, though belonging to an earlier date, should not have been used as a Roman camp-kettle; and the association of the Cardross bucket with a military camp, traditionally believed to be Roman, lends additional support to this view. The general argument on this phase of the subject may be thus briefly stated:—The finding of bronze caldrons of pre-Roman types, and of a wooden roadway, presumably of Roman construction, in association with the *débris* of great forest trees, some of which showed over 300 ring-growths, all buried beneath a bed of peat from 8 to 14 feet thick, affords something more than presumptive evidence that the site of this forest had become dry land at least some centuries before the Christian era.

But before attempting to assign a more precise date to this upheaval, it is desirable to know something of the terrestrial movement which caused it, especially as to the rate of its action. Was



the elevation effected suddenly, or in a few years, or in a few or many centuries? From what I can gather of the history of land oscillations in other parts of the world, the probability is that it was a very slow process, so much so that its progressive littoral changes were too small to be appreciated during the ordinary lifetime of an observer. If that be the case, it follows that there is a corresponding difference in the dates when the shallower and deeper portions of the sea-bottom reached the surface. We have already seen that the upheaval must have been practically completed in the vicinity of Drip Bridge before the wooden roadway was laid down, the carse lands there being only a few feet above present high-water mark. Hence the chronological value of antiquarian relics found within the zone of the 25-feet raised beaches depends to some extent on their position above sea-level. There are several recent discoveries which help to elucidate this point, one of the most instructive being a Bronze Age cemetery near Joppa, the situation of which is thus described by Mr W. Lowson, F.S.A. Scot. :—

“In the beginning of December last (1881) workmen began to excavate a piece of ground, little more than an acre in extent, lying between Magdalen Chemical Works and Eastfield Cottages, Joppa, on the north side of the road from Edinburgh to Musselburgh. The level of the ground is about 12 to 14 feet above high-water mark. On the top was ordinary soil, and beneath that a layer of sea-sand from 4 to 8 feet thick, and beneath that gravel. On the 21st January last I learned from the person who had feued the ground that in the course of removing the sand the workmen had discovered a large cinerary urn, filled with calcined human bones.”\* Subsequently, six other urns, varying in size, and all contained in stone cists, were recovered from the same locality. Besides, there were two or three cists without urns, and one with a skeleton. All these interments were from 4 to 6 feet below the surface of the ground, and about 3 feet down on the bed of sand. “The piece of ground,” writes Mr Lowson, “in which these remains were found lies along the sea-shore, and is now faced with heavy stones towards the sea; but I saw an old man in Fisherrow who remembers that he used to dig out sandmartins’ nests in that bank before the stones were put there. He had seen similar urns taken out in his boyhood.”

These facts conclusively prove that the sea had retreated to close upon its present limits before these interments had taken place. For if the surface of this sandy beach is 12 to 14 feet above high-water mark, and the graves from 4 to 6 feet in depth,

\* *Proc. S.A. Scot.*, vol. xvi. p. 419.

it is evident that the sea-level could not have been much more than 8 feet higher when the interment took place, without occasionally submerging and damaging the cemetery. Unless the high-tide limits were several feet lower, it is not likely that people who paid such respect to their dead would select an exposed beach as the final resting-place of their friends.

The hypothesis that the formation of the 25-foot raised beach on the west of Scotland was not completed till about the beginning of the Bronze Age was first suggested to me some years ago by the discovery of five bronze axes of the flat type (fig. 5), while work-

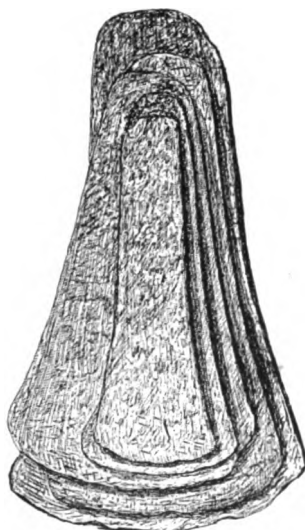


FIG. 5.—Five Bronze Celts found together at the “Maidens,”  
Ayrshire. ( $\frac{1}{2}$ ).

men were excavating the foundations of buildings on the sea-shore near Culzean Castle. These axes—which were bound together by a strong bronze wire, and had the remarkable peculiarity of being graduated in size—evidently formed the ‘kit of tools’ of a Bronze Age workman. They were lying in a crevice beneath a ledge of rock, against which were heaped up a few feet of gravel. The spot was about 100 yards from the sea-shore, and 25 feet above present high-water mark. In recording the discovery (*Proc. S.A. Scot.*, vol. xvii. p. 436), I suggested, as an explanation of

the phenomena, that the rocky ledge under which the axes had been deposited, apparently for temporary concealment, was at that time open towards the shore, and that subsequently, during a storm, the crevice had been covered over with coarse sea-gravel. It does not appear that the owner, when finally parting with his kit of tools, suspected any danger from the proximity of the sea; and hence there is some ground for supposing that the ordinary high tides were not wont to reach the spot. Now, had the relative level of sea and land been the same then as now, a storm could hardly account for their being covered over with sea-gravel. It is not, therefore, unreasonable to suppose that the upheaval had already, *i.e.* at the beginning of the Bronze Age, made considerable progress, for these axes are among the earliest objects of that period known in Scotland.

In conclusion, I have only to express the opinion that the facts and arguments here advanced warrant us in assigning the upheaval which caused the 25-foot raised beaches of Central Scotland to a more restricted chronological range than that expressed in my former theory on the subject, *viz.*, "that it was subsequent to the appearance of man in the district, but prior to its occupation by the Romans." The additional evidence points to the well-founded inference that the process of elevation had been virtually completed about the beginning of the Bronze Age. When it commenced there is little evidence to show, beyond the fact that it was a considerable time posterior to the stranding of the school of whales on the tidal shore of the shallow sea which then covered the carse lands to the west of Stirling.

(*Issued separately June 18, 1904.*)

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*Continued on page iii of Cover.*

**The Complete Solution of the Differential Equation of  $J_{[n]}$ .** By the Rev. F. H. Jackson, H.M.S. "Irresistible."  
*Communicated by Dr Wm. Peddie.*

(MS. received April 23, 1904. Read July 4, 1904.)

In connection with the function  $J_{[n]}$ , it may be of interest to give briefly the complete solution of the differential equation satisfied by the function  $J_{[n]}$ . The method of Frobenius will be employed. Consider the differential equation

$$px^{[2]}f^{(2)} \quad \left\{ 1 - [n] - [-n] \right\} x^{[1]}f^{(1)}(x) \\
+ x^{[2]}f(x^{p^2}) + [n][ -n] f(x) = 0. \quad \dots (a)$$

in which

$$[n] = \frac{p^n - 1}{p - 1} \\
f^{(2)}(x) = \frac{d}{d(x^p)} \left\{ \frac{df}{dx} \right\}$$

If  $p = 1$ , the equation reduces to

$$x^2 f'' + x f' + (x^2 - n^2) f = 0,$$

which is Bessel's equation for functions of order  $n$ .

Substituting an expression

$$f(x) = \sum c_r x^{[\alpha + r]}$$

in the equation (a), we have an indicial equation

$$[\alpha + n][\alpha - n] = 0,$$



and an indicial function

$$f = c_0 \left[ x^{[a]} - \frac{x^{[a+2]}}{[a-n+2][a+n+2]} + \frac{x^{[a+4]}}{[a-n+2][a+n+2][a-n+4][a+n+4]} - \dots \right]$$

The principal roots of the indicial equation are

$$a = +n, \quad a = -n.$$

If  $n$  be not an integer, the corresponding integrals are  $J_{[n]}$  and  $J_{[-n]}$ .

$$J_{[n]}(x) = [2]^n \Gamma_p([n+1]) \left\{ x^{[n]} - \frac{x^{[n+2]}}{[2][2n+2]} + \dots \right\}$$

$$J_{[-n]}(x) = [2]^{-n} \Gamma_p([1-n]) \left\{ x^{[-n]} - \frac{x^{[-n+2]}}{[2][2-2n]} + \dots \right\}$$

If  $n=0$  these integrals are identical, while if  $n$  be an integer, one or other becomes ineffective according as  $n$  is positive or negative. In these cases, then, it is necessary to form a second distinct and effective integral corresponding to Hankel's solution of Bessel's equation.

When  $a$  is integral, we write<sup>1</sup>

$$f(x) = C \left\{ [a+2n] - [n] \right\} \left\{ [a+2n] - [-n] \right\} \left[ x^{[a]} - \frac{x^{[a+2]}}{[a-n+2][a+n+2]} + \dots \right. \\ \left. \dots - (-1)^n \frac{x^{[a+2n-2]}}{\prod_{r=1}^{n-1} \left\{ [a+2r] - [n] \right\} \left\{ [a+2r] - [-n] \right\}} \right] \\ + E \left[ x^{[a+2n]} - \frac{x^{[a+2n+2]}}{\left\{ [a+2n+2] - [n] \right\} \left\{ [a+2n+2] - [-n] \right\}} \right. \\ \left. + \dots \right] \\ = \omega_1 + \omega_2.$$

<sup>1</sup> Cf. Forsyth, *Theory of Differential Equations*, vol. iii. pp. 101, 102.

From

$$(\omega_1 + \omega_2)_{a=-n}, \left( \frac{\partial \omega_1}{\partial a} + \frac{\partial \omega_2}{\partial a} \right)_{a=-n}$$

we obtain

$$(\omega_1)_{a=-n} = 0 \dots \dots \dots (1)$$

$$(\omega_2)_{a=-n} = [2]^n \Gamma_p([n+1]) E J_{[n]}(x) \dots \dots \dots (2)$$

$$\left( \frac{\partial \omega_1}{\partial a} \right)_{a=-n} = C \frac{\log p}{p-1} \sum_{r=1}^{n-1} (-1)^r \frac{[2n]}{[2][4] \dots [2r] \cdot [2-2n] \dots [2r-2n]} x^{[2r-n]} \quad (3)$$

$$\begin{aligned} \left( \frac{\partial \omega_2}{\partial a} \right)_{a=-n} &= E \frac{\log p}{p-1} \log x \sum_{r=1}^{r=\infty} (-1)^r \frac{p^{n+2r} x^{[n+2r]}}{[r]![n+r]!(2)_r(2)_{n+r}} \\ &- E \frac{\log p}{p-1} \sum_{r=1}^{r=\infty} (-1)^r \left\{ \frac{p^2}{[2]} + \frac{p^4}{[4]} + \dots + \frac{p^{2r}}{[2r]} + \frac{p^{2n+2}}{[2n+2]} + \dots \right. \\ &\quad \left. \dots + \frac{p^{2n+2r}}{[2n+2r]} \right\} \frac{x^{[n+2r]}}{[r]![n+r]!(2)_r(2)_{n+r}} \dots \quad (4) \end{aligned}$$

If we give C the value  $-\frac{(2)_{n-1} \Gamma_p([n])}{[n]}$ , so that  $E = \frac{1}{(2)_{n-1} \Gamma_p([n+1])}$

or what is equivalent

$$E = \frac{1}{[2]^{n-1} \Gamma_p([n+1])}$$

we obtain an integral from (3) and (4) which may be termed  $W_1 + W_2$ . If  $p=1$ , this integral reduces to that given on p. 102, vol. iii., Forsyth's *Theory of Differential Equations*.

In the case when  $n=0$ , the integral is

$$\begin{aligned} f = c J_{[0]}(p, x) \log x - c \sum_{s=1}^{s=\infty} \left\{ \frac{2p^2}{[2]} + \frac{2p^4}{[4]} + \dots \right. \\ \left. \dots + \frac{2p^{2s}}{[2s]} \right\} \frac{x^{[2s]}}{[2]^2 [4]^2 \dots [2s]^2} \dots \quad (5) \end{aligned}$$

These functions satisfy the same recurrence equations as the

function  $J_{[n]}$  given in the *Transactions* of the Society, vol. xli., part i., Nos. (1) and (6). The expression for  $\Gamma_p([x])$  given on p. 105, No. 6, vol. xli., should be

$$\frac{p^{\frac{1}{2}x(x-1)}}{\Gamma_p([x])} = [x]e^{px} \prod_{s=1}^{s=\infty} \left\{ \left( 1 + p^{-x} \frac{[x]}{[s]} \right) e^{[s]} \right\}$$

The class of differential equations integrable by Bessel's functions, and discussed by Lommel in vol. xiv., *Mathematische Annalen*, may without difficulty be formally extended in the same way that Bessel's equation and its solutions have been extended in the above work.

(Issued separately August 15, 1904.)

**A Differentiating Machine.** By J. Erskine Murray, D.Sc.

(Read March 21, 1904. MS. received May 28, 1904.)

It was pointed out to me a few months ago, by my friend Professor W. H. Heaton, that our knowledge of the laws of physical variations might be greatly increased if their study were facilitated by the invention of a machine which would automatically deduce the rate of change of a function from the curve representing that function. In cases where the physical law is already known, and is expressible in terms of known mathematical quantities, such a machine is not essential, though it provides an excellent illustration of mathematical laws; there is, however, a vast and ever-increasing mass of numerical results awaiting discussion and co-ordination, and it is in reducing these to law and order that the differentiator should prove a useful tool. As instances of a few cases in which rates of change are of the first importance, I may mention the following:—

- (1) Meteorological observations of Temperature, Pressure, Humidity and Rainfall.
- (2) Terrestrial Magnetic records.
- (3) Experimental results in Physics and Chemistry which involve changes, whether in time or space. The determination of thermal conductivity by Forbes' method is an example.
- (4) Statistics of Population, Mortality, and Migration.
- (5) Statistics of Wages, Prices, and Commerce.
- (6) Medical records.
- (7) Engineering calculations, such as the deduction of Tractive Force from a Time and Space or Time and Velocity diagram.

Up to the present all determinations of rates of change of quantities like those above mentioned have had to be made by laborious arithmetical or graphical methods, involving so great an expenditure of time for their completion that but little has been done. The differentiator reduces enormously the necessary labour,

and even the roughly constructed instrument shown will give results sufficiently accurate for most purposes.

The construction of the differentiator depends on the well-known fact that if the values of a variable quantity be represented on a diagram by the ordinates of a curve, its rate of change, at any point of the curve, is measured by the slope of the tangent at that point.

The machine, then, is guided by hand so that one line on it remains tangent to the curve, while a tracing point describes on a second sheet of paper a curve whose ordinates are proportional to the slope of the tangent. Thus if  $y=f(x)$  be the equation to the original curve, the derived curve will have for ordinates the corresponding values of  $d(f(x))/dx$ . The abscissæ are the same on both curves.

In order that a line may be tangent to a curve it is necessary that two consecutive points on each should coincide. In practice, two black dots on a piece of transparent celluloid are used, the distance between them being about 2 mm.

The plan of the machine is shown in fig. 1. It consists of three parts. Firstly, the large drawing-board A B C D, on which the original curve is placed. Fixed to each long side of this board is a metal rail, one, C E, having a plain surface, and the other, D F, a longitudinal groove of V-shaped section. The second part is a smaller board, C H I, having three spherical feet, two of which run in the groove and the third on the plane rail. This arrangement permits free motion of the smaller board in the direction of the length of the larger one, *i.e.* parallel to the Y coordinate. The small board carries the paper on which the derived curve is traced by the machine. Attached to its edge are guides, J K L M, which hold the principal part of the mechanism, allowing it free motion in a right and left line.

This part, shown in fig. 2, consists of a frame A B C D, at one corner of which is a pin, A, which serves as the vertical axis about which the rod P Q revolves in a horizontal plane. P Q has a slot in it, through which passes the pin R fixed to the rod S T. S T is controlled by guides E and F, so that it can only move in a direction parallel to O Y.

Below the arm P Q, and fixed rigidly to it below A, is a small

plate of celluloid, not shown in the diagram, on the under side of which are two dots by which the machine is guided along the curve. The line through the dots is parallel to  $PQ$ . The celluloid rests on the paper on which the original curve is drawn, thus supporting the outer end of the frame  $ABCD$ .

Since the distance  $AV$  between the pin and the centre line

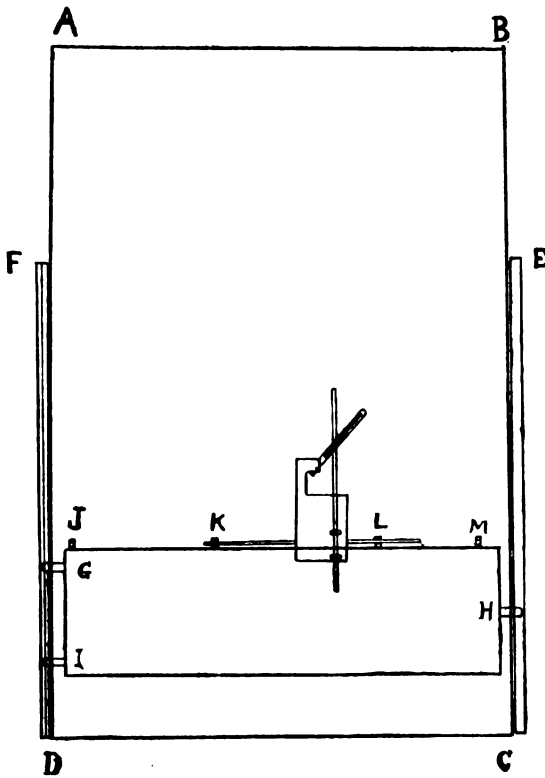


FIG. 1.

of  $ST$  is constant, and since  $RV/AV \doteq dy/dx$ , it is clear that the distance  $RV$  which  $R$  is displaced above or below the zero line  $AV$  measures the tangent of the angle of slope of the curve, *i.e.*  $dy/dx$ . A pen at the end  $T$  of  $ST$  records the movements of  $R$ , and therefore traces a curve of which the ordinates are proportional to the rate of change of the ordinate of the original curve. It should be noticed that the purpose of

the second board is to eliminate the Y coordinate of the original curve. In using the machine the arm P Q is moved so that it remains tangent to the original curve, while the frame A B C D is moved from left to right, and it and the smaller board to and fro as may be necessary in following the curve.

The machine shown has been constructed to deal with curves in which the tangent of the angle of slope does not exceed 5; this is sufficient for almost all experimental or observational results, since

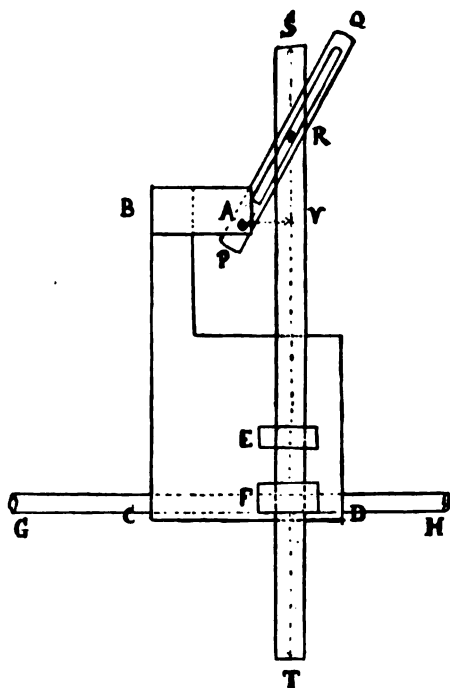


FIG. 2.

it is always possible to flatten out the curve by making the horizontal scale large in proportion to the vertical.

It is, of course, easy to obtain the higher derivatives of the original curve by a simple repetition of the process on the successive curves.

In a future communication I hope to lay before the Society the results of the study of a number of meteorological and other curves by aid of the differentiator.

*(Issued separately August 15, 1904.)*

**On the Thermal Expansion of Dilute Solutions of certain Hydroxides.** By **George A. Carse**, M.A., B.Sc.  
*Communicated by Professor MACGREGOR.*

(Read March 21, 1904.)

In a paper communicated to the Nova Scotian Institute of Natural Science,\* Professor MacGregor has shown that in the case of weak aqueous solutions of certain hydroxides, the volume of a solution is less than the volume of water used in its preparation. At his suggestion I have investigated the hydroxides of sodium, barium, and strontium, to ascertain whether they exhibit this property, and how the excess of the volume of solution over the volume of constituent water varies with the temperature. From the observations made, I have also determined the thermal expansion coefficients, and found how they vary with temperature and with concentration.

*Preparation and Determination of Composition of Solutions.*

The substances were purchased as chemically pure from E. Merck, Darmstadt, and were found to be of sufficient purity, the sodium hydrate being tested for carbonate, chloride, and sulphate, and the barium and strontium hydrates for strontium and calcium, barium and calcium, respectively.

The original solutions were prepared by dissolving the substances in twice-distilled water, and they were analysed volumetrically by titration with acid, phenolphthalein or methyl orange being used as an indicator. The concentration of the acid had been determined by means of sodium carbonate made by heating sodium bicarbonate. The value of the chemical composition of any solution thus analysed was got by taking the mean of several determinations. The values of the atomic weights used were those given by the International Atomic Weight Table of 1904, and the

\* *Trans. Nov. Scot. Inst. Nat. Sc.*, 7, 368, 1889-90.



densities of water at the various temperatures those given by Landolt and Börnstein.\*

Other solutions were made from those prepared directly by mixing measured volumes of the solutions and distilled water at 15° C. The percentage concentration was then got from the formula,  $p = \frac{GV \times 100}{VD + W\Delta}$ , where G is the number of grams of salt per c.c. of original solution at 15° C., V the volume of the solution, D the density of the solution at 15° C., W the volume of water, and  $\Delta$  the density of the water at 15° C. The volumes were measured out by pipettes and burettes which had been certified correct by the Physikalisch-technische Reichsanstalt, Berlin.

The accuracy aimed at in the estimation of the chemical composition of the solutions was the greatest attainable, and in the estimation of the solutions of barium, and, to a lesser degree, of strontium, the errors were greater than in the case of the solutions of sodium. The so-called "probable errors" in the estimation of concentrations were found in no case to exceed '00003 per gram of solution.

#### *Determination of Density.*

The density determinations were made primarily to measure expansion on solution, and I found that the error introduced into the measurement of expansion by the error in the concentration set a limit to the density accuracy necessary. It was found unnecessary to measure densities to any greater degree of accuracy than 5 in the fifth decimal place. Accordingly, the pycnometer method of determining density was adopted.

My attention was drawn to a method devised by Mr Manley † of eliminating the error in a density determination by the pycnometer, due to a difference in the amount of moisture condensed on the glass of the pycnometer in different weighings. The method consists in using as a counterpoise a similar, sealed, pycnometer, which is treated as regards heating, handling, etc. in exactly the same way as the pycnometer containing the liquid whose density is to be measured. Mr Manley finds that "when

\* *Physikalisch-Chemische Tabellen*, 1894.

† *Proc. R.S.E.*, 24, 357, 1902-3.

it is desired to obtain a value for the relative density of a water, which shall be as nearly correct as possible to the fifth decimal place, the use of a counterpoise for automatically eliminating certain incidental errors is absolutely essential."

From calculations I made, based on a paper by Dr G. J. Parks \* "On the Thickness of the Liquid Film formed by Condensation at the Surface of a Solid," it was found that had the pyknometer had maximum deposition of moisture in the one case and none at all in the other, the difference between two weighings of the pyknometer empty could not exceed .004 per cent. Parks found that the thickness of the film of moisture deposited on the surface of the glass after 16 days' exposure, when the maximum was attained, amounted to  $13.4 \times 10^{-6}$  cm. This moisture if all present would increase the weight of my pyknometer by .0008 gms., which is equivalent to .004 per cent. of the weight of the pyknometer empty.

The difference I am dealing with is not the absolute amount of moisture deposited, but the change in the amount of moisture that may occur from experiment to experiment, and therefore is much less than that calculated above.

To find whether it was necessary to use a counterpoise or not, when I wished an estimation of density which should have no greater error than 5 in the fifth decimal place, I determined the specific gravity of a solution at various temperatures, both with and without the counterpoise.

I took two pyknometers of the Sprengel-Ostwald type, of the same kind of glass and of nearly the same external volume. I weighed each one, reducing the weight to weight *in vacuo*. One of the pyknometers was then sealed by closing the end of the tube of large bore and melting the end of the tube of small bore till it was almost closed. The whole pyknometer, except about a quarter of an inch of the capillary tube, was immersed in a beaker of water, and the beaker covered with layers of paper to prevent the heat of the sealing flame reaching the water. The pyknometer was left in the water till the air inside had reached the temperature of the water, and the capillary end was sealed with a fine small flame. Knowing the temperature of the water, the height of the

\* *Proc. Phys. Soc. Lond.*, 18, 410, 1903.

barometer and the internal volume of the counterpoise, we can calculate the weight of the air enclosed.

Let  $w$  be the observed weight of pyknometer with liquid in it using the counterpoise,  $w_1$ ,  $w_2$  the true weights of pyknometer and counterpoise respectively ( $w_2$  including the weight of air inside counterpoise),  $l$  the true weight of liquid in pyknometer,  $v_1$ ,  $v_2$  the volumes of air displaced by pyknometer and counterpoise respectively,  $\lambda$  the density of the air at the particular temperature and pressure at which the observation is made, and  $\rho$  the density of the weights; then

$$l = w + w_2 - w_1 - \lambda \left( \frac{w}{\rho} + v_2 - v_1 \right).$$

The volumes  $v_1$  and  $v_2$  were determined by finding the weight of water in the pyknometer at a given temperature, and thence calculating the volume occupied by the water, and by finding the weight of the pyknometer empty, and the density of the glass, and thence getting the volume of the glass.

All the terms on the right-hand side being known, we can find  $l$ . If the pyknometers have nearly the same surface, then the weights of moisture on their surfaces balance.

I now give my own experiments with and without the counterpoise, showing that the use of the counterpoise was needless in my work. The observations are as follows:—

Temperature degrees Centigrade.	Specific Gravity using Counterpoise.	Specific Gravity not using Counterpoise.
15	1·18566	1·18566
20	1·18415	1·18418
26	1·18269	1·18266
30	1·18174	1·18175

The pyknometers used in the two series of observations given above were different, and each weight of liquid was the mean of two weighings. The pyknometers were not left standing exposed to the air for more than 20 minutes (the time occupied in a weighing). As the differences (the maximum being ·00003) in the specific gravity vary indiscriminately on either side there is no

indication that the one method is any better than the other from my point of view. I therefore did not use the counterpoise.

In the determinations of the densities of the solutions, the pyknometers weighed about 20 grams, and had a capacity of about 20 c.c. The pyknometers, after being filled, were placed in a thermostat, the temperature of which was kept at 15° C., 20° C., 26° C., 30° C., as was required; the bath did not vary more than .04° C. from the required temperature during any experiment. The stirrer was driven by an electric motor, or latterly by a Heinrici hot-air engine. The thermometer which gave the temperature of the bath was graduated to fiftieths of a degree centigrade, and had a table of corrections from the National Physical Laboratory, Kew Observatory. After the pyknometer had been for some time in the bath (the period varying from 2 hours to 20 hours, as the apparatus was kept going day and night), the meniscus was made to coincide with the mark on the stem. A short time after, if the meniscus still coincided with the mark, the pyknometer was taken out, dried with a cloth and weighed. All weighings were corrected for the buoyancy of the air by adding on to the observed weight of the pyknometer the weight of air displaced by the excess of the volume of the pyknometer and liquid over that of the weights.

To get an accuracy of .001 per cent. in a weighing the thermometer in the balance-case should be read to .14° C. and the barometer to .35 mm. The thermometer in the balance-case read to .1° C. and was correct to .02° C., and the air in the case was kept dry by means of sulphuric acid. The barometer, which had been corrected at the National Physical Laboratory, read to .1 mm. In the correction for buoyancy the density of the air was taken from Landolt and Börnstein.\* The error introduced by taking the air in the balance-case as perfectly dry was calculated and found to be negligible.

All weighings were the means of at least two observations, and the deviation of any weighing from the mean of two weighings was found not to exceed .002 per cent. for 94 weighings examined, thus giving a rough estimate of the accuracy in weighing.

The so-called "probable error" in the estimations of density was found not to exceed .00002.

\* *Loc. cit.*

*Expansion on Solution.*

The volume of unit mass of the various solutions examined was calculated, and also the volume which the solvent water contained in unit mass would occupy if its temperature were the same as that of the solution. The amount by which the volume of unit mass of the solution is greater or less than that of the solvent water employed in its preparation is the difference of these quantities. Knowing the density,  $\rho$  (gms. per c.c.), of a solution at  $t^\circ$  C., we can find the volume,  $\frac{1}{\rho}$ , of 1 gm. of the solution at that temperature; and knowing the concentration of a solution ( $c$ ), and the density of water at  $t^\circ$ ,  $\Delta$ , we can find the volume that the water in 1 gm. of solution would occupy if it were free, viz.,  $\frac{1-c/100}{\Delta}$ ; hence the excess of the one volume over the other is  $\frac{1}{\rho} - \frac{1-c/100}{\Delta}$ . This may be called the expansion on solution.

The "probable error" in the determination of the expansion was found to be .00004, the values of the expansion varying from .00957 to .00001.

The following tables give the results found. The headings are self-explanatory.

*Sodium Hydroxide.*

Grams of substance in 100 grams Solution.	Temp. $t^\circ$ C.	Density grams per c.c.	Volume of 1 gram of Solution at $t^\circ$ C. (V c.c.)	Volume at $t^\circ$ C. of water in 1 gram Solution (V' c.c.)	Expansion V-V' c.c.
16.3329	15	1.18463	.84415	.83740	+ .00675
"	20	1.18208	.84596	.83815	+ .00781
"	26	1.17891	.84823	.83935	+ .00888
"	30	1.17676	.84988	.84031	+ .00957
6.0785	15	1.06884	.93559	.94003	- .00444
"	20	1.06699	.93721	.94087	- .00366
"	26	1.06452	.93938	.94222	- .00284
"	30	1.06294	.94078	.94329	- .00251
3.1305	15	1.03532	.96589	.96954	- .00365
"	20	1.03373	.96737	.97041	- .00304
"	26	1.03180	.96918	.97179	- .00261
"	30	1.03074	.97045	.97290	- .00245

It thus appears that for solutions of this hydrate below a certain dilution the expansion is negative, and that this negative expansion becomes less numerically with rise of temperature, *i.e.* it increases algebraically with the temperature, just as is the case when the expansion is positive (see fig. 2).

The following are curves for sodium hydroxide showing expansion on solution plotted against concentration for the various temperatures.

The solution exhibiting the maximum contraction at 15° C. is

*Sodium Hydroxide.*

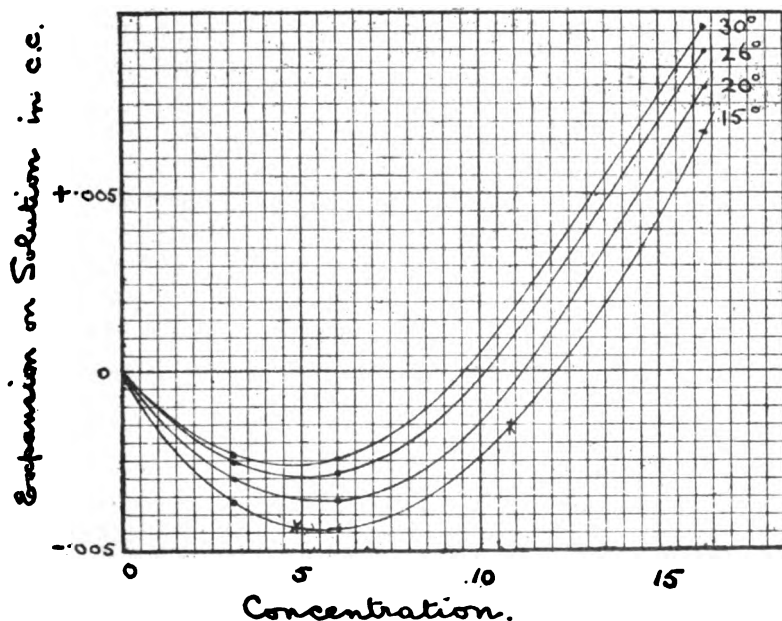


FIG. 1.

one containing 6.07 per cent. of the hydroxide, while the corresponding value deduced by Professor MacGregor is 6 per cent. The maximum contraction, as deduced from the above graph, is .0044 c.c., while that given by Professor MacGregor is .0045. The crosses on the diagram indicate values taken from Professor MacGregor's table. It is also to be noted that contraction decreases with rise of temperature, and that the maximum contraction-

point slowly shifts towards the concentration origin with rise of temperature.

*Barium Hydroxide.*

Grams of substance in 100 grams Solution.	Temp. t° C.	Density grams per c.c.	Volume of 1 gram of Solution at t° C. (V c.c.).	Volume at t° C. of water in 1 gram. Solution (V' c.c.).	Expansion V-V' c.c.
·89387	15	1·01079	·98933	·99192	-·00259
„	20	1·00998	·99010	·99281	-·00271
„	26	1·00847	·99160	·99423	-·00263
„	30	1·00721	·99285	·99537	-·00252
·08212	15	1·00000	1·00000	1·00005	-·00005
„	20	·99913	1·00087	1·00095	-·00008
„	26	·99766	1·00234	1·00238	-·00004
„	30	·99656	1·00345	1·00352	-·00007
·04303	15	·99957	1·00043	1·00044	-·00001
„	20	·99870	1·00130	1·00134	-·00004
„	26	·99728	1·00273	1·00276	-·00003
„	30	·99611	1·00390	1·00392	-·00002

It thus appears that all the solutions of barium hydrate examined have a negative expansion. This hydrate is thus so far analogous to sodium hydrate. The effect of temperature on the expansion is not very marked, and for the last two concentrations the numerical values of the expansions are subject to considerable variations in the fifth decimal place, although they all agree in giving negative expansion (see fig. 2).

*Strontium Hydroxide.*

Grams of substance in 100 grams Solution.	Temp. t° C.	Density grams per c.c.	Volume of 1 gram of Solution at t° C. (V c.c.).	Volume at t° C. of water in 1 gram. Solution (V' c.c.).	Expansion V-V' c.c.
·32744	15	1·00363	·99639	·99759	-·00120
„	20	1·00263	·99738	·99848	-·00110
„	26	1·00114	·99886	·99992	-·00106
„	30	·99996	1·00004	1·00105	-·00101
12162	15	1·00072	·99923	·99965	-·00042
„	20	·99971	1·00029	1·00055	-·00026
„	26	·99831	1·00169	1·00197	-·00023
„	30	·99708	1·00293	1·00313	-·00020
·02354	15	·99946	1·00054	1·00063	-·00009
„	20	·99849	1·00151	1·00153	-·00002
„	26	·99700	1·00306	1·00296	+·00010
„	30	·99600	1·00432	1·00411	+·00021

Here also solutions of strontium hydrate exhibit this negative expansion, and this negative expansion becomes less numerically with rise of temperature, and in the case of the last solution examined it changes from being a negative to a positive expansion with rise of temperature. Strontium hydrate is thus analogous to sodium hydrate (see fig. 2).

The following are curves exhibiting expansion on solution plotted

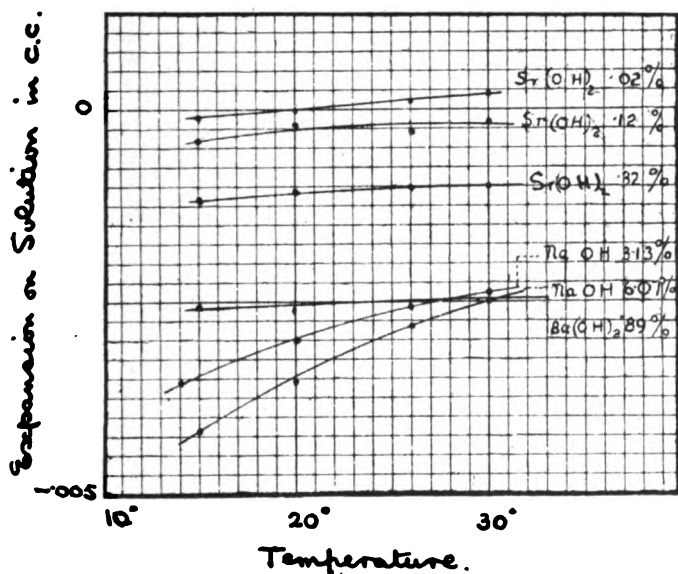


FIG. 2.

against temperature for the hydroxides of sodium, barium, and strontium.

*Thermal Expansion.*

Adopting the formula  $V_t = V_{15}[1 + a(t - 15) + b(t - 15)^2 + c(t - 15)^3]$  where  $V_t$  is the specific volume at  $t^\circ C.$ , and  $a, b$  and  $c$  are constants, I have determined by a modified method of least squares the constants  $a, b, c$ ; the formula gives the volume at any temperature between  $15^\circ C.$  and  $20^\circ C.$  correct to within 5 in the fifth decimal place. By the aid of the above formula the expansion coefficients,  $a_t = \frac{1}{v_t} \frac{dv_t}{dt}$ , where  $a_t$  is the expansion coefficient at  $t^\circ C.$ , were calculated.



The following are the tables :—

*Constants and Coefficients.*

Concentration.	$a \times 10^6$	$b \times 10^8$	$c \times 10^9$	$\alpha_{15} \times 10^5$	$\alpha_{20} \times 10^5$	$\alpha_{25} \times 10^5$	$\alpha_{30} \times 10^5$
<b>Sodium Hydrate.</b>							
16·3329	+430	+000	+116	42	44	47	51
6·0785	+310	+890	-327	31	37	38	36
3·1805	+300	+124	-21	30	31	32	33
<b>Barium Hydrate.</b>							
·8989	+97	+1200	-180	10	20	29	34
·0821	+130	+980	-210	13	21	26	28
·0430	+143	+630	-28	14	20	27	31
<b>Strontium Hydrate.</b>							
·3274	+180	+340	+50	18	21	27	31
·1216	+220	-410	+380	22	21	26	35
·0235	+165	+600	-12	16	22	29	33

The expansion coefficients, since they involve small differences of volume, are subject to large errors in the fifth decimal place, and can only be considered as approximate.

The following curves show expansion coefficient plotted against

*Sodium Hydroxide.*

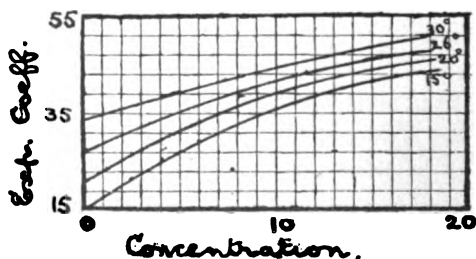


FIG. 3.

concentration, the first set being for sodium hydroxide alone, while the second set are for the three hydroxides. In the second set

the concentrations and expansion coefficients are plotted on scales 20 and 2 times those of the first set respectively.

In the case of sodium hydroxide the expansion coefficient increases with concentration, and does so at a less rapid rate as the temperature rises.

The strontium and barium curves seem to indicate that the rate of variation of expansion coefficient with concentration reaches

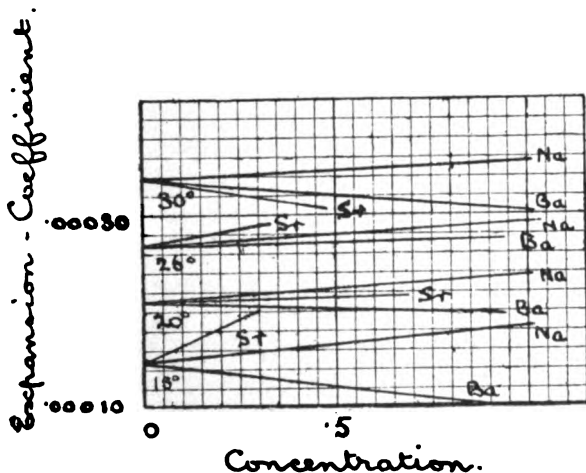


FIG. 4.

stationary values in the range considered, but no great stress can be laid on this conclusion, because of the uncertainty caused by the large errors in the expansion coefficient.

The above experiments were carried out in the Natural Philosophy Laboratory, University of Edinburgh. I have to tender my best thanks to Professor MacGregor for the assistance he has afforded me in this work, both by way of suggestions and advice.

(Issued separately August 15, 1904.)

**Effect of Transverse Magnetization on the Resistance of Nickel at High Temperatures. By Professor C. G. Knott.**

(Read June 20th, 1904.)

*Abstract.*

In a previous communication \* it was pointed out that the effect of transverse magnetization on the resistance of nickel wire was inappreciable in fields below 500 C.G.S. units, thereby differing from the case of longitudinal magnetization, in which the effect was easily measurable in fields below 20.† The reason of this is no doubt to be referred to the thinness of the wire in the direction of the magnetizing force. To measure the effect of transverse magnetization it was necessary to form a flat coil and insert it between the poles of a powerful electro-magnet. Considerable difficulty was experienced in winding this coil with interwound asbestos insulation, for great care had to be taken that no part of the wire cut the lines of force obliquely, otherwise there would be a resolved component of longitudinal effect, which in certain cases might altogether mask the effect looked for. The coil used in the final experiments was suitable in all respects. It was coiled between glass plates, the successive coils being separated by threads of asbestos. Round the coil another coil (of Beacon wire) was wound anti-inductively, so that any current passing through it would have no magnetic action upon the nickel wire inside. By varying the current in this external coil I was able to heat the nickel to any desired temperature up to 400° C. In any one experiment the final temperature came to a steady state, and not till this state was reached was it possible to begin the observations on the resistance change. This was measured in the manner already described in my paper on the effect of longitudinal magnetization, and it will suffice meanwhile to call attention to a remarkable result obtained

\* *Proc.*, vol. xxiv. p. 501 (1903).

† *Trans.*, vol. xli. pp. 39-52 (1904).

when the temperature approached that at which nickel ceases to be strongly magnetic.

The nature of the phenomenon is indicated in the following table, which gives the change of resistance of 100,000 ohms of nickel wire at the temperatures shown when the wire is subjected to a transverse magnetic field of about 3800 units.

Temperature.	Resistance change in Field 3800.	Temperature.	Resistance change in Field 3800
10° C.	750	320° C.	320
100	640	330	270
200	390	335	170
250	250	340	100
290	190	345	40
300	201	350	5 ?
310	250		

The peculiarity consists in the marked minimum at temperature 290° and the still more abrupt maximum at temperature 320°. The very rapid fall off to zero as the temperature rises from 330 to 350 is also worthy of note. So limited is the range of temperature within which these changes take place, that the phenomenon might easily have escaped notice. It was fortunate that in one of the earlier series a temperature very near the minimum point was hit upon. The peculiarity was at first ascribed to the inherently greater difficulties of making the experiments at the higher temperatures: but time after time, by means of small successive changes of temperature between the critical limits, exactly the same results were obtained. There can, therefore, be no doubt as to the existence of a peculiar molecular change as the nickel wire is raised in temperature from about 290° to 350°. In my paper on the effect of longitudinal magnetization (see especially the curves at the highest temperatures, p. 46, *l.c.*), a similar peculiarity was indicated. It was, however, so slight—being merely a slight upward bulging of the isodynamic curves—that it was not at the time regarded as of any moment, but, in the light of the present result, it can no longer be looked upon as due to small errors of measurement.

In this connection I would draw attention to a paper published in the *Philosophical Magazine* for June 1904, bearing on a cognate line of research. In that paper Dr E. P. Harrison shows that pure nickel undergoes curious changes of length as the temperature approaches the temperature at which its magnetic properties are lost. This is strictly analogous to the behaviour of iron at red heat, as discovered long ago by Gore. Tait found that the thermo-electric properties of iron had peculiarities which occurred at this same temperature; and that similar thermo-electric peculiarities were possessed by nickel. He tried, but unsuccessfully, to find a Gore effect in nickel at a temperature of 400°. This has now been very satisfactorily accomplished by Dr Harrison. It is possible, however, that the result obtained by Dr Harrison may be partly due to variation in the magnetic strain caused by the circular magnetization accompanying the strong current used for keeping the nickel wire at the required high temperature.

As to the cause of the curious effects described in this note, more than one hypothesis might be advanced, but it would be premature to attempt any complete discussion until further facts are made out. These I hope to communicate in due course.

*(Issued separately July 30, 1904.)*

**Observations on some Aged Specimens of *Sagartia troglodytes*, and on the Duration of Life in Cœlenterates.** By J. H. Ashworth, D.Sc., Lecturer in Invertebrate Zoology in the University of Edinburgh, and Nelson Annandale, B.A., Deputy-Superintendent of the Indian Museum, Calcutta. *Communicated by Professor J. C. Ewart, M.D., F.R.S.*

(MS. received June 10, 1904. Read June 20, 1904.)

We have, during the last two years, made a series of observations upon specimens of *Sagartia troglodytes* which are at least fifty years old, and have thought it worth while to give a somewhat detailed account of these, as, so far as we can ascertain, there is only one other recorded case of longevity in Cœlenterates (see p. 302), and very few in the whole of the Invertebrata.\*

These specimens of *Sagartia troglodytes* were collected by Miss Anne Nelson (Mrs George Brown) on the coast of Arran, some few years previous to 1862 (the exact date has not been recorded), and were placed in bell-jars containing sea-water. In 1862 they were transferred to the care of Miss Jessie Nelson, in whose possession they still remain, and to whom we are indebted for the opportunities of observing these interesting anemones. Sixteen of the original specimens are still living, so that they have lived in captivity for about fifty years. They are kept in a bell-jar about 13 inches in diameter and 9 in depth. The original specimens are all together on a piece of stone, which bears a number of deep depressions in which the anemones have ensconced themselves. These conditions closely resemble those in which *S. troglodytes* is usually found, the specific name of this anemone being derived from its favourite habit of dwelling in holes and crevices of the rock. These specimens have been under constant observation since 1862, and there can be no doubt that they are the original ones.

\* See the appendix to Weismann's *Essay on the Duration of Life*, 1891, p. 36.

As the conditions under which these anemones have lived for so long may be of interest, the following particulars are given. The bottom of the bell-jar is covered with small rough stones on which several species of green algæ are growing. On these rests the large stone containing the cavities in which the anemones are fixed. The sea-water in the jar (about four gallons) is changed every six or eight weeks, and is usually aerated every morning. From time to time a little fresh water is added to keep the density of the whole constant. The anemones are fed about once a month on small pieces of raw lean beef. They usually reject fish or mutton,\* but appear to digest the beef very thoroughly, a small mass of white flocculent matter being ejected from the mouth a day or two after feeding. In addition, the anemones catch and feed upon the small isopods which abound among the algæ. One of us lately observed a specimen seize and engulf an *Actinia mesembryanthemum* which had freed itself from a neighbouring stone and come into contact with the tentacles of the *Sagartia*. Two days later the victim, almost intact, but quite dead, was ejected. Those tentacles of the captor (*Sagartia*) which had first touched the *Actinia* remained for some days diminished in size and opaque in colour, but finally recovered their usual appearance. *Sagartia troglodytes* is evidently not immune to the poison of *Actinia mesembryanthemum*, but, so far as could be ascertained, only the tentacles of the former suffered from the effects of the poison of the latter. Probably the nematocysts of the latter became inoperative soon after its capture, either owing to the death of the *Actinia* or to some other cause, so that the internal structures of the *Sagartia* remained practically uninjured. Grosvenor (*Proc. R.S.L.*, vol. 72, 1903, pp. 478-479) ascribes the discharge of nematocysts to osmotic action. His experiments show that the contents of the capsule are able to take

\* Owing to the value of these aged specimens, we have not been able to make sufficient experiments upon them to determine whether they have a sense of taste, but the above observations seem to suggest that such a sense is present, though feebly developed. For an account of such experiments see G. H. Parker, "The Reactions of *Metridium* to Food and other Substances," *Bull. Mus. Comp. Zool. Harvard*, vol. xxix., 1896, pp. 107-119. Parker concludes that the tentacles of this anemone when stimulated with meat juice move so as to point to the mouth; similar stimulation to the lips gives rise to peristaltic movements in the stomodæum, reversal of the ciliary action of the lips, and contraction of the sphincter muscle of the oral disc.

up liquid from sea-water until, on the pressure reaching a certain amount, the thread is shot out. Such discharge would probably take place only in sea-water, or in some fluid which differs but slightly in density from sea-water.\* The *Actinia*, on entering the cœlenteron of its captor and becoming surrounded by the denser mucous secretion poured out upon it, would probably be rendered innocuous, its nematocysts becoming inoperative. Even if the mucous secretion merely served to delay the discharge of the nematocysts (as is almost certain, for it would prevent or retard the access of sea-water), it is probable that the density of the fluid in the cœlenteron (after closure of the stomodæum) would, from other causes, soon increase to such an amount as to then render the discharge of nematocysts impossible. That such a change in the contents of the cœlenteron does occur soon after closure of the stomodæum is evident from the behaviour of the young anemones described below. Then, again, the mucous secretion which the captor forms over its prey would also act as a shield against any nematocysts of the latter which might be discharged. We may account in one or other of these ways for the apparently uninjured condition of the internal structures of the captor.

Miss Nelson's specimens of *Sagartia troglodytes* and also of *Actinia mesembryanthemum* have been very prolific, though only a small proportion of the young produced has survived. As a rule, most of them disappear within a week or two after birth, some being devoured by the adults of their own or other species, and the rest disappear in other ways not ascertained. Both species breed in early spring: *Actinia* commences to bring forth young as early as the beginning of February, and *Sagartia* about a month

\* The fact that the nematocysts of Hydroids are able to pass undischarged through a portion of the alimentary canal and into the dorsal processes of *Aolis*, but may be discharged on being extruded into sea-water, supports this view. Again, some fish appear to feed with impunity on anemones and other Cœlenterates, e.g. *Peachia hastata* is found in the stomach of the cod (M'Intosh, *The Marine Invertebrates and Fishes of St Andrews*, p. 37), swarms of an *Edwardsia* in the stomach of the flounder (p. 38), while *Virgularia mirabilis* is also occasionally seen in the cod's stomach (p. 39). Anemones are sometimes used on parts of the Scottish coast as bait for cod, and are found to answer this purpose well (see, for example, M'Intosh, *The Resources of the Sea*, p. 129). Off the south coast of Iceland one of us has seen the stomach of a cod full of specimens of *Pennatulula*.



later. As a rule, only a few young are extruded at one time, and generally early in the morning, and one individual may repeat this operation every morning for several weeks. The young, the majority of which when extruded already possess the first two cycles of tentacles (*i.e.* twelve tentacles), are not expelled with violence, but gently, and usually lie for a time, with their tentacles retracted, on the disc of the parent. They are dispersed in a manner which is no doubt very useful and effective in a tidal pool on the sea-shore. At or soon after extrusion the basal portion of each young anemone is much swollen, owing to the presence of a considerable amount of fluid in the cœlenteron, so that the pedal disc becomes strongly convex. This is probably due to the fact that the tentacles being retracted and the mouth closed, the products of metabolism are unable to escape. In addition to their mere accumulation, the soluble products exert some osmotic action which causes sea-water to diffuse through the thin body-wall into the cœlenteron, thus strongly inflating the basal portion of the young animal. Owing to this basal inflation and the retraction of the oral end the young anemone has an almost globular shape, so that the slightest current in the water causes it to roll off the oral disc of its parent, and often carries it some distance before it sinks to the bottom, as its specific gravity is not much greater than that of sea-water. As soon as the young anemone finds the bottom of the vessel it becomes orientated in the proper direction and fixed by the pedal disc, apparently possessing already that well-marked polarity which is characteristic even of pieces of adult anemones which include a portion of the pedal disc (see A. P. Hazen, *Arch. f. Entwicklungsmechanik d. Org.*, Bd. 14, 1902, pp. 592-599, and Bd. 16, 1903, pp. 365-376, *Sagartia luciae*). We have occasionally seen adult specimens of *Actinia mesembryanthemum* assume this globular and buoyant form, the pedal disc becoming free from its attachment, the basal part of the animal swollen and the oral disc retracted. Both *S. troglodytes* and *A. mesembryanthemum* are frequently found in this condition at birth, but adult specimens of the former rarely adopt it, though a case is mentioned by Gosse (1860, p. 95). *S. troglodytes* seems to rarely change its station when once settled in a cavity which is to its liking.

*S. troglodytes* is, or may be, viviparous. As stated above (p. 298), all the young which we have seen extruded were already provided with six, or more usually twelve tentacles. Our experience agrees with that of Mr Sydney Chaffers, Registrar of the Owens College, Manchester (see also p. 301), whose specimens have invariably reproduced in a similar manner. He informs us that he has seen many batches of young born, and has succeeded in feeding some of them within a few minutes after extrusion. Neither Mr Chaffers nor ourselves have seen any ova or ciliated larvæ issue from the mouth. Oskar Carlgren, however, states ("Die Brutpflege der Actiniarien," *Biol. Centralbl.*, Bd. 21, 1901, p. 469) that in *S. troglodytes*, *S. viduata*, and *S. undata*, fertilisation of the ova takes place in the sea-water outside the parent. It appears, therefore, that *S. troglodytes* may be either oviparous, as in Carlgren's specimens, or viviparous, as in Mr Chaffers' and ours.

The mode of reproduction in anemones is evidently subject to some variation. For example, *Bunodactis (Bunodes) gemmacea* is usually viviparous, "living and well-formed young" with twelve tentacles being brought forth (Gosse, 1860, p. 193, and Carlgren, *Biol. Centralbl.*, Bd. 21, 1901, p. 469). Mr Chaffers, who has also observed the reproduction of anemones of this species, states that he has found them to be in all cases but one viviparous. He observed on one occasion the extrusion of four or five ciliated larvæ, which swam vigorously for some minutes.

We have carefully observed the old specimens of *Sagartia troglodytes* during the last two years, with the view of noting any points of interest in their appearance and physiology. It was not possible to obtain one for dissection or histological examination. On comparing these old ones with younger specimens, there is seen to be little difference in their external characters. Certain younger individuals, the progeny of the old ones, and now about fourteen years old, are living in another aquarium, to which they were removed soon after birth. They have been under very favourable circumstances as regards volume of water, feeding, etc., and are now larger than their parents. The latter are rather more variegated in their coloration than is the case with their offspring, but these differences are not important. The coloration

of this species is, as Gosse has pointed out (1860, pp. 89-92), extremely variable. Specimens of this species collected by one of us in the Faeroës are both smaller and more intensely pigmented than others from the Scottish coast. Specimens kept in captivity show little tendency to increase in size, but become decidedly paler in colour. These old captives are lighter in colour than individuals which have been more recently taken from a rock pool.

All the individuals of this species which we have observed are sensitive to changes of light and of temperature, becoming and remaining semi-contracted during cold weather and at night, but expanding to their fullest in warm, bright weather. The old ones are much more strongly affected by unfavourable conditions than those which are more than thirty years younger, and also are longer in recovering when conditions become again favourable. When the aquaria are examined in early morning or in fine warm weather succeeding a period of cold, it is found that the old specimens remain contracted for some time after their children and grandchildren are fully expanded.

The most notable difference between the old (fifty years) and the younger (fourteen years) individuals of *Sagartia troglodytes* is, as would be expected, in point of fertility. In 1903 the sixteen old ones did not produce altogether more than half a dozen young; indeed, it is doubtful whether they bred at all, as the few young found beside them may not have been their progeny. During the same period their children and grandchildren reproduced in large numbers (hundreds), though, as mentioned above (p. 297), only a few of these survived.

*Sagartia troglodytes*, in these aquaria at any rate, apparently takes three years to reach maturity.

In the early part of 1904 the aquaria were somewhat neglected, the water was aerated less frequently and not changed for over three months, and the animals remained unfed for a longer period than usual. Probably as a result of these less favourable conditions only a few young, much fewer than usual, were produced, even by the younger specimens of *Sagartia*; these younger ones were abnormally thin and transparent, and were not extruded until early in April. The sixteen original specimens produced no offspring whatever in the spring of this year (1904).

Specimens of *Actinia mesembryanthemum* living under identical conditions and in the same aquaria as the *Sagartia* were more fruitful, two in particular being very prolific, though their breeding season was somewhat retarded. It would therefore appear that *S. troglodytes* is more sensitive than *A. mesembryanthemum* to changes in the environment, and that these changes exert a considerable effect on the reproduction, though it is obvious that there is some individual variation in this respect.

In August 1903 two specimens of *S. troglodytes* were brought from Thorshavn in the Faeroës and placed in the aquaria. In the following October each produced several young, and in April 1904 one of them gave birth to a single young anemone. All the other specimens of *S. troglodytes* which were under the same conditions breed only in the spring, and it is improbable that October is the normal breeding-time of specimens under natural conditions in the Faeroës, as by this late season of the year the sea is already running high, and there would be a great risk of the delicate young anemones being unable to fix themselves, and being destroyed. It is probable that the change of environment (perhaps temperature was largely responsible) had induced these anemones to breed out of their usual season (see also p. 303).

We are indebted to Mr Sydney Chaffers for sending to us some particulars regarding anemones which he has kept in captivity for a number of years (see also pp. 299, 303). These specimens have in most cases been returned to the sea. He has kept for a period of eight years, without any difficulty, specimens of *Actinia mesembryanthemum*, *Sagartia troglodytes*, and *Bunodactis (Bunoles) gemmacea* in aquaria containing about seven gallons of sea-water. These anemones were fed regularly twice a week on a portion of the mantle of *Mytilus*, and the water was aerated every other day by means of a glass syringe. Mr Chaffers states that during these eight years there was no appreciable alteration in the size and appearance of these anemones. This supports the view that under favourable conditions they may live to a great age.

Miss Nelson informs us that *Actinia mesembryanthemum* is the only other anemone which she has been successful in keeping for any length of time, and that no specimens of this species have lived in her collection for more than about eight years.

A specimen of this species collected by Sir J. Graham Dalyell (1848, p. 203) at North Berwick in August 1828 reached the age of about sixty-six years. So far as we can ascertain, this is the only recorded example of longevity in anemones, and is quoted by Gosse (1860, p. 182), M'Bain (1878, p. 280), Weismann (1891, pp. 6, 55), and others.

Dalyell computed, after comparison of the size of this specimen with that of others which had been bred in his aquaria, that it must have been at least seven years old at the time of its capture. After Dalyell's death in 1851 this anemone was placed successively under the care of several naturalists, and died in August 1887, being then about sixty-six years old. Unfortunately, nothing is known with certainty as to the cause of its death. The obituary notice which appeared in *The Scotsman* states that the anemone "appeared to be in excellent health up to a few weeks ago, when it was attacked by a parasitic disease, which finally proved fatal." Mr R. Lindsay, who had charge of this anemone during the last five years of its life, informs us that this report is unfounded, and that "the death of the anemone was not due to any parasitic disease," but was apparently "natural." There is also a footnote \* to this effect on p. 55 of Weismann's *Essays* (1891, vol. i.). It was kept in a comparatively small volume of water (the vessel in which it lived is described as a large tumbler), was fed on half a mussel once a fortnight, and the sea-water was changed soon afterwards.

During the first twenty years of its life it produced 334 young (Dalyell, 1848, p. 213), and then remained unproductive for some years, but during the spring of 1857 it gave birth to 230 young during the course of a single night (M'Bain, 1878, p. 286). For the next fifteen years it was unproductive, but in August 1872 it produced a brood of 30, and in December of the same year one of 9. It continued to reproduce each year, the number of its young being from 5 to 20 at a birth. During the seven years beginning August 1872, over 150 living young were born. Two of these were isolated and regularly fed, and at the age of

\* "It died, by a natural death, on August 4th, 1887, after having appeared to become gradually weaker for some months previous to this date."—Footnote by Professor Poulton, from information obtained by Mr J. S. Haldane.

four years produced over 20 young ones, so that the offspring produced by Dalyell's *Actinia* when it had reached the age of fifty years were quite normal and vigorous.

A few of the statements regarding the breeding of these anemones in captivity may be brought together here. As noted above (p. 297), Miss Nelson's specimens of *Sagartia troglodytes*, which are usually fed once a month, breed in the spring. During the spring of this year (1904), however, when they were somewhat neglected, and feeding, aeration and change of water occurred at longer intervals, they were much less productive. A Faerish specimen of this species placed in the same aquarium bred in autumn 1903 and in the spring of 1904, the latter being probably its normal, and the former an unusual breeding season, induced by change in the environment, rise of temperature being probably an important factor (though better feeding may have contributed to the result).\*

Mr Chaffers states that his specimens of *S. troglodytes* and *A. mesembryanthemum*, which are fed twice a week, bring forth young at all times of the year except during the cold weather.

Dalyell (p. 214) states that "feeding certainly promotes fertility" in *Actinia mesembryanthemum*.

From these facts it appears that temperature and feeding exercise a very considerable influence upon the production of young in these forms of life.

*Sagartia troglodytes* and *Actinia mesembryanthemum* are viviparous; the former may also be oviparous (see p. 299). *Bunodactis* (*Bunodes*) *gemmaea* is usually viviparous, but Mr Chaffers has observed, on one occasion, the extrusion of ciliated larvæ.

Little is known concerning the rate of growth and the duration of life in Cœlenterates, but it may be useful to collect here some of the scattered references to these subjects.

*Hydrozoa*.—Evidence shows that Hydroids grow rapidly, for, as Hincks (1868, p. xliii) remarks, "timber immersed in the sea is

\* This specimen was taken from a pool near high-water mark, where food was probably not abundant. We have noticed, on the west coast of Scotland, that the largest specimens are almost invariably found in the pools near low-water mark, those living in pools higher up the beach being distinctly smaller.

soon found to be covered with a luxuriant growth of zoophyte . . . . a *Eudendrium* has been observed to cover the bottom of a boat in fifteen days.

One of us has observed off the coast of the Malay Peninsula hydroid colonies (*Obelia*, sp.) several inches in length attached to the cast skins of sea snakes (*Enhydrina valakadien* and others). These therefore had grown upon the skins before the latter had had time to disintegrate, for such colonies were not present on any of the hundreds of living sea snakes examined.

Hincks states (p. xlv) that some species of hydroids, especially such as grow on fronds and stems of seaweed, are annuals. The larger arborescent masses of the stouter kinds of *Sertularia*, *Helecium*, *Eudendrium*, etc., are, however, probably the growth of several seasons.\*

Some of the Siphonophora are probably annual. A species of *Porpita* † is common in calm warm weather (February to April) in the Indian seas, but completely disappears in the stormy season (about July). This animal has no power of sinking, and its complete disappearance seems to indicate that it has perished, and those which appear in the next warm season probably belong to the following generation.

\* There is a complete absence of hydranths in some forms during the winter, but the cenosarc persists, and new polyps develop by budding in the following spring. Weismann (*Die Entstehung der Sexualzellen bei den Hydromedusee*, p. 102, Jena, 1883) states that in *Eudendrium racemosum* the hydranths are wanting during the winter in those colonies which are situated in exposed stormy places, but they may persist in those which live in more protected situations. The hydranths of *Tubularia indivisa* (Allman, *Gymnoblactic Hydroids*, p. 403, Ray Soc., 1871) are in greatest perfection during spring and summer, and when the racemes of gonophores have attained their greatest size the hydranths are "perpetually cast off and renewed." Towards the end of summer the renewal of the hydranths ceases, and the upper parts of the perisarc tubes are empty, and probably remain so during the winter, new hydranths being formed in the spring. Van Beneden ("Recherches sur la Faune Littorale de Belgique (Polypes)," *Mém. l'Acad. Roy. de Belgique*, t. 36, 1867, p. 101) records specimens of *Tubularia* and *Campanularia* which have lived in his aquaria for several years without any diminution of their powers of growth.

† It may be of interest to refer here to what we believe is the first reference in English to *Porpita*. It occurs in a letter written from Goa by Thomas Steevens in 1579. In describing his voyage to India he says—"The first sign of land was certain fowls which they know to be of India. The second was boughs of palms and sedges. The third, snakes swimming on the water,

*Anthozoa.*

(1) *Actiniaria*.—The instances given earlier in this paper show that the age at which an anemone becomes mature varies with the species and conditions. For example, Dalyell (p. 217) records a specimen of *Actinia mesembryanthemum*, one of the progeny of his famous specimen, which was mature fifteen months after its birth; while M'Bain (p. 287) states that another of the progeny of this same parent, although carefully tended and fed at least once a week, was four years in reaching maturity. *Sagartia troglodytes* seems to be at least three years in reaching maturity, at any rate in captivity. These anemones may continue productive, either regularly or at intervals (this being apparently largely determined by the external conditions and regularity of feeding), for over fifty years. The only information available respecting the actual duration of life in anemones is that derived from the statement that Dalyell's *Actinia* apparently died "a natural death" at the age of sixty-six (see p. 302). Miss Nelson's specimens of *Sagartia*, which are now about fifty years old, show little sign of loss of vegetative vigour, but, as noted above (p. 300), breed either sparingly or not at all.

(2) *Matreporaria*.—The only reference known to us upon the duration of life in corals is contained in a paper by Mr Stanley Gardiner (1902). He describes (pp. 465-468) the life history of *Flabellum rubrum*, and states that by the time the corallum measures 15-17 mm. along the long axis of its calicle, the mesenteries bear testes, and spermatozoa are being discharged from those on the larger mesenteries. Coralla of this size bear "five lines of growth, which correspond probably to annual periods." Later, the male organs gradually disappear and ova are found on the mesenteries. In specimens in which the axis of the corallum is over 25 mm. in length, ripe ova are present. As the two or three large ova on each mesentery are extruded, a similar number of smaller ones take their place, and this

*and a substance which they call by the name of a coin of money as broad and as round as a groat, wonderfully printed and stamped of nature like unto some coin.*"—*Voyages and Travels, mainly during the Sixteenth and Seventeenth Centuries*, C. R. Beazley, 1908, p. 158.



process is continued for a considerable time, there being no dearth or vacuity in the ovary. Mr Gardiner finds, however, that in a specimen 40 mm. long some of the mesenteries bear no ova, but on most of them isolated ova are present. On none of the mesenteries are there any small ova to take the place of those which had escaped or were about to escape. "It seemed obvious that a critical period had been reached, after which ova ceased to develop. . . . There is no direct proof—indeed it is only a presumption—that the polyp now dies." This seems, however, very probable, for the largest specimen among over 600 from the Cape of Good Hope measured 42 mm. in length, and Mr Gardiner dredged eight dead ones in the Maldives which average about 38 mm. His largest living specimen, the one described above, measured 40 mm.

Mr Gardiner has been good enough to re-examine his material, and to give us some valuable information respecting the number of growth-lines on these old specimens. These growth-lines are difficult to count in specimens in which the calicle is longer than 20 mm. He found that the maximum number of lines, allowing for the cut-off base, is about 24 in the largest specimens. We may assume, therefore, that these specimens of *Flabellum*, which were obviously nearing the end of their reproductive powers, and probably also near the end of life, were about twenty-four years old.

Mr Gardiner states (1902, p. 469) that he examined, on the reefs of Rotuma, a large area covered by *Madrepora pulchra*, Brook, var. *alveolata*, Brook, and found that most of the polyps were dead. The living polyps were all female, and the reproductive organs were in the condition described above for the 40 mm. *Flabellum*, that is, the ova were either few and isolated, or had been already discharged. In this and in other similar cases mentioned there were no external conditions, such as silting up, which might account for death. Each colony has presumably originated from single ovum, and the limitations in the size of the colonies point to some reason innate in the organisms themselves. "There can be no rejuvenescence, and the operative cause is probably the same as that which ultimately produces the death of our forest trees," but Mr Gardiner does not consider that he is able to offer an

explanation which is complete or quite satisfactory (but see his paper, 1902, pp. 470, 471).

We are grateful to Mr Gardiner for permission to bring forward here some of his observations, not yet published, on the probable age of certain large colonies of Maldivan corals \* which seemed to be dying. His method of estimating the age of these colonies is as follows:—The number of polyps on colonies, the age of which is approximately known, † is first determined. Each of these colonies presumably originated from a single primary polyp, and the numerous polyps have been produced by successive budding. The number of polyps so produced would increase in approximately geometrical progression. Knowing the period required for the production of the known number of polyps on the colony of known age, it is possible to make an estimate of the age of the old colonies of the same species from the number of polyps of which they are composed. Mr Gardiner finds that the results of his examination of several colonies are strikingly uniform, giving a maximum age of twenty-two to twenty-eight years.

It is therefore probable that the duration of life in solitary corals like *Flabellum* is about twenty-four years, and in colonial corals such as *Goniastrea*, *Prionastrea*, *Orbicella*, and *Pocillopora*, from twenty-two to twenty-eight years.

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\* *Goniastrea retiformis*, *Prionastrea fuscoviridis*, *Orbicella laxa*, and various species or facies of *Pocillopora*.

† These colonies must have grown up (from ova) within a period "certainly less than three years, and probably not more than two years and ten months." They were obtained from a canal cut through the reef of Hulule, which is regularly cleaned out once every three years. See J. S. Gardiner, *The Fauna and Geography of the Maldive and Laccadive Archipelagoes*, vol. i. pp. 329, 330, Cambridge, 1903.

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**Note on the Molecular Condition of Nickel (and Iron)  
demagnetised by decreasing Reversals. By James  
Russell.**

(Read July 18, 1904.)

In a former communication\* it has been shown that iron demagnetised by decreasing reversals of a directional force  $ab$ , develops an induction component at right angles to the subsequent magnetising force  $H$ , when the angle  $\theta$  between these two forces is other than  $0^\circ$  and  $90^\circ$ . This component after reaching a maximum tends to disappear as saturation values are reached.

It has now been found that these transverse induction effects also exist in nickel.

The curves for nickel *resemble* those for iron in the following respects:—

(*First*) They change sign either if the direction of  $H$  be reversed, or if  $ab$  be rotated through an angle of  $90^\circ$ ;

(*Second*) Their maxima are sharpest when  $\theta = 45^\circ$ ; and

(*Third*) They vanish in the horizontal axis when  $\theta = 0^\circ$  and  $90^\circ$ .

The curves for nickel *differ from* those for iron in the following respects:—

(*First*) The smallness of the transverse induction is extreme. When  $\theta = 45^\circ$ , the nickel curves reach a maximum of about 13 C.G.S. units only. In iron, under the same conditions, the maximum attained is equal to fully 230 C.G.S. units. In order therefore to compare by superposition the curves obtained for nickel and iron, the nickel ordinates require to be increased *eighteen* times.

(*Second*) If  $ab$  be rotated so that  $\theta$  is gradually reduced from  $45^\circ$  to  $0^\circ$ , and the values of  $H$  be not too small, the curves are relatively increased in value to an extent greater than the corresponding curves for iron. Further, if  $\theta$  be not too small, the  $45^\circ$  maximum is even exceeded.

\* "The Molecular Condition of Iron demagnetised by various Methods," *Proceedings Roy. Soc. Edin.*, vol. xxiv. p. 544.

The above results may be equally well illustrated if transverse induction be plotted not against  $H$  for various values of  $\theta$ , but against  $\theta$  increasing from  $0^\circ$  to  $90^\circ$  for various values of  $H$ . If the values of  $H$  be low, the curves for both metals appear to reach their maxima when  $\theta$  is approximately equal to  $45^\circ$ . If, however,  $H$  be taken higher, maximum values are rapidly displaced to the left, the curves rising very abruptly between  $0^\circ$  and  $15^\circ$ . In iron, on the other hand, this displacement occurs slowly, and is (within present experimental limits) much less in amount.

The above experiments were made with hollow cylinders, so constructed that the shell of each cylinder was itself hollow. Or, they may be described as hollow anchor rings flattened so that the difference between the internal and external radii was less than 1 in 10. The width of each hollow ring was made nearly equal to  $\pi$  times its average radius. The smallness of the transverse effect in nickel necessitated the elimination of the demagnetising effect of the ends of the hollow cylinders previously used.

I take this opportunity of acknowledging my indebtedness to the Royal Society of London for placing at my disposal a Government grant for the purposes of this research.

*(Issued separately August 22, 1904.)*

**On the Front and Rear of a Free Progression of Waves in Deep Water.** (*Continued from Proc. R.S.E., Feb. 1st, 1904.*) By Lord Kelvin.

(Read June 20, 1904.)

§ 11. The present communication is substituted for another bearing the same title, which was read before the Royal Society of Edinburgh on January 7th, 1887, because the result of that paper was rendered imperfect and unsatisfactory by omission of the exponential factor referred to in § 10 of my paper of February 1st, 1904. I shall refer henceforth to the last-mentioned paper as §§ 1 . . . . 10 above.

§ 12. I begin by considering progressions produced by superposition of static initiating disturbances, of the type expressed in (12) of § 4 above; graphically represented by fig. 1; and leading to motion investigated in §§ 1-3, 5-10. The particular type of that solution which I now choose, is that chosen at the end of § 4, which we, with a slight but useful modification,\* may now write as follows:—

$$\left. \begin{aligned}
 -\zeta = \phi(x, t) &= \sqrt{\frac{2}{\rho}} \cos\left(\frac{gt^2x}{4\rho^2} - \frac{1}{2}\chi\right) \epsilon^{\frac{-gt^2x}{4\rho^2}} \\
 \text{where } \rho &= \sqrt{(z^2 + x^2)}, \text{ and } \chi = \tan^{-1}(x/z)
 \end{aligned} \right\} \dots (17).$$

Here  $-\zeta$  denotes the upward vertical component of the displacement of the fluid at time  $t$  from its undisturbed position at point  $(x, z)$ , which may be either in the free surface or anywhere below it. Taking  $t=0$  in (17), we have, for the initial height of the free surface above the undisturbed level,

$$-\zeta_0 = \phi(x, 0) = \sqrt{\frac{2}{\rho}} \cdot \sqrt{\frac{\rho+z}{2\rho}} = \frac{\sqrt{(\rho+z)}}{\rho} \dots (18).$$

§ 13. We shall first take, as initiating disturbance, a row extending from  $-\infty$  to  $+\infty$  of superpositions of (18); alternately

\* The substitution of  $\frac{1}{2}\chi$ , for  $\frac{1}{2}\pi - \tan^{-1}\sqrt{\frac{\rho+z}{\rho-z}}$ , saves considerable labour and use of logarithms; especially when, as in our calculations,  $z=1$ .

positive and negative ; and placed at equal successive distances  $\frac{1}{2}\lambda$  : so that we now have

$$-\zeta_0 = P(x, 0) = \sum_{i=-\infty}^{i=+\infty} (-1)^i \phi\left(x + i\frac{\lambda}{2}, 0\right) . . . (19),$$

or, as we may write it,

$$-\zeta_0 = P(x, 0) = \sum_{i=-\infty}^{i=+\infty} D(x + i\lambda, 0) . . . (19'),$$

where

$$D(x, 0) = \phi(x, 0) - \phi\left(x + \frac{\lambda}{2}, 0\right) . . . (20).$$

In (19), P denotes a space-periodic function, with  $\lambda$  for its period. This formula, with  $t$  substituted for 0, represents  $-\zeta_t$ , being the elevation of the surface above undisturbed level at time  $t$ , in virtue of initial disturbance represented by (19).

§ 14. Remark now that whatever function be represented by  $\phi$ , the formula for P in (19) implies that

$$P(x + \lambda, 0) = P(x, 0) . . . (21),$$

which means that P is a space-periodic function with  $\lambda$  for period. And (19) also implies that

$$P(x + \frac{1}{2}\lambda, 0) = -P(x, 0) . . . (22) ;$$

which includes (21). And with the actual function, (18), which we have chosen for  $\phi(x, 0)$ , the fact that  $\phi(x, 0) = \phi(-x, 0)$  makes

$$P(x, 0) = P(-x, 0) . . . (23).$$

Thus (19) has a graph of the character fig. 5, symmetrical on each

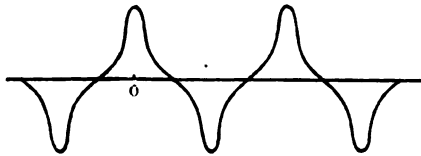


FIG. 5.

side of each maximum and minimum ordinate. The Fourier harmonic analysis of  $P(x, 0)$ , when subject to (22) and (23), gives

$$P(x, 0) = A_1 \cos \frac{2\pi x}{\lambda} + A_3 \cos 3\frac{2\pi x}{\lambda} + A_5 \cos 5\frac{2\pi x}{\lambda} + \dots (24).$$

§ 15. *Digression on periodic functions generated by addition of*

values of any function for equidifferent arguments. Let  $f(x)$  be any function whatever, periodic or non-periodic; and let

$$P(x) = \sum_{i=-\infty}^{i=+\infty} f(x+i\lambda) \dots \dots \dots (25);$$

which makes

$$P(x) = P(x+\lambda) \dots \dots \dots (26).$$

Let the Fourier harmonic expansion of  $P(x)$  be expressed as follows:—

$$P(x) = A_0 + A_1 \cos \alpha + A_2 \cos 2\alpha + A_3 \cos 3\alpha + \dots \dots \dots \left. \begin{array}{l} \text{where } \alpha = \frac{2\pi x}{\lambda} \\ + B_1 \sin \alpha + B_2 \sin 2\alpha + B_3 \sin 3\alpha + \dots \dots \end{array} \right\} \dots \dots (27).$$

Denoting by  $j$  any integer, we have by Fourier's analysis

$$\frac{1}{2} \lambda \frac{A_j}{B_j} = \int_0^\lambda dx P(x) \frac{\cos j \frac{2\pi x}{\lambda}}{\sin j \frac{2\pi x}{\lambda}} \dots \dots \dots (28);$$

which gives

$$\left. \begin{array}{l} \frac{1}{2} \lambda A_j = \sum_{i=-\infty}^{i=+\infty} \int_0^\lambda dx f(x+i\lambda) \cos j \frac{2\pi x}{\lambda} = \int_{-\infty}^{+\infty} dx f(x) \cos j \frac{2\pi x}{\lambda} \\ \frac{1}{2} \lambda B_j = \sum_{i=-\infty}^{i=+\infty} \int_0^\lambda dx f(x+i\lambda) \sin j \frac{2\pi x}{\lambda} = \int_{-\infty}^{+\infty} dx f(x) \sin j \frac{2\pi x}{\lambda} \end{array} \right\} \dots \dots (29).$$

§ 16. Take now in (29), as by (19'), (20),

$$f(x) = \phi(x, 0) - \phi\left(x + \frac{\lambda}{2}, 0\right) \dots \dots \dots (30).$$

This reduces all the  $B$ 's to zero; reduces the  $A$ 's to zero for even values of  $j$ ; and for odd values of  $j$  gives, in virtue of (22),

$$\frac{1}{2} \lambda A_j = 2 \int_{-\infty}^{+\infty} dx \phi(x, 0) \cos j \frac{2\pi x}{\lambda} \dots \dots \dots (31).$$

Go back now to §§ 3, 4, (6), (12), above; and, according to the last lines of § 4, take

$$\phi(x, 0) = \left\{ RS \right\} \frac{\sqrt{2}}{\sqrt{z+\alpha}} = \frac{\sqrt{(\rho+z)}}{\rho} \dots \dots \dots (32).$$

Hence, for the harmonic expansion (24) of  $P(x, 0)$ , we have

$$A_j = \frac{4}{\lambda} \int_{-\infty}^{+\infty} dx \frac{\sqrt{(\rho+z)}}{\rho} \cos j \frac{2\pi x}{\rho} = \frac{4}{\lambda} \left\{ RS \right\} \int_{-\infty}^{+\infty} dx \frac{\sqrt{2}}{\sqrt{z+\alpha}} \cos j \frac{2\pi x}{\lambda} \dots \dots \dots (33).$$

The imaginary form of the last member of this equation facilitates the evaluation of the integral. Instead of  $\cos j \frac{2\pi x}{\lambda}$  in the last



factor, substitute

$$\cos j \frac{2\pi x}{\lambda} + i \sin \frac{2\pi x}{\lambda}, \quad \text{or } e^{j \frac{2\pi x}{\lambda}} \dots \dots \dots (34).$$

The alternative makes no difference in the summation  $\int_{-\infty}^{+\infty} dx$ , because the sine term disappears for the same reason that the sine terms in (29) disappear because of (30). Thus (33) becomes

$$A_j = \frac{4}{\lambda} \left\{ \text{RS} \right\} \int_{-\infty}^{+\infty} dx \frac{\sqrt{2}}{\sqrt{z + ix}} e^{j \frac{2\pi x}{\lambda}} \dots \dots \dots (35);$$

put now  $\sqrt{z + ix} = i\sigma$ ; whence  $\frac{dx}{\sqrt{z + ix}} = 2d\sigma$ , and  $ix = -\sigma^2 - z$ .  
 $\dots \dots \dots (36).$

Using these in (35) we may omit the instruction {RS} because nothing imaginary remains in the formula: thus we find

$$A_j = \frac{8\sqrt{2}}{\lambda} \int_{-\infty}^{\infty} d\sigma e^{-\frac{2\pi j}{\lambda} \sigma^2} \cdot e^{-\frac{\pi j z}{\lambda}} = e^{-\frac{2\pi j z}{\lambda}} \cdot \frac{8\sqrt{2}}{\lambda} \cdot \sqrt{\frac{\lambda}{2\pi j}} \cdot \sqrt{\pi} \dots \dots \dots (37),$$

$$= e^{-\frac{2\pi j z}{\lambda}} \cdot \frac{8}{\sqrt{j\lambda}} \dots \dots \dots (38).$$

The transition in (37) is made in virtue of Laplace's celebrated discovery  $\int_{-\infty}^{\infty} d\sigma e^{-m\sigma^2} = \sqrt{\frac{\pi}{m}}$ .

§ 17. Equation (38) allows us readily to see how near to a curve of sines is the graph of  $P(x, 0)$ , for any particular value of  $\lambda/z$ . It shows that

$$A_1 = \frac{8}{\sqrt{\lambda}} e^{-\frac{2\pi z}{\lambda}}; \quad A_3/A_1 = \sqrt{\frac{3}{5}} \cdot e^{-\frac{4\pi z}{\lambda}}; \quad A_5/A_3 = \sqrt{\frac{5}{7}} \cdot e^{-\frac{4\pi z}{\lambda}}; \quad \dots (38).$$

Suppose for example  $\lambda = 4z$ ; we have

$$e^{-\frac{4\pi z}{\lambda}} = e^{-\pi} = \cdot 043214; \quad A_3/A_1 = \cdot 02495; \quad A_5/A_3 = \cdot 03347 \dots (39).$$

Thus we see that  $A_3$  is about 1/40 of  $A_1$ ; and  $A_5$ , about  $\frac{1}{30}$  of  $A_3$ . This is a fair approach to sinusoidality; but not quite near enough for our present purpose. Try next  $\lambda = 2z$ ; we have

$$A_1 = \frac{8}{\sqrt{\lambda}} \cdot 043214; \quad e^{-2\pi} = \cdot 001867; \quad A_3/A_1 = \cdot 001078 \dots (40).$$

Thus  $A_3$  is about a thousandth of  $A_1$ ; and  $A_5$  about  $1\frac{1}{2} \times 10^{-6}$  of  $A_1$ . This is a quite good enough approximation for our present purpose:  $A_5$  is imperceptible in any of our calculations:  $A_3$  is negligible, though perceptible if included in our calculations (which

are carried out to four significant figures): but it would be utterly imperceptible in our diagrams. Henceforth we shall occupy ourselves chiefly with the free surface, and take  $z = h$ , the height of O, the origin of coordinates above the undisturbed level of the water.

§ 18. To find the water-surface at any time  $t$  after being left free and at rest, displaced according to any periodic function  $P(x)$  expressed Fourier-wise as in (27); take first, for the initial motionless surface-displacement, a simple sinusoidal form,

$$-\zeta_0 = A \cos(mx - c) \dots \dots \dots (41).$$

Going back to (2), (3), and (4) above, let  $w(z, x, t)$  be the downwards vertical component of displacement. We thus have, as the differential equations of the motion,

$$g \frac{dw}{dz} = \frac{d^2 w}{dt^2} \dots \dots \dots (42),$$

$$\frac{d^2 w}{dx^2} + \frac{d^2 w}{dz^2} = 0 \dots \dots \dots (43).$$

These are satisfied by

$$w = C e^{-mz} \cos(mx - c) \cos t \sqrt{gm} \dots \dots \dots (44),$$

which expresses the well-known law of two-dimensional periodic waves in infinitely deep water. And formula (44) with  $C e^{-mh} = A$  and  $t = 0$ , agrees with (41). Hence the addition of solutions (44), with  $jm$  for  $m$ ; with  $A$  successively put equal to  $A_1, A_2, \dots, B_1, B_2, \dots$ ; and, with  $c = 0$  for the  $A$ 's, and  $= \frac{1}{2}\pi$  for the  $B$ 's, gives us, for time  $t$ , the vertical component-displacement at depth  $z - h$  below the surface, if at time  $t = 0$  the water was at rest with its surface displaced according to (27). Thus, with (38), and (24), we have  $P(x, t)$ .

§ 19. Looking to (44) and (27), and putting  $m = 2\pi/\lambda$ , we see that the component motion due to any one of the  $A$ 's or  $B$ 's in the initial displacement is an endless infinite row of standing waves, having wave-lengths equal to  $\lambda/j$  and time-periods expressed by

$$\tau_j = \frac{2\pi}{\sqrt{j}m} = \sqrt{\frac{2\pi\lambda}{jg}} \dots \dots \dots (45).$$

The whole motion is not periodic because the periods of the constituent motions, being inversely as  $\sqrt{j}$ , are not commensurable. But by taking  $\lambda = 2h$  as proposed in § 17, which, according to (40), makes  $A_3$ , for the free surface, only a little more than 1/1000 of  $A_1$ , we have so near an approach to sinusoidality that in our illus-

trations we may regard the motion as being periodic, with period (45) for  $j = 1$ . This makes  $\tau = \sqrt{\pi}$  when, as in § 5, we, without loss of generality (§ 10), simplify our numerical statements by taking  $g = 4$ ; and  $h = 1$ , which makes the wave-length = 2.

§ 20. Toward our problem of "front and rear," remark now that the infinite number of parallel straight standing sinusoidal waves which we have started everywhere over an infinite plane of originally undisturbed water, may be ideally resolved into two processions of sinusoidal waves of half their height travelling in contrary directions with equal velocities  $2/\sqrt{\pi}$ .

Instead now of covering the whole water with standing waves, cover it only on the negative side of the line (not shown in our diagrams)  $YOY'$ , that is the left side of  $O$  the origin of coordinates; and leave the water plane and motionless on the right side to begin. At all *great* distances on the left side of  $O$ , there will be in the beginning, standing waves equivalent to two trains of progressive waves, of wave-length 2, travelling rightwards and leftwards with velocity  $2/\sqrt{\pi}$ . The smooth water on the right of  $O$  is obviously invaded by the rightward procession.

§ 21. Our investigation proves that the extreme perceptible rear of the leftward procession (marked  $R$  in fig. 10 below) does not, through the space  $OR$  on the left side of  $O$ , broadening with time, nor anywhere on the right of  $O$ , perceptibly disturb the rightward procession.

§ 22. Our investigation also proves that the surface at  $O$  has simple harmonic motion through all time. It farther shows that the rightward procession is very approximately sinusoidal, with simple harmonic motion, through a space  $OF$  (fig. 9) to the right of  $O$ , broadening with time; and that, at any particular distance rightwards from  $O$ , this approximation becomes more and more nearly perfect as time advances. What I call the front of the rightward procession, is the wave disturbance beyond the point  $F$ , at a not strictly defined distance rightwards from  $O$ , where the approximation to sinusoidality of shape, and simple harmonic quality of motion, is only just perceptibly at fault. We shall find that beyond  $F$  the waves are, as shown in fig. 9, less and less high, and longer and longer, at greater and greater distances from  $O$ , at one and the same time; but that the wave-height does not at

any time or place come abruptly to nothing. The propagational velocity of the beginning of the disturbance is in reality infinite, because we regard the water as infinitely incompressible.

§ 23. Thus we see that the front of the rightward procession, with sinusoidal waves following it from O, is simply given by the calculation, for positive values of  $x$ , of the motion due to an initial motionless configuration of sinusoidal furrows and ridges on the left side of O. Fig. 8 represents a static initial configuration, which we denote by  $Q(x, 0)$ , approximately realising the condition stated in § 20. Fig. 9 represents on the same scale of ordinates the surface displacement at the time  $25\tau$  in the subsequent motion due to that initial configuration; which, for any time  $t$ , we denote by  $Q(x, t)$  defined as follows:—

$$Q(x, t) = \frac{1}{2}\phi(x, t) - \phi(x + 1, t) + \phi(x + 2, t) - \dots \text{ad. inf.} \quad (46),$$

where  $\phi$  is the function defined by (17), with  $z=1$  and  $g=4$ .

§ 24. The wave-height, at all distances so far leftward from O that the influence of the rear of the leftward procession has not yet reached them at any particular time,  $t$ , after the beginning, is simply the  $P(x, t)$  of § 13 calculated according to §§ 18, 17; and the motion there is still merely standing waves, ideally resolvable into rightward and leftward processions. Let I, beyond the leftward range of fig. 10, be the point of the ideally extended diagram, not precisely defined, where the leftward procession at any particular time,  $t$ , becomes sensibly influenced by its own rear. Between I and R the whole motion is transitional in character, from the regular sinusoidal motion  $P(x, t)$  of the water on the left side of I, to regular sinusoidal motion of half wave-height  $\frac{1}{2}P(x, t)$ , from R to O; and on to F of fig. 9, the beginning of the front of the disturbance in the rightward procession. Hence to separate ideally the leftward procession from the whole disturbance due to the initial configuration, we have only to subtract  $\frac{1}{2}P(x, t)$  from  $Q(x, t)$  calculated for negative values of  $x$ . Thus the expression for the whole of the leftward procession is

$$Q(x, t) - \frac{1}{2}P(x, t) \text{ for negative values of } x \dots (47).$$

Fig. 10 represents the free surface thus found for the leftward procession alone at time  $t = 25\tau$ .

§ 25. The function  $D(x, t)$ , which appears in § 13 as an item in one of the modes of summing shown for  $P(x, 0)$  in (19'), and indicated for  $P(x, t)$  at the end of § 13, and which has been used in some of our summations for  $Q(x, t)$ ; is represented in figs. 6 and 7, for  $t=0$ , and  $t=25\tau$  respectively.

§ 26. Except for a few of the points of fig. 6, representing  $D(x, 0)$ , the calculation has been performed solely for integral values of  $x$ . It seemed at first scarcely to be expected that a fair graphic representation could be drawn from so few calculated points; but the curves have actually been drawn by Mr Witherington with no other knowledge than these points, except information as to all zeros (curve cutting the line of abscissas), through the whole range of each curve. The calculated points are marked on each curve: and it seems certain that, with the knowledge of the zeros, the true curve must lie very close in each case to that drawn by Mr Witherington.

§ 27. The calculation of  $Q(x, t)$ , for positive integral values of  $x$ , is greatly eased by the following arrangements for avoiding the necessity for direct summation of a sluggishly convergent infinite series shown in (46), by use of our knowledge of  $P(x, t)$ . We have, by (46) and (19),

$$Q(0, t) = \frac{1}{2}\phi(0, t) - \phi(1, t) + \phi(2, t) - \dots \dots \dots ad. \text{ inf.} \quad (48),$$

$$P(0, t) = \sum_{i=-\infty}^{i=+\infty} (-1)^i \phi(i, t) \dots \dots \dots (49).$$

Hence, in virtue of  $\phi(-i, t) = \phi(i, t)$ ,

$$P(0, t) = 2Q(0, t) \dots \dots \dots (50).$$

Again going back to (46), we have

$$Q(x, t) = \frac{1}{2}\phi(x, t) - \phi(x+1, t) + \phi(x+2, t) - \phi(x+3, t) + \dots \dots \dots$$

$$Q(x+1, t) = \frac{1}{2}\phi(x+1, t) - \phi(x+2, t) + \phi(x+3, t) - \dots \dots \dots$$

By adding these we find

$$Q(x+1, t) + Q(x, t) = \frac{1}{2}[\phi(x, t) - \phi(x+1, t)] = \frac{1}{2}D(x, t) \quad (51).$$

By successive applications of this equation, we find

$$2Q(x+i, t) = (-1)^i 2Q(x, t) - (-1)^i D(x, t) \pm \dots + D(x+i-1, t) \quad (52).$$

Hence by putting  $x=0$ , and using (50), we find finally

$$2Q(i, t) = (-1)^i P(0, t) - (-1)^i D(0, t) \pm \dots + D(i-1, t) \quad (53).$$

This is thoroughly convenient to calculate  $Q(1, t)$ ,  $Q(2, t)$  . . . . successively; for plotting the points shown in fig. 9.

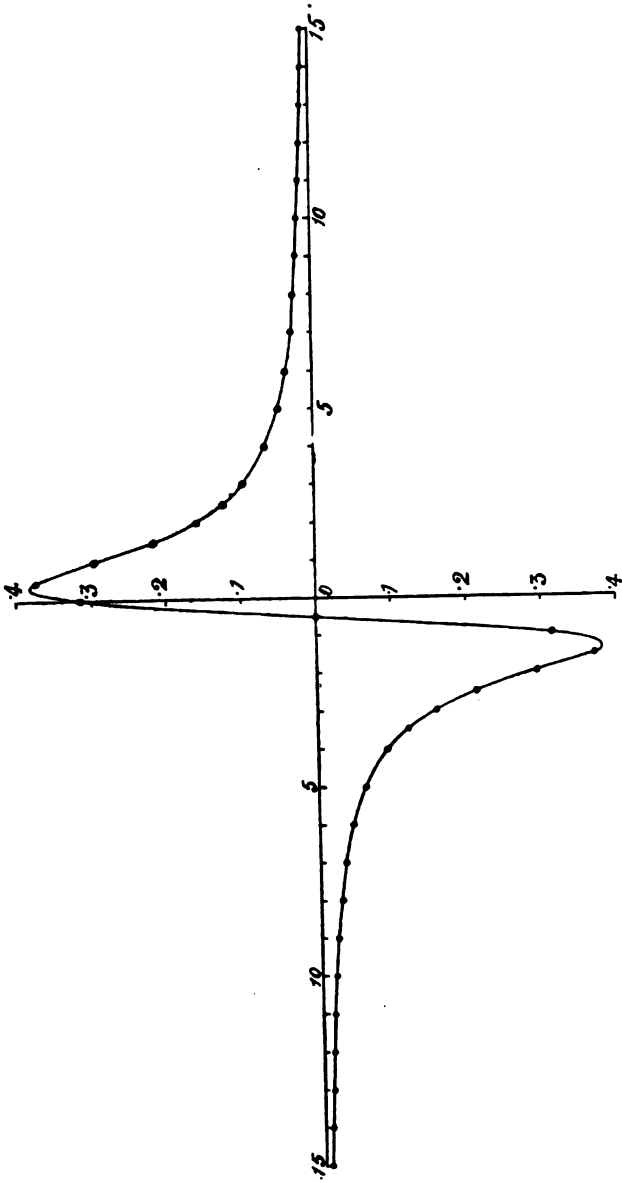
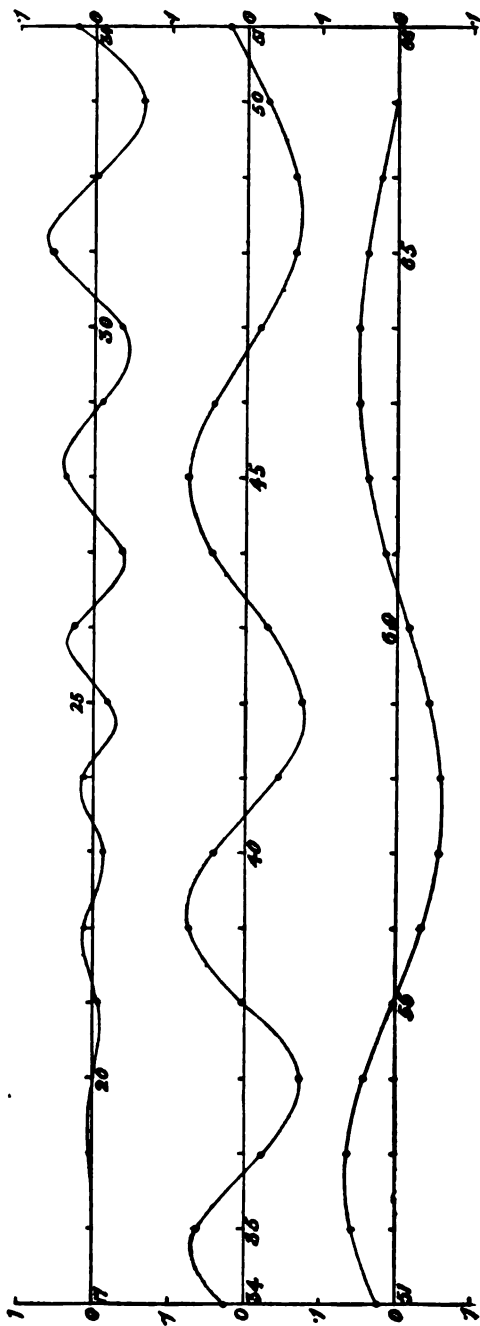


FIG. 6 ;  $D(x, 0)$

FIG. 7 ;  $D(x, 25\pi)$ .

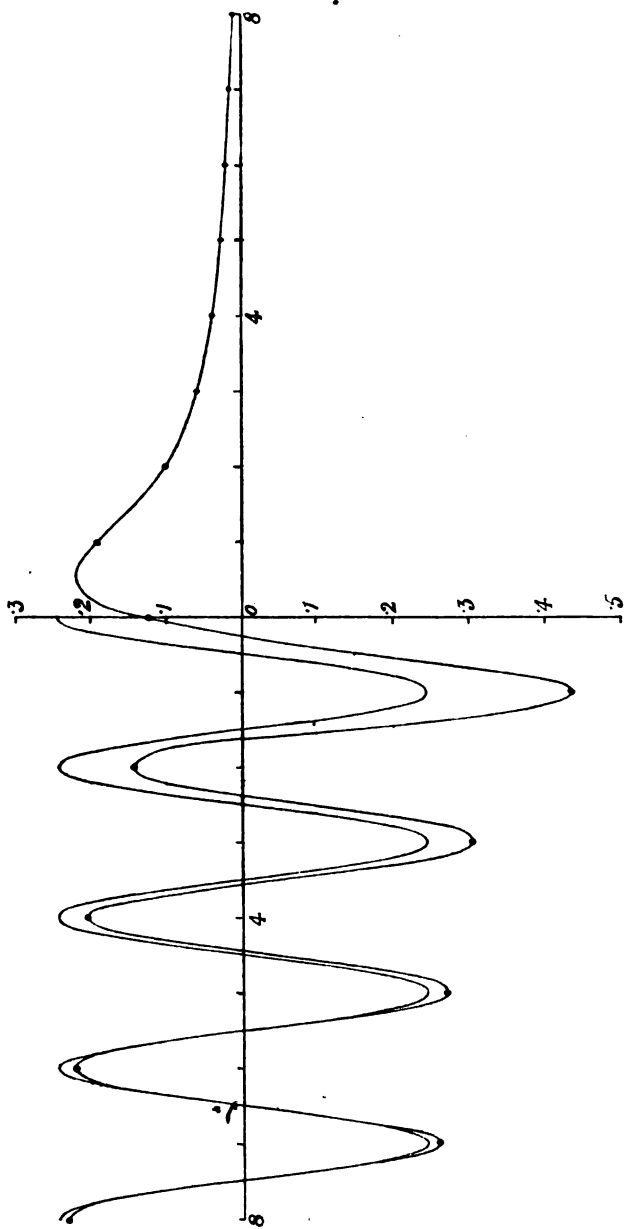


FIG. 8 ;  $Q(x, 0)$ , and a portion of the curve of sines which very approximately agrees with it at great leftward distances.



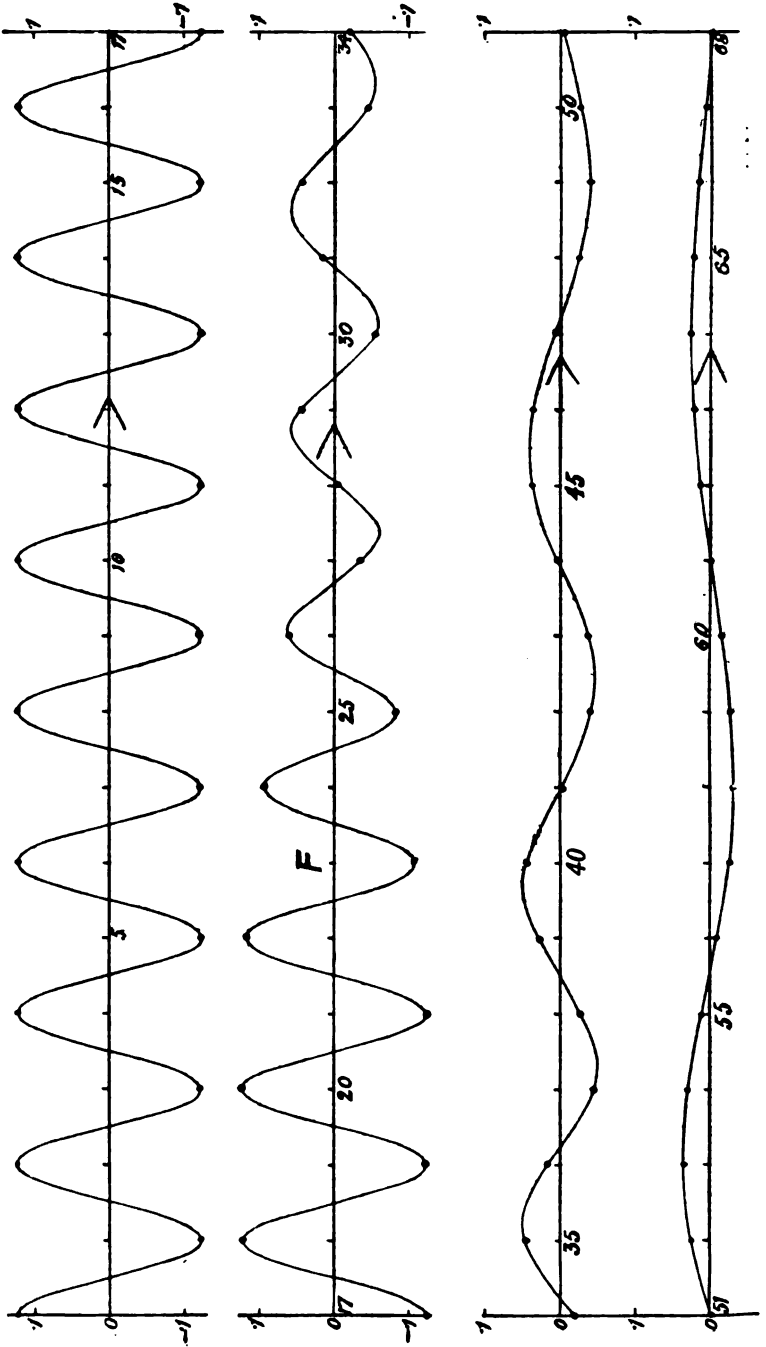


FIG. 9; Head and front of rightward procession.

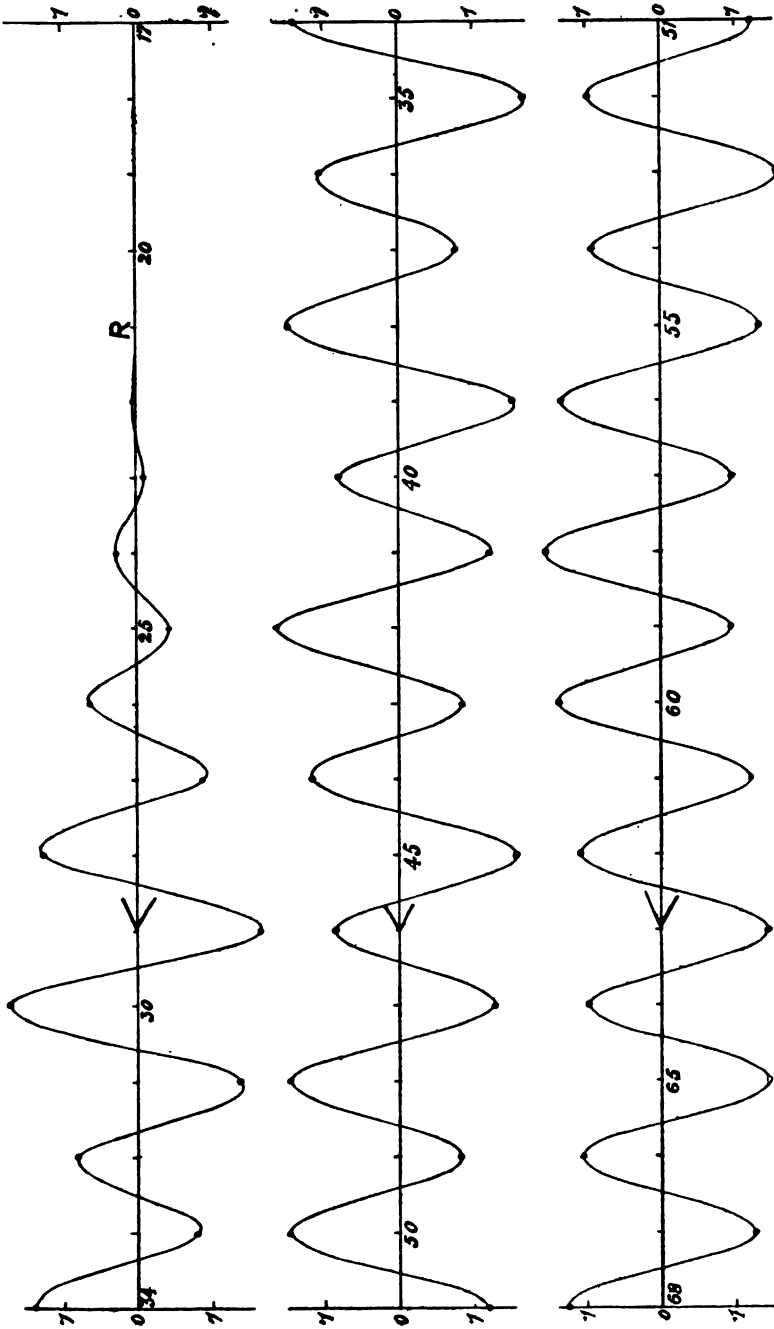


Fig. 10 ; Tail and rear of leftward procession.

§ 28. For fig. 10, instead of assuming as in (47) the calculation of  $Q(x, t)$  for negative values of  $x$ , a very troublesome affair, we may now evaluate it thus. We have by (46)

$$Q(x, t) = \frac{1}{2}\phi(x, t) - \phi(x + 1, t) + \phi(x + 2, t) - \dots$$

$$Q(-x, t) = \frac{1}{2}\phi(-x, t) - \phi(-x + 1, t) + \phi(-x + 2, t) - \dots$$

Hence

$$Q(x, t) + Q(-x, t) = \phi(x, t) - \phi(x + 1, t) + \phi(x + 2, t) - \dots$$

$$- \phi(-x + 1, t) + \phi(-x + 2, t) - \dots \quad (54).$$

Now by the property of  $\phi$ , used in the first term of (54), that its value is the same for positive and negative values of  $x$ , we have  $\phi(-x + i, t) = \phi(x - i, t)$ . Hence (54) may be written

$$Q(x, t) + Q(-x, t) = \sum_{i=-x}^{x+x} (-1)^i \phi(x + i, t) = P(x, t) \quad (55).$$

Hence  $Q(-x, t) = P(x, t) - Q(x, t) \quad (56).$

Using this in (47) we find

$$\frac{1}{2}P(x, t) - Q(x, t) \quad (57),$$

for the elevation of the water due to the leftward procession alone at any point at distance  $x$  from O on the left side,  $x$  being any positive number, integral or fractional. Having previously calculated  $Q(x, t)$  for positive integral values of  $x$ , we have found by (57) the calculated points of fig. 10 for the leftward procession.

§ 29. The principles and working plans described in §§ 11 - 28 above, afford a ready means for understanding and working out in detail the motion, from  $t=0$  to  $t=\infty$ , of a given finite procession of waves, started with such displacement of the surface, and such motion of the water below the surface, as to produce, at  $t=0$ , a procession of a thousand or more waves advancing into still water in front, and leaving still water in the rear. To show the desired result graphically, extend fig. 10 leftwards to as many wave-lengths as you please beyond the point, I, described in § 24. Invert the diagram thus drawn relatively to right and left, and fit it on to the diagram, fig. 9, extended rightwards so far as to show no perceptible motion; say to  $x=200$ , or 300, of our scale. The diagram thus compounded represents the water surface at time  $25\tau$  after a commencement correspondingly compounded from fig. 8, and another

similar figure drawn to represent the rear of the finite (two-ended) procession which we are now considering.

§ 30. Direct attack on the problem thus indirectly solved, gives, for the case of 1000 wave-crests in the beginning, the following explicit solution,

$$-\zeta = \sum_{i=0}^{i=2000} (-1)^i \psi(x-i, t) \dots \dots \dots (58),$$

where  $\psi$  is a function found according to the principles indicated in § 4 above, to express the same surface-displacement as our function  $\phi$  of § 12, and the proper velocities below the surface to give, in the sum, a rightward procession of waves. Our present solution shows how rapidly the initial sinusoidality of the head and front of a one-ended infinite procession, travelling rightwards, is disturbed in virtue of the hydrokinetic circumstances of a procession invading still water. Our solution, and the item towards it represented in figs. 6 and 7, and in fig. 2 of § 6 above, show how rapidly fresh crests are formed. The whole investigation shows how very far from finding any definite "group-velocity" we are, in any initially given group of two, three, four, or any number, however great, of waves. I hope in some future communication to the Royal Society of Edinburgh to return to this subject in connection with the energy principle set forth by Osborne Reynolds,\* and the interferential theory of Stokes † and Rayleigh ‡ giving an absolutely definite group-velocity in their case of an infinite number of mutually supporting groups. But my first hydrokinetic duty, the performance of which I hope may not be long deferred, is to fulfil my promises regarding ship-waves, and circular waves travelling in all directions from a place of disturbance in water.

§ 31. The following tables show some of the most important numbers which have been calculated, and which may be useful in farther prosecution of the subject of the present paper.

\* *Nature*, vol. xvi., 1877, pp. 343-4.  
 † *Smith's Prize Paper, Camb. Univ. Calendar*, 1876.  
 ‡ *Sound*, ed. 1, vol. i., 1877, pp. 246-7.

[TABLE I.

TABLE I.

$$t = 0; \phi(x, 0) = \frac{\sqrt{(\rho + z)}}{\rho}; \quad \Gamma(x, 0) = \phi(x, 0) - \phi(x + 1, 0).$$

$x$	$\phi(x, 0)$	$\frac{\Gamma(-1, 0) = -\Gamma(0, 0)}{\Gamma(x, 0)}$	$x$	$\phi(x, 0)$	$\frac{\Gamma(-1, 0) = -\Gamma(0, 0)}{\Gamma(x, 0)}$
0	1·4142	·3155	34	·1740	·0026
1	1·0987	·2942	35	·1714	·0025
2	·8045	·1593	36	·1689	·0023
3	·6452	·0962	37	·1666	·0023
4	·5490	·0647	38	·1643	·0022
5	·4843	·0468	39	·1621	·0021
6	·4375	·0357	40	·1600	·0020
7	·4018	·0284	41	·1580	·0019
8	·3734	·0232	42	·1561	·0019
9	·3502	·0194	43	·1542	·0018
10	·3308	·0166	44	·1524	·0017
11	·3142	·0143	45	·1507	·0017
12	·2999	·0125	46	·1490	·0016
13	·2874	·0111	47	·1474	·0016
14	·2763	·0100	48	·1458	·0015
15	·2663	·0089	49	·1443	·0015
16	·2574	·0081	50	·1428	·0014
17	·2493	·0073	51	·1414	·0014
18	·2420	·0068	52	·1400	·0014
19	·2352	·0062	53	·1386	·0013
20	·2290	·0058	54	·1373	·0013
21	·2232	·0053	55	·1360	·0012
22	·2179	·0050	56	·1348	·0012
23	·2129	·0047	57	·1336	·0012
24	·2082	·0043	58	·1324	·0011
25	·2039	·0041	59	·1313	·0011
26	·1998	·0039	60	·1302	·0011
27	·1959	·0036	61	·1291	·0011
28	·1923	·0035	62	·1280	·0010
29	·1888	·0033	63	·1270	·0010
30	·1855	·0031	64	·1260	·0010
31	·1824	·0029	65	·1250	·0010
32	·1795	·0028	66	·1240	·0009
33	·1767	·0027	67	·1231	0009

TABLE II.

$$t = 25\tau; \tau = \sqrt{\pi}; \chi = \tan^{-1} \frac{x}{z}; \phi = \sqrt{\frac{2}{\rho}} \cos \left[ x \left( \frac{25}{\rho} \right)^2 \pi - \frac{1}{2} \chi \right] \epsilon^{-\left( \frac{25}{\rho} \right)^2 \pi}$$

$x$	$\left( \frac{x^2}{\rho^2} + \theta \right)$	$\frac{-t^2}{\epsilon \rho^2}$	$\phi(x, 25\tau)$	$D(x, 25\tau)$
15	41π + 133° 43'	·0002	·0000	+·0001
16	39π + 30° 41'	·0005	-·0001	-·0002
17	36π + 161° 31'	·0011	+·0001	-·0002
18	34π + 157° 22'	·0024	+·0003	+·0006
19	33π + 11° 3'	·0044	-·0003	+·0020
20	31π + 77° 25'	·0075	-·0023	-·0018
21	29π + 171° 23'	·0118	-·0005	-·0055
22	28π + 109° 24'	·0174	+·0050	+·0117
23	27π + 68° 20'	·0246	-·0067	-·0136
24	26π + 45° 33'	·0333	+·0069	+·0146
25	25π + 39° 6'	·0434	-·0077	-·0188
26	24π + 46° 38'	·0550	+·0111	+·0281
27	23π + 67° 0'	·0679	-·0170	-·0386
28	22π + 98° 45'	·0820	+·0216	+·0377
29	21π + 140° 41'	·0917	-·0161	-·0101
30	21π + 11° 47'	·1131	-·0060	-·0372
31	20π + 71° 11'	·1299	+·0312	+·0558
32	19π + 138° 6'	·1472	-·0246	-·0032
33	19π + 2° 50'	·1651	-·0214	-·0626
34	18π + 111° 49'	·1832	+·0412	+·0267
35	18π + 17° 29'	·2016	+·0145	+·0637
36	17π + 108° 23'	·2201	-·0492	-·0266
37	17π + 24° 6'	·2385	-·0226	-·0713
38	16π + 124° 14'	·2569	+·0487	+·0021
39	16π + 48° 27'	·2752	+·0466	+·0728
40	15π + 156° 27'	·2934	-·0262	+·0425
41	15π + 87° 58'	·3112	-·0687	-·0410
42	15π + 22° 44'	·3287	-·0277	-·0751
43	14π + 140° 32'	·3459	+·0474	-·0290
44	14π + 81° 8'	·3629	+·0764	+·0434
45	14π + 24° 24'	·3794	+·0330	+·0741
46	13π + 150° 6'	·3956	-·0411	+·0429
47	13π + 98° 9'	·4112	-·0840	-·0190
48	13π + 48° 20'	·4267	-·0650	-·0642
49	13π + 0° 32'	·4416	-·0008	-·0657
50	12π + 134° 40'	·4560	+·0649	-·0282
51	12π + 90° 36'	·4702	+·0931	+·0224
52	12π + 48° 10'	·4840	+·0707	+·0582
53	12π + 7° 25'	·4973	+·0125	+·0643
54	11π + 148° 9'	·5101	-·0518	+·0417
55	11π + 110° 18'	·5226	-·0935	+·0035
56	11π + 73° 48'	·5348	-·0970	-·0332
57	11π + 38° 35'	·5464	-·0638	-·0556
58	11π + 4° 34'	·5580	-·0082	-·0578
59	10π + 151° 43'	·5690	+·0496	-·0421
60	10π + 119° 57'	·5797	+·0917	-·0152
61	10π + 89° 14'	·5900	+·1069	+·0141
62	10π + 59° 30'	·6001	+·0928	+·0373
63	10π + 30° 43'	·6098	+·0555	+·0501
64	10π + 2° 50'	·6193	+·0054	+·0506
65	9π + 155° 48'	·6284	-·0452	+·0403
66	9π + 129° 35'	·6372	-·0855	+·0226
67	9π + 104° 9'	·6459	-·1081	+·0022
68	9π + 79° 28'	·6540	-·1103	

(Issued separately August 22, 1904.)

**Some Results in the Mathematical Theory of Seiches.**  
By Professor Chrystal.

(Read July 18, 1904. MS. received July 29, 1904.)

(Abstract.)

I propose in this preliminary communication to lay before the Society some results of investigations in the theory of Seiches in a lake whose line of maximum depth is approximately straight, and whose depth, cross section, and surface breadth do not vary rapidly from point to point.

As the seiche disturbance is small compared with the length of the lake, I shall make the assumptions usual in the theory of long waves:—viz., that the squares of the displacements and of their derivatives are negligible.

The  $x$ -axis,  $O X$ , is taken in the undisturbed level of the lake, in the average direction of the line of maximum depth; the  $z$ -axis,  $O Z$ , is taken vertically upwards. The horizontal and vertical displacements of a water particle originally in the undisturbed surface, at a distance  $x$  from the origin, are denoted by  $\xi$  and  $\zeta$ .  $A(x)$  and  $b(x)$  are used to denote the area and the surface breadth of the cross section at a distance  $x$  from  $O$ .

We suppose that the vertical disturbance at every point in the surface line of any cross section of the lake is the same; in other words, we neglect the dynamical effect of any flow perpendicular to  $O X$  due to the gradual increase or diminution of the area of the cross section of the lake. As in the theory of long waves, the vertical acceleration is also neglected; and we also neglect the (usually small) effect due to the shelving of the shore.

With these assumptions, the equations which determine  $\xi$  and  $\zeta$  are found to be

$$\frac{\partial^2 u}{\partial t^2} = g\sigma(v) \frac{\partial^2 u}{\partial v^2}, \dots \dots \dots (1)$$

$$\zeta = - \frac{\partial u}{\partial v}, \dots \dots \dots (2)$$

where  $u = A(x) \xi$ ,  $v = \int_0^x dx b(x)$ ,  $\sigma(v) = A(x) b(x)$ ; and  $g$  and  $t$  have the usual meanings.

A natural\* seiche of frequency  $n$  is therefore determined by the equations

$$A(x) \xi = u = P \sin nt + Q \cos nt, \dots (3);$$

where  $P$  and  $Q$  are solutions of

$$\frac{d^2 P}{dv^2} + \frac{n^2}{g\sigma(v)} P = 0 \dots (4)$$

Since  $\sigma(v)$  is a slowly varying function of  $v$ , we might take it to be either a linear or a quadratic integral function of  $v$ . On the former assumption the solution of (4) is found to depend on Bessel's Functions. It is found, however, that the assumption  $\sigma(v) = h(1 - v^2/a^2)$  is more convenient for obtaining approximate representations of the cases that occur in nature. The solution in this case is found to depend on certain functions which we may call the *Seiche Functions*, defined, for  $-1 \leq w \leq +1$ , by the following convergent series:—

$$C(c, w) = 1 - \frac{c}{1.2} w^2 + \frac{c(c-1.2)}{1.2 \times 3.4} w^4 - \frac{c(c-1.2)(c-3.4)}{1.2 \times 3.4 \times 5.6} w^6 + \dots$$

$$S(c, w) = w - \frac{c}{2.3} w^3 + \frac{c(c-2.3)}{2.3 \times 4.5} w^5 - \frac{c(c-2.3)(c-4.5)}{2.3 \times 4.5 \times 6.7} w^7 + \dots$$

$$\mathcal{C}(c, w) = 1 - \frac{c}{1.2} w^2 + \frac{c(c+1.2)}{1.2 \times 3.4} w^4 - \frac{c(c+1.2)(c+3.4)}{1.2 \times 3.4 \times 5.6} w^6 + \dots$$

$$\mathcal{S}(c, w) = w - \frac{c}{2.3} w^3 + \frac{c(c+2.3)}{2.3 \times 4.5} w^5 - \frac{c(c+2.3)(c+4.5)}{2.3 \times 4.5 \times 6.7} w^7 + \dots$$

The functions  $C$  and  $S$  are synectic integrals of the differential equation

$$(1 - w^2) \frac{d^2 P}{dw^2} + cP = 0 \dots (5);$$

\* As opposed to a *forced* seiche, whose period depends partly on the period of the disturbing agency. Some of the seiches on Lake Erie are, I believe, of this nature.



and are connected by the relation

$$C(c, w)S'(c, w) - C'(c, w)S(c, w) = 1 \dots (6),*$$

where the dashes denote differentiation with respect to  $w$ . On account of the fact that  $C$  and  $S$  have certain of the properties of  $\cos w$  and  $\sin w$ , and in a certain limiting case reduce to these functions, we may call them the *seiche-cosine* and the *seiche-sine* respectively. From another point of view they are limiting cases of the hypergeometric function; but from this fact no practical advantage has been found hitherto.

In like manner  $\mathfrak{G}(c, w)$  and  $\mathfrak{S}(c, w)$ , which we may call the *hyperbolic seiche-cosine* and *hyperbolic seiche-sine*, are integrals of

$$(1 + w^2) \frac{d^2P}{dw^2} + cP = 0, \dots (7)$$

and

$$\mathfrak{G}(c, w) \mathfrak{S}'(c, w) - \mathfrak{G}'(c, w) \mathfrak{S}(c, w) = 1 \dots (8)$$

For the particular values  $w = 1$  and  $w = i$  (where  $i$  is the imaginary unit) we have

$$\left. \begin{aligned} C(c, 1) &= \left(1 - \frac{c}{1.2}\right) \left(1 - \frac{c}{3.4}\right) \left(1 - \frac{c}{5.6}\right) \dots \dots \dots \\ S(c, 1) &= \left(1 - \frac{c}{2.3}\right) \left(1 - \frac{c}{4.5}\right) \left(1 - \frac{c}{6.7}\right) \dots \dots \dots \end{aligned} \right\} \dots (9)$$

$$\left. \begin{aligned} \mathfrak{G}(c, i) &= \left(1 + \frac{c}{1.2}\right) \left(1 + \frac{c}{3.4}\right) \left(1 + \frac{c}{5.6}\right) \dots \dots \dots \\ \mathfrak{S}(c, i) &= i \left(1 + \frac{c}{2.3}\right) \left(1 + \frac{c}{4.5}\right) \left(1 + \frac{c}{6.7}\right) \dots \dots \dots \end{aligned} \right\} \dots (10)$$

It follows from Sturm's Oscillation Theorem regarding the solutions of a linear differential equation, such as (5), that, for any given real value of  $v \leq 1$ , there are an infinite number of positive real values of  $c$  which satisfy the equations

$$\begin{aligned} C(c, v) &= 0, & S(c, v) &= 0; \\ \mathfrak{G}(c, v) &= 0, & \mathfrak{S}(c, v) &= 0; \end{aligned}$$

and that the roots of either of the equations of one of these pairs separate the roots of the other.

\* The analogue of the relation  $\cos^2 x + \sin^2 x = 1$  for the circular functions.

It appears at once from (9) that the real positive roots of  $C(c, 1) = 0$  are

$$c = 1.2, 3.4, 5.6, \dots \text{i.e. } 2, 12, 30, \dots \quad (11)$$

and of  $S(c, 1) = 0$

$$c = 2.3, 4.5, 6.7, \dots \text{i.e. } 6, 20, 42, \dots \quad (12)$$

The roots of  $\mathfrak{G}(c, 1) = 0$  and  $\mathfrak{S}(c, 1) = 0$  are neither commensurable nor so easily found. A somewhat laborious arithmetical calculation, in which I have been kindly assisted by Dr Burgess and Mr E. M. Horsburgh, has given for the smallest positive root of  $\mathfrak{G}(c, 1) = 0$   $c = 2.77 \dots$ , and for the corresponding root of  $\mathfrak{S}(c, 1) = 0$   $c = 12.34 \dots$ .

It should also be observed that, when  $c$  has one of the values (11),  $C(c, v)$  reduces to an integral function of  $v$ ; and the same happens to  $S(c, v)$  when  $c$  has one of the values (12).

If we assume  $\sigma(v) = h(1 \mp v^2/a^2)$ , the equation for  $P$  is

$$\left(1 \mp \frac{v^2}{a^2}\right) \frac{d^2 P}{dv^2} + \frac{n^2}{gh} P = 0,$$

which, if we put  $w = v/a$ , and take

$$c = n^2 a^2 / gh,$$

reduces to either (5) or (6). Hence  $A(x)\xi$  can be expressed in terms of the seiche functions; and  $\zeta$  is given by

$$\zeta = -\frac{1}{a} \frac{\partial u}{\partial w}.$$

In the case where the breadth of the lake is constant and the cross section rectangular, but the depth variable, say  $h(x) = h(1 - x^2/a^2)$ , we can replace the variable  $v$  by  $x$ . The constants  $h$  and  $a$  are then linear magnitudes (whose meanings are obvious) instead of a volume and an area as in the general case. It will be observed, therefore, that all the general features of the phenomena of seiches are to be found in this more special case, regarding which we now give some particulars.

LAKE WITH SYMMETRIC LONGITUDINAL SECTION OF PARABOLIC  
CONCAVE FORM  $h(x) = h \times (1 - x^2/a^2)$ .

If  $c = \nu(\nu + 1)$ , and  $T_\nu$  be the period of the  $\nu$ -nodal seiche, then

$$T_\nu = 2\pi/\mu = 2\pi a/\sqrt{(c_\nu g h)} = \pi l/\sqrt{\{\nu(\nu + 1)gh\}} \quad \dots (13)$$

where  $l (= 2a)$  is the whole length of the lake.

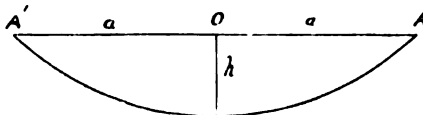


FIG. 1.

For seiches with odd and even numbers of nodes we have

$$\xi = \frac{A}{h} \frac{C(c_{2\nu-1}, w)}{1 - w^2} \sin nt, \quad \zeta = -\frac{A}{a} C'(c_{2\nu-1}, w) \sin nt; \quad \dots (14)$$

and

$$\xi = \frac{B}{h} \frac{S(c_{2\nu}, w)}{1 - w^2} \sin nt, \quad \zeta = -\frac{B}{a} S'(c_{2\nu}, w) \sin nt, \quad \dots (15)$$

respectively.

UNINODAL SEICHE.

$$c_1 = 1.2; \quad T_1 = \pi l/\sqrt{(2gh)} \quad \dots \dots \dots (16)$$

$$\xi = \frac{A}{h} \sin nt, \quad \zeta = \frac{2Ax}{a^2} \sin nt, \quad \dots \dots \dots (17)$$

Note  $x = 0$ .

If  $\bar{\xi}$ ,  $\bar{\zeta}$  denote the maximum horizontal and vertical displacements of a particle on the surface at the end of the lake, and  $\bar{\xi}$  the maximum horizontal velocity of displacement, then

$$\bar{\xi} = l\bar{\zeta}/4h, \quad \bar{\zeta} = \pi l\bar{\xi}/2hT_1 \quad \dots \dots \dots (18)$$

It should be observed that here, and in the cases that follow under the present head, the boundary condition at A and A' is not that  $\xi = 0$ , but that the motion be tangential to the shore.

BINODAL SEICHE.

$$c_2 = 2.3 ; \quad T_2 = \pi l / \sqrt{(6gh)} , \quad . . . . . (19)$$

$$\xi = \frac{Bx}{a^2 l} \sin nt , \quad \zeta = \frac{B(3x^2 - a^2)}{a^3} \sin nt , \quad . . . (20)$$

Nodes  $x = \pm a / \sqrt{3} = \pm .57 \dots a . . . . . (21)$

We have

$$T_2/T_1 = \sqrt{2}/\sqrt{6} = .574 \dots . . . . . (22)$$

Hence the period of the binodal seiche in a concave lake of symmetric parabolic section is greater than half the period of the uninodal seiche.

Also the nodes are more than half way towards the ends ; *i.e.* they are displaced towards the shallows.

If  $\bar{\xi}$ ,  $\xi$ , and  $\bar{\zeta}$  have the same meanings as before, we have

$$\bar{\xi} = l\bar{\zeta}/4h , \quad \bar{\xi} = \pi l \bar{\zeta} / 2h T_2 . . . . . (23)$$

at the ends of the lake. At a node the values of  $\bar{\xi}$  and  $\xi$  are reduced in the ratio .57 . . . : 1. At the centre  $\xi = 0$  at all times ; and  $\bar{\zeta}$  has half its value at the end of the lake.

TRINODAL SEICHE.

$$c_3 = 3.4 ; \quad T_3 = \pi l / \sqrt{(12gh)} .$$

$$\xi = \frac{A}{h a^2} (a^2 - 5x^2) \sin nt , \quad \zeta = \frac{A}{a^3} (12a^2 x - 20x^3) \sin nt , \quad . (24)$$

Nodes  $x = 0, x = \pm a \sqrt{3} / \sqrt{5} = \pm .7746 \dots a , \dots (25)$

$$T_3/T_1 = \sqrt{2}/\sqrt{12} = .4082 \dots . . . . . (26)$$

QUADRINODAL SEICHE.

$$c_4 = 4.5 ; \quad T_4 = \pi l / \sqrt{(20gh)} , \quad . . . . . (27)$$

$$\xi = \frac{1}{h a^3} x (3a^2 - 7x^2) \sin nt , \quad \zeta = \frac{B}{a^5} ( - 3a^4 + 30a^2 x^2 - 35x^4 ) \sin nt (28)$$

Nodes  $x = \pm .3400 \dots a , \pm .8621 \dots a , \dots (29)$

$$T_4/T_1 = .3162 \dots . . . . . (30)$$

QUINQUINODAL SEICHE.

$$c_5 = 5.6; \quad T_5 = \pi l \sqrt{(30gh)}. \quad \dots \quad (31)$$

$$\xi = \frac{A}{ha^4} (a^4 - 14a^2x^2 + x^4) \sin nt,$$

$$\zeta = \frac{A}{a^6} (30a^4x - 140a^2x^3 + 126x^5) \sin nt, \quad \dots \quad (32)$$

Nodes  $x = 0, \pm .5384 \dots a, \pm .9058 \dots a, \dots \quad (33)$

$$T_5/T_1 = .2582 \dots, \quad \dots \quad (34)$$

LAKE WITH SYMMETRIC LONGITUDINAL SECTION OF PARABOLIC CONVEX FORM  $h(x) = h \times (1 + x^2/a^2)$ .

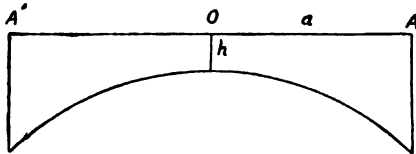


FIG. 2.

If  $c_1, c_2, c_3, \dots, c_r, \dots$  be the real positive roots taken in order of magnitude of the equations  $\mathfrak{G}(c, 1) = 0$  and  $\mathfrak{E}(c, 1) = 0$ , so that  $c_1$  is the smallest positive root of  $\mathfrak{G}(c, 1) = 0$ ,  $c_2$  the smallest positive root of  $\mathfrak{E}(c, 1) = 0$ , and so on, then, for seiches with an odd number of nodes,

$$\xi = \frac{A}{h} \frac{\mathfrak{G}(c_{2s-1}, w)}{1+w^2} \sin nt, \quad \zeta = -\frac{A}{a} \mathfrak{G}'(c_{2s-1}, w) \sin nt, \quad \dots \quad (35)$$

for seiches with an even number of nodes

$$\xi = \frac{B}{h} \frac{\mathfrak{E}(c_{2s-1}, w)}{1+w^2} \sin nt, \quad \zeta = -\frac{A}{a} \mathfrak{E}'(c_{2s-1}, w) \sin nt, \quad \dots \quad (36)$$

UNINODAL SEICHE.

$$c_1 = 2.77 \dots, \quad T_1 = \pi l / \sqrt{(2.77 \dots gh)}, \quad \dots \quad (37)$$

Hence  $T_1 < T_1$ ; that is to say, for the same central depth and the same length, the uninodal period is less when the lake is convex than when it is concave.

**BINODAL SEICHE.**

$$c_2 = 12 \cdot 34, \quad \mathfrak{T}_2 = \pi l / \sqrt{(12 \cdot 34 \dots gh)}, \dots \quad (38)$$

Hence  $\mathfrak{T}_2 < T_2$ .

$$\text{Also } \mathfrak{T}_2 / \mathfrak{T}_1 = \sqrt{\{2 \cdot 77 \dots / 12 \cdot 34 \dots\}} = \cdot 474 \dots \quad (39)$$

In other words, in a convex lake of symmetric parabolic section the period of the binodal seiche is less than half the period of the uninodal seiche.

It follows, of course, from the fact that the seiche functions degenerate into the circular functions when the curvature of the bottom is infinitely small, that when the lake bottom is flat  $T_2 / T_1 = \frac{1}{2}$ , etc., as in the case of vibrating rods, or strings.

**CASE OF CONCAVE LAKE WITH UNSYMMETRIC BIPARABOLIC SECTION.**

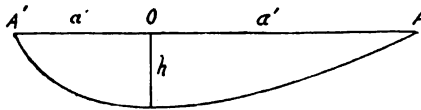


FIG. 3.

The depth from O to A is given by  $h(x) = h(1 - x^2/a^2)$ ; from O to A' by  $h(x) = h(1 - x^2/a'^2)$ .

If  $w = x/a, w' = x'/a'; c = n^2 a^2 / gh, c' = n^2 a'^2 / gh$ , then for the two portions O A and O A' we have respectively

$$\xi h(1 - w^2) = \frac{A}{S(c, 1)} \{S(c, 1)C(c, w) - C(c, 1)S(c, w)\} \sin nt,$$

$$\zeta = -\frac{A}{aS(c, 1)} \{S(c, 1)C'(c, w) - C(c, 1)S'(c, w)\} \sin nt; \dots \quad (40)$$

and

$$\xi' h(1 - w'^2) = \frac{A}{S(c', 1)} \{S(c', 1)C(c', w') + C(c', 1)S(c', w')\} \sin nt,$$

$$\zeta' = -\frac{A}{a'S(c', 1)} \{S(c', 1)C'(c', w') + C(c', 1)S'(c', w')\} \sin nt \dots \quad (41)$$

The values of C and C' which determine the periods are given by  $c/c' = a^2/a'^2$  together with the period-equation

$$\alpha' C(c, 1) S(c', 1) + \alpha C(c', 1) S(c, 1) = 0 \quad (42)$$

If we put  $a'^2 c = a^2 c' = n^2 a^2 \alpha^2 / gh = z$ , the period equation may be written

$$\alpha \left(1 - \frac{z}{1.2a^2}\right) \left(1 - \frac{z}{3.4a^2}\right) \dots \left(1 - \frac{z}{2.3a^2}\right) \left(1 - \frac{z}{4.5a^2}\right) \dots + \alpha' \left(1 - \frac{z}{1.2a'^2}\right) \left(1 - \frac{z}{3.4a'^2}\right) \dots \left(1 - \frac{z}{2.3a'^2}\right) \left(1 - \frac{z}{4.5a'^2}\right) \dots = 0 \quad (43)$$

UNSYMMETRIC LAKE WITH ONE SHALLOW AND TWO MAXIMUM DEPTHS.

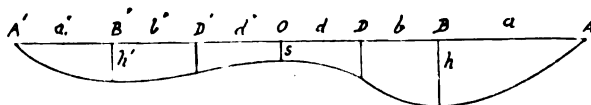


FIG. 4.

A good approximation to the form of lake section in many cases that occur in nature can be obtained by piecing together six parabolas, as in figure (4), so as to form one continuous curve. If  $s$  be the minimum, and  $h$  and  $h'$  the two maximum depths,  $D$  and  $D'$  the points of inflexion;  $AB = a_1$ ,  $A'B' = a'_1$ ,  $BD = b$ ,  $B'D' = b'$ ,  $OD = d$ ,  $OD' = d'$ , then we may represent the portions  $AB$ ,  $BD$ ,  $DO$ ,  $OD'$ ,  $D'B'$ ,  $B'A'$  by the six parabolas:— $h(x) = h(1 - x^2/a_1^2)$ ;  $h(x) = h(1 - x^2/a_2^2)$ ;  $h(x) = s(1 + x^2/a_3^2)$ ;  $h(x) = s(1 + x^2/a'_3^2)$ ;  $h'(x) = h'(1 - x^2/a'_2^2)$ ;  $h(x) = h'(1 - x^2/a'_1^2)$ .

The conditions of continuity lead to

$$\begin{aligned} a_2^2 &= hb(d + b)/(h - s), & a_3^2 &= sd(d + b)/(h - s); \\ a'_2^2 &= h'b'(d' + b')/(h' - s), & a'_3^2 &= s'd'(d' + b')/(h' - s). \end{aligned} \quad (44)$$

All the magnitudes marked in the figure may be arbitrarily determined; but after this has been done the depths at the points of inflexion are not at our disposal.

The formulæ for  $\xi$  and  $\zeta$  and the period-equation have been worked out for this case. They involve all the four seiche-

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[Continued on page iii of Cover.]

1903-4.] Prof. Chrystal on *Mathematical Theory of Seiches*. 337

functions; and are naturally somewhat complicated. We therefore omit them from this preliminary communication.

In a more detailed paper which I propose to submit hereafter to the Society I shall give particulars regarding the establishment of the above results, further developments of their application, a discussion of the agreement of the results in particular cases with observation, and a comparison of the above theory with that given by Du Boys in his "Essai Théorique sur les Seiches" (*Arch. d. Sc. Phys. et Nat. d. Genève, Pér.* iii. t. xxv., 1891).

In the meantime I cherish a hope that the above summary may help to encourage and to guide the ardent observers who are now engaged in procuring for us accurate data regarding the interesting natural phenomena with which they deal.

(Issued separately October 6, 1904.)

**A New Form of Spectrophotometer.** By J. R. Milne,  
B.Sc., Carnegie Scholar in Natural Philosophy, Edinburgh  
University.

(Read July 4, 1904. MS. received August 1, 1904.)

The present paper is the continuation of a note sent to the Society in July of last year,\* and is for the purpose of describing the developed form of the spectrophotometer whose principle was indicated in that communication.

The former paper described the employment of a divided spherical lens to bring together the two slightly separated spectra seen in any ordinary form of spectrophotometer. This divided lens is placed at about twice its focal length behind the two spectra

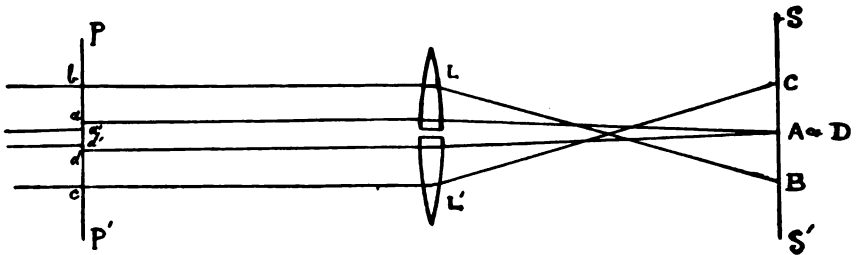


FIG. 3.

produced by the objective of the telescope, and, when suitably adjusted, gives rise to two spectra in contact with each other, as shown in fig. 1 of the former paper. It has been found, however, to be better to modify the action of the divided lens, and to use it as indicated in fig. 3. The defect of the former arrangement can be seen from fig. 1,† where the point *b* is beneath O, permitting light from *b* to pass straight along beneath the lens-half L, to prevent which an opaque stop is required to fill up the space OO', the stop being so contrived that freedom of relative motion is still preserved to the two halves of the lens L and L'. In the present arrangement, which is depicted in fig. 3, no such device is

\* *Proc. Roy. Soc. Edin.*, vol. xxiv. p. 496, 1903.

† See former note.

needed, provided that the light rays from any the same point of  $ab$  and  $cd$  are all in a horizontal plane through that point, and this condition, as will be seen later on, is actually fulfilled.

The edges  $a'$  and  $d'$  of the two spectra formed by the objective of the telescope are necessarily somewhat hazy and ill-defined, whether the gap between the spectra has been produced by the menisci of a liquid or by the edge of a solid. To remedy this, a strip of metal  $ad$  is placed so as to cut off the extreme edges of the two spectra, and by this means the edges which are afterwards brought into contact in the plane  $SS'$  are beforehand made perfectly straight, and are sharply delimited. This strip of metal or "trimmer"  $ad$  (fig. 3) really consists of two similar pieces, which by means of a slow motion screw can be arranged in such a way that the compound strip is slightly wider at one end than at the other. This arrangement the author has found to be necessary, as in his model instrument, for reasons of economy, the divided lens is a simple one, and so the neighbouring edges of the images of the two spectra formed by the divided lens are not parallel to each other. This difficulty is perfectly overcome, however, by making the trimmer slightly wider at one end or the other as may be required. The point is mentioned because even with a more perfect lens the device might be necessary to obtain the most exact results.

It inevitably happens that the two beams of light falling on the trimmer  $ad$  in fig. 3 suffer marked diffraction, and if (say) the lower beam be stopped off, obvious diffraction bands at the lower edge of the remaining spectrum may in general be seen on looking through an ordinary telescope eyepiece, placed behind the divided lens at a distance of about twice the focal length of the latter. If, however, the eyepiece, originally somewhat too far off to focus objects in the plane  $SS'$ , be slowly pushed nearer that plane, the diffraction bands, which in this case are dark and are situated upon the bright strip, are observed to begin closing in towards the edge of the image, and when the eyepiece is exactly focussing the plane  $SS'$  no bands are to be seen at all. On continuing to move the eyepiece towards the plane  $SS'$  the bands reappear, being now bright lines situated outside the bright strip, and they continue to move out from its edge with the motion of the eyepiece. These

results show then, that when the eyepiece is correctly focussed no trouble will be experienced from diffraction effects.

In the former note all that was contemplated was a device for attachment to an ordinary spectrophotometer to bring the two spectra exactly together, that the judging of their relative intensities might be made more accurate. The author, however, had in view the object of measuring the light intensities of various liquids, which were to be contained in tubes about a metre long, and it was found that for this purpose, in addition to the above device, a further modification in the form of spectrophotometer was desirable. This new design of instrument also presents advantages for general spectrophotometrical work.

Fig. 4 is intended to give a diagrammatic view of such an

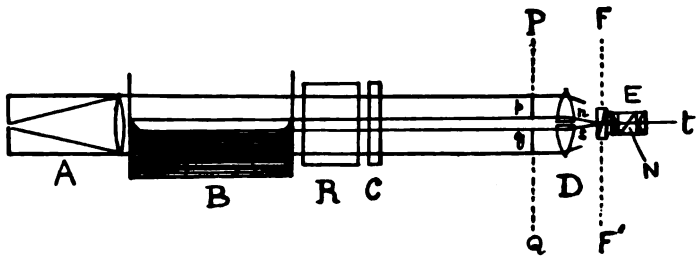


FIG. 4.

[That some parts may be more easily seen, this diagram is not drawn to scale.]

apparatus. The collimator A is so far distant from the prism R, that there is room to insert between the two the long tube B containing the liquid. The ends of the tube are made of plane parallel glass, so as not to interfere with the parallelism of the rays of light passing through it. Before the customary vertical slit of the collimator, there is placed a thin piece of opaque metal pierced with another slit whose opening is horizontal, so that the effective aperture of the two is a very small rectangular hole. This arrangement results in the production of a beam of light from the collimator lens, which is sensibly parallel, and, the tube B being only half filled with liquid, all the upper half of the beam of light passes entirely clear of the latter, while all the under half of the beam passes through the full length of the liquid.

Without this arrangement, and using the light as it comes

naturally from the light source with its different rays inclined in various directions, there would be the following difficulty. Some of those rays which are inclined downwards would enter the tube above the liquid, but before leaving the tube, they would pass into the liquid, and so the emergent lower beam would consist only partially of rays that have passed through the whole length of the liquid.

At first sight it might be supposed that this error is compensated by a similar addition from the lower to the upper beam, but this is not the case, for, as will be seen on reflection, each beam would thus gain *equal* quantities of light, whereas, did complete compensation occur, the gains of the lower and of the upper beams respectively would bear a ratio to one another which is equal to the fraction of the total light, incident upon it, which is transmitted by the absorbing liquid.

In reality too the number of rays passing from the lower to the upper beam within the absorption vessel is not equal to the number passing from the upper to the lower, because a large proportion of the former rays will be totally reflected down again at the surface of the liquid; and consideration will show that this fact will make the error spoken of above still greater.

There is also the further point that with non-parallel light and a long absorption tube the number of rays that pass out through the sides of the tube will be different for the upper and for the lower part of the tube, owing to the presence of the liquid in the latter.

With a non-parallel beam not only do the two above noted difficulties arise, but there comes in the additional error that the source of light is in effect brought some distance nearer in the case of the beam that passes through the liquid, and hence the light intensity of that beam is increased, that of the other beam being left unchanged.\*

Even were the beam of light employed to be the cone of rays proceeding from a very small hole in an opaque screen placed immediately in front of the light source and at the level of the

\* In this connection see a paper entitled "On the Absorption Spectra of some Copper Salts in Aqueous Solution," by Thomas Ewan, B.Sc., Ph.D., *Phil. Mag.* (6), No. 203, p. 331, April 1892.



liquid in the tube, of these three errors just noted only the first would be done away with, besides which such a plan would give less light intensity than the arrangement of the collimator described above.

In a spectrophotometer as ordinarily made there is no room between the collimator and the prism for an absorption vessel, and to comply with the above parallel light condition it becomes necessary to take off the collimator and to mount it by itself in front of the spectrophotometer at such a distance as permits of inserting the absorption vessel between the two.

In the ordinary type of spectrophotometer there are two difficulties that would arise were parallel light to be used. It will be seen that as all the rays of both the beams of light which emerge from the absorption vessel are parallel to the general optic axis of the instrument, these two beams of light, after duly passing through the prism and the object glass of the telescope, will give rise to one and the same spectrum; and that the width of this spectrum will be very small.

Taking the latter difficulty first, the width of the spectrum produced by any spectroscope must be equal to the length of the collimator slit multiplied by the focal length of the telescope objective and divided by the focal length of the collimator lens. Now in the above arrangement the "length" of the small hole which acts as a collimator slit may be about  $\frac{1}{10}$ th of an inch, so that the spectrum formed by the two beams of light will have a quite insufficient width for our purpose. Besides, we require each beam to give rise to a separate spectrum, and we must not have the two spectra formed in the same position one upon the other. Both difficulties, however, are readily solved by using as the telescope objective a cylindrical lens (C, fig. 4) whose axis of figure is placed vertically: the focal length of the lens being identical with that of the spherical lens whose place it has taken. In this way, while using a strictly parallel beam of light to pass into the absorbing vessel, we obtain two separate spectra placed one above the other, and formed respectively by the "comparison" and by the "absorbed" beams of light; and the widths of these spectra are amply sufficient, for they are respectively equal to the heights of the cross sections of each beam of light. See

$p$  and  $q$ , fig. 4, which represent the intersections by the plane of the paper of the two spectra formed by the cylindrical lens C.

In the case of a spectrophotometer where the light intensities are regulated and measured by means of a Vieroidt double slit on the collimator, the latter cannot be removed and placed in front of the absorption vessel without the loss of this means of controlling the light intensity. Of course the collimator might be left on the spectrophotometer and another collimator might be arranged in front of the absorption vessel, the two beams of light from the latter being directed upon the two Vieroidt slits respectively. With such an arrangement, however, the intensity would be reduced by the narrow openings of the Vieroidt slits, as well as by the small rectangular opening of the first collimator. The writer tried a modification of the above plan designed to obviate this loss, in which the second collimator being provided with Vieroidt slits, the latter were made to open at the maximum to a width equal to that of the two beams of light, while the lens of this collimator was discarded. Those changes are legitimate because the light rays have already been made parallel by the first collimator, and all that we wish to retain of the Vieroidt double slit collimator is its power to regulate the intensities of the two beams. The difficulty with this plan is that the beam of light produced by the first collimator is apt not to have the same intensity at every point across a normal section, and if, for example, the jaws of one of the slits be closed together till only the half of that beam is permitted to pass through, we shall not in general have reduced the total light intensity of that beam by one half. The uniformity of the distribution of intensity in the cross section of the beam of light after leaving the first collimator depends to a large extent on what source of light is employed; lime light, owing to the small area of its light source, being markedly inferior for such a purpose to a flat acetylene flame. Even with the latter, however, a doubt may exist as to the perfect equality of the intensity throughout the cross section of the beam, and so this modification of the Vieroidt double slit was abandoned and another device for intensity regulation was substituted which will be discussed later.

The plane P and Q (fig. 4) in which the two spectra  $p$  and  $q$  are formed, is occupied by a screen whose function is to limit the field of view of the eyepiece to two narrow strips taken one from each spectrum for the purpose of having their intensities compared. This screen is shown diagrammatically in fig. 7. By means of the sliding piece A, the colour of the strip taken from the upper spectrum can be altered at pleasure, while by means of the second sliding piece  $a$ , mounted on the first, the width of the strip can be altered. The sliding pieces B and  $b$  perform similar offices for the lower spectrum. Through the opening of the slides the trimmer T may be seen. The latter is fixed at the side of the screen adjacent to the divided lens, and fulfils a function that has already been explained.

After the screen there follows at a distance of about twice\* its

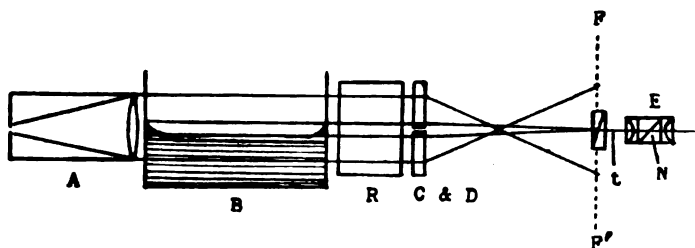


FIG. 5.

[That some of the parts may be more easily seen, this diagram is not drawn to scale, nor does C show the true cross section of the lens at that place.]

focal length the divided spherical lens D (fig. 4), and, as shown above, the resulting images in the plane  $FF'$  (which is conjugate to the screen in the plane  $PQ$ ) of the strips of the two spectra  $p$  and  $q$  can be arranged by adjusting the lens-halves so that their edges are in complete contact.

It may be mentioned here that there is an alternative arrangement of the parts just described which has the merit of shortening the telescope tube. The latter point is important, because if an ordinary spectroscopic prism be employed, all the parts of the

\* It will be recollected that the minimum distance between an object and the image of it formed by a convergent lens is equal to four times the focal length of the lens; the divided lens has been placed at a distance of twice its focal length from the two spectra  $p$  and  $q$  (fig. 4), so that the telescope tube may be as short as possible.

optical train that follow must be capable of rotation about a vertical axis through the prism to an extent sufficient to cause all the various colours of the spectrum in their turn to fall upon the eyepiece and to be seen by the eye of the observer. In the usual form of spectrophotometer this is achieved by supporting the telescope only at its objective end, which is pivoted so as to have the required rotatory motion. Now in this instrument the telescope tube must have a length equal to the focal length of the cylindrical telescope objective, plus a further length, equal to four times the focal length of the divided lens. Such a length of tube makes it difficult to secure the necessary rigidity without resorting to a cumbrous form of mounting.

The alternative form of apparatus just mentioned, which is shown diagrammatically in fig. 5, is provided with a single lens, C and D, which takes the place of the two lenses C and D of fig. 4, with the result that the telescope tube is shortened by a length equal to the distance between the planes PQ and FF'. In order to find the specification of the lens required in this case two points must be borne in mind. In the first place the lens, when placed behind the prism R (fig. 4), must give rise to two pure spectra formed from the two beams of light respectively. Now a cylindrical lens with its axis of figure upright will fulfil the above condition. Its focal length may equal the distance from C to the line PQ, so that the spectra will be formed in a plane normal to the paper through the latter line. In the second place, as already explained, to avoid diffraction effects the trimmer must be situated in a plane conjugate to that in which the spectra are formed. To fulfil this condition along with the other the lens, having its front face ground to the cylindrical curvature determined above, must have its back face ground as a cylindrical lens whose axis of figure is horizontal. The exact focal length of the curvature on the back face of the lens we shall discuss later. At present it will merely be specified that it is to be less than the focal length of the curvature formed on the front face. This lens will bring the beam of parallel light ABCD (fig. 6a) to a line focus EF, where EF is situated as before at a distance from the lens equal to the distance from C to the line PQ (fig. 4). Before reaching EF, however, the beam is first brought to

another line focus GH, the distance from GH to the lens being equal to the focal length of the curvature on the back face of the lens. Now this lens, if placed behind the prism R in fig. 4, will form two pure spectra in the plane normal to the paper through the line PQ. Further, if in that figure the strip of metal called the trimmer be placed immediately behind the lens of the collimator, we can arrange, by properly choosing the radius of curvature of the back face of the lens, that the plane in which the trimmer is placed shall be conjugate to the plane in which the spectra are produced; and this fulfils our second condition.

The two spectra so formed from the two beams respectively will exhibit a dark gap between them, and therefore, as before,

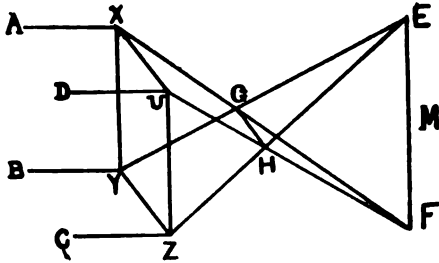


FIG. 6a.

the lens is to be cut through the centre in a horizontal plane, and then on separating the lens-halves to the required extent the two spectra can be moved towards each other till their edges come into perfect contact. In fig. 6 $\beta$ , there are shown two beams of homogeneous light, and the resulting lines EM, MF (which are two elements of the two spectra that would be formed in the general case) are drawn as they would be if brought with their ends just to touch each other by an appropriate separation of the lens-halves L and L'.

With a simple lens, on bringing the edges of the two spectra near each other, it can be seen that they are not parallel. This is due to a mixture of the errors of distortion and of chromatic aberration of the lens. To remedy this it would be of no avail to make the trimmer wider at one end, as explained on page 339; for reflection will show that that would merely reduce the intensity of the light which forms the adjacent edges of the

two spectra, but would not alter the positions of those edges. Each spectrum, however, may be tilted to the required amount by slightly rotating the corresponding lens-half about the general optical axis of the instrument. In order to preserve symmetry the respective rotations in opposite directions of the lens-halves should be to equal amounts. Even with so-called achromatic lenses this device will probably be found necessary.

The limitation of the field of view seen by the eye to a similar narrow strip from each spectrum is obtained in this form of the instrument by an appropriate screen in the eyepiece.

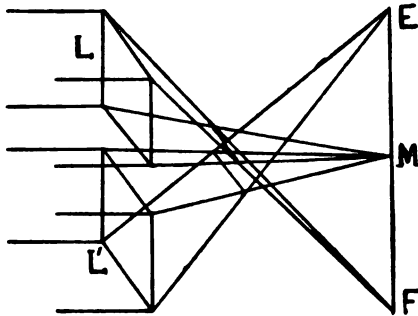


FIG. 6β.

Finally, it may be noted that it is desirable, in the interests of good definition, to use spherical lenses in preference to cylindrical, and to avoid curvatures of too small radius. Accordingly, instead of the theoretical lens discussed above, it is better to substitute one having the curvature on one of its faces spherical, and having the other face a convergent cylindrical lens whose axis of figure is horizontal. The proof that such a lens can be equivalent to the former is part of the general theory of optics, and neither this proof nor any details as to the necessary focal lengths of the curvatures, etc., need be entered upon here.

The author's experiments with this form of the instrument have not been numerous, because he found that the cheap divided lens used by him in the model gave less satisfactory definition than the spherical divided lens employed in the model of the instrument first described. He believes, however, that with a well-made lens this second arrangement of instrument might perhaps be better than the other.

It may, however, be noted that the chief objection to a somewhat long telescope tube can be done away with by the use in the spectrophotometer of a "constant deviation prism,"\* a construction of prism which permits the telescope of the instrument to be permanently fixed, while the prism alone rotates to bring the different parts of the spectrum to the observer's eye. In the case of a spectrophotometer furnished with such a prism the rigid mounting of even an unusually long telescope tube of course presents no difficulty.

Either of the above described arrangements of spectrophotometer having been adopted, it might be supposed that the similar strips of the two adjacent spectra could be satisfactorily observed on looking at them through any ordinary eyepiece. What is thus

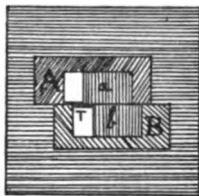


FIG. 7.

seen, however, is unsatisfactory. The two luminous strips are not like natural objects, which give out rays of light in all directions from every point, but on the contrary the edge of each of the strips brought into contact gives out rays of light only in a single plane, as indicated in fig. 8. From any point *a* of the upper edge of the lower image rays proceed only in the plane normal to the paper which passes through the line *aB*, and similarly from any point *b* of the lower edge of the upper image the rays proceed only in the plane normal to the paper which passes through the line *bA*. Accordingly, the coincident edges of the two images are seen by means of two sets of rays which respectively fall on the optical system of the eye at places some distance apart. Now through the effects of the eye's spherical aberration, and probably also because of general irregularities in the refractive parts of the eye, the two sets of rays from the coincident edges of the strips will not be brought to the same line on the retina. Any slight move-

\* As employed, for example, by Messrs Hilger, Ltd., on certain of their spectrometers.

ment of the head will alter the paths of the two sets of rays through the optical system of the eye, and the effect of such a movement will be to cause an apparent relative motion, as seen by the observer of the really coincident edges of the two spectra. As a matter of fact the edges of the two spectra are seen by an observer to be slightly overlapping each other at one moment, while a moment later a slight gap will have made its appearance between them. This, no doubt, is due to movements of the head or eye.

The author at first sought to remedy this defect by giving to the divided lens a focal length of about half a metre, which caused a reduction of the angle  $AaB$  of fig. 8, and a consequent reduction in the distance between the two sets of rays  $aB$ ,  $bA$  when

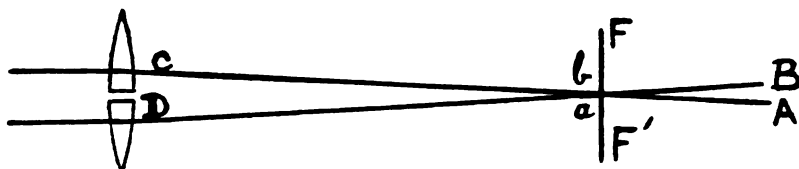


FIG. 8.

entering the eye. A specially short eyepiece also was used, so that the eye of the observer might come as near the diverging point  $a$  as possible. These alterations, while undoubtedly effecting much improvement, were after all only palliative in their effect, and the comparatively great focal length of the divided lens necessitated a somewhat unwieldy length of telescope tube, a point that has already been dwelt upon.

After various other methods had been considered without success the following means of overcoming the difficulty was finally discovered. Advantage was taken of the well-known fact that if a ray of light fall normally upon one of the faces of a Wollaston double image prism there proceeds from the other face two divergent rays which are polarised in planes at right angles to each other. If now—reversely—there fall on one of the faces of the Wollaston prism two converging rays of light inclined at the proper angle, these two rays will emerge from the opposite face of the prism in one and the same straight line normally to the face. It is true, of course, that unless the entering rays be each



polarised, and polarised in the proper planes respectively, then in addition to the two coincident exit rays there will be two other non-coincident exit rays, making four exit rays in all, but the divergent rays have no connection with our present purpose, and may be disregarded, as will be shown later. Suppose now that a suitable Wollaston prism be placed in the plane  $FF'$ , fig. 4, then all the rays which go to form the edges of the two spectra in the plane  $FF'$  (two of which rays are indicated by  $Cb$  and  $Da$ , fig. 8) proceed, after passing through the Wollaston prism, in one and the same horizontal plane through the eyepiece  $E$ . In this way all the rays from any point common to the coincident edges of the two spectra fall on the cornea of the observer's eye in one

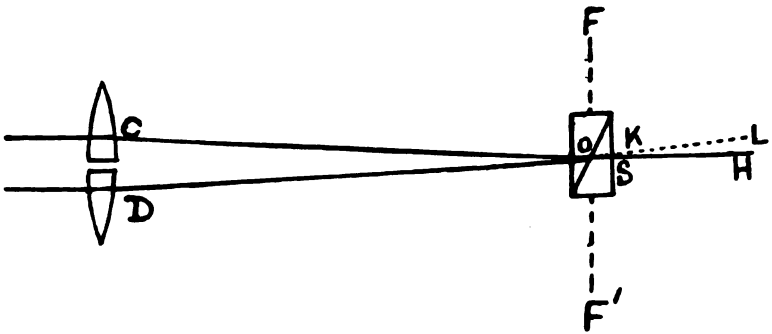


FIG. 9.

and the same straight line, so that the optical defects of the eye spoken of before do not cause any difficulties.

As mentioned above, each ray incident on the Wollaston prism gives rise to two emergent rays. Considering then, for example, the point  $b$  (fig. 8), we see that the ray proceeding from it in the plane of the paper will, after passing through the prism, give rise to two emergent rays  $SH$  and  $KL$  (fig. 9). The ray  $KL$  will not be seen at all by the observer unless the angle  $COI$  be small and the power of the eyepiece low. In the model that the author has had constructed the distance  $CO$  is about 6.5 inches and  $CD$  is equal to .6 inch, while the eyepiece is one of moderate power, and such rays as  $KL$  can only be seen by moving the eyepiece either up or down until the junction of the two bright strips has passed out of the field of view, so that only one of the two bright strips

can be seen by the observer. It will then be noticed that this strip has superimposed upon it another bright strip (C or D, fig. 10), at the end which has just been brought into view by this movement of the eyepiece. Hence it appears that really there are in all four bright strips, disposed as shown in fig. 10, the two with which we are concerned being the middle pair with their edges in contact along the line AB. Now the two additional bright strips C and D are formed by KL and the other rays whose refraction is analogous, and it will be seen that the statement made above—that the rays so refracted may for our purpose be ignored—is justified.

But there is a further advantage to be gained by such a use of a Wollaston prism. It will be remembered (see p. 343) that the use of a Vieroidt double slit in connection with this instrument

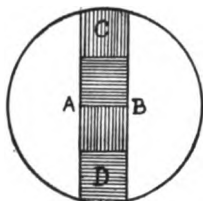


FIG. 10.—Under normal conditions only the middle portion of the above can be seen through the eyepiece, C and D lying outside the field of view.

to regulate and measure the light intensities of the two beams was found to be unsatisfactory owing to the great loss of light which it entailed, while a modified arrangement of the same kind had also to be discarded. Now, with the arrangement of apparatus described above, by the mere addition of a Nicol prism to the eyepiece there is provided the necessary appliance for regulating the intensities of the two strips of light seen by the observer until a perfect match is attained. The rays  $r$  and  $s$  (fig. 4), after transmission through the Wollaston prism, pass out along the same straight line  $t$ , but remain distinct in this, that they are polarised in planes at right angles to each other. Accordingly, because of the Nicol in the eyepiece, rotation of the latter about its axis causes every possible variation from zero to infinity of the ratio of the intensities of the two strips of light seen by the observer. By means of a circular vernier or other device the position of the

eyepiece as regards its rotation is ascertained after each setting to equal intensity of the two bright strips. The ratio of the brightness of the two strips is equal to the square of the tangent of the angle of displacement of the eyepiece, if the zero of the latter be that position in which the light of the under strip is completely extinguished. As the under strip is that due to the comparison beam, that is, to the beam that does not pass through the absorbing liquid, the tangent of the displacement angle is equal to the fraction of the incident light transmitted by the absorbing substance.

It is to be noted that, in common with other polarising spectrophotometers, this instrument suffers from the defect that the light in passing through the main prism is partially polarised in a vertical plane, for which reason, when there is no absorbing substance in the path of either beam, and when accordingly the analysing Nicol ought to give equality of illumination when set at an angle of  $45^\circ$ , it is found that the Nicol has to be turned round slightly from that position before the intensities of the two beams will exactly balance. The amount of this error, which depends on the refractive index of the glass of the prism, etc., can be calculated by Fresnel's formulæ, and in a case computed by the author it is about  $4^\circ 40'$ . As, however, it is hoped later on to publish some experiments on this subject, the mathematical discussion need not be entered into here. It only remains to be said, that the observations made with such instruments are to be reduced by assuming that a certain (constant) absorbing body has been permanently placed in the path of one of the beams.

It should be noted that the two beams of light are in some ways asymmetric as they pass through the Wollaston prism, and hence it is possible that different fractions of the light may be transmitted in each case. As regards absorption the existence of such a crystal as tourmaline shows that this may be very different in the case of the ordinary and of the extraordinary rays. With the crystal mentioned the ordinary ray is practically non-existent after transmission through one or two millimetres of the substance, while the extraordinary ray in the same circumstances is only slightly absorbed. In the case of this instrument, however, the Wollaston prism is of quartz, which is a substance where no such

marked disparity in the absorption coefficients for the ordinary and the extraordinary rays exists; and, further, any difference in the intensity of the transmitted beams after passing through the Wollaston prism could, in any case, only be a small one, because the rays of each beam are transmitted for the length of approximately half their path through the crystal as ordinary (extraordinary) rays, and for the remaining half as extraordinary (ordinary) rays.

Another possible source of asymmetric error lies in the fact that the rays from any, the same point of the image  $p$  (fig. 4) may, after passing through the Wollaston prism, diverge to a different extent from the rays from the corresponding point in the other image  $q$ , after they have passed through the Wollaston prism. Were this the case, and were the difference sufficiently marked, the eye would see the strip due to the less divergent beam to sensibly greater advantage as regards intensity than the strip due to the other beam. And indeed the two images themselves, because they are formed inside the Wollaston prism, may not correspond in brightness to the original beams, for the rays of the two beams respectively may be converged to a different extent on entering the prism.

Any such errors, however, did they exist could be at least very approximately got rid of as follows. The light absorption of any liquid for any particular wave length would be twice measured, once with the Wollaston prism emitting the upper beam as the ordinary ray, and then with the Wollaston prism turned upside down and emitting the same beam as the extraordinary ray. The mean of these two measurements would give the true absorption very nearly.

The model instrument which has been made, while it shows the general soundness of the principles involved, is not capable of measurements of the accuracy required to definitely settle this question. All that can be said in the circumstances is that no such discrepancy can be seen with the present apparatus.

In the note of last year the use of the instrument for Murphy's method of mapping the visual intensity of a spectrum was pointed out, and it only needs to be said that the necessary adjustments of the apparatus are those described in that communication in the

case of the form of this instrument which is depicted in fig. 5. In the case of the form which is depicted in fig. 4 the sliding pieces A and B of the screen (fig. 7) are first set respectively to the two neighbouring strips of the spectrum whose intensity it is desired to compare, and then the lens-halves L and L' (fig. 3) are moved sideways normal to the plane of the paper to bring the two images of these strips one above the other in the plane SS'. The perfect contact of the edges is secured by moving the lens-halves vertically either nearer together or further apart, as has already been explained.

(*Issued separately November 5, 1904.*)

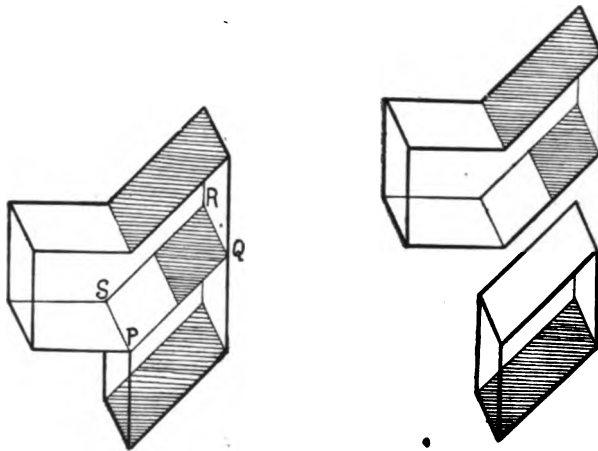
**A New Form of Juxtapositor to bring into Accurate Contact the Edges of the two Beams of Light used in Spectrophotometry, with an application to Polarimetry. By J. R. Milne, B.Sc., Carnegie Scholar in Natural Philosophy.**

(Read June 20, 1904. MS. received June 23, 1904.)

In the ordinary spectrophotometer and in Laurent's "half-shade" polarimeter, two neighbouring patches of light of the same colour but of different intensities are presented to the eye of the observer, who by an appropriate means reduces the intensity of the brighter until in his judgment it is brought down to the same intensity as the other. The accuracy of such a measurement must depend on two factors. The first factor is the accuracy with which the observer's eye can judge of the equality of the two patches of light, and the second factor is the accuracy with which the instrumental reading indicates the intensity of the comparison beam, *i.e.*, of the beam whose brightness is reduced till it becomes equal to that of the other. Now it is found that in ordinary cases the error of the eye's judgment in such measurements amounts to about 4% or 5%, while the measurement of the instrumental regulation of the light can be made much more accurately. Accordingly, the error in the measurements made with a spectrophotometer cannot be much less than 4% or 5% unless some special means be employed for improving the eye's power of judgment in such a case, and the mere provision of a finer instrumental graduation will not meet the difficulty. Considerable assistance would be rendered to the eye were the two patches of light, whose equality the eye is to judge, brought with their edges accurately to touch each other so that no hiatus existed between them. As a rule however such a hiatus does exist, for should the two lights be from different sources, the edge of the mirror or other appliance which directs the comparison beam into

the instrument invariably shows as a more or less badly defined dark space between the two spectra; while in those cases where only one light source is employed, one part of the beam being absorbed by any given substance and the other part used for comparison, the edge of the substance, if the latter be a solid, or the meniscus, if it be a liquid, brings about the same result. The object of the present paper is to describe an appliance by which this difficulty may be overcome.

The instrument (see fig. 1) is constructed of two separate pieces of glass which are cut from the same block to ensure



The two glass blocks cemented together.

FIG 1.

The two glass blocks shown apart as they are before being cemented.

similarity of optical properties. These pieces having been worked truly plane on the faces which transmit the light, are silvered over the portions shaded in the figure, and are then cemented together along their common interface PQRS. The effect of the cement, whose refractive index is practically the same as that of the glass, is to make the joint nearly optically homogeneous with the glass blocks on each side. As will be seen from the diagrams, in every case the various faces of the blocks are either perpendicular, or are inclined at an angle of  $45^\circ$  to each other.

The glass block thus built up is encased in a metal shell, with

appropriate openings for the entrance and exit of the light. The best place for the attachment of the apparatus to the spectrophotometer will, of course, vary to some extent with the pattern of the instrument—in the author's case it is mounted immediately in front of the collimator slit. When the juxtapositor is so situated with regard to the spectrophotometer, the upper or "comparison" beam of light enters face AB (fig. 2a) and meets the interface CD at an angle of 45°, and the part of it falling on the area OC is reflected upwards by the silvering. The other part, which falls on the unsilvered surface OD, passes straight on and out through the face CF, and is not used. In the same

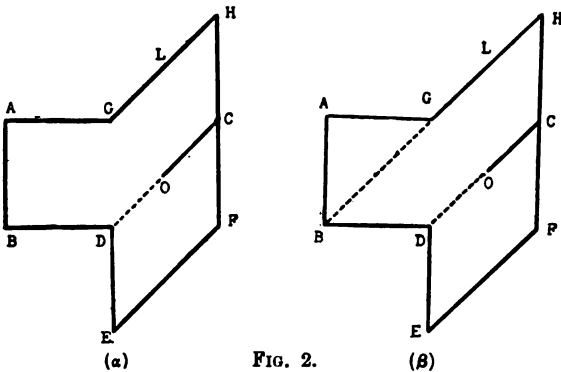


FIG. 2.

way the lower or "absorbed" beam enters face DE, and is reflected upwards by the silvering on the face EF, and the part of it incident on the lower half OD of the interface DC continues on its vertical course upwards. The other part, which falls on the silvered surface OC, is reflected out through the face CF, and is not used. The two beams which are respectively reflected and transmitted by OC and OD pass upwards in a common vertical direction, and have their edges in complete contact along a plane through OL normal to the paper. The beams thus brought into contact are reflected once more at the silvering on the face GH, and pass out through the face HC parallel to their original direction.

In the above discussion the action of the juxtapositor has been explained in a particular case—namely, when attached to a spectrophotometer immediately in front of the collimator slit—and we



have spoken of the "comparison" beam as entering the face AB, while the absorbed beam was supposed to enter the face DE. But obviously these are merely the special circumstances of a particular mode of application of the apparatus, and the latter might be attached to a spectrophotometer in any other way, and would work equally well, provided that one of the beams—it does not matter which—is made to fall normally on the face AB, and the other to fall normally on the face DE; and provided also that the point O (fig. 2) be in a plane optically conjugate to the retina of the observer's eye. The latter condition is necessary to avoid the appearance of diffraction effects caused by the cutting off of the edges of the two beams at the edge of the silvering on the interface; and also because the juxtapositor cannot be so exactly made that the two beams emerge from it quite parallel to each other; but as can easily be seen, their edges in such a case will once more be brought in contact in any plane where a real image of the point O is produced by the parts of the optical train of the spectrophotometer.

An important, and indeed one may almost say essential, principle of such an apparatus has been successfully observed, namely, that each of the two beams of light should pass through exactly the same length of glass. When this condition is not fulfilled the light from one beam will be absorbed to a greater extent than the light from the other, and an error will thus be introduced. Of course, in theory at least, an appliance faulty in this respect might be used correctly were its differential absorption found accurately beforehand; but the correction would have to be ascertained for a great number of different wave-lengths throughout the visible spectrum, and every observation made with the spectrophotometer when the appliance was in use would have to be individually corrected. That the passage through even a short length of glass causes marked absorption in a beam of light, particularly at the blue end of the spectrum, has been shown by various workers, among others by Nichols and Snow;\* and the knowledge of this fact caused the author to reject an earlier design which, though

\* "Note on the Selective Absorption of Light by Optical Glass and Calc-spar." By Edward L. Nichols and Benjamin W. Snow. *Phil Mag.* (5), No. 208, pp. 379-382, April 1892.

otherwise satisfactory, could not claim to be entirely symmetrical as regards absorption with respect to the two beams of light. Of course, as regards symmetry, absorption is not the only thing to be taken into account: the reflections and refractions of the two beams must be the same; but an examination of the figures will show that each of the two beams of light in this apparatus suffers two reflections and four refractions (*i.e.* into the glass, into and out of the cement, and finally out of the glass). This form of juxtapositor, as the author originally designed it and had it constructed, was arranged in what at first sight appears to be a symmetrical manner, and the fallacy involved was not observed till later on.

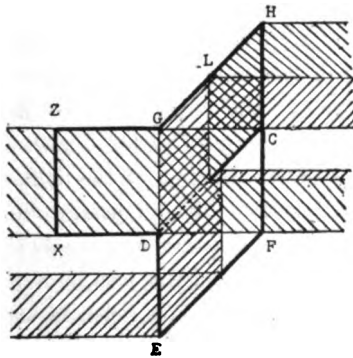


FIG. 3.—The letter O cannot be shown in the above diagram, but its position is the same as in fig. 2 ( $\alpha$ ) and ( $\beta$ ).

In this older form the upper of the two blocks of glass which compose the apparatus was cut through at an angle of  $45^\circ$ , as shown by the line GB (fig. 2 $\beta$ ). The triangular corner so detached was cemented on again, the cemented junction GB in the path of the upper beam being for the purpose of balancing the cemented junction DO in the path of the lower beam. The reasoning as to the symmetry of this form with regard to the two beams of light is as follows:—Each beam is twice reflected at a silvered surface. Each beam passes once from air to glass and once from glass to air. Each beam passes through the same total amount of glass. Each beam passes through one cemented junction. Hence the juxtapositor is symmetrical with respect to the two beams. In this reasoning, however, we are assuming the effect of a junction to be the absorption of the light owing to its cement layer, while

in reality this effect is inappreciable, the cement layer being so extremely thin; and we are leaving out of account the effect of the reflection by such a junction, which is not inappreciable, as will be shown later. Now, were it the case that all the light of the upper beam fell on the silvered part OC of the interface DC, and none of it on the unsilvered part OD, then each of the two beams would lose the same fraction of its light as it passed through the cemented joint in its path, *i.e.*, as the upper beam passed through the junction GB and the lower beam passed through the junction DO. It is necessary, however, that the lower edge of the upper beam should fall at least some distance below the point O in the figure, because only in this way can the full intensity of light be ensured right up to the edge of the silvered part OC of the interface CD. Assuming then that we have the lower part of the upper beam of light falling on the unsilvered part of the interface DO, there must exist the following state of affairs:—A certain fraction of the light of the upper beam is reflected by the junction GB and passes out through the face AG, leaving the beam that passes on towards the interface CD so much the less intense. The light lost in a similar manner by the lower beam, however, by being reflected at the junction DO and sent out through the face CF is more or less made up for by the light of the lower part of the upper beam which is reflected vertically upwards from the same junction DO.

If, however, the junction BG (fig. 2 $\beta$ ) were to be omitted, and the upper beam of light arranged to cover the whole face AB (fig. 3), then the gain and the loss to the light of the lower beam, caused by the interface at OD, would exactly balance each other. Provided always that is, that the juxtapositor is placed in the optical train after that piece of apparatus, whatever its particular form, whose function it is to equalise the intensity of the two beams of light, for then we have two beams of equal intensity falling on the same surface (OD) at the same angle, and accordingly the reflections will be of exactly the same magnitude.

In those cases where the juxtapositor is not so placed we have the loss or gain of intensity of the lower beam given by a quantity which is the reflection at the cement of the difference of the intensities of the two beams, and even here the error introduced

will, in general, be less than it would be were such a junction as BG (fig. 2β) arranged in the path of the upper beam. Moreover, in such cases to make the junction OD optically more homogeneous, such a liquid as the well-known α-monobromonaphthaline might be used between the faces of the glass blocks instead of the ordinary cement.

The following experiment was undertaken with the view of approximately ascertaining the amount of light reflected by such a cemented junction OD as occurs in this juxtapositor. The face DE of the latter was blocked up by an opaque screen, so that no light could pass through. The apparatus was then brought near a window, and the image of the latter produced by reflections at the silvered part CO of the interface, and at the silvering on the face HG was observed by looking into the face HC. No image whatever could be observed caused by a reflection from the unsilvered part OD of the interface, and not even an increased darkness could be seen corresponding to the places where the images of the window bars would fall. As a still more stringent test, the juxtapositor, with the lower face DE blocked up as before, was brought quite close to an incandescent electric lamp. In this case an image caused by reflection from the unsilvered part OD of the interface could be seen, but the image did not show the glass or brass fittings of the lamp, but only the glowing filament itself. Accordingly, it is clear that while there must be some difference between the refractive indices of the glass and of the cement used in the juxtapositor, which gives rise to reflection of light at the cemented surface, the fraction of the total light so reflected is very small indeed. It was noted also that the colour of that part of the glowing filament which was reflected by the unsilvered part OD of the interface appeared to be unchanged, which indicates that the small difference in the refractive indices of the glass and of the cement must be at least approximately constant for different wave-lengths.

The edge of the silvered part of the face PQRS (fig. 1) of the upper block of glass is cut off very trim and sharp by means of an ivory chisel and nitric acid. This is a most important point in the construction, because it is at this place that the two beams of light unite, and on the abruptness of the termination of the

reflecting surface depends the perfectness of the joining of the beams.

It is to be noted that the use of polarisation for regulating and measuring the light intensities is not prohibited by the adoption of this appliance, even when the light is polarised before being passed through the latter. No change of polarisation or production of polarisation can be caused by the entrance to or exit from the glass, for that only takes place normally to the various faces. If the two beams are plane polarised vertically and horizontally before entrance, with a view to the adjustment of their relative intensities later on by means of a Nicol prism, then because the plane of polarisation in each case is either in or normal to the plane of incidence on the silver surfaces no change of polarisation can occur. On the other hand, if the two beams have their respective planes of polarisation inclined to the vertical and to the horizontal, these beams, because they are each twice reflected at parallel silver surfaces, will emerge plane polarised still, though the plane of polarisation of each has been rotated to some extent. Hence in both cases the analysing Nicol can be used as before for the purpose of measuring the light intensity, although the zero will have been permanently displaced through a definite angle.

A suggested application of the juxtapositor described above will be readily understood by anyone conversant with the construction of Laurent's "half-shade" polarimeter. In that instrument two parallel beams of light polarised in planes at an angle to one another are passed through a substance whose rotative power it is desired to measure, and are then analysed by means of a Nicol prism. By properly adjusting the position of the latter, the two half-circles of light seen in the eyepiece of the instrument, due to the two beams, can be made equally bright. It is found that in this way a much more accurate setting of the rotating Nicol can be obtained than when, as in the ordinary case, only one beam of light is employed and the Nicol is set to extinction. But the accuracy of the measurement in Laurent's improved form of instrument turns on the degree of precision with which the eye is able to determine when the two halves of the circle seen in the eyepiece are equally bright. Now, these two halves are separated by a dark line, and accordingly, as explained above in the case of the

spectrophotometer, increased accuracy of measurement would result from getting the two bright semi-circles into perfect contact along their common diameter. By the use of this juxtapositor it is hoped this may be accomplished, and the accuracy of polarimetical measurements correspondingly improved.

The experiments which led up to the designing of this form of juxtapositor were made in the Physical Laboratory of the University of Edinburgh. The apparatus employed was in part supplied by a grant from the Moray Endowment Fund, to the trustees of which the author's best thanks are due.

*(Issued separately January 17, 1905.)*

### The Three-line Determinants of a Six-by-Three Array.

By Thomas Muir, LL.D.

(Second copy of MS. received September 12, 1904.\*  
Read November 7, 1904.)

(1) If the array in question be

$$\begin{array}{cccccc} a_1 & b_1 & c_1 & f_1 & g_1 & h_1 \\ a_2 & b_2 & c_2 & f_2 & g_2 & h_2 \\ a_3 & b_3 & c_3 & f_3 & g_3 & h_3, \end{array}$$

its score of three-line determinants  $|a_1 b_2 c_3|$ ,  $|a_1 b_2 f_3|$ ,  $\dots$  may be viewed as consisting of two complementary sets of ten, each of the first set containing at least two columns taken from  $|a_1 b_2 c_3|$ , and each of the second set at least two columns taken from  $|f_1 g_2 h_3|$ . Further, either set of ten may be viewed as consisting of one unique member and three sub-sets of three members each, the members of a sub-set being derivable from one another by performing the cyclical substitutions



In this way a convenient notation for the twenty determinants will be found to be

$$\left. \begin{array}{l} |a_1 b_2 c_3| \\ |a_1 b_2 f_3|, |b_1 c_2 g_3|, |c_1 a_2 h_3| \\ |a_1 b_2 g_3|, |b_1 c_2 h_3|, |c_1 a_2 f_3| \\ |a_1 b_2 h_3|, |b_1 c_2 f_3|, |c_1 a_2 g_3| \end{array} \right\} \equiv \begin{array}{l} 0 \\ 1, 2, 3 \\ 4, 5, 6 \\ 7, 8, 9 \end{array}$$

$$\left. \begin{array}{l} |f_1 g_2 h_3| \\ |c_1 g_2 h_3|, |a_1 h_2 f_3|, |b_1 f_2 g_3| \\ |c_1 h_2 f_3|, |a_1 f_2 g_3|, |b_1 g_2 h_3| \\ |c_1 f_2 g_3|, |a_1 g_2 h_3|, |b_1 h_2 f_3| \end{array} \right\} \equiv \begin{array}{l} 0' \\ 1', 2', 3' \\ 4', 5', 6' \\ 7', 8', 9' \end{array}$$

\* The original MS. was despatched by the author from Cape Town on 20th March 1904, but was lost in transit through the post.—[Sec. R.S.E.]

(2) *The product of any two complementary determinants of a six-by-three array is expressible in six different ways as an aggregate of three similar products.*

Taking as an example the product  $|a_1 b_2 c_3| |f_1 g_2 h_3|$  i.e.  $00'$ , we have from a well-known theorem by interchanging  $f, g, h$  in succession with  $a$

$$a_1 b_2 c_3 |f_1 g_2 h_3| = |f_1 b_2 c_3| |a_1 g_2 h_3| + |g_1 b_2 c_3| |f_1 a_2 h_3| + |h_1 b_2 c_3| |f_1 g_2 a_3|,$$

i.e.  $00' = 88' + 22' + 55'.$

By interchanging  $f, g, h$  in succession with  $b$  and  $f, g, h$  in succession with  $c$  two similar identities are obtained, viz.

$$00' = 99' + 33' + 66',$$

$$00' = 77' + 11' + 44',$$

which, however, it is simpler to view as derivatives of the first by cyclical substitution. On altering the order of the factors in the given product the same procedure leads us to

$$0'0 = 8'8 + 6'6 + 1'1,$$

$$0'0 = 9'9 + 4'4 + 2'2,$$

$$0'0 = 7'7 + 5'5 + 3'3.$$

It is clear (1) that what is here done with  $00'$  can be done with any similar product; (2) that each product on the right, by reason of the mode of obtaining it from the product on the left, will consist of factors that are *complementary*; (3) that the theorem used will not give more than six expressions, because the interchanging of two letters with two,—which is the remaining possibility,—is the same in effect as interchanging one with one.

(3) *The nine products in the first triad of expressions for  $00'$ , . . . are the same as the nine in the second triad, and further can be so arranged that a row-and-column interchange will produce the latter triad, any five of the expressions thus giving the sixth.*

Thus in the case of  $00'$  such an arrangement is

$$\begin{array}{ll} 88' + 22' + 55' & 88' + 66' + 11' \\ 66' + 99' + 33' & \text{and } 22' + 99' + 44' \\ 11' + 44' + 77' & 55' + 33' + 77' \end{array}$$



That this must in all cases hold is evident from considering that the interchanges, which when made on 00' produce the nine products of the first triad, are

$$\begin{matrix} \binom{f}{a}, & \binom{g}{a}, & \binom{h}{a}, \\ \binom{f}{b}, & \binom{g}{b}, & \binom{h}{b}, \\ \binom{f}{c}, & \binom{g}{c}, & \binom{h}{c}, \end{matrix}$$

and that these when taken in columns are exactly the interchanges which need to be performed on 00' to produce the products of the second triad.

(4) The ten groups of such sets of six expressions may thus be compactly exhibited as follows:—

00'			11'			22'			33'																																						
<table border="1" style="margin: auto; border-collapse: collapse;"> <tr><td>11'</td><td>44'</td><td>77'</td></tr> <tr><td>88'</td><td>22'</td><td>55'</td></tr> <tr><td>66'</td><td>99'</td><td>33'</td></tr> </table>			11'	44'	77'	88'	22'	55'	66'	99'	33'	<table border="1" style="margin: auto; border-collapse: collapse;"> <tr><td>00'</td><td>-44'</td><td>-77'</td></tr> <tr><td>-66'</td><td>55'</td><td>22'</td></tr> <tr><td>-88'</td><td>33'</td><td>99'</td></tr> </table>			00'	-44'	-77'	-66'	55'	22'	-88'	33'	99'	<table border="1" style="margin: auto; border-collapse: collapse;"> <tr><td>00'</td><td>-55'</td><td>-88'</td></tr> <tr><td>-44'</td><td>66'</td><td>33'</td></tr> <tr><td>-99'</td><td>11'</td><td>77'</td></tr> </table>			00'	-55'	-88'	-44'	66'	33'	-99'	11'	77'	<table border="1" style="margin: auto; border-collapse: collapse;"> <tr><td>00'</td><td>-66'</td><td>-99'</td></tr> <tr><td>-55'</td><td>44'</td><td>11'</td></tr> <tr><td>-77'</td><td>22'</td><td>88'</td></tr> </table>			00'	-66'	-99'	-55'	44'	11'	-77'	22'	88'
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Of course in the sixty equations here implied every distinct equation is repeated four times; for example, the equation  $00' - 11' - 44' - 77' = 0$  occurs under each of the headings  $00'$ ,  $11'$ ,  $44'$ ,  $77'$ . The number of distinct equations is thus 15.

(5) These fifteen equations are not all independent, the fact being that any one of the ten sets of six gives rise to all the remaining nine equations. Thus, taking the first set of six, viz.

$$\begin{aligned} 00' - 11' - 44' - 77' &= 0, \\ 00' - 88' - 22' - 55' &= 0, \\ 00' - 66' - 99' - 33' &= 0, \\ 00' - 11' - 88' - 66' &= 0, \\ 00' - 44' - 22' - 99' &= 0, \\ 00' - 77' - 55' - 33' &= 0, \end{aligned}$$

we can eliminate from pairs of them the nine binomials

$$\begin{aligned} 00' - 11', \quad 00' - 44', \quad 00' - 77', \\ 00' - 22', \quad 00' - 55', \quad 00' - 88', \\ 00' - 33', \quad 00' - 66', \quad 00' - 99', \end{aligned}$$

thus obtaining nine other equations of the same form, which are the nine in question. It is thus seen that the connecting equations will be better viewed as statements of the equality of binomials; and the theorem which this view leads to is that either the sum or the difference of any two of the products  $00'$ ,  $11'$ , . . . . is expressible in two ways as the sum or difference of other two. The forty-five possible binomials may be arranged as follows to show these equalities:—

$$\begin{aligned} 00' - 11' &= 77' + 44' = 88' + 66' \\ 00' - 22' &= 88' + 55' = 99' + 44' \\ 00' - 33' &= 99' + 66' = 77' + 55' \\ 00' - 44' &= 11' + 77' = 22' + 99' \\ 00' - 55' &= 22' + 88' = 33' + 77' \\ 00' - 66' &= 33' + 99' = 11' + 88' \\ 00' - 77' &= 11' + 44' = 33' + 55' \\ 00' - 88' &= 22' + 55' = 11' + 66' \\ 00' - 99' &= 33' + 66' = 22' + 44' \end{aligned}$$

$$\left. \begin{aligned} 11' - 22' = 55' - 66' = 99' - 77' \\ 22' - 33' = 66' - 44' = 77' - 88' \\ 33' - 11' = 44' - 55' = 88' - 99' \end{aligned} \right\}$$

$$11' - 55' = 22' - 66' = 33' - 44',$$

$$11' - 99' = 22' - 77' = 33' - 88',$$

$$44' - 88' = 55' - 99' = 66' - 77'.$$

It will be seen that the second line is derivable from the first, and the third from the second, by the cyclical substitution: and that the number of such triads is four. The last three lines are not so related: the cyclical substitution if performed on any one of these would simply reproduce that one.

(6) It is interesting to note that to each of the foregoing fifteen sets of three equivalents a fourth equivalent of a different form may be added. Thus for the seventh line we have the additional equivalent

$$\left| \begin{array}{ccc} | a_1 b_2 | & | a_2 b_3 | & | a_3 b_1 | \\ | f_1 g_2 | & | f_2 g_3 | & | f_3 g_1 | \\ | c_1 h_2 | & | c_2 h_3 | & | c_3 h_1 | \end{array} \right|;$$

for this can be shown equal to

$$\left| \begin{array}{cc} | a_1 b_2 g_3 | & | a_1 b_2 f_3 | \\ | c_1 h_2 g_3 | & | c_1 h_2 f_3 | \end{array} \right| \quad i.e. \quad 44' + 11',$$

and as the interchanges

$$\left( \begin{array}{cc} a & b \\ f & g \end{array} \right), \quad \left( \begin{array}{cc} c & h \\ f & g \end{array} \right)$$

alter only the sign of the three-line determinant,\* the latter must also be equal to

$$- \left| \begin{array}{cc} | f_1 g_2 b_3 | & | f_1 g_2 a_3 | \\ | c_1 h_2 b_3 | & | c_1 h_2 a_3 | \end{array} \right| \quad i.e. \quad 3'3 + 5'5$$

and

$$- \left| \begin{array}{cc} | a_1 b_2 h_3 | & | a_1 b_2 c_3 | \\ | f_1 g_2 h_3 | & | f_1 g_2 c_3 | \end{array} \right| \quad i.e. \quad -77' + 00.$$

\* The other similar interchange  $\left( \begin{array}{cc} a & b \\ c & h \end{array} \right)$  gives nothing new.

(7) Turning now from the products whose factors are complementary to those whose factors are not, we see that the taking of 0 along with any other of its own set (*e.g.* 01, 02, . . .) would be nugatory, because the two factors of any such product would have two columns in common. But 01, 02, . . . , 09 being on this account unfruitful, it follows that the same cannot be said of 01', 02', . . . , 09'. As for the products which begin with 1, they must be nine in number, because if they cannot be taken along with any particular one that follows it in its own set, this very fact ensures fruitfulness if taken along with the corresponding one of the other set : as a matter of fact the useful cases are

$$12, 13, 14', 15, 16', 17', 18', 19.$$

Similarly the useful products beginning with 2 are

$$23, 24', 25', 26, 27, 28', 29';$$

those beginning with 3,

$$34, 35', 36', 37', 38, 39':$$

and so on. It is thus seen that if we confine ourselves to the products whose first factor at least is taken from the first set of ten and is represented by a smaller integer than the second factor, the number of fruitful products is

$$9 + 8 + 7 + \dots + 3 + 2 + 1.$$

From every one of these products, however, another fruitful product is obtainable by changing each factor into its complementary. The total number is thus 90.

(8) Taking the first of the ninety, viz. 01', we have on interchanging *c*, *g*, *h* in succession with *a*

$$\begin{array}{l} | a_1 b_2 c_3 | | c_1 g_2 h_3 | = | g_1 b_2 c_3 | | c_1 a_2 h_3 | + | h_1 b_2 c_3 | | c_1 g_2 a_3 |, \\ \text{i.e.} \qquad \qquad 01' = \qquad \qquad 23 \qquad \qquad - \qquad \qquad 59. \end{array}$$

Now, no new result is got by interchanging *c*, *g*, *h* in succession with *b*, nor by interchanging *c*, *g*, *h* in succession with *c*. Further, by reversing the order of the factors in 01' and applying our theorem, we merely repeat the same result. We thus learn that *each of the ninety products of pairs of non-complementary three-line minors formed from a six-by-three array can*

be expressed in one and only one way as a sum or difference of two other such products.

(9) We are thus prepared to learn that if we take either of the two products whose sum or difference has been obtained in this way as an equivalent for a given product of the same kind, and apply our theorem as before, we shall merely get another repetition of the previous result. Thus

$$|g_1 b_2 c_3| |c_1 a_2 h_3| = |a_1 b_2 c_3| |c_1 g_2 h_3| + |h_1 b_2 c_3| |c_1 a_2 g_3|$$

i.e.  $23 = 01' + 59,$

and

$$|h_1 b_2 c_3| |c_1 g_2 a_3| = |g_1 b_2 c_3| |c_1 h_2 a_3| + |a_1 b_2 c_3| |c_1 g_2 h_3|,$$

i.e.  $-59 = -23 + 01'.$

It follows therefore that since there are ninety products and each can only occur once in an identity along with two others, the number of such identities is *thirty*. Probably the best arrangement of the thirty is that which brings into juxtaposition those that form a triad, and places opposite to each other those that are complementary. The result of this is:—

$$\begin{aligned} 01' - 23 + 59 &= 0 = 0'1 - 2'3' + 5'9' \\ 02' - 31 + 67 &= 0 = 0'2 - 3'1' + 6'7' \\ 03' - 12 + 48 &= 0 = 0'3 - 1'2' + 4'8' \end{aligned}$$

$$\begin{aligned} 04' - 56 + 38 &= 0 = 0'4 - 5'6' + 3'8' \\ 05' - 64 + 19 &= 0 = 0'5 - 6'4' + 1'9' \\ 06' - 45 + 27 &= 0 = 0'6 - 4'5' + 2'7' \end{aligned}$$

$$\begin{aligned} 07' - 89 + 26 &= 0 = 0'7 - 8'9' + 2'6' \\ 08' - 97 + 34 &= 0 = 0'8 - 9'7' + 3'4' \\ 09' - 78 + 15 &= 0 = 0'9 - 7'8' + 1'5' \end{aligned}$$

$$\begin{aligned} 14' + 82' + 69' &= 0 = 1'4 + 8'2 + 6'9 \\ 25' + 93' + 47' &= 0 = 2'5 + 9'3 + 4'7 \\ 36' + 71' + 58' &= 0 = 3'6 + 7'1 + 5'8 \end{aligned}$$

$$\begin{aligned} 16' + 49' + 73' &= 0 = 1'6 + 4'9 + 7'3 \\ 24' + 57' + 81' &= 0 = 2'4 + 5'7 + 8'1 \\ 35' + 68' + 92' &= 0 = 3'5 + 6'8 + 9'2. \end{aligned}$$

(10) The relations between products of *three* factors it is less necessary to study, the foundation of them being laid in what precedes. For example, there are numerous results like

$$1(01' + 59) = 2(02' + 67) = 3(03' + 48)$$

which is clearly obtainable from the first triad of § 8. Less easily verifiable from the foregoing are the pair

$$\begin{vmatrix} 1 & 8 & 6 \\ 4 & 2 & 9 \\ 7 & 5 & 3 \end{vmatrix} = 000', \quad \begin{vmatrix} 1' & 8' & 6' \\ 4' & 2' & 9' \\ 7' & 5' & 3' \end{vmatrix} = 0'0'0',$$

the process being—

$$\begin{aligned} \begin{vmatrix} 1 & 8 & 6 \\ 4 & 2 & 9 \\ 7 & 5 & 3 \end{vmatrix} &= 1(23 - 59) + 4(56 - 38) + 7(89 - 26) \\ &= 1 \ 01' \quad + 4 \ 04' \quad + 7 \ 07', \\ &= 0(11' + 44' + 77') \\ &= 000'. \end{aligned}$$

(Issued separately January 20, 1905.)

**The Sum of the Signed Primary Minors of a Determinant.**  
By Thomas Muir, LL.D.

(MS. received July 25, 1904. Read November 7, 1904.)

(1) The fundamental propositions in regard to the sum of the signed primary minors of a determinant are—

(A) *An expression for the negative sum of the signed primary minors of any determinant is got by taking a determinant of the next higher order whose first element is zero with the given determinant for complementary minor, and whose remaining elements are units all positive or all negative.*

(B) *The sum of the signed primary minors of any determinant is expressible as a determinant of the next lower order, any element (r, s) of the latter being the sum of the signed elements of a two-line minor of the former, viz., the sum (r, s) - (r, s + 1) - (r + 1, s) + (r + 1, s + 1).*

(C) *If the elements of a determinant be all increased by the same quantity  $\omega$ , the determinant is thereby increased by  $\omega$  times the sum of its signed primary minors.\**

(2) By the application of the first of these the following results are readily obtained—

*The sum of the signed primary minors of the alternant  $|a^{\alpha}b^{\beta}c^{\gamma} \dots|$  is equal to the alternant itself.* (I)

*The sum of the signed primary minors of a circulant of the  $n^{\text{th}}$  order is equal to  $n$  times the quotient of the circulant by the sum of its variables.* (II)

Thus, the sum of the signed primary minors of  $C(a, b, c)$

$$\begin{aligned}
 &= - \begin{vmatrix} . & 1 & 1 & 1 \\ 1 & a & b & c \\ 1 & c & a & b \\ 1 & b & c & a \end{vmatrix} = - \begin{vmatrix} . & 1 & 1 & 1 \\ a+b+c & a & b & c \\ a+b+c & c & a & b \\ a+b+c & b & c & a \end{vmatrix} \div (a+b+c), \\
 &= - \begin{vmatrix} -3 & 1 & 1 & 1 \\ . & a & b & c \\ . & c & a & b \\ . & b & c & a \end{vmatrix} \div (a+b+c), \\
 &= 3C(a, b, c) \div (a+b+c).
 \end{aligned}$$

\* *Proceedings Roy. Soc. Edinburgh*, xxiv. pp. 387-392.

The sum of the signed primary minors of a zero-axial skew determinant is equal to a similar determinant of the next higher order, and therefore is zero if the order of the original determinant be even, and is the square of a Pfaffian if the order be odd. (III)

(3) By the application of the second fundamental result (B) the case of a centro-symmetric determinant can be equally easily dealt with, the result being—

The sum of the signed primary minors of a centrosymmetric determinant is equal to a similar determinant of the next lower order, and therefore is resolvable into two factors. (IV)

Thus, the sum of the signed primary minors of

$$\begin{vmatrix} a & b & c \\ d & e & d \\ c & b & a \end{vmatrix}$$

$$= \begin{vmatrix} a-b-d+e & b-c-e+d \\ d-e-c+b & e-d-b+a \end{vmatrix} = (a-b-d+e)^2 - (b-c-e+d)^2, \\ = (a-c)(a-2b-c-2d+2e).$$

(4) The case of a continuant requires and is worthy of a little more consideration. Restricting ourselves, merely for shortness' sake, to the six-line continuant

$$\left( \begin{array}{cccccc} & b_1 & b_2 & b_3 & b_4 & b_5 \\ a_1 & a_2 & a_3 & a_4 & a_5 & a_6 \\ & c_1 & c_2 & c_3 & c_4 & c_5 \end{array} \right),$$

and denoting the sum of its signed primary minors by prefixing to it an M, we know to begin with that this sum equals

$$- \begin{vmatrix} . & 1 & 1 & 1 & 1 & 1 & 1 \\ 1 & a_1 & b_1 & . & . & . & . \\ 1 & c_1 & a_2 & b_2 & . & . & . \\ 1 & . & c_2 & a_3 & b_3 & . & . \\ 1 & . & . & c_3 & a_4 & b_4 & . \\ 1 & . & . & . & c_4 & a_5 & b_5 \\ 1 & . & . & . & . & c_5 & a_6 \end{vmatrix}.$$

Fixing the attention on the last column and last row, the non-



zero elements of which are (1, 7), (6, 7), (7, 7), (7, 6), (7, 1), we obtain the equivalent expression

$$(7, 7)\text{cof} + (6, 7)(7, 6)\text{cof} + (1, 7)(7, 1)\text{cof} + (1, 7)(7, 6)\text{cof} + (7, 1)(6, 7)\text{cof}$$

i.e.

$$a_6 M \begin{pmatrix} b_1 & \dots \\ a_1 & \dots & a_5 \\ c_1 & \dots \end{pmatrix} - b_5 c_5 M \begin{pmatrix} b_1 & \dots \\ a_1 & \dots & a_4 \\ c_1 & \dots \end{pmatrix} + \begin{pmatrix} b_1 & \dots \\ a_1 & \dots & a_5 \\ c_1 & \dots \end{pmatrix}$$

$$- c_5 \begin{vmatrix} 1 & a_1 & b_1 & . & . \\ 1 & c_1 & a_2 & b_2 & . \\ 1 & . & c_2 & a_3 & b_3 \\ 1 & . & . & c_3 & a_4 \\ 1 & . & . & . & c_4 \end{vmatrix} - b_5 \begin{vmatrix} 1 & 1 & 1 & 1 & 1 \\ a_1 & b_1 & . & . & . \\ c_1 & a_2 & b_2 & . & . \\ . & c_2 & a_3 & b_3 & . \\ . & . & c_3 & a_4 & b_4 \end{vmatrix}.$$

Of the two determinants here written at full length the first is seen to be

$$= \begin{pmatrix} b_1 & \dots \\ a_1 & \dots & a_4 \\ c_1 & \dots \end{pmatrix} + c_4 \begin{vmatrix} 1 & a_1 & b_1 & . \\ 1 & c_1 & a_2 & b_2 \\ 1 & . & c_2 & a_3 \\ 1 & . & . & c_3 \end{vmatrix},$$

$$= \begin{pmatrix} b_1 & \dots \\ a_1 & \dots & a_4 \\ c_1 & \dots \end{pmatrix} - c_4 \begin{pmatrix} b_1 & \dots \\ a_1 & \dots & a_3 \\ c_1 & \dots \end{pmatrix} + c_4 c_3 \begin{pmatrix} b_1 \\ a_1 a_2 \\ c_1 \end{pmatrix} - c_4 c_3 c_2 (a_1) + c_4 c_3 c_2 c_1,$$

and the second

$$= \begin{pmatrix} b_1 \\ a_1 \dots a_4 \\ c_1 \end{pmatrix} - b_4 \begin{pmatrix} b_1 \dots \\ a_1 \dots a_3 \\ c_1 \dots \end{pmatrix} + b_4 b_3 \begin{pmatrix} b_1 \\ a_1 a_2 \\ c_1 \end{pmatrix} - b_4 b_3 b_2 (a_1) + b_4 b_3 b_2 b_1.$$

It thus follows that

$$M \begin{pmatrix} b_1 & \dots \\ a_1 & \dots & a_6 \\ c_1 & \dots \end{pmatrix} = a_6 M \begin{pmatrix} b_1 & \dots \\ a_1 & \dots & a_5 \\ c_1 & \dots \end{pmatrix} - b_5 c_5 M \begin{pmatrix} b_1 & \dots \\ a_1 & \dots & a_4 \\ c_1 & \dots \end{pmatrix}$$

$$+ \begin{pmatrix} b_1 & \dots \\ a_1 & \dots & a_5 \\ c_1 & \dots \end{pmatrix} - (c_5 + b_5) \begin{pmatrix} b_1 & \dots \\ a_1 & \dots & a_4 \\ c_1 & \dots \end{pmatrix}$$

$$+ (c_5 c_4 + b_5 b_4) \begin{pmatrix} b_1 \\ a_1 \dots a_3 \\ c_1 \end{pmatrix}$$

$$- (c_5 c_4 c_3 + b_5 b_4 c_3) \begin{pmatrix} b_1 \\ a_1 a_2 \\ c_1 \end{pmatrix}$$

$$+ (c_5 c_4 c_3 c_2 + b_5 b_4 c_3 c_2) (a_1)$$

$$- (c_5 c_4 c_3 c_2 c_1 + b_5 b_4 b_3 b_2 b_1), \quad (V)$$

and consequently that the sum of the signed primary minors of a continuant of the  $n^{\text{th}}$  order can be got when the corresponding sums for the cases of the  $(n - 1)^{\text{th}}$  and  $(n - 2)^{\text{th}}$  order are known.

(5) By repeated application of the preceding result we obtain ultimately an expression involving only the continuants

$$\begin{pmatrix} b_1 \dots \\ a_1 \ a_2 \dots a_n \\ c_1 \dots \end{pmatrix}, \begin{pmatrix} b_1 \dots \\ a_1, a_2, \dots, a_n \\ c_1 \dots \end{pmatrix}, \dots \text{ and their co-}$$

efficients. The following is the general theorem thus reached:—

*If the cofactors of  $a_n, a_n a_{n-1}, a_n a_{n-1} a_{n-2}, \dots$  in the continuant*

$$\begin{pmatrix} b_1 \dots \\ a_1, a_2, \dots, a_n \\ c_1 \dots \end{pmatrix} \text{ be denoted by } K_{n-1}, K_{n-2}, K_{n-3}, \dots, \text{ and}$$

*the cofactors of  $a_1, a_1 a_2, a_1 a_2 a_3, \dots$  be denoted by  $H_{n-1}, H_{n-2}, H_{n-3}, \dots$ , the sum of the signed primary minors of the continuant  $K_n$  is*

$$\begin{aligned} & K_{n-1} + K_{n-2}(1, b_{n-1} + c_{n-1} \checkmark H_1, -1) && \text{(VI)} \\ & + K_{n-3}(1, b_{n-2} + c_{n-2}, b_{n-1} b_{n-2} + c_{n-1} c_{n-2} \checkmark H_2, -H_1, 1) \\ & + K_{n-4}(1, b_{n-3} + c_{n-3}, b_{n-2} b_{n-3} + c_{n-2} c_{n-3}, b_{n-1} b_{n-2} b_{n-3} \\ & \qquad \qquad \qquad + c_{n-1} c_{n-2} c_{n-3} \checkmark H_3, -H_2, H_1 - 1) \\ & + \dots \dots \dots \\ & + (1, b_1 + c_1, b_2 b_1 + c_2 c_1, \dots \checkmark H_{n-1}, -H_{n-2}, \dots). \end{aligned}$$

For example, the sum of the signed primary minors of the con-

tinuant  $K_3$ , i.e.  $\begin{pmatrix} b_1 & b_2 \\ a_1 & a_2 & a_3 \\ c_1 & c_2 \end{pmatrix}$  is

$$\begin{aligned} & \begin{pmatrix} b_1 \\ a_1 & a_2 \\ c_1 \end{pmatrix} + (a_1)(1, b_2 + c_2 \checkmark a_3, -1) \\ & + (1, b_1 + c_1, b_2 b_1 + c_2 c_1 \checkmark a_2 a_3 - b_2 c_2, -a_3, 1), \end{aligned}$$

i.e.

$$\begin{aligned} & a_1 a_2 - b_1 c_1 + a_1(a_3 - b_2 - c_2) \\ & + a_2 a_3 - b_2 c_2 - a_3(b_1 + c_1) + (b_2 b_1 + c_2 c_1), \end{aligned}$$

i.e.

$$\begin{aligned} & a_1 a_2 + a_2 a_3 + a_3 a_1 - a_1(b_2 + c_2) - a_3(b_1 + c_1) \\ & - b_1 c_1 - b_2 c_2 + b_1 b_2 + c_1 c_2. \end{aligned}$$

(6) For the case of a 'simple' continuant, *i.e.* when each of the  $b$ 's is 1 and each of the  $c$ 's is  $-1$ , the expression (VI) becomes

$$\begin{aligned} & (a_1 a_2 \dots a_n) \\ & + (a_1 a_2 \dots a_{n-1}) \cdot a_n \\ & + (a_1 a_2 \dots a_{n-2}) \cdot \{(a_{n-1} a_n) + 2\} \\ & + (a_1 a_2 \dots a_{n-3}) \cdot \{(a_{n-2} a_{n-1} a_n) + 2a_n\} \\ & + (a_1 a_2 \dots a_{n-4}) \cdot \{(a_{n-3} a_{n-2} a_{n-1} a_n) + 2(a_{n-1} a_n) + 2\} \\ & + \dots \dots \dots \end{aligned}$$

and therefore, like the continuant itself, has all its terms positive. (VII)

For example, the sum of the signed primary minors of the continuant  $(a_1, a_2, a_3, a_4)$  is

$$\begin{aligned} & (a_1 a_2 a_3) + (a_1 a_2) \cdot a_4 \\ & \quad + a_1 \cdot \{(a_3 a_4) + 2\} \\ & \quad + \{(a_2 a_3 a_4) + 2a_4\} \end{aligned}$$

*i.e.*

$$\begin{aligned} & a_1 a_2 a_3 + a_1 + a_3 + (a_1 a_2 + 1)a_4 + a_1(a_3 a_4 + 3) \\ & \quad + a_2 a_3 a_4 + a_2 + 3a_4, \end{aligned}$$

*i.e.*

$$\begin{aligned} & a_1 a_2 a_3 + a_1 a_2 a_4 + a_1 a_3 a_4 + a_2 a_3 a_4 \\ & \quad + 4a_1 + a_2 + a_3 + 4a_4. \end{aligned}$$

(7) If the expression (VI) in § 5 be arranged in the order of the  $H$ 's and their cofactors, it becomes

$$\begin{aligned} & H_{n-1} + H_{n-2}(1, b_1 + c_1 \delta K_1, -1) \\ & \quad + H_{n-3}(1, b_2 + c_2, b_1 b_2 + c_1 c_2 \delta K_2, -K_1, 1) \\ & \quad + \dots \dots \dots \end{aligned} \tag{VIII}$$

which according to (VI) is the sum of the signed primary minors

of 
$$\begin{pmatrix} & b_{n-1} & \dots & & b_1 \\ a_n & a_{n-1} & \dots & a_2 & a_1 \\ & c_{n-1} & \dots & & c_1 \end{pmatrix},$$

--a result to be expected, since generally

$$\begin{vmatrix} \cdot & 1 & 1 & 1 & \dots & \cdot \\ 1 & a_1 & a_2 & a_3 & \dots & \cdot \\ \cdot & 1 & b_1 & b_2 & b_3 & \dots & \cdot \\ \cdot & 1 & c_1 & c_2 & c_3 & \dots & \cdot \\ \cdot & \cdot & \cdot & \cdot & \cdot & \dots & \cdot \end{vmatrix} = \begin{vmatrix} a_1 & a_2 & a_3 & \dots & 1 \\ b_1 & b_2 & b_3 & \dots & 1 \\ c_1 & c_2 & c_3 & \dots & 1 \\ \cdot & \cdot & \cdot & \dots & \cdot \\ 1 & 1 & 1 & \dots & 1 \end{vmatrix}$$

$$= \begin{vmatrix} \cdot & \dots & 1 & 1 & 1 \\ \cdot & \cdot & \cdot & \cdot & \cdot \\ 1 & \dots & c_3 & c_2 & c_1 \\ 1 & \dots & b_3 & b_2 & b_1 \\ 1 & \dots & a_3 & a_2 & a_1 \end{vmatrix},$$

and therefore

$$M\{ | a_1 b_2 c_3 \dots | \} = M\{ | \dots c_2 b_2 a_1 | \}. \tag{IX}$$

(8) When each of the  $a$ 's is equal to  $a$ , each of the  $b$ 's to  $b$ , and each of the  $c$ 's to  $c$ , the  $H$ 's and  $K$ 's are no longer distinguishable, and the expression (VI) becomes

$$\begin{aligned} & K_{n-1} + K_{n-2} \cdot (1, b + c \checkmark K_1, -1) \\ & + K_{n-3} \cdot (1, b + c, b^2 + c^2 \checkmark K_2, -K_1, 1) \\ & + \dots \\ & + (1, b + c, b^2 + c^2, \dots \checkmark K_{n-1}, -K_{n-2}, \dots). \end{aligned}$$

This, however, is best arranged in portions containing  $1, b + c, b^2 + c^2, \dots$ , and their respective cofactors, the result then being

$$\begin{aligned} & (K_{n-1}, K_{n-2}, K_{n-3}, \dots, K_1, 1 \checkmark 1, K_1, \dots, K_{n-3}, K_{n-2}, K_{n-1}) \\ + (b + c) & (K_{n-2}, K_{n-3}, \dots, K_1, 1 \checkmark 1, K_1, \dots, K_{n-3}, K_{n-2}) \\ + (b^2 + c^2) & (K_{n-3}, \dots, K_1, 1 \checkmark 1, K_1, \dots, K_{n-3}) \\ + \dots & \dots \tag{X} \end{aligned}$$

or, say,

$$\chi_{n-1} - (b + c)\chi_{n-2} + (b^2 + c^2)\chi_{n-3} - \dots$$

Now since

$$\chi_n = K_n + K_{n-1}K_1 + K_{n-2}K_2 + \dots$$

and the known ultimate form of  $K_n$  is an expression consisting of terms descending by second powers of  $a$  and ascending by first powers of  $bc$ , viz.

$$a^n - C_{n-1,1} a^{n-2}bc + C_{n-2,2} a^{n-4}b^2c^2 - \dots,$$

it follows that there must be for  $\chi_n$  an expression of similar

character. Towards finding this we first note the following property of continuants, viz.

*The cofactors of the elements in the places*

$$(n, n), (n-1, n), (n-2, n), \dots, (1, n)$$

*of the continuant*

are 
$$\begin{pmatrix} b_1 & b_2 & & & \\ a_1 & a_2 & a_3 & \dots & a_n \end{pmatrix} \text{ or } \kappa_n$$

$$\kappa_{n-1}, \kappa_{n-2}, \dots, \kappa_1, 1. \tag{XI}$$

Changing  $K_n$  into the form  $\begin{pmatrix} -b_1c_1 & -b_2c_2 & & & \\ a_1 & a_2 & a_3 & \dots & a_n \end{pmatrix}$

and putting in the said places of it

$$1, K_1, K_2, \dots, K_{n-1}$$

we thus learn that the resulting determinant is equal to

$$(K_{n-1}, K_{n-2}, \dots, 1 \text{ \textcircled{X}} 1, K_1, \dots, K_{n-1})$$

and therefore is equal to  $\chi_{n-1}$ . In other words we have

$$\chi_{n-1} = \begin{vmatrix} a & -bc & . & \dots & . & K_{n-1} \\ -1 & a & -bc & \dots & . & K_{n-2} \\ . & -1 & a & \dots & . & K_{n-3} \\ . & . & . & \dots & . & . \\ . & . & . & \dots & a & K_1 \\ . & . & . & \dots & -1 & 1 \end{vmatrix}. \tag{XII}$$

This determinant, however, may be developed in another way, viz., in terms of the elements of the first row and their respective cofactors; and doing this we obtain

$$\chi_{n-1} = a\chi_{n-2} - bc\chi_{n-3} + K_{n-1} \tag{XIII}$$

—a recurrence-formula which readily gives

$$\chi_{n-1} = na^{n-1} - (n-1)C_{n-2,1}a^{n-2}bc + (n-2)C_{n-3,2}a^{n-3}b^2c^2 - \dots \tag{XIV}$$

In illustration let us take the case where  $n=4$ . We then have

the sum of the signed primary minors of  $\begin{pmatrix} b & b & b \\ a & a & a \\ c & c & c \end{pmatrix}$

$$\begin{aligned} &= \chi_3 - (b+c)\chi_2 + (b^2+c^2)\chi_1 - (b^3+c^3), \\ &= 4a^3 - 6abc - (b+c)(3a^2 - 2bc) + (b^2+c^2)2a - (b^3+c^3), \\ &= 4a^3 - 3a^2(b+c) + 2a(b^2 - 3bc + c^2) - (b+c)(b^2 - 3bc + c^2). \end{aligned}$$

(9) If it be desired to have the general result arranged according to descending powers of  $a$ , we have only got to substitute in  $\chi_{n-1} - (b+c)\chi_{n-2} + (b^2+c^2)\chi_{n-3} - \dots$  the expressions for  $\chi_{n-1}, \chi_{n-2}, \dots$  obtained from (XIV), and then collect the coefficients of like powers of  $a$ . The theorem thus arrived at is—

The sum of the signed primary minors of  $\begin{pmatrix} b & b \\ a & a & \dots & a \\ c & c \end{pmatrix}_n$  is

$$\begin{aligned} & na^{n-1} \\ & - (n-1)a^{n-2}\{b+c\} \\ & + (n-2)a^{n-3}\{(b^2+c^2) - C_{n-1,1}bc\} \\ & - (n-3)a^{n-4}\{(b^3+c^3) - C_{n-2,1}(b+c)bc\} \\ & + (n-4)a^{n-5}\{(b^4+c^4) - C_{n-3,1}(b^2+c^2)bc + C_{n-2,2}b^2c^2\} \\ & - (n-5)a^{n-6}\{(b^5+c^5) - C_{n-4,1}(b^3+c^3)bc + C_{n-3,2}(b+c)b^2c^2\} \\ & + \dots \dots \dots \end{aligned} \tag{XV}$$

The cofactors here of  $na^{n-1}, -(n-1)a^{n-2}, \dots$  are related to one another in a curious way, which is worth noting if only for use as a check in computation. Denoting them by  $X, X_1, X_2, \dots$  we have

$$\left. \begin{aligned} X_{2m+1} &= (b+c)X_{2m} \\ X_{2m} &= (b+c)X_{2m-1} - (n+1)\frac{1}{m}C_{n-m,m-1} \cdot b^m c^m \end{aligned} \right\} \tag{XVI}$$

the demonstration of both resting on the facts

$$\begin{aligned} (b^r+c^r)(b+c) &= (b^{r+1}+c^{r+1}) + bc(b^{r-1}+c^{r-1}), \\ C_{p,q} &= C_{p-1,q} + C_{p-1,q-1}. \end{aligned}$$

(10) It is thus suggested to examine the result of multiplying the whole expression by  $a+(b+c)$ . Taking it in its original form

$$\chi_{n-1} - (b+c)\chi_{n-2} + (b^2+c^2)\chi_{n-3} - \dots$$

we readily see, to begin with, that the product is

$$\begin{aligned} & a\chi_{n-1} - a(b+c)\chi_{n-2} + a(b^3+c^3)\chi_{n-3} - \dots \\ & + (b+c)\chi_{n-1} - \left. \begin{aligned} & (b^2+c^2) \\ & - bc(b^0+c^0) \end{aligned} \right\} \chi_{n-2} + \left. \begin{aligned} & (b^3+c^3) \\ & + bc(b+c) \end{aligned} \right\} \chi_{n-3} - \dots \end{aligned}$$

This, however, if arranged in parts containing  $b^0 + c^0$ ,  $b^1 + c^1$ ,  $b^2 + c^2$ , . . . . and their respective cofactors, is

$$\begin{aligned} & \{a\chi_{n-1} - 2bc\chi_{n-2}\} + (b+c) \{\chi_{n-1} - a\chi_{n-2} + bc\chi_{n-3}\} \\ & \quad - (b^2 + c^2) \{\chi_{n-2} - a\chi_{n-3} + bc\chi_{n-4}\} \\ & \quad + (b^3 + c^3) \{\chi_{n-3} - a\chi_{n-4} + bc\chi_{n-5}\} \\ & \quad - \dots \end{aligned}$$

Now it can be shown that

$$a\chi_{n-1} - 2bc\chi_{n-2} = nK_n,$$

and, as we have already seen,

$$\chi_{n-1} - a\chi_{n-2} + bc\chi_{n-3} = K_{n-1};$$

we thus reach the following interesting result—

The sum of the signed primary minors of  $\begin{pmatrix} b & b & & \\ a & a & a & \dots & a \\ & c & c & & \end{pmatrix}_n$  is the quotient of  $nK_n + (b+c)K_{n-1} - (b^2+c^2)K_{n-2} + (b^3+c^3)K_{n-3} - \dots$  by  $a+b+c$ . (XVII)

(11) It has recently been proved \* that

$$\begin{vmatrix} a+d & b+d & d & d & \dots \\ c+d & a+d & b+d & d & \dots \\ d & c+d & a+d & b+d & \dots \\ d & d & c+d & a+d & \dots \\ \dots & \dots & \dots & \dots & \dots \end{vmatrix}_n = \begin{matrix} (-)^{n+1}c^n \\ s(a-\beta) \end{matrix} \begin{vmatrix} 1 & 1 & s+(n+1)d \\ 1 & a^{n+1} & 1+a+\dots+a^n \\ 1 & \beta^{n+1} & 1+\beta+\dots+\beta^n \end{vmatrix}$$

where  $s = a + b + c$  and  $\alpha, \beta$  are the roots of the equation  $cx^2 + ax + b = 0$ . Now, in the first place, the determinant on the left here is

by the third theorem (C) of § 1, the continuant  $\begin{pmatrix} b & b & & \\ a & a & a & \dots & a \\ & c & c & & \end{pmatrix}_n$

increased by  $d$  times the sum of its signed primary minors: that is to say, is equal to

$$K_n + d \cdot M(K_n).$$

In the second place, the determinant on the right is equal to

$$s \begin{vmatrix} 1 & a^{+1} \\ 1 & \beta^{n+1} \end{vmatrix} + d \begin{vmatrix} 1 & 1 & n+1 \\ 1 & a^{n+1} & 1+a+\dots+a^n \\ 1 & \beta^{n+1} & 1+\beta+\dots+\beta^n \end{vmatrix}.$$

\* By Dr F. S. Macaulay in *Math. Gazette*, iii. pp. 44-45.

It follows, therefore, because of the known result

$$K_n = (-)^{n+1}c^n \cdot \frac{\begin{vmatrix} 1 & a^{n+1} \\ 1 & \beta^{n+1} \end{vmatrix}}{a - \beta} = (-c)^n \frac{a^{n+1} - \beta^{n+1}}{a - \beta},$$

that

$$M(K_n) = \frac{(-)^{n+1}c^n}{s(a - \beta)} \begin{vmatrix} 1 & 1 & n+1 \\ 1 & a^{n+1} & 1+a+\dots+a^n \\ 1 & \beta^{n+1} & 1+\beta+\dots+\beta^n \end{vmatrix} \quad \text{(XVIII)}$$

This curious result ought to agree with (XVII): in other words we ought to be able to show that

$$\frac{(-)^{n+1}c^n}{a - \beta} \begin{vmatrix} 1 & 1 & n+1 \\ 1 & a^{n+1} & 1+a+\dots+a^n \\ 1 & \beta^{n+1} & 1+\beta+\dots+\beta^n \end{vmatrix} = nK_n + (b+c)K_{n-1} - (b^2+c^2)K_{n-2} + \dots$$

Towards doing so it has first to be noted that the determinant on the left

$$\begin{aligned} &= \begin{vmatrix} 1 & 1 & n \\ 1 & a^{n+1} & a+a^2+\dots+a^n \\ 1 & \beta^{n+1} & \beta+\beta^2+\dots+\beta^n \end{vmatrix} \\ &= n \begin{vmatrix} 1 & a^{n+1} \\ 1 & \beta^{n+1} \end{vmatrix} + \begin{vmatrix} 1 & 1 \\ 1 & a^{n+1} & a+a^2+\dots+a^n \\ 1 & \beta^{n+1} & \beta+\beta^2+\dots+\beta^n \end{vmatrix}, \\ &= n \begin{vmatrix} 1 & a^{n+1} \\ 1 & \beta^{n+1} \end{vmatrix} + \begin{vmatrix} a^{n+1} & a+a^2+\dots+a^n \\ \beta^{n+1} & \beta+\beta^2+\dots+\beta^n \end{vmatrix} - \begin{vmatrix} 1 & a+a^2+\dots+a^n \\ 1 & \beta+\beta^2+\dots+\beta^n \end{vmatrix}, \\ &= n \begin{vmatrix} 1 & a^{n+1} \\ 1 & \beta^{n+1} \end{vmatrix} + \begin{vmatrix} a^{n+1} & a \\ \beta^{n+1} & \beta \end{vmatrix} + \begin{vmatrix} a^{n+1} & a^2 \\ \beta^{n+1} & \beta^2 \end{vmatrix} + \dots \\ &\quad - \begin{vmatrix} 1 & a^n \\ 1 & \beta^n \end{vmatrix} - \begin{vmatrix} 1 & a^{n-1} \\ 1 & \beta^{n-1} \end{vmatrix} - \dots \\ &= n \begin{vmatrix} 1 & a^{n+1} \\ 1 & \beta^{n+1} \end{vmatrix} - (a\beta+1) \begin{vmatrix} 1 & a^n \\ 1 & \beta^n \end{vmatrix} - (a^2\beta^2+1) \begin{vmatrix} 1 & a^{n-1} \\ 1 & \beta^{n-1} \end{vmatrix} - \dots \\ &= -n(a^{n+1} - \beta^{n+1}) + \left(\frac{b}{c} + 1\right)(a^n - \beta^n) + \left(\frac{b^2}{c^2} + 1\right)(a^{n-1} - \beta^{n-1}) + \dots \end{aligned}$$

Multiplying this now by  $(-)^{n+1}c^n/(a - \beta)$  and substituting  $K_n$  for  $(-c)^n(a^{n+1} - \beta^{n+1})/(a - \beta)$  we obtain



$$nK_n + (b+c)K_{n-1} - (b^2+c^2)K_{n-2} + \dots$$

as was desired.

Since  $s = c(1-\alpha)(1-\beta)$  the result (XVIII) may also be written in the more symmetrical form

$$M(K_n) = \frac{(-c)^n}{(\gamma-\alpha)(\gamma-\beta)(\beta-\alpha)} \begin{vmatrix} 1 & \alpha^{n+1} & \alpha + \alpha^2 + \dots + \alpha^n \\ 1 & \beta^{n+1} & \beta + \beta^2 + \dots + \beta^n \\ 1 & \gamma^{n+1} & \gamma + \gamma^2 + \dots + \gamma^n \end{vmatrix}$$

where  $\alpha, \beta, \gamma$  are the roots of the equation

$$cx^2 - (c-a)x^2 + (b-a)x - b = 0;$$

and, noting that the coefficients of this equation are the non-unit elements of the determinant

$$\begin{vmatrix} 1 & b-a & -b & . & . & . & . & . & . & . \\ 1 & a-c & b-a & -b & . & . & . & . & . & . \\ 1 & c & a-c & b-a & -b & . & . & . & . & . \\ 1 & . & c & a-c & b-a & . & . & . & . & . \\ 1 & . & . & c & a-c & . & . & . & . & . \\ . & . & . & . & . & . & . & . & . & . \end{vmatrix}_n$$

which is another form of  $M(K_n)$ , we have at once suggested the problem of evaluating the determinant

$$\begin{vmatrix} 1 & c & d & . & . & . & . & . & . & . \\ 1 & b & c & d & . & . & . & . & . & . \\ 1 & a & b & c & d & . & . & . & . & . \\ 1 & . & a & b & c & . & . & . & . & . \\ 1 & . & . & a & b & . & . & . & . & . \\ . & . & . & . & . & . & . & . & . & . \end{vmatrix}_n$$

in terms of the roots of the equation

$$ax^3 + bx^2 + cx + d = 0.$$

After doing this, however, we should only have reached a simple case of a known theorem of wide generality.\*

\* *Vide* my Text-Book of Determinants, p. 173, § 127.

(Issued separately January 20, 1905.)

**Crystallographical Notes.**  
By **Hugh Marshall**, D.Sc., F.R.S.

(MS. received November 21, 1904. Read December 5, 1904.)

*I. Axes of Compound Symmetry of the Second Order.*

In recent years, since the more or less general adoption of the systematic classification of crystals under the thirty-two possible types of symmetry, it has become usual to dispense with the 'centre of symmetry' as one of the elements of crystal symmetry, and to adopt in its place the 'axis of compound symmetry of the second order.' The derivation of symmetrical directions from any given one by means of a compound axis of order  $n$  involves not

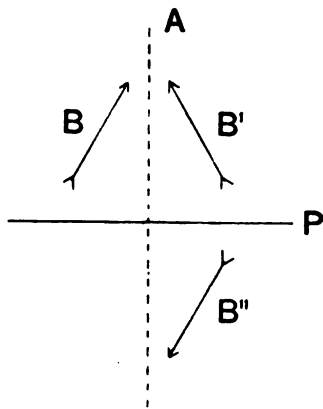


FIG. 1.

merely rotation through the angle  $2\pi/n$  about that axis, but also reflection in the plane at right angles to the axis. If the axis A (fig. 1) is of second order, then B, by rotation about A through  $\pi$ , would give B', but this by reflection in the normal plane P gives B'', and the latter (not B') is therefore symmetrical to B with reference to the compound axis A. But B'' is evidently parallel to B, and oppositely directed, so that it follows that in crystals possessing an axis of compound symmetry of the second order (or

of order  $n$  such that  $n$  is divisible by 2 but not by 4), opposite directions are equivalent to one another. So far as external shape is concerned, this involves, essentially, the occurrence of parallel faces on every form. These are the characters of centro-symmetrical bodies, however, so that at first sight it appears as if the symmetry might be referred indifferently either to the compound axis or to the centre. There is, unfortunately, one grave objection to the former method which seems to be generally overlooked. In all other cases an axis of symmetry is some perfectly definite direction in the crystal, and the number of axes is never large—not exceeding six of any one order, even in the most symmetrical classes. An axis of *compound* symmetry of the *second* \* order, however, is not a definite direction in the crystal, and every centro-symmetrical crystal possesses not one such axis, but an infinitude of them, because any direction whatsoever may be chosen as the axis without affecting the final result. It is therefore much better to avoid this lack of definitiveness in the expression ‘axis of symmetry’ by giving up the use of the ‘axis of compound symmetry of the second order,’ and restoring the ‘centre of symmetry’ to its former position.

## II. *The Classification of Trigonal and Hexagonal Crystals.*

For teaching and ordinary crystallographical purposes, the classification of crystals is largely a matter of practical convenience; questions of structure or arrangement of crystal molecules may be entirely overlooked in this connection. Bearing this in mind, it is a matter of some importance that the crystal systems which resemble one another in possessing one principal axis of symmetry (the trigonal, tetragonal, and hexagonal systems) should be so arranged as to accentuate their similarities; by doing so it becomes, for students beginning the subject, a much easier matter to appreciate and remember the various classes (nineteen out of the total of thirty-two) included in these three systems.

The tetragonal system is defined quite sharply, and the seven classes belonging to it present no characters which would lead to

\* This does not apply to compound axes of higher order, because an axis of compound symmetry of order  $n$  is necessarily an axis of ordinary symmetry of order  $n/2$ .

the inclusion of them in any other group. We may therefore take the tetragonal system as a standard, and compare the trigonal and the hexagonal with it. The seven tetragonal classes and their characteristic symmetry are as follows:—

1. *Bi-sphenoidal class*.—One axis of compound tetragonal symmetry. (Representatives of this class are not actually known, however.)

2. *Pyramidal class*.—One axis of tetragonal symmetry.

3. *Trapezohedral class*.—One axis of tetragonal symmetry; two pairs of lateral axes of digonal symmetry.

4. *Scalenohedral class*.—One axis of compound tetragonal symmetry; one pair of lateral axes of digonal symmetry; one pair of planes of symmetry intersecting each other, normally, along the principal axis.

5. *Di-tetragonal pyramidal class*.—One axis of tetragonal symmetry; two pairs of planes of symmetry intersecting, all at equal angles, along the axis of symmetry.

6. *Tetragonal bi-pyramidal class*.—One axis of tetragonal symmetry; one plane of symmetry normal to the axis.

7. *Di-tetragonal bi-pyramidal class*.—One axis of tetragonal symmetry; two pairs of lateral axes of digonal symmetry, all equally inclined to one another; one principal plane of symmetry and two pairs of planes of symmetry, each plane normal to an axis of symmetry.

At first sight it might be expected that, corresponding to these, there would be possible seven classes in each of the other two systems, the only differences being those due to the lower or higher order of the principal axis of symmetry. So far as regards the classes in which the axis is not one of compound symmetry, this is the case; but not so when the symmetry is compound. Axes of compound symmetry of *even* order are possible, but axes of compound symmetry of *odd* order are not possible merely as such, therefore two classes must be lacking in the trigonal system. This is easily seen by a reference to the usual symmetry diagrams representing projections on a plane at right angles to the principal axis of symmetry.

Fig. 2 represents the case in which the only symmetry assumed is that of a trigonal axis of compound symmetry. An upper face,

1, on rotation through  $\frac{2\pi}{3}$  and reflection in the normal plane, would give a lower face, 2; and by repeating these operations an upper face, 3, would result. Repeating the rotation once more would bring the face back to its original position, but the ensuing reflection would give a new face, immediately below the first. It is therefore evident that in order to return to the original position, by repeating the operations characteristic of the symmetry, two complete revolutions are necessary, and this produces six faces, as shown in fig. 3—three above and three below. The diagram now

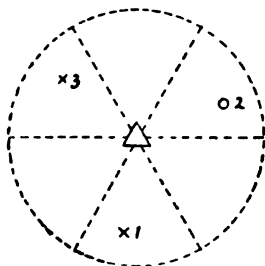


FIG. 2.

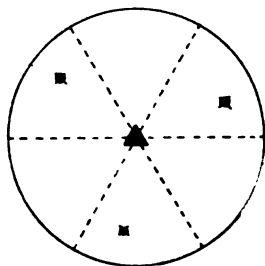


FIG. 3.

exhibits the higher symmetry of an ordinary trigonal axis combined with a plane of symmetry at right angles to it; but this is the symmetry of the trigonal bi-pyramidal class which corresponds to the tetragonal bi-pyramidal class. There can, therefore, be no trigonal class corresponding to the tetragonal bi-sphenoidal class.

Similarly, there can be no trigonal class corresponding to the di-tetragonal scalenohedral class, as a trigonal axis of compound symmetry combined with vertical planes of symmetry leads necessarily to the symmetry of the di-trigonal bi-pyramidal class. Each of these two classes—the trigonal bi-pyramidal and the di-trigonal bi-pyramidal—therefore represents, in a sense, two classes of the tetragonal system. It is noteworthy that not a single substance is known to crystallise in either of them; they are only 'theoretically possible.'

As hexagonal axes of compound symmetry are possible, there are the full number of seven classes possible in the hexagonal system. The classes corresponding to the tetragonal bi-sphenoidal and scalenohedral are the rhombohedral class and the *hexagonal* scalenohedral. Representatives of both are known, especially of the

latter; they are the classes of diopase and calcite respectively. Instead of being classed in the hexagonal system, however, they are generally placed in the trigonal system, the scalenohedral one being known as the di-trigonal scalenohedral class. The principal axis of symmetry is, of course, a simple trigonal axis, as well as one of hexagonal compound symmetry, but that is no sufficient reason for departing from the strictly systematic method of treatment. The result of doing so is to complicate matters for the student quite unnecessarily.

For the purpose of introducing the student to the various crystal classes, it would therefore appear to be best, after treating of the triclinic, the monoclinic, and the rhombic systems, to take up the tetragonal system, and, after this has been gone over, to proceed to the hexagonal and, lastly, the trigonal systems: the close analogies, allowing for the exceptions in the trigonal system as referred to above, render the study of the latter systems quite simple.

The various classes might then be tabulated as follows, the symmetry of the different systems being expressed in general terms:—

Symmetry.	Systems and Classes.		
	$n=4$ Tetragonal.	$n=6$ Hexagonal.	$n=3$ Trigonal.
$n$ -gonal axis of compound symmetry } $n$ -gonal axis	Bi-sphenoidal Pyramidal	Rhombohedral Pyramidal	... Pyramidal
$n$ -gonal axis; $n$ lateral axes (digonal) }	Trapezohedral	Trapezohedral	Trapezohedral
$n$ -gonal axis of compound symmetry; $n/2$ planes intersecting in axis; $n/2$ lateral axes (digonal) }	Scalenohedral	Scalenohedral	...
$n$ -gonal axis; $n$ planes intersecting in axis }	<i>Di</i> -tetragonal pyramidal	<i>Di</i> -hexagonal pyramidal	<i>Di</i> -trigonal pyramidal
$n$ -gonal axis; one plane normal to axis }	Bi-pyramidal	Bi-pyramidal	Bi-pyramidal
$n$ -gonal axis; $n$ lateral axes (digonal); plane normal to each axis }	<i>Di</i> -tetragonal bi-pyramidal	<i>Di</i> -hexagonal bi-pyramidal	<i>Di</i> -trigonal bi-pyramidal

When considered in this way the trigonal and hexagonal systems are referred to the Bravais axes, using the appropriate symbols. It is important, however, that students should be made acquainted with the mode of referring certain crystals to rhombohedral axes, with Miller's original symbols; therefore those classes for which such axes can be adopted should subsequently be brought together into a rhombohedral system by themselves. The classes to which this is applicable are those, belonging to the trigonal and hexagonal systems, which do not possess elements of symmetry higher than those pertaining to a (geometrical) rhombohedron. Consequently, all classes possessing a simple hexagonal axis, and also those which possess a principal plane of symmetry, are excluded from the rhombohedral system, which therefore includes—

The trigonal pyramidal class  
 „ „ trapezohedral class  
 „ di-trigonal pyramidal class  
 „ hexagonal rhombohedral class  
 „ „ scalenohedral class

The above list contains all the represented classes which are usually included in the trigonal system, and doubtless this is the principal reason why two classes which are, strictly speaking, hexagonal, are generally placed in the trigonal system. It appears to me, however, that considerable advantage is obtained by first deducing the trigonal and hexagonal classes in a strictly systematic manner, and, after the student has become acquainted with them, introducing the use of rhombohedral axes as an alternative method of dealing with a certain group, represented in both of the preceding systems, before passing on to the cubic system.

*(Issued separately February 1, 1905.)*

**The Effect of Simultaneous Removal of Thymus and Spleen in young Guinea-pigs.** By D. Noël Paton and Alexander Goodall. (*From the Laboratory of the Royal College of Physicians, Edinburgh.*)

(Read December 5, 1904.)

We have already shown that removal of the spleen (1) or of the thymus (2) has very little effect on the animal economy. Since the spleen and thymus together comprise the largest amount of lymphoid tissue in the body of young animals, it would appear not improbable that although removal of either of these organs causes no marked disturbance, their simultaneous extirpation might be expected to give rise to some more manifest change. Friedleben (3) states that, while in his series of experiments no dog died of removal of the thymus, and that the removal of the spleen in young dogs does not influence the course of life, the simultaneous removal of the thymus and spleen causes a marked deterioration of blood formation, and leads to death.

Since his experiments were made without aseptic precautions, and since his results may therefore have been due to sepsis, it appeared desirable to repeat these observations on young guinea-pigs, in which animals removal of the thymus and of the spleen separately has been found by us to cause no disturbance of importance.

In the following series of observations D. Noël Paton is responsible for the operations, which were performed under full anaesthesia. The animals invariably recovered rapidly. There was never suppuration, or any evident discomfort to the animal.

The observations on the blood were made by A. Goodall.

*Experiment I.*—On 9th April two female guinea-pigs were brought under observation. A. weighed 200 grms and B. 160 grms.

A. had thymus and spleen removed:—thymus .3 gm., spleen .18 gm.



On 25th April A. weighed 330 grms. and B. 230 grms. A. had 11,200 and B. 7800 leucocytes per c.cm. Both were killed on 5th October. A. weighed 870 grms. The thymus was completely gone. A small piece of splenic tissue weighing .67 gm. was found. B. weighed 550 grms. Thymus .55 gm., spleen .93.

*Experiment II.*—A guinea-pig weighing 260 grms. had thymus (.32 gm.) and spleen (.18 gm.) removed on 25th April. On 2nd May it weighed 290 grms. and had 6800 leucocytes. On 12th May it had 6600 leucocytes. On 2nd June it weighed 410 grms. and had 13,000 leucocytes. It became pregnant, and aborted on 26th July, giving birth to three young, weighing in all 123 grms. It was killed the same day. The thymus was completely removed, while a small scrap of spleen was found.

*Experiment III.*—Two female guinea-pigs had thymus and spleen removed on 2nd May.

A. weighed	220 grms.	.	Thymus	.275	Spleen	.345
B. „	280 „	.	„	.280	„	.405
On 12th May	A. = 280	with	1200	leucocytes.		
„	B. = 310	„	8800	„		
26th	A. = 355	„	5000	„		
„	B. = 385	„	7200	„		
2nd June	A. = 370					
„	B. = 400					

Both were killed on 6th June. Removal of thymus and spleen was complete.

The number of leucocytes compared with that of normal animals of the same age showed the same slight diminution that we have noticed after removal of the thymus alone, but, as in the case of removal of thymus only, this leucopenia does not persist after the animal has attained the age of three months.

Differential counts of the leucocytes showed no departure from physiological limits.

We conclude that simultaneous removal of the thymus and spleen in the young guinea-pig in no way interferes with nutrition, blood formation, growth and development of the animal.

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- (2) " " " " xxxi., 1904, p. 49.
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(*Issued separately February 1, 1905.*)

**Networks of the Plane in Absolute Geometry.** By  
**Duncan M. Y. Sommerville, M.A., B.Sc.,** University of  
 St Andrews. *Communicated by Professor P. R. SCOTT LANG.*

(Read December 19, 1904.)

(*Abstract.*)

The problem to divide the plane, without overlapping, into a network of regular polygons with the same length of side, has been completely worked out for the three geometries for the case in which the polygons are all of the same kind. The resulting networks are called *regular*.

On the Elliptic plane there are five regular networks. These correspond to the five regular polyhedra in ordinary space. On the Euclidean plane there are three, consisting respectively of triangles, squares, and hexagons. On the Hyperbolic plane there exist an infinite number.

To investigate the extension of this problem to the case where the polygons are of different kinds, *i.e.* to find the *semi-regular* networks, I consider first how the space about a point can be exactly filled with regular polygons. I take the three geometries separately.

**I. THE EUCLIDEAN PLANE.**—The angle of a regular polygon is definite. If there are  $p_1$   $n_1$ -gons,  $p_2$   $n_2$ -gons, etc. at a point, the condition that the sum of the angles at the point is  $360^\circ$  leads to an indeterminate equation which may be denoted by  $\Delta = 0$ ,  $\Delta$  being an integral function of the  $n$ 's and  $p$ 's. The solutions of this equation in integers give the possible combinations of polygons. Of these there are 17. I call them the "kinds of angles." They are divided into three Classes according to the number of kinds of polygons involved. The development of some of the kinds of angles leads to impossible combinations of polygons. Rejecting these, there are left 11, involving triangles (T), squares (S), hexagons (H), octagons (O), and dodecagons (D). They may be denoted as follows :—

Class A. 1.  $T_6$ . 2.  $S_4$ . 3.  $H_3$ .

Class B. 4.  $T_3S_2$ . 5.  $T_2H_2$ . 6.  $T_4H$ . 7.  $TD_2$ . 8.  $SO_2$ . 9.\*

Class C. 10.  $TS_2H$ . 11.  $T_2SD$ . 12.  $SHD$ .

Out of these all the semi-regular networks must be built up. I distinguish *types* of networks according to the kinds of angles of which they are composed. If there is only one kind of angle the type is called *simple*, otherwise it is *composite*. The types are classified into Groups according to the kinds of polygons which are involved, and the groups into Classes according to the number of kinds of polygons. There are four classes. Class A. consists of the regular networks.

The simple types are first considered. There are four unique types,  $T_4H$ ,  $TD_2$ ,  $SO_2$ , and  $SHD$ .  $T_3S_2$  admits of an infinite number of varieties of the simple type. In  $T_2H_2$  two distinct varieties can be recognised, an infinite number of varieties being obtained as mixtures of the two. With  $TS_2H$  there are three distinct varieties with an infinite number of mixtures.  $T_2SD$  does not admit of a simple type, nor, of course, does Class D.

The composite types in general admit of infinite variation. In any group a composite type corresponds to a possible combination of the kinds of angles contained in the group. Thus in the group of triangles and squares there are the three angles 1, 2, 4, and the composite types 1, 4; 2, 4; 1, 2, 4; the combination 1, 2 being impossible. The method of investigating these is chiefly experimental, and consists in testing the various combinations. It is easily seen, however, that certain combinations are impossible. For example,  $H_3$  must be accompanied by  $T_2H_2$  in order that the gap of  $120^\circ$  may be filled up. The following are the numbers of composite types in the various groups:

B. I. (T, S) 3; II. (T, H) 8. C. I. (T, S, H) 47; II. (T, S, D) 10.  
D. (T, S, H, D)  $169 + \dagger$

II. THE ELLIPTIC PLANE.—Here we get a relation of the form  $\Delta > 0$ , and by giving positive integral values to  $\Delta$  an infinite number of kinds of angles are found. Only a few of these, however, can be developed. For example, if there are at a point

\* No. 9 is 2 pentagons and 1 decagon, but this is not a developable angle.

† I have not exhausted all the composite types in this class. There cannot be more than 222.

an  $n_1$ -gon, an  $n_2$ -gon, and an  $n_3$ -gon,  $n_1$ ,  $n_2$ , and  $n_3$  must all be even, for the  $n_1$ -gon must be surrounded alternately with  $n_2$ -gons and  $n_3$ -gons. With the angles which remain there are thirteen simple types, two with two varieties each and one with five, and two infinite series of simple types, one corresponding to right prisms on a regular polygonal base, the other with triangles instead of quadrilaterals.

Of composite types it is probable that none exist, if we make the condition that the angle of a regular polygon must be less than  $180^\circ$ . When a polygon occurs in a particular combination its angle is thereby determined, and if it occurs in another combination its angle must be the same, which is not in general the case.

III. THE HYPERBOLIC PLANE.—The number of simple types here is infinite. For example, one  $n$ -gon and two  $2m$ -gons at a point determine a simple hyperbolic network for all values of  $n$  and  $m$  for which the network is neither Euclidean nor Elliptic.

As regards the composite types, the same considerations hold here as in the case of the spherical networks.

(*Issued separately February 1, 1905.*)

**A Specimen of the Salmon in transition from the Smolt to the Grilse Stage.** By W. L. Calderwood. (With Two Plates.)

(Read December 19, 1904.)

In October of this year (1904) there came into my hands a very interesting specimen of a young salmon. In round terms, the fish is 1 pound in weight and nearly 14 inches long.

Up to the present time very little is known of the life history of the salmon during the transition from the stage of the smolt leaving the river, a fish of about 3 ounces, and that of the grilse returning to the river for the first time, a fish of 3, 6, or 9 pounds in weight.

A great deal of speculation has arisen as to the length of time occupied in this change, and most of the earlier writers have upheld the view that three or four months is sufficient, or, in other words, that the smolt of May or June is the grilse which appears in the summer of the same year. This view was mainly based, I believe, upon results which it was held had been obtained by marking the fish by the mutilation or removal of the adipose fin. But since the adipose fin grows again to a greater or less extent, a considerable amount of uncertainty in recognising the recaptures was inevitable; and I may add that recent observations made in Devonshire by the instructions of the Duke of Bedford, in which the marking was carried on in precisely the same manner, have been held to show that the grilse do not come back the same season, or within four months or so of the seaward smolt migration. All the recaptured grilse obtained in the Tavy were caught in the succeeding season. If any still remained in the sea and ascended during the second season succeeding, they would probably be unrecognisable. Further, the few smolts which have been recaptured after being marked by the attachment of a foreign body of some sort—I refer to those of the Early Tweed Experiments—have been got as grilse in the summer of the year after that in which they were marked.

The specimen (Pl. I.) now exhibited throws some light upon the question of rate of growth at the period between smolt and grilse. It is an Irish fish, and was taken on a small fly on 25th August of this year (1904). It was caught by Mr W. N. Milne when angling a quarter of a mile above tide reach in the river Galway. It was therefore taken at the season when grilse proper are commonly found to be several pounds in weight, and when, if the old observers were correct, the fish could not have weighed, as it does, only  $15\frac{1}{2}$  ounces. It is more than a smolt, is evidently a quite young fish, and cannot fairly be called a grilse. It has attained, I believe, about a third of the growth of the grilse, as this stage is commonly recognised, and requires another year of sea feeding to accomplish the transition. I am not aware of any similar specimen existing in this country, if we except a few that have been artificially reared, and, as smolts, transferred to salt water aquaria or sea ponds. In this way Dahl in Norway has reared examples up to 31.5 cm.; and recently in Scotland a sea pond at the mouth of the Spey, belonging to the Duke of Richmond and Gordon, has produced rather larger examples. I am able to show one of these, which is 33 cm., or almost the size of the Galway fish (Pl. II.).

I have heard of two occasions on which fish approaching the stage of the Galway fish have, in the wild state, been caught in Scotland. In other cases which have been brought to my notice the identification is uncertain. A specimen weighing  $\frac{3}{4}$  pound was, Mr S. Gurney Buxton informs me, caught by him when spinning with natural sand eel in the Kyle of Tongue in 1886; and two fish, each weighing  $\frac{3}{4}$  pound, were reported to me by the late Mr Anderson, salmon tacksman in the Forth district, as having been taken by his father in 1863, he himself being present, in the Dundas net which used to be fished between Hopetoun and Queensferry. The fish were not preserved, or, so far as I can find, identified scientifically, but the reports are, I consider, worthy of record, my informants being in each case men with long experience in salmon fishing.

Dahl, in his valuable report of inquiries into the early stages of the sea trout and salmon,<sup>1</sup> refers to three young salmon which

<sup>1</sup> *Ørret og Unglaks*, Christiania, 1902.

were sent to him by mackerel fishers near Oksø. Those measured 43, 36, and 17.5 cm. Two other specimens he found in the Zoological Collection of Bergen University, which, though undescribed, are believed by Professor Collett to have been found amongst young mackerel in the Christiania fish market. They measure 23.5 and 28 cm.

Dahl's special netting in Norwegian fjords and some special netting which I have carried on in Scotland have as yet produced only negative results. Sea trout can easily be obtained in all stages at and near the mouths of rivers, but it is clear that on entering salt water the salmon smolt separates himself from the sea trout, and has a habitat in the sea which has not yet been discovered.

The particulars of measurement, etc. respecting the Galway fish are given below. They are those most approved by the British Museum authorities for the purpose of identifying the species of salmonidæ. I may add that I have already submitted the specimen to Mr Boulenger in London, and that he and his colleague Mr C. T. Regan, who made a separate identification, agree that the fish is a salmon.

The measurements are given in millimetres.

- |   |                       |
|---|-----------------------|
| 1. Sex, . . . . .   | ♂                     |
| 2. Length to centre of caudal fin, . . . . .  | 350                   |
| 3. Weight, . . . . .  | 15½ ounces            |
| 4. Length of head from end of snout to posterior border of gill cover, . . . . .  | 77                    |
| 5. Length of head to anterior border of eye, . . . . .  | 21                    |
| 6. Diameter of eye, . . . . .   | 11                    |
| 7. Length of mouth from end of snout to posterior border of maxillary bone, . . . . .   | 35                    |
| 8. Length of caudal peduncle, measured in a straight line from base of last ray of anal fin to base of lowermost ray of caudal fin, . . . . . | 47                    |
| 9. Least depth of caudal peduncle, . . . . .  | 29                    |
| 10. Length of longest ray of anal fin, . . . . .  | 39                    |
| 11. Shape of posterior border of tail, . . . . .  | Fully notched         |
| 12. Number of scales, counting from posterior extremity of base of adipose fin downwards and forwards to lateral line, . . . . .              | 12                    |
| 13. Number of gill-rakers, . . . . .  | 7 + 6 (gills damaged) |
| 14. Presence or absence of black spots below the lateral line in the region of the 'shoulder' . . . . .                                       | Present.              |

In general appearance (Pl. I.) the fish has to my eye certain



sea trout characteristics. Owing to the more or less familiar appearance of artificially reared specimens, a series of which I show in Pl. II., one is prepared for the presence of spots below the lateral line (although in the largest specimen which I show, and which has been twelve months in a sea water pond, spots are less conspicuous), but the rather noticeable breadth of the caudal peduncle in the Galway specimen is certainly not in keeping with the shapely specimens which can be reared artificially, as it is opposed to the characteristics of young salmon, as insisted upon and figured by Dahl in Norway.

The measurement of the caudal peduncle is contained in the length of the fish only  $11\frac{2}{3}$  times.

In a Fochabers smolt retained in fresh water till three years old similar measurements give 13·6 times. In the Fochabers smolt placed for a year in a sea pond the measurements give 15·2 times.

In a small Beaulieu grilse of 1 lb.  $15\frac{1}{2}$  oz. similar measurements give 15 times.

In a small Tay grilse of 2 lbs.  $\frac{1}{2}$  oz. the measurements give 15·9 times.

These two grilse are exceptionally small, and have been preserved by me on this account. Without any doubt the caudal peduncle is broad, but on inquiry I am informed by Mr Milne, who has had a wide professional experience as a salmon fisher both in Scotland and Ireland, that the fish of the Galway river "are thicker above the tail than the East of Scotland grilse. They are rougher altogether, fins and tail larger in proportion." In spite, however, of this unusual depth of caudal peduncle, the number of scales, counted forwards and downwards from the posterior margin of the adipose fin to the lateral line, is on each side 12. This, in my view, is by far the most reliable test by which to distinguish between salmon and sea trout, the former having almost invariably 11 or 12, the latter having almost invariably 14 or 15 scales in the line indicated. The present specimen, therefore, in spite of its sea-trout-like caudal peduncle, has the salmon number in the matter of scales. Mr Milne reports that on capture the scales came off very freely. This accounts for the rather patchy appearance of the side in the photograph of the fish which accompanies

this paper. The smolt, as is well known, has this characteristic, as also has the newly run grilse and the spring salmon. In other words, when the salmon is found in a very silvery condition, at a time remote from the season of its spawning, the scales are very deciduous. Grilse and salmon, in a more or less gravid condition, after a stay in fresh water, do not show this peculiarity, the scales being apparently enclosed firmly in the skin pockets.

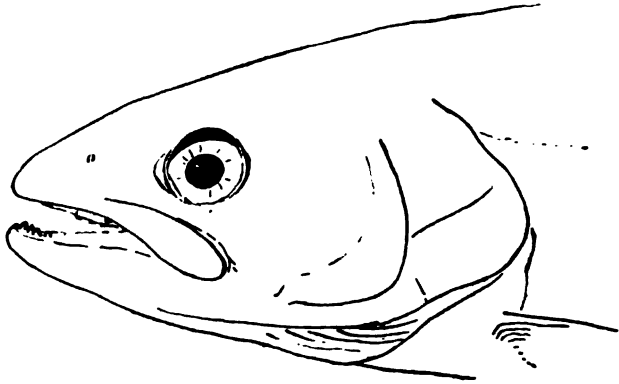
A number of scales from the fish have been examined by my friend Mr H. W. Johnston, Strathtay, who has recently made a special study of salmon scales, and is more able than I am to deal with the question of age and growth as shown by scales.

From notes he has kindly sent me, it appears that in his opinion this little salmon has attained the age of rather less than two and a half years, and that fully two years have been spent in fresh water. Mr Johnston writes—"The area of the fresh-water scale growth is larger than is usual in Tay fish, and corresponds more to that of hand-fed smolts from a hatchery." I am informed that no hatchery exists in the Galway district. It is possible, however, that the conditions of feeding may vary greatly in different localities. "In the early part of the third year," that is, when the fish is two years old and has reached the migratory stage, "there is slightly improved growth, owing perhaps to (a) tidal feeding or (b) increased temperature, followed immediately by probably continuous sea feeding, and corresponding growth of comparatively brief duration, resembling from half to three quarters of that generally shown by a grilse in its *first* summer in the sea. There is no trace of river feeding after the sea growth."

I am therefore inclined to the view that the presence of the fish in the Galway river, a quarter of a mile above tide reach, is not indicative of any habit which the salmon at this stage develops. The presence of the fish in fresh water at this stage I am inclined to regard as exceptional, or at least unusual. The specimen is a male, with genitalia quite undeveloped. The stomach is empty and contracted, but the pyloric appendages are fairly well surrounded with fat. The vomer bone has the usual complement of teeth on the head, while on the shaft of the bone two pairs of teeth are still present. The dorsal and caudal fins are blackish

as in the grilse, and the caudal fin still carries several spots. The adipose fin is, like the dorsum of the fish, a dark steel colour. The fork in the tail fin is well marked. Measurements taken with the caudal fin unextended, as in the photograph, show that the lower lobe of the fin extends 2.5 cm. beyond the central part of the fin.

The length of the head is contained  $4\frac{1}{11}$  in the total length. The maxillary bone shows a condition midway between that notice-



able in the parr or smolt and that of the grilse or salmon. In the parr the posterior margin of the bone reaches to a point vertically below the centre of the eye. In the adult fish the maxillary bone is prolonged backwards to a point vertically below the posterior margin of the eyeball, or beyond the eye altogether. In the Galway fish the point at which the maxillary bone ends is vertically below the posterior margin of the pupil of the eye. This and the arrangement of the opercular bones will be seen from the accompanying outline drawing of the head.

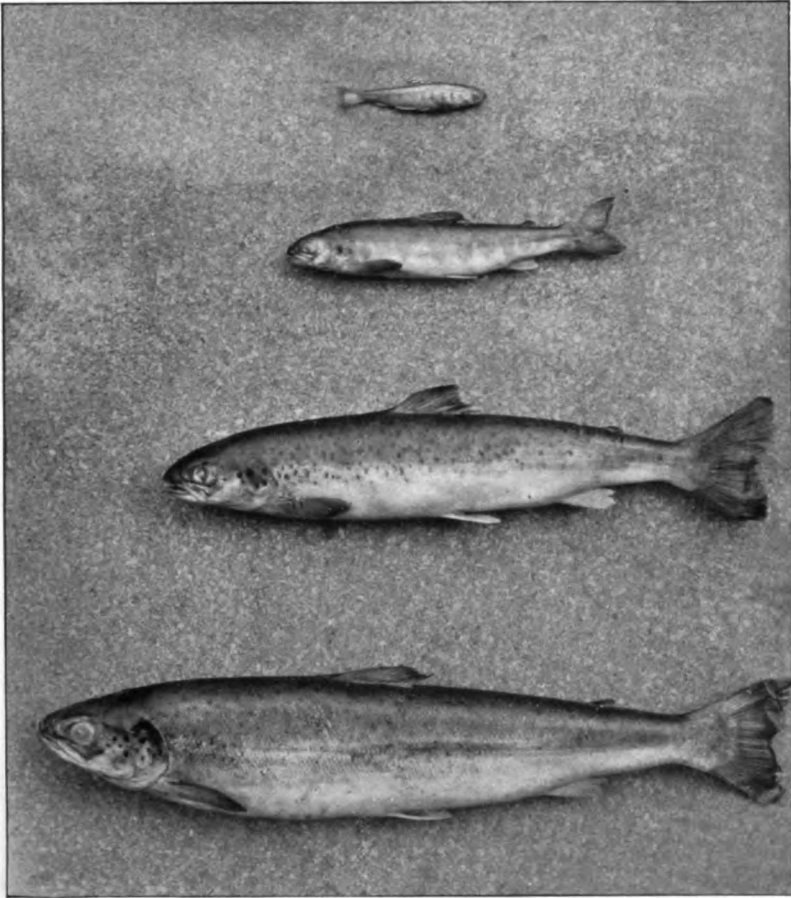
(Issued separately February 1, 1905.)



PLATE I.—The Galway R. specimen.

MR W. L. CALDERWOOD.





**PLATE II.**—Artificially reared salmon, one, two, and three years old. The largest fish is, like the fish shown immediately above it, three years old, but has been one year in a sea-water pond. It is 33·0 cm. long.

**MR W. L. CALDERWOOD.**



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[Continued on page iii of Cover.]

**A Comparative Study of the Lakes of Scotland and Denmark.** By Dr C. Wesenberg-Lund, of the Danish Fresh-water Biological Station, Frederiksdal, near K. Lyngby, Denmark. *Communicated by Sir JOHN MURRAY, K.C.B., F.R.S. (From the Danish Fresh-water Biological Laboratory, Frederiksdal.) (With Two Plates.)*

(MS. received January 13, 1905. Read January 23, 1905.)

#### INTRODUCTION.

In June 1904 I received an invitation from Sir John Murray to visit Scotland and spend three or four weeks in exploring the Scottish lakes, in order to make a comparison between them and the Danish lakes: he was of opinion that such a comparison of the lakes of a highland and a lowland country, which had hitherto not been attempted, would lead to some interesting results. The admirable bathymetrical and physical explorations carried on by Sir John Murray in Scotland, and more especially in Loch Ness, being far advanced, the question as to the scope of the biological observations called for consideration; so he desired me to indicate, from the impressions derived during my visit, my views as to the most useful lines of investigation that might be taken up with reference to the biology of the Scottish lakes. I was much interested in the task imposed upon me, and at the same time gratified at the prospect of assisting in the design of the biological explorations in the lakes of a foreign country; and as it was of the greatest significance to me to learn the nature of alpine lakes, I immediately accepted the invitation. I spent three weeks in Scotland,—the first two at Fort Augustus, on Loch Ness, and the third in Edinburgh. From Fort Augustus I explored the lakes of the Caledonian Canal, and thus became acquainted with alpine lakes; from Edinburgh I explored a few lowland lakes, especially Loch Leven. The steamer *Mermaid*, belonging to the Marine Biological Station at Millport, fully equipped for deep-sea work,

under the direction of Dr Gemmill, was sent into the Caledonian Canal, and many hauls were taken with the dredge and trawl, as well as with different kinds of tow-nets, in Lochs Lochy, Oich, and Ness, down to the greatest depths (500 and 750 feet).

Before entering on the subject of this paper, I beg to tender to Sir John Murray my most cordial thanks for his invitation and for his kindness to me during my stay in Scotland. As regards limnological explorations, Scotland was a few years ago a complete *terra incognita*, but when the work of the Lake Survey is completed there will undoubtedly be no other country in which the lakes have been better studied than in Scotland. On Loch Ness I learnt the methods employed in taking the temperature and other physical observations; and when the numerous observations and enormous mass of material have been worked out, I think that Loch Ness, as regards the bathymetrical and other physical conditions, will be one of the best explored lakes in the world—perhaps only equalled by the Lake of Geneva.

It has hitherto been difficult to give equal prominence to the physico-chemical investigations, on the one hand, and the biological investigations, on the other, in the study of the lakes in different countries, owing mainly to the lack of scientists versed in the different branches of limnology, and interested alike in these two great departments. The admirable explorations carried on by Professor F. A. Forel and his pupils show what excellent results may be obtained when the investigations are planned on a uniform basis. I trust that Sir John Murray and Mr Laurence Pullar will agree with me in expressing the hope that, on the completion of the bathymetrical and physical survey so admirably commenced by Sir John Murray, and continued at the joint expense of both gentlemen, the work may be still further carried on in such a manner as to utilise the results yielded as to the biological study of lakes. I am quite well aware, as will be seen from the following pages, that the study of organisms, and especially of the influence of organic life upon the general conditions of a lake and its environs, presents greater difficulties in alpine countries than in lowland countries. The problems presented by the local conditions of lakes can perhaps be better studied in Scotland than in any other country; and I sincerely hope that

the investigations relating to the extremely interesting plankton, the bottom-fauna, the Diatom flora of the shores, and the influence of water rich in humic acid upon fresh-water organisms, may be studied in accordance with the knowledge which has been gained of the life-conditions common to all organic life. It would be most unfortunate for the study of fresh-water and its organisms if, in a country where the knowledge of the life-conditions is so prominent, this knowledge should not be fully utilised.

During the last fifteen years I have spent most of my time in the study of our own lakes and their organic life, and I hope that my statements in the following condensed and brief account of the Danish lakes may prove reliable; time will show whether I have carried my generalisations too far. What I learnt regarding the Scottish lakes brought to light many differences between them and our own lakes; and I had occasion to make some observations which, if carried further, would have served as starting-points upon which to base my working theories. My knowledge of the Scottish lakes is, of course, very limited, but I hold it to be the duty of a scientist not only to make known the actual facts observed by him, but also his ideas as to the bearing of these facts. Strictly speaking, new ideas should be regarded not so much from the standpoint as to whether they may be right or wrong, but rather as to their value in the promotion of scientific knowledge; and I hope the following pages may contain ideas useful in some measure in future investigations.

## I.

### GENERAL REMARKS ON THE NATURAL CONDITIONS OF THE DANISH AND SCOTTISH LAKES.

#### A. *The Danish Lakes.*

My explorations have shown the most remarkable differences between the Danish and the larger Scottish lakes in nearly all particulars, which was to be expected, considering the wide divergence in the geological structure of the two countries. I would here merely point out that Denmark is a lowland country, the highest eminences not exceeding 500 to 550 feet above sea-level, and,

geologically speaking, it is of recent origin, being built up of very light and friable soil—mostly the moraines of those enormous glaciers which covered Denmark and the surrounding seas during the Ice Age. It is probable that lime strata of different geological ages occur nearly everywhere beneath the soil, rising in certain places to the surface, and in other places not far below the surface. The soil itself is commonly very rich in lime, which is washed out by the rivers and carried into the lakes. The rainfall is not great, only about 614 mm. (24 inches) per annum; and this, in conjunction with the lowness of the country and the friable soil, accounts for the fact that the rivers are all small—rarely more than about 50 feet in breadth, with level courses (falls being quite unknown), and transporting only a small quantity of water. The outflow of water from the rivers is greatest in spring after sudden thaws, and least in summer (especially in dry seasons) and in autumn, increasing considerably in November and December, with their abundant rainfall. As an example we may take the river Skern in Jutland, which in summer discharges at its outlet only about 500 cubic feet per second, while in spring it may discharge about 7500 cubic feet per second.

Denmark is now rather deficient in lakes, though at an earlier period they must have been more numerous. They are all very small, the largest covering an area of only about 40 square kilometres (about  $14\frac{1}{2}$  square miles), while the great majority are much smaller. Their depth is inconsiderable, as was to be expected in a low and flat country; exceptionally, depths of about 120 feet have been recorded, but the majority are only 40 to 60 feet in depth, while some of the largest lakes are in fact merely great pools, with a maximum depth of only 10 to 12 feet. Denmark is, on the whole, a flat country, with no deep depressions, and most of the lakes are roundish in outline, long and narrow lakes being rare; formerly the lakes were much more irregular, but owing to the silting up of the bays and shallower parts the shore-lines show very few sinuosities, though some of the larger lakes are very irregular.

The renewal of the water in the lakes goes on very slowly. As the amount of water carried into the lakes by rivers is always greatest in spring and slowly diminishes in summer, it will be understood that the level of the lakes is highest in spring and

lowest in August and September, the difference amounting to 2 or 3 feet in the two seasons. Hence it follows that in our shallow lakes the breadth of the beach increases in summer and autumn to the extent of several hundred feet, and in winter and spring the ice or the waves cover places over which one might walk dry-shod in summer.

The sides of the lakes are gently sloping; and the same remark applies to what the Germans term "uferbank," and the French and Swiss term "beine." The deeper parts of the lakes are floored by more or less level plains, the greatest depth being often found near the centre. Islands are not common, though both islands and banks occur. Owing to the small amount of detritus carried down by the rivers, deltas are usually inconspicuous, and well-marked banks at the embouchures of the rivers are rare.

Erosion by waves upon the shores is seldom conspicuous, as the force of the waves is broken in rolling over the shallow plains, often covered and bound together by vegetation. The wind-blown sides of the lakes (especially the east-south-east shores) are frequently sandy, or covered with stones and pebbles, while the west and north-west shores are often peaty. On the other hand, certain parts of the lake-shores show remarkable indications of erosion, and these are most conspicuous where the shores are covered with wood; here one may see trees with scars and rifts 2 to 3 feet from the ground, and often showing remarkably irregular forms. Further, one may find many overthrown trees and dead shrubs standing high upon their washed-out white roots. In the few cases where the shores rise precipitously from the water's edge marks of erosion are often found, and abundance of stones and pebbles washed down from the slopes above. This erosion, however, is to be ascribed rather to the action of ice than to that of waves. In spring, when the ice breaks up, it is often piled into heaps 2 to 4 feet high, in front of which one always finds a very conspicuous "end-moraine," consisting of gravel, stones, broken *Phragmites*, shells of mussels and *Limnæa*, and various drift-materials. It may be pushed 20 to 25 feet from the shore, and even—to the amazement of a naturalist—remain there from one year to another. The ground over which the ice has travelled will show, after the disappearance of the ice, a very



conspicuous "bottom-moraine," consisting of the shells of *Anodonta*, etc., which may be scattered over the ground in thousands; stones are polished, and the ice, striking against the trees, causes the rifts and wounds referred to above. Many trees, on the prominent points bordering the lakes, are killed by these heaps of ice, which are piled up year after year on the shores of our lakes (see fig. 1). We may also mention that the ice-slabs in spring often break great apertures in the closed stocks of *Phragmites* and *Scirpus*, detaching large patches of rhizomes a square metre (over a square yard) in extent and throwing them on shore; the ice may in the course of a few hours cover over a peaty shore with sand, or cover a sandy beach with peat-forming material.

With reference to the *temperature* of the Danish lakes, it is to be regretted that the observations are rather deficient. Still, it may be stated generally that the temperature of the water follows very closely the changes in the temperature of the air. Having exemplified this statement in my Plankton paper, I shall here only remark that the surface waters of our lakes are generally very warm in summer, often attaining a temperature as high as 23° C. (73° F.), and in hot summers the water may maintain a temperature of 20° to 23° C. (68° to 73° F.) for more than a month: it is very rarely that the surface temperature in summer falls below 16° C. (61° F.). Almost every winter most of the lakes are frozen over, though the length of the period during which they are ice-bound varies greatly in different years, but never exceeds more than about four months. The observations I have made show that the lakes are usually frozen for one or two months, generally from about 15th January to 15th March, but exceptionally they may not be frozen at all. As we have often a short spell of frost in November and December, followed by thaw, usually followed again by the customary long period of frost in January to March, the smaller lakes may have two ice-bound periods—a short one in December and a longer one in January to March, but in the larger and deeper lakes only the latter period prevails. As most of our lakes resemble each other as regards height above the sea, latitude, depth, and form of basin, it will be understood that they vary little in temperature. It may generally be said that the deeper and narrower the lake and the steeper the

sides, the more will the temperature of the water differ from that of the air; it will take a longer time to freeze over, but will remain ice-bound much longer than a shallow lake, and the temperature of the water will rise more slowly, never attaining the high temperature of the shallower lake. Only in one Danish lake (Haldsö) does the temperature of the water appear to differ essentially from that of the other lakes. Thus the mean temperature of the air in July 1901 was extremely high,—19·9° C. (67·8° F.), and the surface temperature of all our lakes except Haldsö was 21° to 23° C. (70° to 73° F.), while in Haldsö the temperature never exceeded 18° C. (64° F.); in the winter of 1901-2 the other lakes were ice-bound from 39 to 65 days, whereas Haldsö was only ice-bound for 35 days. It may be added that Haldsö is one of our deepest lakes (about 120 feet), and has more precipitous shores than any of the others.

The *transparency* of the water in our lakes is small, and varies regularly with the season of the year, being always greatest in spring, diminishing during the last days of April, and least in August. During the ice-bound period the water becomes much clearer, all the detritus and huge masses of phytoplankton being precipitated to the bottom.

The *colour* of the water in the Danish lakes in April, after the ice has broken up, is nearly always a bright blue, but this colour only continues till the beginning of May, when most of the lakes become of a yellowish-green colour, which continues to be the predominant colour till the frost sets in. In hot summers the surface is generally covered by a coating of "wasserblüthe," and then the colour changes to blue-green or green; in cold summers no "wasserblüthe" appears on the deeper and colder lakes.

As regards the chemical composition of the water, very few observations have as yet been made, but I hope this will soon be remedied.

#### B. *The Scottish Lakes.*

Comparing the natural conditions of the Danish lakes, as indicated in the foregoing pages, with those of the Scottish lakes, we shall find the greatest differences in nearly every detail. It must be borne in mind that geologically Scotland is a very old

country, for the most part built up of hard rocks. Nearly all the lakes belong to the Highlands, the highest mountain peaks attaining an elevation of more than 4000 feet above sea-level. It is unnecessary in this short paper to enter into the chemical composition of the rocks, but I think I am right in stating that, as compared with Denmark, lime generally plays a subordinate rôle in the chemical composition of the Scottish Highlands, and I am of opinion that the amount of lime washed out by rivers and carried into the lakes is nearly everywhere inconsiderable. The Scottish rivers, with their rapid currents, their sources high up in the mountains, their great eroding powers and waterfalls, are quite different from our little brooks. As far as I could gather from the members of the Lake Survey staff, there are no special seasons in which the rivers carry exceptional quantities of water into the lakes or into the sea. At different times of the year, though probably mostly in spring, the rivers after heavy rains become swollen, and after periods of drought they become low, but this rise and fall are not, to the same extent as in Denmark, restricted to certain seasons, and the suddenness with which the Scottish rivers come down in flood has no parallel with us.

These differences are closely connected with the wide divergence in the geological structure and climatological conditions of the two countries—the one a low country, with moderate rainfall; the other mountainous, with a heavy rainfall,\* the hilltops shrouded in mists, and the hills themselves clothed with peat or peat-mosses, which suck up the water like a sponge and feed the rivers. While Denmark has few lakes, Scotland has very many; and though generally of moderate size, many of them are much larger than the Danish lakes. The main difference is the great depth of the Scottish lakes, often exceeding 500 feet, and in one case (Loch Morar) exceeding 1000 feet, and they are nearly all long and narrow, none of the larger ones being circular, as is the case with many of the Danish lakes. Their narrow form facilitates the renewal of the water, and the sudden flooding of the rivers at nearly all seasons of the year causes rapid changes in the level of the lakes. With these phenomena we have hardly anything to

\* In the western Highlands the rainfall is five to seven times greater than in Denmark.

compare in Denmark ; and the regular, but slow and comparatively slight, rise in the level of our lakes in spring, and the fall in summer, have, generally speaking, as far as my information goes, no, or only a slight, counterpart in Scotland.

I consider the steep and precipitous shores of the Scottish lakes to be one of their most prominent characters (see figs. 3 and 4). From what I know (unfortunately only from the literature) of the alpine lakes of Switzerland, the Scottish lakes generally surpass them in this respect ; in Scotland the mountains often descend almost vertically into the lakes, and depths of 500 feet may be found only a few yards from shore. Consequently there may be no beach, or only a very narrow one, and I suppose the same may be said of the "beine."

The Scottish lakes resemble the Danish ones in that the greatest depth is generally found near the centre of the lake, and that banks and well-marked deep holes are rare. Owing to the large amount of detritus carried down by the rivers, banks are common opposite the mouths of the rivers, and well-defined delta formations seem to be a frequent feature. Where beaches occur, they very often consist of pebbles and cobblestones, which during storms are agitated by the waves ; the erosion of the waves upon the rocks is often very conspicuous.

With reference to the *temperature* of the water, the excellent observations of the Lake Survey show great differences between the Scottish and Danish lakes. The larger Highland lakes are never ice-bound, the surface temperature in winter being generally from 5° to 7° C. (41° to 45° F.). On the other hand, the maximum temperature in the same lakes in summer will never (I am informed) exceed 18° C. (64° F.). It will thus be seen that, while the surface temperature of the Danish lakes varies from a little below zero to 23° or 25° C. (73° or 77° F.), the amplitude of the variation in the surface temperature of the larger Highland lakes is only from about 5° - 7° C. (41° - 45° F.) to 18° C. (64° F.).

The *transparency* of the water in the Scottish lakes is, strange to say, not much greater than in the Danish lakes. Forel's disc in Loch Ness disappears at 24 or 25 feet, and I am told that in other Scottish lakes the transparency is even less. This fact is very remarkable, and, so far as I know, at variance with what one might

expect from the observations in other alpine lakes. As regards the transparency, there is still this great difference between the lakes of the two countries—that in the Danish lakes the transparency is always and everywhere greatest in spring, and slowly diminishes with increasing temperature, whereas in the Scottish lakes, according to my informants, the transparency is nearly constant all the year round, but may at any season, especially after heavy rains, be suddenly greatly reduced.

As to the *colour* of the water, another great difference between Danish and Scottish lakes is to be noted; for while the colour of our lakes undergoes a regular alternation, strictly dependent on the different seasons, the colour of the Scottish lakes varies very little at all seasons. The larger Scottish lakes never show that turbid yellowish-green colour so characteristic of nearly all our lakes from May to November, nor the deep blue colour displayed by our lakes in April, neither are they covered with “wasserblüthe” caused by blue-green *Algæ*. The water in all the Scottish lakes seems to be very clear, but has a yellowish-brown colour, quite different from the blue colour of most of the alpine lakes in Switzerland, which are also characterised by the great transparency of the water: in both the Swiss and Danish lakes the transparency is much greater in winter and spring than in summer and autumn.

As will be noted in a later chapter, the colouring of the Danish waters is due to the plankton; the colouring of the Scottish lakes has quite a different origin. It must be remembered that the Scottish rivers nearly always drain through peaty bogs and the moss-covered sloping sides of the mountains, and only very rarely, and for a short period of the year, do the rivers obtain their water directly from the snow. I am told that the layer of peat on the mountains may attain a thickness of 1 to 2 feet, and it will therefore be easily understood that the water of the Scottish lakes must necessarily be peaty and very rich in humic acid, and this fact accounts for their yellow-brown colour and very slight transparency. In my opinion we have here the most striking and the most interesting difference between the alpine lakes of Switzerland, with their clear blue water, their rivers fed directly from the vast eternal glaciers, and the alpine lakes of Scotland, with their yellowish-brown water, their rivers rising in bogs and travers-

ing the moss-covered precipitous mountain sides. I have been told that Loch Morar, the deepest of all Scottish lakes, has the clearest water, Forel's disc being visible at a depth of 44 feet; and in this connection it is of great interest to note the fact that the rocks along the shores of Loch Morar and all over the drainage area are not covered with peat and mosses, but are for the most part quite bare. As far as I know, we have no particularly peaty water in any of our larger lakes, though it is, of course, a very common feature in the smaller lakes surrounded by peat, and whose floors are covered by peaty mud, many of which are quite artificial, being due to the digging of peat.

The foregoing remarks refer only to the character of the Danish and Scottish lakes, but I feel convinced that many of the facts stated are common to lakes belonging respectively to the great Central European plain and to alpine countries. As traits common to all the first-mentioned lakes, I would specially point to their shallowness, their gently sloping shores, their roundish outline, the high temperature of the surface water in summer and the freezing over in winter, the ice-erosion on the shores, the small transparency, and the yellow or yellow-green colour of the water in summer, due to the huge plankton-masses. Differences may be looked for with regard to the chemical composition of the water and bottom-mud, owing to the varying chemical composition of the soil in different countries; I anticipate that further investigations will prove that the large amount of lime carried by streams into our lakes is one of the most characteristic peculiarities of the Danish lakes. On the other hand, I am of opinion that the features mentioned in connection with the Scottish lakes are common to alpine lakes in general. Especially would I call attention to their great depth and long and narrow form, their precipitous shores, the sudden flooding of the rivers and the rapid changes in the level of the lakes, and the slight amplitude in the annual variation of the surface temperature. Peculiar to the Scottish lakes are the small transparency and yellowish-brown colour of the water, to which may undoubtedly be added the large amount of humic acid. These peculiarities may be traced to, and are closely connected with, the strongly-marked climatological and geological conditions common to the whole country.

## II.

THE ORGANISMS, AND THEIR RELATIONS TO THE DIFFERENT  
LIFE-CONDITIONS, IN THE DANISH AND SCOTTISH LAKES.

It will be easily understood that the life-conditions offered to fresh-water organisms differ widely in the Danish and Scottish lakes respectively, and that there are great differences between the vegetable and animal life in each case. Generally speaking, it may be said that the low temperature and freezing over of the Danish lakes in winter have not hindered the immigration of most of the fresh-water organisms distributed over the entire temperate region of Europe, while the usually high summer temperature, due to the shallowness of our lakes, is undoubtedly one of the main factors to which we must ascribe the extremely rich organic life, both as to the number of species and of individuals, characteristic of our own as well as most of the lakes in the northern part of the Central European plain. We shall now consider the vegetable and animal life in the Danish and Scottish lakes respectively, according to the three main regions that may be recognised in every lake, viz., the Littoral region, the Pelagic region, and the Abyssal region. As far as possible, we shall endeavour to indicate how the different characters of the lakes in the two countries have produced great differences in their associations of animals and plants.

A. *The Danish Lakes.*

1. *The Littoral Region.*—Owing to the gently sloping shores, the smooth wash of the waves, the sandy beaches, often covered with decaying vegetable matter, and the high summer temperature of the coastal waters, most of our lakes are bordered by dense and luxurious bands of vegetation, which in shallow bays may attain a considerable width, merging imperceptibly into the vegetation of the adjoining land. Thus our lakes are often in certain parts bordered by humid meadows, which in winter and spring are covered by ice or water, while in hot summers they may be quite dry, so that it is frequently difficult to say where the land ends and the lake begins.

As the depth of the lakes increases very regularly from the shore outward, and as the different plants are on the whole limited to certain depths, the vegetation arranges itself in zones (see fig. 2). For details I may refer to the excellent work of Professor Warming (1895),\* and will here restrict myself to the following remarks. In most of our larger lakes we have a narrower or wider shore-zone, mainly characterised by *Scirpus lacustris* and *Phragmites communis*. Further out we shall find zones of *Potamogeton lucens* and *perfoliatus* and some other plants, especially *Batrachium*, *Myriophyllum*, and *Ceratophyllum*. Still further out, by dredging on the bottom, we find a zone formed of Characea and some *Fontinalis*, which extend to a depth of 8 or 9 metres (25 or 30 feet), and beyond this limit we usually find no higher plants. With the exception perhaps of the outer border of the Characea zone, all these zones of vegetation die off in winter, leaving only their resting organs, their rhizomes, etc., on the bottom. The higher plants are in summer nearly always covered by a very rich epiphytic vegetation of blue-green Algæ, Diatoms, and green Algæ. On the windward side of the lakes the vegetation is, of course, less abundant, and here we often find beaches of stones and gravel, without any higher plants. The stones themselves in all our lakes are in winter covered with a rich brown coating of Diatoms, which in summer often disappears, but in several lakes its place is taken by a crust of greyish lime deposited from the blue-green Algæ, as in many of the Swiss lakes.

The plentiful vegetation is the home of an abundant and remarkable animal life: of the higher invertebrate groups we specially notice many larvæ of insects,—of Diptera, Phryganidæ, Ephemeriidæ, Libellulidæ, certain Coleoptera, and a few Neuroptera (*Sialis*); of the Crustacea there are Amphipoda (*Gammarus pulex*, *Pallusiella quadrispinosa*), *Asellus*, Daphnids and Copepods in great abundance; besides many Rhabdocœla, a few Dendrocœla and Oligochœta, very many Rotifera, a very rich Protozoan fauna, and a great many snails and mussels. Beneath the stones we also find numerous organisms, especially Phryganidæ, Ephemeriidæ, and Planaria, and on the upper sides of the stones snails are nearly everywhere found.

\* This work will shortly appear in English, translated by Professor Balfour.



A stranger unacquainted with our lakes on reading these lines might form the impression that the shores of our lakes were for the most part inhabited by the common fresh-water fauna to be found in every shallow pond with rich vegetation. This impression would be incorrect, for a closer examination would certainly show that, while many species are common to ponds and to the vegetation zone of the lakes, still it would appear that most of the Phryganidæ, Libellulidæ, Ephemeridæ, some of the Crustacea, many Planaria, some Oligochæta and Rotifera are quite peculiar to the lake-shores, and rarely appear in ponds. Further, it would seem that several species of snails common to the ponds and the shores of the larger lakes are represented in the lakes by special forms differing from those found in ponds. I cannot in this short paper discuss this point in greater detail, but will content myself by remarking that the fauna of the littoral zone of our lakes is on the whole very different from that of our ditches and ponds.

In winter the greater part of this rich fauna disappears. In November and December many of the organisms, especially snails and some insect larvæ, migrate into deeper water before the shores are covered with ice; other organisms, for instance many insect larvæ, go ashore and burrow holes in the ground, while a great many other species, especially Daphnids and Rotifers, make resting organs\* and, by means of them, survive the freezing in the ice. Still, there are numerous organisms which appear to live in winter beneath the ice as they do in summer in water having a temperature of about 25° C. (77° F.); for example, Planaria, Phryganidæ, Amphipoda, *Nepheleis*, etc.

2. *The Pelagic Region.*—With regard to the plankton, I may refer to my Plankton studies (1904), and restrict myself in this place to the following brief remarks. Our lakes are nearly always extremely rich in plankton, so much so that throughout the greater part of the year—from April to December—it affects the colour and transparency of the water, and is doubtless one of the main factors in determining the varying amounts of oxygen and carbonic acid dissolved in the water. It will thus be understood that the plankton of our lakes—its composition and its abundance—must necessarily greatly influence the other organisms in the lakes.

\* Hibernating buds, ehippia, or eggs.

With regard to the fresh-water plankton of the world, two remarkable characteristics should be noted. Firstly, that generally speaking it seems to be very homogeneous from pole to pole. The plankton of the Greenland lakes is similar to that of the North African lakes, only certain groups of plankton-Algæ being apparently rare, or perhaps entirely absent, near the pole. From this general rule we know only a few exceptions, especially as regards some Crustacea. Very many species are common to the fresh-waters of Iceland and those of North Italy. Secondly, that the central domain for the full development of all fresh-water plankton is apparently in the temperate zone, and not in the tropics. If these characteristics hold good, the fresh-water plankton differs essentially in both these respects from all other associations of organisms in the sea or on the land. These two points cannot, however, be held as proved until the tropical fresh-water plankton has been fully explored; and I consider it extremely desirable that one of the great nations having possessions in the tropics should despatch an expedition with the main object of investigating the tropical fresh-water plankton.

The plankton of our lakes does not differ, on the whole, from that to be found in any of the larger lakes in the northern parts of the Central European plateau, but, as Forel justly remarks, all these lakes scarcely merit the name. In most of these comparatively shallow lakes the plankton is characterised by a great development of *Melosira* and blue-green Algæ, by the presence of *Bosmina coregoni*, and perhaps by the occurrence of the only two common species of the Copepod genus *Diatomus*, *D. gracilis* and *graciloides*. The *Cyclotella* and *Oscillatoria*, so characteristic of alpine lakes, are usually rare, and often entirely absent, while certain species of *Diatomus* and some peculiar species of Chlorophyceæ, common in southern alpine lakes, have never been found in the Central European plateau.

The plankton of the Danish lakes differs somewhat perhaps from that of the lakes in the surrounding lowland countries in the rich development of the Diatom genus *Stephanodiscus*, of the blue-green Alga genus *Lyngbya*, and of the Conferva *Tribonema bombycinum*. As our lakes are usually shallow and the littoral zone very extensive, it will be readily understood that many

organisms from the Littoral region find their way to the central parts of the Pelagic region, and that many of the forms peculiar to the central parts of the larger ponds, especially many Chlorophyceae, may be carried by the rivers into the Pelagic region of the lakes; still, the majority of these organisms never play a prominent part in the composition of the plankton.

Out of about 150 plankton organisms which have been recognised in the Danish lakes, very few appear in such vast quantities as to give the plankton a monotonous character, or to influence the life-conditions of the lake during the greater part of the year. Among these are *Melosira crenulata* and *granulata*, *Asterionella gracillima*, *Aphanizomenon flos aquae*, *Ceratium hirundinella*, the species of *Diaptomus*, *Daphnella brachyura*, *Hyalolaphnia cucullata*, *Bosmina coregoni*, and *Leptodora kindtii*. From April to December there are in almost every lake, besides the above-mentioned species, others which may predominate during a shorter period. Among these I would mention *Fragilaria crotonensis*, and other Diatoms, *Coelosphaerium kützianum*, *Polycystis*, and a few other Cyanophyceae, a very few Chlorophyceae and Protozoa, some Rotifera, and of Crustacea especially *Cyclops oithonoides*, *Bosmina longirostris*, and *Daphnia hyalina*. Besides those organisms whose home is in the littoral zone, or in the central parts of ponds, which are always rare in the Pelagic region of the lakes, there are other rare forms found in this region that only appear in the summer months. These organisms, as far as I know, have apparently reached or nearly reached their northern limit with us; this applies especially to some Rotifers, Cyanophyceae, etc.

Though the life-conditions in our lakes do not vary very much, still there is a good deal of difference in the plankton of the different lakes: this refers mostly to the Diatoms and Cyanophyceae, those two great groups of organisms which, in my opinion, affect more than any other the common life-conditions of the lake. As a general rule, we may say that these two groups rarely attain their maximum development in the same lake or simultaneously. Most of the fresh-water Diatoms reach their highest development at a relatively low temperature (below 12° or 10° C. = 54° or 50° F.) and in the colder of our lakes; on the other hand, the Cyano-

phycea—except *Oscillatoria*—usually reach their greatest development at the highest summer temperature (between 19° and 23° C. = 66° and 73° F.) and in the warmer lakes. Accordingly, we find a great development of Diatoms in the cold northern lakes as well as in the southern alpine lakes, and an almost complete absence of the Cyanophyceæ in both these localities, the only exception being the *Oscillatoria* and partly *Anabæna flos aquæ*, which are both common in the alpine lakes of Switzerland. In our colder lakes a great development of Diatoms occurs in the last days of April, when the lakes are ice-free, and continues till June; then a great development of *Ceratium hirundinella* sets in, and in September a second development of Diatoms appears. On the other hand, in our shallower and warmer lakes the great development of Diatoms is discontinued a little earlier, then the Cyanophyceæ appear, and often predominate throughout the rest of the year; still, in these lakes also the development of *Ceratium* and a second development of Diatoms occur, but rarely to such an extent as in the deeper and colder lakes.

The deep cold lakes rarely present the phenomenon of “wasserblüthe”; and if it appear, it is only for a short time in June, caused by *Anabæna flos aquæ*. As the chromatophores of the Diatoms, as well as those of *Ceratium hirundinella*, are a yellowish-green, the colour of the water in nearly all our colder lakes is also yellow-green. The colour of the water in the shallower and warmer lakes is in spring also yellow-green, owing to the first great development of Diatoms; but when the maximum development of the Cyanophyceæ sets in, the colour becomes more bluish-green, owing to the blue-green colour of the Cyanophyceæ cells, and the surface of the water on calm days is covered by a thick layer of “wasserblüthe”: in August and September, when the great development of Cyanophyceæ is intermixed with that of *Ceratium hirundinella* and the second development of Diatoms, the water in these lakes changes somewhat towards yellow-green.

As a rule, we may say that the colour of the water in our lakes throughout the greater part of the year is determined by the colour of the plankton-organisms, especially by that of the chromatophores of the Diatoms and of the Cyanophyceæ. Only in April, immediately after the breaking up of the ice, is the quantity

of plankton so insignificant that one may decide as to the original colour of our fresh-waters: to determine the colour of the water at any other season it would be necessary to filter it. This probably applies to all the lakes of the Central European plain, but, as far as I am aware, the colour of the water in all these lakes has never been determined from filtered samples; and if so, it must be remembered that such determinations may have been greatly influenced by a foreign factor, viz., the colour of the chromatophores of the plankton-organisms in greatest profusion at the time. Until the colour of the water has been determined from filtered samples, we cannot, in my opinion, directly compare the colour of the water in these lakes with that of the water in the alpine lakes, in which the amount of plankton, especially in the surface layers of water, is altogether insignificant as compared with our lakes.

In winter a great many plankton-organisms totally disappear from the water: this is the case with certain species which in more southern latitudes occur all the year round (*Ceratium hirundinella*), but with us they produce their resting organs in autumn and disappear. I think it is very probable that those resting organs which, before winter sets in, are precipitated to the bottom in the deepest parts of the lakes, never rise to the surface again, but sooner or later die off, not finding the necessary conditions for germination. In my opinion, the plankton-organisms of the following year are mostly derived from those resting organs which were deposited in shallower water nearer the shore, where the waves during the spring gales sweep the bottom, carrying away the resting organs and scattering them over the lake. In our lakes the resting organs of the different plankton-organisms are most plentiful in April and May, after the heavy storms; and I have shown in my Plankton paper that many plankton-organisms are in May most abundant near shore, and that their distribution over the whole lake does not take place till later in the year.

I may here remark that very probably—though direct observation is very difficult—various plankton-organisms, especially certain Diatoms (*Tabellaria fenestrata*, *Diatoma elongatum*), may have alternately a fixed littoral stage and a free-swimming or free-floating pelagic stage, and these two stages may be restricted to certain seasons, the shape of the colonies in the littoral stage

(chains) being different from that of the colonies in the pelagic stage (stars). These remarks may prove of some importance, inasmuch as future investigations may show how littoral organisms become transformed into pelagic organisms, and as they support the hypothesis, now commonly adopted, that the fresh-water plankton is derived from the common microscopical littoral and bottom fauna and flora, very few organisms having immigrated directly from the sea.

As a character common to all our plankton, I may add that the seasonal variations of the organisms are very conspicuous, and more especially those of *Daphnia* (*Hyalodaphnia*) *cucullata*, *Bosmina coregoni*, *Asplanchna priodonta*, *Ceratium hirundinella*, *Asterionella gracillima*, *Melosira crenulata*, *Fragilaria crotonensis*, *Pediastrum*, etc. I shall return to the investigations on this point after treating of the plankton of the Scottish lakes.

I may point out that the vivid red colour characteristic of many Crustacea in other countries is not with us very conspicuous; several Copepoda do, as a rule, in winter, change from yellowish-white into a deep red colour.

With regard to the *vertical distribution* of the plankton, I only venture to remark that the greatest profusion of plankton is to be found in the upper layers of water. Like most of the naturalists who have studied the plankton in the lakes of the northern part of Central Europe, I have not been able to distinguish any vertical wanderings at different hours of the day; I venture to think that such wanderings are rather inconspicuous with us, but further investigations with improved appliances will be necessary to decide this question.

3. *The Abyssal Region.*—In my paper on the bottom-exploration of the Danish lakes (1901), I have pointed out that there are reasons for fixing the limit between the Littoral region and the Abyssal region at about 9 or 10 metres (30 or 35 feet). In speaking of our shallow lakes we cannot, of course, strictly use the term "abyssal region"; the principal conditions laid down by Forel regarding this region, especially the uniformity of all the life-conditions, are never fully realised in the Danish lakes. Still, it may be maintained that we can speak of an abyssal fauna, inasmuch as this is quite different from the littoral fauna, and apparently similar to the abyssal fauna in deep alpine lakes.

Outside the 9-metre (30-feet) contour we find no plants except certain species of *Oscillatoria* and bottom Diatoms; all higher vegetation is limited within this contour, and the slight transparency of the water is probably the main factor in determining this distribution. The majority of the snails also are limited by this contour, only *Valvata piscinalis* extending a little beyond; the pulmonary snails never cross this boundary, the abyssal *Limnæa* known from the Lake of Geneva being entirely absent from our lakes. The same contour also marks the boundary of nearly all the insect larvæ, only *Sialis* penetrating so far.

The deep bottom of our lakes is chiefly inhabited by *Pisidium*, the larvæ of *Chironomus* and *Tanyptus*, the Oligochæte *Psammoryctes fossor*, Ostracoda (*Limnocythere relicta* and some species of *Candona*), a few Planaria (*Plagiostoma lemani*), etc. The Daphnidæ and the very minute forms of animal life, such as Protozoa, have not been studied. On the whole, I think I may say that our abyssal fauna, though imperfectly known, is still undoubtedly very like the abyssal fauna of the Swiss lakes.

#### B. The Scottish Lakes.

In comparing the associations of fresh-water organisms in the Scottish lakes with those in the Danish lakes, we shall find in nearly every particular the greatest contrast.

1. *The Littoral Region.*—With regard to this region we may, in the first place, point out that the belt of vegetation which nearly always surrounds our lakes is often entirely absent from the larger Scottish alpine lakes, due to the precipitous or stone-covered shores, devoid of deposits of sand or decaying vegetable matter: even river deltas and other sandy flats are often almost bare of vegetation, partly, I suppose, because of the powerful erosion of the waves, and partly because the sudden changes in the level of the lakes is destructive to the amphibial plants. In the smaller and shallower lakes, for instance Loch Oich, in which we find some higher vegetation along the shore, this vegetation is not arranged in those elegant zones so characteristic of the Danish lakes.

As far as I am aware, the stones have never been found clothed with blue-green Algæ; but when I had the opportunity

of examining them, they were always covered with coatings, often thick, of Diatoms. I found such coatings at the height of summer, at a time when they never occur in our country, owing to the high temperature of the water; and from what I have observed in Danish lakes, I suppose they may possibly also occur in the Scottish lakes in winter. I visited Scotland at an extremely dry season of the year, when the rivers were only moderately supplied with water and the level of the water in the lakes singularly low; on the stony shores and precipitous mountain sides I often found a more or less distinct whitish band, which on closer examination proved to be due to dried Diatoms and other plants, the upper stripe being identical with high-water mark. We find a similar band on the stones in our lakes in May, but later on the Diatoms are often covered over by blue-green Algæ.

The animal life in the littoral region of the larger Highland lochs seemed to me, compared with the Danish lakes, to be extremely poor, but it must be kept in mind that I only examined the lakes during the season when the animal life of the littoral zone is almost everywhere at a minimum; most of those insects which, as larvæ, live in the littoral zone, disappear in summer as full-grown insects, though they may possibly have been numerous at an earlier season. Still, the animal life whose home is in the vegetation zone, living or resting on the vegetation, is rare compared with our lakes. When I had occasion to examine the vegetation, for example in Loch Oich, I always found it extremely void of the epiphytic organisms so characteristic of most of our submerged fresh-water plants; still, in rapid streams the leaves of *Potamogeton natans* often constitute a support for a great many larvæ of *Chironomus*, *Phryganea*, and of the family *Hydroptilidæ* (I suppose *Hydroptila maclachlani*), as well as for *Stylaria proboscidea* and *Sida crystallina*. Along the shores of the lakes I observed very little of the extremely rich winged insect life, consisting of swarms of imagos of all those insects which as larvæ abound in the water, and which both in bright sunshine and on calm moonlight nights are characteristic of our lakes, and highly attractive to the student. Beneath the stones I only found a few Planarians and one or



two species of Ephemeridæ and Phryganidæ. In the small bays of the lakes, where the bottom may be seen well covered with vegetation, for example *Littorella*, *Myriophyllum*, etc., we often find a comparatively rich fauna of insect larvæ, Cladocera, and Rotifera; in such localities the fauna in these respects does not seem to be much inferior to that found in the Danish lakes.

Between the littoral fauna of the Highland lakes as compared with that of the Danish lakes, the main difference appears to be in the Mollusca, which play a very prominent part in our lakes, but are extremely rare in the Highland lakes. Along the shores of Loch Ness and the other lochs of the Caledonian Canal I never found a single mollusc shell, and on exploring the shores only a few living specimens of *Limnæa ovata* and *Planorbis contortus* were to be found. Still, I expect that a closer examination by a malacologist would reveal more species, and that in the shallow water, in depths of 15 or 20 feet, species of *Valvata*, *Bithynia*, etc. would be found, but all the larger species of *Planorbis* and *Limnæa* seem to be entirely wanting. At any rate the molluscan life in the Highland lakes generally is so extremely poor that it cannot possibly influence the general conditions of life in the zone in which it is principally found.

This special difference between the Scottish and Danish lakes I consider to be due to the large amount of humic acid in the water of the Scottish lakes, to the total absence of lime in the water and on the floor of these lakes, to the absence of all lime-secreting Algæ and of lime-encrusted blue-green Algæ covering the stones, of Characea, etc., on which the snails in our lakes principally feed, and to the, generally speaking, extremely poor vegetation. That the first-mentioned is the principal cause is evident from the fact that even in lakes rich in vegetation the molluscan life is greatly inferior to that in the Danish lakes.

2. *The Pelagic Region.*—The investigations of Mr James Murray,\* assistant on the Lake Survey staff, of Messrs West, and my own cursory examinations, have shown that there is a great resemblance, and at the same time a great difference, between the plankton of the Scottish and of the Danish lakes. Nearly all the common plankton-organisms of the Scottish lakes also occur in the

\* I desire to express my thanks to Mr Murray for information supplied to me.

Danish lakes, while, on the other hand, many forms found in the Danish lakes have not hitherto been observed in the Scottish lakes. I may here give a short account of the commoner plankton-forms, based on the investigations above referred to.

The Cyanophyceæ play an altogether inferior part in the composition of the plankton in the larger Highland lakes, the only rather common forms being *Anabaena flos aquæ* and *Coelosphaerium nægelianum*. With regard to *Lyngbya* and *Oscillatoria* further explorations may give information, but as Mr Murray often speaks of "filamentous Algæ in abundance" they are probably common.

Of the Diatoms, it may be pointed out that *Melosira*, as in many other mountain lakes, seems to be relatively rare, and never forms those huge masses of plankton found in the Danish and other lowland lakes. *Stephanodiscus astræa* has not yet been observed as a plankton-organism; and *Cyclotella*, which has often been considered as characteristic of alpine lakes, was not so common as might have been expected, yet I suppose that closer examination at other seasons may prove that it is abundant; *Fragilaria crotonensis* also seems to be rare in the Scottish lakes. The commonest forms are:—*Asterionella gracillima*, *Tabellaria fenestrata*, var. *asterionelloides*, *T. flocculosa* (in chains), and a remarkably large number of bottom and shore Diatoms (Naviculoidæ and Surirelloidæ).

With regard to the Chlorophyceæ, Chodat has observed that nearly all the small forms belonging to the Euchlorophyceæ are warm-water plants, having their home in small ponds, the water of which is rich in disintegrated organic matter; in the Pelagic region of the greater lakes they are nearly all rare, and must be considered as merely chance visitors, introduced by streams and rivers, soon finding their graves in the Pelagic region of the lakes: to this rule we find only a few, but very peculiar, exceptions. A study of the Chlorophyceæ in the Danish lakes has shown this view of Chodat's to be quite correct: as all our lakes are shallow, and the water in summer very warm, they should, according to Chodat, be extremely rich in Chlorophyceæ, and this is exactly the case. With regard to the Scottish lakes, we find some very remarkable features. All the Euchlorophyceæ seem, from my own observations, to be rare, and Messrs West (1904, p. 554) have

also pointed out the very "remarkable scarcity of many of the free-swimming Protococcoideæ." Still, it must be remembered that these organisms, judging from Chodat's investigations, could by no means be expected, all these plants, except *Sphaerocystis* and a few others, being extremely rare in lakes: the numerous species recorded by Lemmermann, Bruno Schröder, and others, all inhabit the shallower and warmer lakes (see West, p. 554).

On the other hand, the explorations of Messrs West have proved that the Desmidiaceæ play a most prominent and remarkable part in the Pelagic region of a considerable number of the larger lakes. The authors state that the Scottish phytoplankton "is unique in the abundance of its Desmids. No known plankton can compare with it in the richness and diversity of the Desmid flora." In the present state of our knowledge, I consider the presence of these numerous Desmids to be one of the most peculiar traits in the composition of the plankton of the Scottish lakes. As far as I know, very few of them have hitherto been recorded in the Pelagic region of any of the greater European lakes, and their common occurrence is quite the reverse of what might have been expected from Chodat's and my own observations. In the other European lakes only two species, viz., *Staurastrum gracile* and *S. paradoxum*, are common. The manner in which, I think, we may endeavour to account for their frequent occurrence will be referred to after the plankton groups have been treated of.

As the Flagellata, Heliozoa, and Infusoria have not hitherto been specially studied, and I myself have had no opportunity of visiting the lakes in the season during which many of the Flagellata and Infusoria are generally most abundant, I do not venture to deal with these groups in detail, but restrict myself to the following remarks. I have found *Dinobryum* in all the lakes explored, and in a few instances also species of the genera *Mallomonas* and *Gymnodinium*. *Ceratium hirundinella* seems to be common, and the frequent occurrence of *Clathrulina* is very remarkable—usually empty shells, for only once, I think, did I see a living animal. As far as one may judge from the investigations of Mr James Murray, it seems that the plankton Rotifers are quite similar to those in other countries, but the absence of *Mastigocerra capucina* is remarkable.

With regard to the geographical distribution, none of the plankton-organisms present points of so much interest as the Crustacea. It has been mentioned that the plankton-organisms have an extremely wide distribution, and may be regarded as cosmopolitan; most of the exceptions belong to the Copepoda and Cladocera. Steuer (1901) was the first to draw attention to the fact that the Diaptomidæ and some of the plankton Cladocera seem to have well-marked areas of distribution. Steuer's views have been corroborated and modified or enlarged by the excellent investigations of Ekman (1904) in the northern part of Sweden; Ekman's results fully accord in all the main points with my observations in the Danish lakes (1904). Having referred to these papers, I shall here restrict myself to those points having special reference to the fauna of the Scottish and Danish lakes.

It may be regarded as a fact that there exists a peculiar association of Arctic plankton Crustacea, mainly restricted to the Arctic or North European lakes. This association is characterised by the common occurrence of *Holopedium gibberum*, *Daphnia hyalina*, *Bosmina obtusirostris*, *Bythotrephes longimanus*, *Diaptomus laciniatus*, the genus *Heterocope* (perhaps), and certain other species of Copepoda. *Bosmina coregoni*, as well as *B. longirostris* and *Hyalodaphnia cucullata*, are almost entirely absent; these are the particular forms, besides several others, especially *Diaptomus gracilis* and *D. graciloides*, *Daphnella brachyura*, *Leptodora kindtii*, which constitute the huge masses of zooplankton in the Central European plains. Of the sub-arctic association, some of the species, especially *Diaptomus laciniatus*, are also common in the alpine lakes of Switzerland and other lakes in the Central European alpine zone, but most of them (*Holopedium gibberum*, *Bosmina obtusirostris*) are never, or only exceptionally, found there. It seems to me that these southern alpine lakes are mostly inhabited by the same species which are characteristic of the Central European plains, and that the arctic elements are on the whole subordinate.

The following facts may be briefly stated, from the explorations of Mr James Murray, and the excellent papers of Mr Scourfield and Mr Scott quoted in the Bibliography, as well as from my own investigations:—

*Holopedium gibberum* is very common, and frequently “so

abundant that it chokes up the nets in a short time, and makes it impossible to get a fair proportion of the other animals present" (Murray, 1904A, p. 42); it may be added that the animals are extremely large.

Of the genus *Daphnia* the common species is *Daphnia hyalina* in different varieties (*lacustris*, *galeata*, etc.). *D. (Hyalodaphnia) cucullata* is very rare, and only found in one locality (a lowland lake). *Bosmina coregoni* is almost entirely absent, and it seems as though the genus *Bosmina* were only represented by one species, *B. obtusirostris*. *Bythotrephes longimanus* occurs generally in the Highland lakes, and is extremely large. *Leptodora kindtii* and *Daphnella brachyura* are common in nearly all the lakes. Of the Copepoda, the Diaptomidæ are represented by *D. gracilis*, the commonest species, as well as by *D. laciniatus*, *D. laticeps*, and the peculiar *D. wierzejskii*; of the *Cyclops*, *C. strenuus* is the main form.

As will readily be seen, the common occurrence of *Leptodora kindtii* and *Daphnella brachyura* is the only feature that gives the otherwise almost entirely sub-arctic association of Scottish plankton Crustacea a more southern facies. Otherwise we may point to a very close connection between the associations of plankton Crustacea in the Scottish and the sub-arctic lakes—a connection much closer than that between the plankton Crustacea of the Scottish lakes and of the lakes of the Central European plain and of Switzerland. This result is only what might have been expected, considering the situation of the Scottish lakes and the geological structure of the country, but still it seems to me not without interest.

With regard to the other plankton-organisms, I shall only point out that *Corethra plumicornis* has been found by Mr James Murray as a plankton-organism in Loch Oich, and that different species of Hydrachnids are common in most of the lakes.

As regards the *quantity* of plankton in the Highland lakes, it can only be regarded as extremely poor when compared with that in the Danish lakes. It appears that the plankton in the larger Highland lakes affects the transparency or colour of the water only to a very slight extent, therefore the plankton can only slightly influence the general conditions of life for all the other organisms in these lakes. Only in small lakes has Mr James Murray

observed the transparency and the colour of the water to be influenced by the plankton. Further, I should think it is exceptional to find in the Highland lakes a single plankton-organism giving the entire plankton the uniform monotonous character frequently observed in our lakes due to *Melosira*, *Aphanizomenon*, and others. And it will be easily understood that the marked changes which almost invariably take place in our lakes when the great development of Diatoms ceases and the maximum development of the Cyanophyceae sets in are never so conspicuous in the Scottish lakes. Finally, I am inclined to think that many of the plankton-organisms in the Scottish lakes show a less marked maximum and minimum development than is the case in our lakes; and should further explorations confirm this supposition, the fact must be ascribed to the much lesser amplitude in the annual variation of temperature in the Highland lakes, where the water never attains those very low or very high temperatures at which life in an active form, owing to the structure of the organisms, becomes impossible; the organisms may therefore not be forced to form resting organs, but may remain in the layers of water as free swimmers.

According to the observations of Mr James Murray and myself, the *seasonal variations* of the plankton-organisms are never so conspicuous in the Scottish as in the Danish lakes. I have pointed out (1900) that in several very different plankton-organisms the longitudinal axis is simultaneously lengthened during summer and shortened during winter, and that the formation of all the various structures (spines, floating apparatus, etc.) considered necessary to enable the organism to float are most distinctly visible in summer-forms and summer-individuals. I also pointed out that the explanation must be looked for in the varying external conditions, which, so to speak, compel the organisms to vary regularly in accordance therewith. I ascribed these variations mainly to the annual changes in the specific gravity of the water, occasioned by the regular annual fluctuations in the temperature, starting from the supposition that if the velocity of the falling motion of the plankton-organisms be not the same at all seasons, the organisms must, in order to exist as such during the season when the velocity of the falling motion is

invariably greatest, of necessity be capable of developing properties tending to reduce the velocity of the falling motion. Knowing, as we now do, that the spherical form in all bodies has the quickest falling velocity, and seeing that so many organisms, with the increasing temperature and decreasing specific gravity of the water, often obviously became lengthened in form, the thought struck me that very probably the seasonal variations in the specific gravity of the water were the main factor in determining the seasonal variations in the shape of the organisms. Subsequently Ostwald (1903) pointed out that the lengthening of the longitudinal axis with increase of temperature, and the shortening of the longitudinal axis with decrease of temperature, cannot be attributed solely to the variations in the specific gravity of the water consequent upon the rising temperature in spring and falling temperature in autumn; he draws attention to the fact that the oscillations in the specific gravity of the water with a temperature varying from 0° to 24° C. (32° to 75° F.) are too slight to account for these great seasonal variations in the form of the organisms. He agrees with me in taking it for granted that these seasonal variations in so many very different plankton-organisms can only be due to variations in the external conditions, but he believes them to be due to the varying viscosity of the water, which, like the specific gravity, is dependent on the oscillations in the temperature of the water, while the variations in viscosity are far more perceptible than the variations in specific gravity. I think that Ostwald's modification of my views is quite correct.

The conclusions arrived at by Ostwald and myself have been greatly strengthened by recent observations. It is evident that if the seasonal variations are occasioned by variations in the external conditions, in accordance with the variations in the temperature of the water, these seasonal variations must be most conspicuous in those lakes having the most pronounced annual variations in temperature. It has now been shown that the seasonal variations are very conspicuous in a great many lakes in Denmark, South Sweden, and North Germany, and many interesting facts regarding these seasonal variations, the sinking of short-spined individuals during the early summer months, etc. (Max Voigt, 1904, p. 113), have been brought to light by the explorers in these countries,

with their shallow and, in summer, warm lakes. On the other hand, from Ekman's explorations in the northern alpine lakes in the Sarek, we know that the seasonal variations are by no means so conspicuous there as in the more southerly parts of Sweden. Brehm (1902) arrives at a similar result as regards the Daphnids in the Achensee, North Tyrol. From my own observations in the Icelandic lakes (which will be published shortly), I know that the seasonal variations are there extremely inconspicuous, and now the investigation of the Scottish lakes has given the same result. From these facts, and in accordance with the observations of Ostwald and myself, we may conclude that the seasonal variations are of slight importance in arctic and cold alpine lakes, while, as might have been expected, they are conspicuous in the lakes of the Central European plain, characterised by the great annual variations in the temperature of the water. In this connection it will be seen how interesting a thorough exploration of the great tropical lakes would prove to be.

According to the published papers by the investigators of the alpine lakes and the lakes of the European plains, it may be considered as a general rule that many animals always display more vivid *colours* in the cold alpine lakes than in the warm lakes of the plains, and that the animals retain their bright colours in the alpine lakes throughout the year, whereas in the lakes of the plains the vivid colouring is only observed in winter when the temperature is low. Brehm supposes that the red colouring of alpine organisms is a means of protection against the cold, and gives good reasons for this supposition. The examination of the plankton in the Scottish lakes has now shown that the Crustacea, for instance *Daphnia hyalina*, *Diaptomus gracilis*, *Cyclops strenuus*, as in other alpine lakes, are frequently in summer of a deep red or deep blue colour; in my own country I have only seen these vivid colours in winter, and never in the summer months.

With regard to the *vertical distribution* of the plankton-organisms in the Scottish lakes we know very little, and further observations on this point are necessary. It is an interesting fact that what is known of the vertical movements of the plankton shows that these movements are very conspicuous in the alpine lakes, but inconspicuous, and often hardly traceable, in the lakes of the



plains. Seeing, however, that no thorough investigations have as yet been carried out on this point in the lakes of the plains, or the facts have not been sufficiently elucidated, I consider any discussion on this subject as rather premature. Mr James Murray has told me that at night a very great accumulation of plankton takes place in the surface waters of the Highland lakes, and we may therefore conclude that very conspicuous movements occur at different times of the day and night; in this particular also the plankton of the Highland lakes agrees with that of other alpine lakes.

Before leaving the plankton of the Scottish lakes I wish to draw attention to a very peculiar feature. The singular abundance of Desmids has been already mentioned, and needs an explanation. To suppose that the Scottish lakes should be the only known home of an entire plankton-flora of Desmids seems to me, at first sight, from my knowledge of fresh-water planktons, on the whole an odd idea. I presume that the occurrence of the Desmids in the plankton must be regarded in connection with the appearance of a good many other organisms in the Pelagic region of the lakes; for instance, *Polyphemus pediculus*, *Sida crystallina*, *Chydorus sphaericus*, *Clathrulina*, several Rotifers, and very many Diatoms of the sub-orders Naviculoideæ and Surirelloideæ. All these organisms may be considered as littoral forms, washed out by the waves from the precipitous hillsides, blown out by the wind from the few shallow bays, and carried out into the deeper part of the lakes by rivers and currents. Knowing that the original home of the Desmids is in peat-moors, and that the sloping sides of the hills in Scotland are almost everywhere covered with mosses, which are quite moist for the greater part of the year, and in many places all the year round, the thought immediately struck me that the plankton Desmids must have been originally derived from the hillsides, or from tarns and moors on the hilltops, and, associated with the littoral species above named, have been carried by the rivers out into the centre of the lakes. Later on, when I read the most interesting paper of Messrs West, I observed that, according to these gentlemen, the plankton Desmids of the Scottish lakes "are also known to us from the bogs and rocky pools of north-west Scotland and the Outer Hebrides" (1904, p. 553). Further, the authors report the very interesting fact

that "the majority of the species of *Staurastrum* and *Arthrodesmus* which occur in the plankton are remarkable for their long spines, or long processes with spinate apices. Even those species which are normally long-spined increase the length of their spines when in the plankton" (1904, p. 554).

From the results of these thorough explorations I think we may conclude, on the one hand, that the home of the plankton Desmids is in fact in the pools and moss-covered sides of the hills, from which the plankton-flora of the lakes is nowadays recruited, and, on the other hand, that some of those forms which, according to their primeval structure, were best adapted to plankton-life, are now in fact, under the new conditions, about to develop those processes (spines, etc.), common to very many exclusively plankton-organisms, that we always regard as a floating apparatus. The adoption of a pelagic life by the Desmids—a process really going on as regards so many species in the Scottish lakes—may be more easily understood when we remember that these lakes, unlike most other large lakes, offer one of those great life-conditions which so many of the Desmids seem to require, viz., peaty water rich in humic acid. What I have here set forth is, of course, only a theory, but one which may perhaps prove a starting-point for further investigations.

3. *The Abyssal Region.*—Our knowledge of the abyssal fauna of the Highland lakes is at the present time very deficient. Before my arrival in Scotland, Mr James Murray had been dredging a good deal, especially in Loch Ness. As mentioned in the Introduction, opportunities were afforded me for dredging in Loch Lochy, Loch Oich, and Loch Ness, and from a good steamer I used all the various apparatus employed in deep-sea trawling. I thus, of course, obtained some idea of the abyssal fauna in the lakes mentioned, but still I consider my impressions to be altogether insufficient, and the results at which I have arrived need in a great measure to be tested and corrected by further explorations.

The distance from shore at which the alluvial deposits settle on the bottom depends in the first instance, of course, upon the declivity of the shore. As the shores of Loch Lochy and Loch Ness are very precipitous, with depths of 300 to 500 feet only a few hundred yards from shore, I suppose that the alluvial deposits

settle on the bottom only at remarkably great depths. It is very difficult to dredge upon these almost vertical planes; and in the few instances where a dredging gave any result, I never got any finer alluvial deposits, but only stones and gravel, upon which I never found any sign of animal life. At the present moment we have no knowledge of the animal life on the precipitous sides of the lochs from 100 to about 300 feet, but I expect that further investigations will show that it is extremely poor. Mr James Murray has shown me samples from 300 feet in Loch Ness, containing many insect larvæ, especially Perlidæ, Coleoptera, and Ephemeridæ, as well as many Daphnidæ and Rotifera. In the dredgings in Loch Ness I never found these animals, and I conclude that, especially during the spring, they will be found to accumulate in the abyssal region. These forms must certainly be regarded as having fallen down the precipitous sides of the bordering hills, washed out by the waves, and carried out into deep water. Further, I think it quite probable that the rivers, especially after heavy rains, may be able to sweep away the river-fauna from the rocks and carry it out into the lakes so far from shore that it does not subside until depths exceeding 200 or 300 feet have been reached. Further observations may show whether this littoral fauna of the great depths will be starved out, or will be able to reach its primary home again.

I had hoped to find in the lakes of the Caledonian Canal traces of the fauna of relict animals, first discovered by Lovén in the great Swedish lakes, subsequently observed in Finland, Norway, Iceland, and North America, and in recent years also in Germany and Denmark (1902). I had expected to find both the relicts common in all these countries (*Mysis relicta*, *Pallasiella quadrispinosa*, *Pontoporeia affinis*), and also those whose home is in very deep and very cold water (*Idothea entomon* and *Gammarus loricatus*), hitherto recorded only from the Swedish lakes and Lake Ladoga. It is most extraordinary that the deep fauna of the great Swedish lakes has never been investigated since Lovén drew the attention of the entire scientific world to the existence of marine animals in their great depths. I thought that the sources of knowledge regarding this peculiar fauna could not have been exhausted with Lovén's discoveries, and that modern

appliances would have brought to light quite new fresh-water organisms. I hoped, further, that the explorations might reveal some of those species found by Forel in the deep water of the Lake of Geneva—*Niphargus foreli*, *Asellus foreli*, *Limnæa profunda* and *abyssicola*, etc. It will thus be understood that I began the deep bottom dredgings with great expectations, which were, of course, nourished by Mr James Murray's discoveries, larvæ of Perlidæ and Ephemeridæ never having previously been found in the abyssal region. All my expectations, however, fell short of realisation. While I think it necessary to emphasise the fact that the explorations hitherto carried on have been quite fragmentary, yet I consider it most extraordinary that with our excellent apparatus we were unable to procure one specimen either of the relict fauna or of the deep-water fauna taken by Forel in the Lake of Geneva. I may add, that in the exploration of Loch Ness I used the very same net with which I have taken the relict fauna in our Danish lakes.

The genuine abyssal fauna of the Highland lakes appears to be poor, consisting mainly of *Chironomus* larvæ, a very few species of Oligochæta, Ostracoda, and *Pisidium* (probably *Plagiostoma lemani* was found in Loch Ness), and the number of individuals seemed to me inconsiderable. The microscopic abyssal fauna is imperfectly known; but seeing that many Rhizopods are most common in peaty water, I think it probable that further investigations will reveal a great many species as inhabitants of the abyssal region of the Scottish lakes. As probably pointing to the cause of the apparently extreme poverty of organic life in the abyssal region of the Scottish lakes, I would draw attention to a fact well known in our country, viz., that in all our peat-moors the animal life at the bottom of the moors is extremely poor; we find only a few snails, larvæ of Chironomidæ, while the Oligochæta are often almost entirely absent, and only the Rhizopods are numerous. For my part I have always thought that this must be due to the large amount of humic acid, which acts as poison to many animals; and if this be the true explanation, it may indicate the principal reason why the abyssal region of the Scottish lakes is so thinly populated, the peaty water being a hindrance to the development of life.

Once more calling to mind the mist-wrapped, moss-covered Scottish hills, with their peaty moors and precipitous sides, I think we must seek the main cause of the general extreme poverty of animal and vegetable life in the Highland lakes in the general geographical conditions of the country itself.

From this sketch of the organic life in the Danish and Scottish lakes it will appear that the differences are extremely great. I suppose that what has been said with regard to the life in the Danish lakes will hold good also as to the lakes of the northern part of the Central European plain. On the other hand, the very imperfect sketch I have given of the Highland lakes can by no means be taken as applicable also to alpine lakes in general. It would indeed have been fortunate could we have drawn a comparison between the Highland lakes of Scotland, their nature and their organic life, and the Norwegian alpine lakes, many of which are similar in some respects; but this is impossible, since the Norwegian lakes have been very insufficiently explored, and we can only compare the Scottish lakes with the southern alpine lakes, especially the well-explored Swiss lakes. I may refer to the admirable works of Forel (1892-1902), Zschokke (1900), and others, relating to the fauna and flora of the Swiss lakes. Anyone who has read these, and knows something of the life in the Scottish lakes, will be aware that in every respect life is much richer in the Swiss lakes than in the Scottish lakes.

### III.

#### THE INFLUENCE OF THE ORGANIC LIFE UPON THE LAKES THEMSELVES AND THEIR SURROUNDINGS.

##### A. *The Danish Lakes.*

It stands to reason that the organic life will always exercise the greatest influence upon the surrounding medium where the organisms are in excess, both as regards the number of species and the number of individuals. When we remember that Denmark is built up of friable soil, while Scotland, on the other hand, consists for the greater part of hard rocks, it will be evident that the influence of organic life is far more intense, and

consequently more conspicuous, in Denmark than in Scotland. Every year the wide zone of vegetation which surrounds our lakes decays in October and November, is broken up by the waves, partly pulverised on the shore, and, as detritus, carried out over the whole lake; the vegetation which withstood the force of the autumn gales is frozen in the ice, and in spring, when the ice breaks up, is scattered over the lake as leaves and stems. The lime-crusts, derived from the blue-green Algæ covering the stones, are peeled off by the action of the ice, and as powder carried out from the shore. As stated by Forel (1892-1902), Kirchner (1896), myself (1901), and others, the blue-green Algæ and the fauna living in the Algæ-crusts corrode the stones, so that the stones become brittle, decay, and are pulverised. Every spring, after the first heavy storms, we find the shores strewn with thousands of dying snails or empty shells, which are broken up, pulverised, and as a fine lime powder, colouring the water in calm bays a whitish-grey, are scattered over the lake; the lime incrustations on *Potamogeton* and other plants will, especially in spring and autumn, share the same fate. During these seasons the waves reach the bottom in depths of 10 to 15 feet, and the great Characea growths, which often cover the bottom, are uprooted, cast on to the beach, and undergo the same process of pulverisation. The pulverised material remains in suspension in the water for a long time, and as detritus affects the transparency of the water,—the amount of detritus, especially in spring after heavy gales, being very considerable. It may be added, that by no means all the material thrown up on the beach is subjected to pulverisation, for a larger or smaller proportion is deposited in shallow bays, and forming peat, fills them up, and thus diminishes the size of the lake.

The huge masses of plankton will also in the course of time reach the bottom. I have shown (1900) that we can often detect beneath the layers of living plankton—I think below the “sprungschicht”—layers of dead plankton, which three or four weeks previously had been living plankton in the upper layers of water. This dead plankton mostly consists of skeletons, and by means of vertical hauls I have followed it on its way to the lake-bottom. I have shown, further, that nearly all the protoplasm of

the cells in the plankton is eaten away by Phycomycetes before reaching the bottom: my observations prove that an organism in the latter part of the period of maximum development may very often be infected by Phycomycetes, which feed upon the protoplasm and kill it, leaving the skeleton intact.

All the decayed matter derived from the plankton or from the littoral organisms, on settling upon the bottom, will be mixed with the inorganic material washed out by the waves from the shores or carried by the rivers out into the lakes. In our country this material consists mainly of lime and clay, but as yet the inorganic constituents of our lake-bottoms have not been thoroughly studied. The percentage of lime in our deeper lake-deposits is very variable, but in most cases it is extremely high, often 15 to 25 per cent., and in the Furesö 35·30 per cent., while in other lakes it may rise to 46·98, and even 59·44 per cent. (see my bottom explorations, 1901, p. 93). We have no chemical analyses of the water of the greater lakes, and therefore cannot speak of any deposits due to chemical precipitations from the water of the lakes.

The rich bottom-fauna, consisting mainly of *Chironomus*, *Oligochæta*, *Ostracoda*, and *Pisidium*, obtains its nutriment from the rain of organic and inorganic matter which drops down through the water and reaches the bottom. I have studied the life of this fauna in aquaria at the fresh-water biological laboratory at Furesö. If we take the mud from the greatest depths of our lakes and place it in aquaria, we shall observe, after the lapse of some days, upon the surface of the mud, elevations consisting of granules, as well as some jelly tubes covered with mud, and surrounded by similar granules. Beneath the elevations and in the tubes we find respectively *Oligochæta* and *Chironomus* larvæ; we can detect the granules being pushed out, and we know them to be excrementa. If we take some mud from the deep lake-bottoms and sift it through a very fine sieve we shall find enormous quantities of these granules, and if we allow the mud to remain sufficiently long in the aquaria the whole surface becomes converted into granules, that is, into excrements. From these observations we conclude that the upper layers of the deeper lake-bottoms become, consequent upon the digestive action of the fauna, converted into layers of excrements.

As far back as 1862 these layers were termed "gytje" in an admirable paper by the eminent Swedish naturalist H. v. Post, and this term is very much used in North European and Danish literature. V. Post distinguishes different forms of "gytje," but we shall here only deal with the so-called "Lake-gytje," which is formed principally in clear, limpid water. In my paper (1901) I have pointed out that the main condition for the formation of this "gytje" appears to be, that no greater quantities of organic matter be precipitated than the bottom-fauna and the bacteria conjointly may be capable of digesting. If the supply of organic matter be superabundant, black fetid mud-formations (river-deltas, common sewers, etc.) result, while, on the other hand, where the organic matter, owing to the presence of humic acid, remains undecayed and is preserved, peat is formed. Owing to the digestive processes, the excrements are generally of a lighter colour than that of the lake-bottom itself. This might be accounted for by supposing that the animals of the upper layers feed mainly on the organic dark-coloured débris, allowing the inorganic matter, which in our lakes consists especially of lime and clay, to pass through their alimentary canals. By means of bore samples from shallow lakes I have shown that the colour of the lake-bottom grows lighter the deeper we go down; it may be greyish-white 4 feet beneath a surface which is often quite black. I am of opinion that layers of almost pure lime or clay—so-called coprogenic lime and clay layers—may result from the digestive action of the bottom fauna and flora.

With regard to the process of formation, these layers are not identical with those layers of clay which, during and immediately after the Ice Age, were formed on the primary sandy bottoms of our lakes, and were one of the first conditions for the development of a higher and more specialised organic life in the lakes. Nowadays, in all our lakes, and probably in many of the lakes of the Central European plains, the precipitation of organic matter—débris from the littoral zone as well as plankton—is very copious. In all our deeper lakes it is mainly the plankton which determines the composition of the lake-gytje; and as the plankton varies in the different lakes, it will be understood that the lake-gytjes consequently also differ from each other.

In our lakes I have been able to distinguish three different



forms of lake-gytje, viz., Diatom-gytje, Cyanophycea-gytje, and Chitin-gytje. The first-named, which occurs mostly in the colder lakes, contains enormous quantities of plankton Diatom frustules, and may consist almost exclusively of these; skeletons of bottom Diatoms are very rare. From gytjes of this composition the Diatom clay may arise. According to Forel it seems that the Diatom skeletons in deeper lake-bottoms may be dissolved and disappear, but this is not the case in our shallower lakes. The Cyanophycea-gytje is a black, fetid substance, consisting of decaying plankton Cyanophycea, and mostly occurs in warm shallow lakes. The Chitin-gytje contains enormous quantities of the valves of Daphnids, and is generally formed in small lakes devoid of Cyanophycea. Lately, Holmboe (1903) has found Diatom-gytje as well as Chitin-gytje fossil in Norwegian peat-moors.

The constituents of the lake-gytje are not the same all over the lake-floor, notable differences being recognisable on the two sides of the 30-feet contour-line. Outside this contour we hardly ever find stems, shells, and Mollusca (except *Pisidium*), and very seldom leaves, the deposit nearly always consisting of fine mud. On the other hand, inside the 30-feet contour we often find the whole bottom strewn with shells; leaves and stems are common, and the deposit is much coarser in texture, often containing considerable quantities of sand and gravel, which are rarely found outside the 30-feet contour. As already stated, the material inside the 30-feet contour is either deposited, and forms, for example, peat, or is, sooner or later, pulverised by the action of the waves dashing it against the stones and sandy bays of the beach; hand in hand with this mechanical action a chemical process goes on, especially as regards the lime deposits. A close study of the mollusc shells from the shore and shallow water shows a very conspicuous corrosion, caused by different factors. On this point I may refer to my bottom explorations (1901, p. 152), and would here only observe that hitherto the corrosion of the shells of living animals has been studied chiefly as a conchological curiosity, without full appreciation of the fact that the corroding influences are nature's principal instruments in the pulverisation and dissolution of lime secreted by organisms. The process of pulverisation and dissolution of all the waste material inside the 30-feet contour is greatly accelerated

by the operations of the abundant littoral fauna, which feeds alike on the living vegetation and on the decayed matter; a large part of these passes through the alimentary canals of animals, and is transformed into excrements. The animals which cause this transformation are not the same as those found in deeper water, but consist mostly of insect larvæ and molluscs; very often we find the bottom covered with long greyish-white excrements of snails, especially *Limnæa auricularia*, *ampla*, and *ovata*.

In our lakes the space between the 16-foot and 30-foot contours is marked by a remarkable and often very conspicuous elevation of the bottom. Explorations show that in two of the lakes at some distance from shore a series of *banks* occur, consisting chiefly of mollusc shells embedded in a bluish-grey lake-marl. There is no doubt that the molluscs here act as reef-forming factors, and it will be understood that in our lakes the molluscs must act as such. In the Danish lakes molluscan life (except *Pisidium*) does not extend beyond the 30-foot contour. The shells in the vegetation zone are in great measure dissolved or pulverised by the powerful action of the various erosive agencies of this zone. In the zone occupying the space between the vegetation zone and the outer limit of molluscan life on the lake-floor the erosive power of these agencies is much diminished, and in the deeper part of the zone almost *nil*. In the tranquil water here the accumulation of shells may go on undisturbed by the grinding and dissolving forces, and thus banks of mollusc shells are formed. These banks consist of the shells of those mollusca which can live outside the vegetation zone, especially *Valvata piscinalis*, *Bithynia*, *Anodonta*, and *Unio*, but only to a slight extent of the shells of *Limnæa* and *Planorbis*, which live mostly in the vegetation zone. The accumulation of shells in the "shell-zone" is often enormous, and apparently there is a striking disproportion between the large amount of empty shells and the relatively few specimens of living molluscs; yet it must be remembered that vast accumulations of shells may result from a slow process of deposition during long periods of time, as from a more rapid deposition during a shorter period.

Inside the shell-zone and closer to the shore we often find more local and very peculiar formations, among which may be mentioned

the great lime-deposits, consisting solely of lime-incrustations formed by the Characea, composed of very conspicuous broken stems and leaves. These lime-deposits, in which the percentage of lime may amount to 88·50, are dug out of the lakes by machinery and used as manure on the fields.

In other localities within the 30-feet contour a high percentage of lime is found, but very often it is impossible to discover from what source the lime originates. In our lakes we often find lime-incrustations upon other plants besides Characea, especially *Potamogeton*, *Elodea*, etc. In studying these lime-incrustations (1901) I arrived at the following result :—In clear, calm weather the lime accumulates in thick flakes on the leaves and stems of *Potamogeton*, etc.; in stormy weather it is swept off by wave action. The precipitation of lime upon the leaves probably goes on unceasingly during assimilation; and the leaves not being able to carry the full weight of the lime, broken particles are continually dropping off, which sink to the bottom at a greater or less distance from the plant. In order to show, as far as practicable, that the precipitations of lime from *Potamogeton* and *Elodea* play a prominent part in the formation of lake-lime, two bottom-samples were taken in the Furesö; one from a bed of *Potamogeton lucens*, the other from a depth of 100 feet, the former containing 72·41 per cent., the latter 35·30 per cent. of lime. On separately weighing the dried leaves of *P. lucens* and their coatings, it appeared that a leaf often carried more lime than its own weight; one leaf weighing 0·35 gram carried no less than 4·1 grams of lime. As one plant has often some thirty leaves, it will be easily understood that the percentage of lime on the lake-floor beneath the dense growths of *Potamogeton* may be considerably raised by means of the constant rain of lime-powder dropping down from the leaves.

Other local formations are the often extensive layers of peat arising from the decaying-vegetation along the protected shores and in the shallow bays, often bordered by abundant growths of *Phragmites* and *Scirpus*. In the shell-zone lime-deposits likewise occur, abounding in mollusc shells; and in certain lakes these shells are transformed into limonite, so that considerable layers of "bohnenerz" have been formed; on this point I may refer to

my bottom explorations, where such transformations are figured (1901, p. 159, tab. iii.).

The preceding pages will have shown to what a large extent the organic life of a lake may influence the lake itself and its environs. We observe the vegetation of the littoral zone being transformed into peat, or in other localities being pulverised, and as detritus scattered over the lake, reducing the transparency of the water, and ultimately find it on the deeper lake-floor, constituting a part of the general precipitation. We see the blue-green Algæ of the shore corroding the stones, reducing them in size, and the Algæ-crusts in turn broken off and pulverised by the ice. We are able to follow the accumulation, as well as the pulverisation, of shells near the shore, and to see the white powder colouring the water a greyish-white. We observe whole layers of lime (often several feet thick) arising from the precipitated stems and leaves of Characea, and are also able to show that the percentage of lime on the bottom is raised by the lime dropping down from the great leaves of *Potamogeton*. We see the huge plankton masses determining the colour of the water, affecting the quality of the air contained in the water, causing accumulations of gases unfit for the respiration of animals, and greatly reducing the transparency of the water. We are able to recognise the once-living plankton as skeletons in the deeper layers of water, and to show how the nature of the lake-bottom is mainly determined by the character of the plankton, and, furthermore, that whole layers are derived from the accumulation of Diatom skeletons. We also note how the different precipitations are eaten by the bottom-fauna and converted into excrementa, and that the excremental processes result in layers having a lesser amount of organic matter and a greater amount of inorganic matter than if the precipitations had not been subjected to the digestive action of the bottom-fauna.

#### B. *The Scottish Lakes.*

As the result of my investigations on the Danish lakes, I have dwelt at some length upon the manner in which the fauna and flora influence and react upon the general character of the lakes themselves, thereby transforming the conditions of life common to all organisms in the lakes and their surroundings. From the

impressions I formed of the Scottish lakes, I shall next endeavour to show how the organic life here also influences the lakes and their environs. I have, of course, seen too little of Scotland to be able to do so as satisfactorily and exhaustively as I should wish. From what I did see, I gathered that, owing to the extreme paucity of organic life and the hardness of the soil, as well as the lesser amplitude of the variations in the temperature of the water, the intensity of all those processes due to the influence of organic life is much less marked than in the Danish lakes.

As a zone of higher vegetation in the larger Highland lakes is almost entirely wanting, peat formation along the shores is almost out of the question; only a very small amount of organic material from the shores is scattered over the lakes, in the form of detritus, diminishing the transparency of the water. The stones, as far as I am aware, are never covered with lime-incrustations derived from blue-green Algæ; the Potamogetons, etc., are never seen covered with lime-crusts; and the shells of mussels or snails never abound in such quantities on the beach that their pulverised fragments, in the shape of lime-powder, are scattered over the lakes, or influence the percentage of lime in the water or in the deposits on the lake-floor. The amount of plankton in the larger Highland lakes is never or very rarely so great as to affect the colour of the water in any notable degree; most probably it may affect, to a relatively slight extent, the transparency, and the amount and quality of the air in the water.

From my studies of the deposits in Loch Ness, Loch Oich, and Loch Lochy, I suppose that the precipitation of decayed or decaying matter derived from the plankton is very insignificant. Of course, I never found any great quantities of blue-green mud derived from blue-green plankton Algæ, but even the chitinous valves of Daphnids are rare. In Loch Lochy, at a depth of 500 feet, I most frequently found the carapaces with long antennæ of *Bosmina*. A most remarkable and interesting thing is that the frustules of Diatoms, as in the Swiss lakes, are comparatively rare, and the skeletons that do occur are, to my knowledge, only those of bottom and shore Diatoms, the plankton Diatoms being almost entirely absent. It has long been an enigma to me why the skeletons of the plankton Diatoms accumulate on the bottom in

our lakes at 120 feet, while in the certainly much deeper alpine lakes they always appear to be dissolved before reaching the bottom. I can hardly imagine that the solution in the alpine lakes is solely due to the greater depth, because of which the deposition would occupy a longer period of time. On becoming acquainted with the plankton Diatoms of the Scottish lakes, it struck me that the Diatoms in nearly all alpine lakes are the thin-shelled *Cyclotella*, *Asterionella*, and *Fragilaria*. Of these the two last-mentioned are also common in our lakes, but there also their skeletons never produce Diatom-ooze; in many hundreds of samples I have observed very few frustules of these forms, and I suppose that in the Danish lakes also they are dissolved before sedimentation. The Diatom-ooze in our lakes is produced by thick-shelled plankton Diatoms (*Melosira*, *Stephanodiscus astræa*, etc.), species which are rare in the plankton of the alpine lakes, but still occurring in the littoral zone. Provisionally, I am inclined to believe that the formation of plankton Diatom-ooze in our lakes may perhaps be explained by the presence of thick-shelled Diatoms in the plankton. The circulation of silicates in the lakes is a study of the greatest interest, and one regarding which we know very little.

I think it very probable that a future more exhaustive exploration will only further prove that the precipitation of organic matter derived from the littoral zone and plankton in the Scottish lakes is only relatively small. The greater part of the organic matter ultimately reaching the bottom in a more or less pulverised state is, as far as I can make out, derived from the tops and sides of the mountains, carried into the lakes by the rivers. In the preceding pages I have made it my object to point out that, according to my view, *the organic life in the Scottish lakes, both as regards the littoral fauna, the bottom fauna, and the plankton, to a very considerable extent likewise originally belonged to the adjoining country, and not to the lake itself. Regarding the deposits on the lake-floor, we shall arrive at a similar conclusion.* With us it is a common rule that the deposits already at about 50 feet mainly consist of fine mud, mingled with very few stems, shells, or leaves. When dredging at 300 feet in Loch Ness I was greatly surprised to find the bottom mainly consisting of very coarse material, mixed

with large stems, leaves, etc. ; it was only at about 500 feet that I found the deposits to be as finely pulverised as at about 50 feet in the Danish lakes. This phenomenon is easily accounted for—in our lakes everything in the shallow water between the shore and the 30-foot contour is pulverised by the dash of the waves, whereas in the Scottish lakes, owing to the precipitous hillsides, everything is carried away from shore by the rivers and waves, and subsides in depths of 200 to 300 feet, without being exposed to the eroding force of the waves on a shallow coast.

From all the bottom-samples I have seen it appears that the deposition of organic matter is not nearly so abundant as in the Danish lakes, the deposits consisting principally of inorganic materials ; there is further a total absence of lime—a very conspicuous difference between the lake-bottoms in the two countries. Further observations may show in what manner the bottom fauna deals with the deposited material, and the changes to which this material is, in consequence, subjected ; I cannot but think that here also layers of “gytje” are being formed.

I suppose that most of the observations on the influence of organic life upon the general conditions of the lakes and their surroundings in our own country will hold good also with regard to most of the lakes in the southern part of Sweden and in the northern part of Germany ; my investigations of the Danish lake-gytjes are in accordance with v. Post’s explorations of Swedish gytjes, and most of my observations with regard to the lime-deposits have been corroborated by Passarge. The explorations of Halbfass among the lakes of Pomerania show that the natural conditions of those lakes are very similar to those of our own.

#### MY VISIT TO THE LOWLAND LAKES.

Subsequent to my examination of the Highland lakes, I visited some lakes in the Lowlands, as well as some smaller lakes near Edinburgh, including Loch Leven—famous for its excellent trout. These lakes presented many points of similarity with those of our own country. I found in Loch Leven the same gently sloping shores, a very slight transparency of the water, and a considerable amount of detritus ; the mud was very fine, and the large amount

of organic matter, on the whole, very similar to that at the bottom of our lakes. The organic life also has some resemblance to that of the Danish lakes, but still I noticed some very striking differences. The band of vegetation visible above water was narrow, but the evenly sloping sandy shores, especially along the north-east coast, were covered with dense growths of Characea: strangely enough, in the deepest parts of the lake, in depths of about 80 feet, I found the mud covered with long filaments of blue-green Algæ. From the explorations of the Lake Survey (1901A, p. 124) we know that the mud contains no carbonate of lime. The animal life has been studied by Mr T. Scott, to whose paper I refer. The molluscan life I found to be much richer than that in the Highland lakes, but still by no means so rich as with us. *Limnea* and *Planorbis* were, both as regards species and individuals, relatively few in number; only in the Characea-growths were there great quantities of *Valvata*, and in the bottom-mud *Sphærium*, *Pisidium*, and *Anodonta* abounded. The Crustacea, especially the Cladocera, were represented by numerous species, and in the Characea-growths the animal life was extremely rich.

The quantity of plankton was enormous: I do not remember to have seen, even in our lakes, such huge masses of *Leptodora*. The plankton, at the time I visited the lake, consisted chiefly of this Daphnid, with *Cyclops strenuus* and other Entomostraca. The phytoplankton was less conspicuous, *Anabæna flos aque* being the most predominant, and it might have formed "wasserblüthe."

#### GENERAL CONCLUSIONS.

It will easily be understood that where the alluvial deposits in shallow lakes are as copious as in our country, the lakes will in the course of time become silted up and overgrown, and will finally disappear. When looking at old maps and when studying nature we meet with traces of numerous former lakes. Many have been drained by man and converted into arable land, but yet in such cases man has only forestalled what nature would have accomplished in a relatively short period of time. All our lakes were formerly much larger, and their form and coast-lines far more irregular, the bays having in many cases been silted up, and at the



end of the more elongated lakes we generally find more or less extensive marshy ground. Many of our existing lakes are apparently doomed, and it is difficult to imagine how in our country new lakes could be formed.

There can be little doubt that in Scotland the coast-lines of the lakes have altered very little during thousands of years, and that the lakes themselves will remain through long ages.

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PLATE I.



FIG. 1.—Ice erosion on the shores of the Furesö.  
(Photo. by Dr C. Wesenberg-Lund.)



FIG. 2.—Furesö with its zones of *Phragmites* and *Scirpus*.  
(Photo. by Dr C. Wesenberg-Lund.)

DR WESENBERG-LUND



PLATE II.



**FIG. 3.**—Loch Ness from Borlum, looking north-east.  
(Photo. by Mr G. West.)



**FIG. 4.**—Loch Killin (near Loch Ness), looking north, showing steep escarpment on the western shore.  
(Photo. by Mr G. West.)

DR WESENBERG-LUND.



**Variations in the Crystallisation of Potassium Hydrogen Succinate due to the presence of other metallic compounds in the Solution.** (*Preliminary Notice.*) By **Alexander T. Cameron, M.A.** Communicated by Dr **HUGH MARSHALL, F.R.S.**

(MS. received January 9, 1905. Read January 23, 1905.)

In the summer of 1902, while working as a student in the Chemistry Department of Edinburgh University, I prepared a quantity of potassium chromoxalate (Gregory's salt) as an ordinary exercise, and this led me to attempt the preparation of a similar derivative of succinic acid, since such derivatives apparently had not been obtained.

For this purpose a solution of potassium hydrogen succinate (prepared by half-neutralising succinic acid with potassium carbonate) was boiled for some time with freshly precipitated chromic hydroxide (prepared by adding ammonia to a boiling solution of chrome alum, filtering, and washing thoroughly). The undissolved hydroxide was filtered off, and the filtrate subjected to the same treatment with fresh chromic hydroxide; the whole process was repeated two or three times, the final filtrate being dark green in colour. A portion of this solution was evaporated to small bulk by boiling; on cooling, potassium hydrogen succinate first crystallised out, and then a green crystalline powder was obtained. The remainder of the solution was concentrated only to about half its volume and allowed to stand for three days; at the end of that time dark green crystals were deposited. These showed the striking peculiarity of being bounded only by curved surfaces, plane faces being entirely absent; from their shape they might be described as oblique elliptical double cones, possessing monoclinic symmetry (plane of symmetry with digonal axis normal to it). A perfect cleavage, yielding highly lustrous faces, was observed parallel to the plane of symmetry, and the parallelogram



formed by the outline of the cleavage face had an obtuse angle of about  $135^\circ$ .

Until recently I was unable to continue the investigation, but, owing to the publication of a paper by Werner on "The Behaviour of Chromic Hydroxide towards Oxalic Acid and certain other Organic Acids" (*J. Chem. Soc.*, 1904, 85, p. 1438), I have considered it desirable to publish a preliminary note, although the results so far obtained can only be stated generally.

Several preparations have been made similar to that described above, and the crystalline products analysed for chromium. The percentage varies considerably in the different preparations, and as yet it is impossible to state what is the maximum, but specimens hitherto analysed show considerably less than 1 per cent. Since potassium hydrogen succinate crystallises in monoclinic crystals possessing a plane of symmetry and showing perfect cleavage faces parallel to it, the small amount and the fluctuation in that amount of chromium present in these crystals lead to the assumption that they are potassium hydrogen succinate, the external surfaces being modified by the presence of some chromium compound, possibly in solid solution.

Attempts have also been made to dissolve other hydroxides and certain carbonates in potassium hydrogen succinate solution.

When copper carbonate was taken a precipitate of copper succinate was first produced; the filtrate from this was coloured slightly blue, and after standing for some time deposited crystals of the acid succinate. These showed six-sided prism faces, and also, superimposed on these, curved faces similar to those observed with chromic hydroxide.

Crystals showing traces of these curved faces have been obtained from solutions in which aluminium hydroxide had been dissolved.

Equal quantities of fairly concentrated solutions of potassium hydrogen succinate and ferric chloride (containing a few drops of hydrochloric acid) were mixed, and in a few minutes a brick-red precipitate appeared. The solution and precipitate were boiled with another equal quantity of potassium hydrogen succinate, the precipitate filtered off, and the filtrate, which was slightly yellow in colour, set aside to crystallise. At the end of three weeks pale yellow crystals were found at the bottom of the crystallising

dish, elliptical in form, and growing in towards the centre. Their appearance was that of truncated cones. They were removed, and a month later a single elliptical biconical crystal was obtained; it was brownish-yellow in colour, and resembled those obtained with chromic hydroxide, but was much more perfect in form.

Chromic hydroxide dissolves in potassium hydrogen malate much more readily than in the corresponding succinate, and gives finally a very dark green solution, from which crystals similar to those already described have been obtained.

I am continuing the investigation, and hope to be able to publish a detailed examination of these crystals at an early date.

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*(Issued separately February 4, 1905.)*

**A. Laboratory Apparatus for Measuring the Lateral Strains in Tension and Compression Members, with some Applications to the Measurement of the Elastic Constants of Metals.** By E. G. Coker, M.A. (Cantab.), D.Sc. (Edin.), F.R.S.E., Professor of Mechanical Engineering and Applied Mathematics, City and Guilds Technical College, Finsbury, London. (With a Plate.)

(MS. received October 26, 1904. Read November 21, 1904.)

The recognition of the importance of lateral strain in the theory of elasticity, as now taught in most engineering colleges, makes it very desirable that students should make experiments upon the lateral contraction of tension specimens and the lateral extension of compression pieces with the same facility that they now determine the values of Young's modulus and the modulus of shear.

With this purpose in view, the author designed an instrument which has been very thoroughly tested by student-use for the past two years in the testing laboratory of M'Gill University.

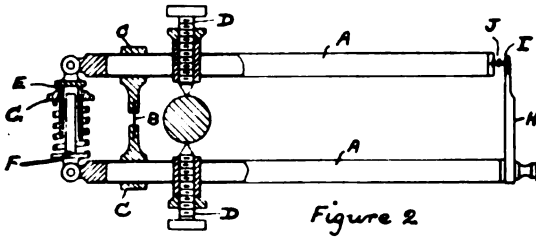
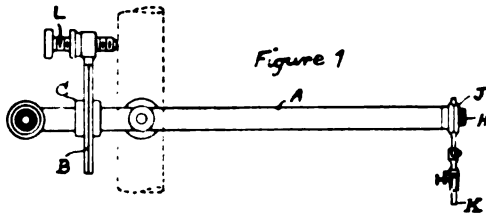
For the object in view it was necessary to make an apparatus of simple construction, easily operated and understood, and capable of standing a considerable amount of wear and tear without injury, while at the same time it must be capable of measuring with accuracy linear strains of the order of  $\frac{1}{20,000}$  of an inch.

After some minor alterations, an apparatus was constructed which fulfilled these requirements.

The instrument is shown in sectional elevation by fig. 1, and in part sectional plan by fig. 2, and it consists essentially of a pair of tubular arms *A*, connected by a flexible steel plate *B*, which forms the fulcrum. This plate is very thin, in order to allow the arms to turn in the plane passing through their axes, and is very deep, to give the necessary rigidity perpendicular to the plane of motion, and thereby ensure that the arms have no other motion. The plate is gripped by a pair of collars *C*, mounted on the arms *A*, and provided with grooved ends and tightening screws.

The instrument is attached to the specimen by a pair of screws *D*, threaded through nuts formed on the arms and provided

with lock nuts, and the pressure of the screw points on the specimen is regulated by a spring threaded over a hollow spindle *E* pivoted to one arm, this spindle being guided by a second, *F*, pivoted to the other arm: the compression of the spring is regulated by a nut *G* upon the outer spindle.



The free ends of the tubes are prolonged beyond the screw grips, and one of them is fitted with an ebony finger *H*, having a thin steel plate *I* secured to its outer end, which presses against a double knife-edge *J*, seated in a shallow V-notch cut in the end of the other arm.

This knife-edge carries a mirror *K* pivoted upon a vertical spindle, and capable of adjustment about an horizontal axis also. An adjusting screw *L*, secured in one of the collars, bears against the specimen, and keeps the instrument from swinging round on the points of the screws.

With this arrangement any alteration in the diameter of the specimen between the screw points causes a movement of the outer end of one arm relatively to the other, and a proportional rotation of the knife-edge and its attached mirror is obtained. This rotation is observed by a telescope and scale placed at a convenient distance away, and a measure of the change in the

diameter of the specimen is thus obtained. A photograph of the apparatus is shown in fig. 3 mounted upon a tension specimen.

The value of a unit of the scale was obtained by calibrating the instrument by a Whitworth measuring machine, and it was found that, with the scale 24·8 inches distant from the mirror, one division of the scale corresponded to one-millionth of an inch.

At first some minor difficulties were experienced owing to the longer branches of the tubes being insufficiently rigid, and they were therefore trussed, with good effect, and afterwards pieces of hard wood, of square section, were forced down the tubes; this overcame the difficulty completely. As the instrument was wholly of brass, some difficulty was experienced owing to small changes of temperature in the laboratory, which sometimes altered the zero of the instrument during a test; this error was guarded against by lagging with chamois leather.

The instrument, when used in conjunction with an apparatus for measuring longitudinal strain, gives a measure of Poisson's ratio  $\frac{1}{m}$  if the material fulfils the conditions assumed by the theory of elasticity; and knowing the value of Young's modulus  $E$ , we can easily calculate the modulus of shear  $C$  and the bulk modulus  $D$  from the formulæ

$$C = \frac{1}{2} \frac{m}{m+1} E$$

$$D = \frac{1}{3} \frac{m}{m-2} E$$

As an example of this we may quote a test of a piece of machinery steel in tension, when the lateral extensometer above described and a Ewing extensometer were secured to the specimen.

The experiment gave the following results:—

Steel specimen 1·01 inches in diameter.

Length under test 8·00 inches.

Ewing Extensometer, one division =  $\frac{1}{100,000}$  of an inch.

Lateral Extensometer, one division =  $\frac{1}{1,000,000}$  of an inch.

The accompanying table of observations (page 455) shows that the mean longitudinal strain per unit of length is ·0000825 inches, and the mean lateral strain ·0000206, corresponding to a value for  $m$  of 4·01, and the value of  $E$ , obtained in the usual manner, is 30,250,000, the units being pounds and inches.

Load Pounds.	Longitudinal Strain.		Lateral Strain.	
	Reading.	$\Delta$	Reading.	$\Delta$
1,000	0	- 34	0	- 20
3,000	34	- 32	20	- 22
5,000	66	- 33	42	- 21
7,000	99	- 33	68	- 22
9,000	132	- 33	85	- 21
11,000	165		106	
0	0		0	

The values of C and D are respectively 12,378,000 and 20,603,000, with the same units.

As a further example we may quote the case of a wrought-iron bar in tension, having a diameter of 1 inch, the test being similar to the one previously described. The readings obtained were—

Load Pounds.	Longitudinal Strain.		Lateral Strain.	
	Reading.	$\Delta$	Reading.	$\Delta$
1,000	0	- 37	0	- 25
3,000	37	- 85	25	- 25
5,000	72	- 34	50	- 25
7,000	106	- 37	75	- 24
9,000	143	- 36	99	- 25
11,000	179	- 36	124	- 22
9,000	143	- 85	102	- 24
7,000	108	- 36	78	- 24
5,000	72	- 35	54	- 25
3,000	37	- 37	29	- 25
1,000	0		4	

And from these readings we derive the following values:—

$$\begin{aligned}
 m &= 3.64 \\
 E &= 28,450,000 \\
 C &= 11,160,000 \\
 D &= 21,048,000
 \end{aligned}$$

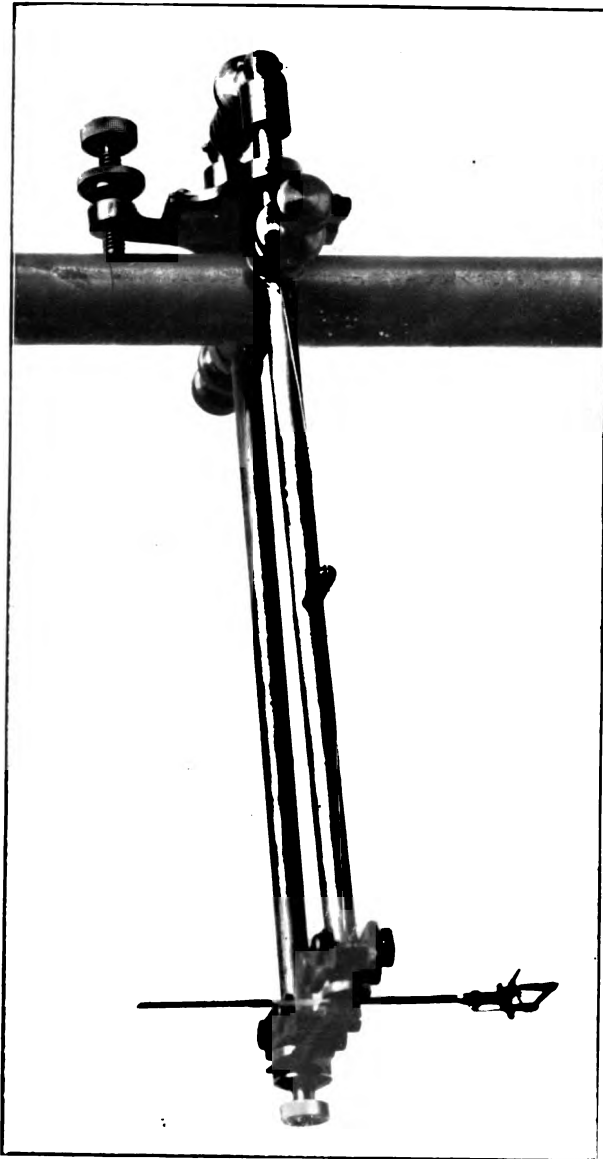
Other metals were also experimented upon, and in some cases under compression, when the longitudinal strain was measured by a compressometer of Professor Ewing's design. It will be sufficient to quote the results of these experiments without the detailed observations, which present no peculiarity except that, in the cases of cast-iron, brass, and copper, the stress strain curve for a complete cycle of stress was a very narrow loop. In these cases the mean value of the strains for the whole range of stress was taken for calculating the values of the constants. The results, including the tests above cited, were as follows:—

*Tension Experiments.*

Specimen.	$m$	E	C	D
Machinery Steel, . . .	4.01	30,250,000	12,378,000	20,608,000
Wrought-Iron, . . .	3.64	28,450,000	11,160,000	21,048,000
Rolled-Brass, . . .	3.10	14,700,000	5,557,000	13,809,000
Rolled-Copper, . . .	3.02	10,100,000	3,794,000	9,640,000

*Compression Experiment.*

Specimen.	$m$	E	C	D
Machinery Steel, . . .	4.09	29,600,000	11,891,000	19,310,000
Wrought-Iron, . . .	3.58	28,100,000	11,000,000	21,200,000
Rolled-Brass, . . .	3.12	14,820,000	5,620,000	13,750,000
Cast-Iron, . . .	4.07	14,900,000	5,950,000	9,750,000



PROFESSOR E. G. COKER.





These results correspond with those obtained by Bauschinger,\* Stromeyer,† Morrow ‡ and others.

It should be noted, in conclusion, that the specimens of material for the tension and compression specimens were not identical, but they were taken from the same consignments.

\* *Der Civilingenieur*, vol. xxv., 1879.

† "Experimental Determination of Poisson's Ratio," *Proc. R. S.*, 1894.

‡ "On an Instrument for Measuring the Lateral Contraction of the Bars, and on the Determination of Poisson's Ratio," *Phil. Mag.*, 1903.

(*Issued separately March 3, 1905.*)

**On Astronomical Seeing.** By Dr J. Halm,  
Lecturer in Astronomy in the University of Edinburgh.

(Read May 6, 1904. MS. received October 14, 1904.)

In the Annual Report of the Smithsonian Institution for 1902 Prof. Langley has published an important note on "Good Seeing," in which he describes some experiments undertaken with the view of improving the definition of telescopic images, so far as it depends on the conditions of the air in the vicinity of the instrument. Up to now the belief has prevailed among astronomers that in order to obtain good definitions the air inside the telescope-tubes should be kept as much as possible not only at a uniform temperature but also in a state of perfect tranquillity. Langley, however, shows that this view is not quite correct, and that maintaining constant and uniform temperature inside the tube, while preventing circulation between the air inside and outside the instrument, is not sufficient to produce satisfactory telescopic images. Particularly, this method does not diminish the troublesome boiling which in solar observations proves so often to be a source of grave inconvenience to the observer. But he shows that if the air inside and near the telescope-tube is agitated by stirring, the definition becomes at once markedly better. The improvement has in all cases been so decided that the reality of this beneficial effect of stirring cannot well be doubted.

This result has led me to investigate the question as to whether a similar conclusion may perhaps be drawn with regard to the great mass of atmosphere which is traversed by the luminous rays of the celestial object before they reach our telescopes. Is there any reason for assuming that stirring this mass of air would improve the definition, sharpness, and steadiness of the star images? The question, I think, has not been asked before; and

I should like, therefore, to discuss it here in a few words, especially as the answer to it seems to be simple and conclusive.

Let us first get an insight into the cause of the blurrings of telescopic images, so far as atmospheric circumstances are responsible for it. We feel no hesitation to look for this cause in the incessant motions of our atmosphere, in the spontaneous, fitful, and ever varying displacements of air from one place to another, in consequence of local changes of temperature and pressure. Now, the motion itself can have no direct effect on the definition. The cause of the blurring must be looked for in sudden changes of the index of refraction of the air resulting from its internal motions. If, for instance, a volume of heated air rises from the surface of the soil to a higher layer, and arrives there with a temperature higher or lower than that of the layer itself, the temperature and density of that particular point of the atmosphere, and thus its index of refraction, will be momentarily altered. Hence the direction of a ray of light passing through this point must suffer a corresponding change; the consequence being, that among the rays which, under undisturbed and perfectly ideal conditions, would all reach the object-glass in parallel directions, those passing through the affected area will be thrown into slightly different paths, and will therefore be focussed at different points of the field of view.

Now, we may ask: If the definition of telescopic images depends on these fitful changes of the index of refraction which are caused by the unavoidable movements and displacements of air in the atmosphere, are there conditions under which these movements have a minimum disturbing effect? It is well known that there is indeed one particular state of the atmosphere in which these conditions seem to be present, viz., the so-called state of adiabatic equilibrium. In this state a volume of air carried from one layer to another will arrive at its new position with exactly the same temperature and density which were previously possessed by the mass of air whose place it has taken. Hence motion of air, in whatever direction it may take place, is not accompanied by change of the index of refraction. We may compare the atmosphere in this particular state to a liquid in which bodies are suspended, of any size and shape, but of the

same transparency and refrangibility as the liquid itself. Whatever may be the motions of these bodies within the liquid, they can have no disturbing effect on the course of the rays passing through the medium, which will behave as an homogeneous substance.

This reasoning leads us to expect the most perfect telescopic images whenever the atmosphere traversed by the light of the star is in the state of adiabatic equilibrium. Now, it is a well-known fact that this state is reached, or at least approached, when air is agitated by convection. It is for this reason that Lord Kelvin long ago proposed to call this equilibrium 'convective,' instead of 'adiabatic' or 'indifferent.' Hence we conclude that seeing should be most favourable when the air has been previously stirred by convection-currents. With regard to the general atmosphere, we reach therefore the same conclusion at which Langley has arrived by his experiments where he considered the comparatively small mass of air in the immediate vicinity of the instrument.

Several facts may be mentioned which seem to corroborate this explanation, and in some measure to bear out its validity. For instance, we know that on clear summer days, especially at continental stations, convection between the upper and lower layers of the atmosphere takes place during the daytime, being most energetic in the afternoon. Hence we infer that convective equilibrium is most nearly attained in the early evening, and consequently that the definition of stellar images should be best during the first hours of the night. In the later hours the seeing must become worse, because, in consequence of nocturnal radiation, the vertical distribution of temperature changes gradually so as to become incompatible with the conditions of adiabatic equilibrium. Towards the morning hours conditions become, therefore, more and more prevalent under which spontaneous displacements of masses of air must be accompanied by fitful changes of its refrangibility. My experience as an observer at Strasbourg is in perfect accord with these conclusions. As a rule, the seeing in the early summer evenings at the time of sunset was excellent, while after two o'clock in the morning the images had usually become so bad that the observations had to be discontinued.

The worst definition was commonly experienced shortly before sunrise. Professor Copeland tells me that at Parsenstown the seeing was specially good during a gale, and my own experience here in Edinburgh confirms this statement.

The superiority of the definition in summer over that in winter which is very marked at continental observatories is readily explained by the fact that convection is much more energetic in the former season. Indeed, at continental stations the atmosphere in winter is on the whole very far from the condition of adiabatic equilibrium, the temperature-gradient being much too small, and often even reversed.

The question is doubtless of practical importance, and should receive attention when sites for new observatories are selected. The erection of observatories on or near mountains may be advocated from this point of view, because horizontal movements of the atmosphere are deflected at the mountain sides into more vertical directions, thus enhancing that "stirring" of the atmosphere above the station which leads to the establishment of convective equilibrium. The atmosphere on mountains, besides being more transparent, must also be steadier, in an optical sense, not from the absence of motions, but because these motions, by taking place under adiabatic conditions, exert little or no disturbing influence on the normal refrangibility of the air.

Meteorologists may perhaps give us definite and practical hints as to the more or less favourable conditions under which convection takes place in our atmosphere. Astronomers should be guided by these advices in the selection of localities for their observatories. Clearly, we have no means to prevent the incessant general and local movements of the vast gaseous ocean above us. But knowing that under one certain condition these uncontrollable motions, otherwise so much inclined to impair our vision, may be rendered optically ineffective, we must avail ourselves of every possible chance by which this ideal condition may be approached,—on the one hand, by taking full advantage of favourable topographic and climatic features, and on the other, by designing mechanical devices for inducing convection in the neighbourhood of our instruments.

It would be interesting to hear the opinion of practical astronomers on this question, and to see how far their experiences

confirm my conclusions. I also wish to induce observers to take regular notes of the conditions of seeing, and to enter into their notebooks such remarks on the meteorological conditions prevailing at the time of observation as may enable us to test the views here expressed.

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*(Issued separately March 3, 1905.)*

**On the Graptolite-bearing Rocks of the South Orkneys.**

By J. H. Harvey Pirie, B.Sc., M.B., Ch.B. *Communicated by* Dr HORNE, F.R.S. With a Note by Dr PEACH on Specimens from the South Orkneys.

(MS. received February 7, 1905. Read February 20, 1905.)

The South Orkneys are a small group of islands situated in the Southern Ocean, in about 62° S. lat. and 45° W. long., roughly 800 miles S.E. of Cape Horn. A single landing was made from the "Scotia" on Saddle Island, a small island on the north side of the group, and another on Coronation Island, the largest and most westerly. With these two exceptions all the rock specimens were obtained on Laurie Island, the most easterly of the group.

The rock got on Coronation Island is a coarse conglomerate, in which the bedding is well marked, the individual beds averaging about 2 feet in thickness, and dipping at about 30° in a north-easterly direction. The rock is composed of a mixture of rounded waterworn pebbles and of angular fragments of dark-coloured shale and mica-schist. Whether this rock belongs to the same series as the Laurie Island beds or not I do not know, but the strike is approximately the same.

Saddle Island is composed of a massive greenish greywacke, very similar to the Laurie Island rocks. The typical rock of Laurie Island is a fine-grained greywacke of a blue-grey or greenish-grey colour. To the naked eye it appears almost homogeneous: the only constituents that can be recognised are some minute rounded quartz grains, small black shaly particles, and a few specks of pyrites. Thin quartz and calcite veins traverse the rock irregularly. A microscopic section shows that the derived constituents consist of angular and sub-angular grains, with a mean diameter of about 0.2 mm. The great majority of these are quartz, originally of plutonic origin; there are also a goodly number of small crystals of plagioclase, wonderfully fresh, some grains of both sphene and zircon, and a few minute flakes of biotite. The



cementing material is very largely obscured by a dusty-grey or brown amorphous substance and by black carbonaceous matter. Where the grains are fairly large and well packed this forms a sort of network, in the meshes of which lie the quartz grains. Where the grains are not so close it is more distinct, and under crossed Nicols has a crypto-crystalline appearance, practically identical with that of chalcedony. A few chlorite flakes occur in it here and there. Small veins traverse the section, some containing calcite, others a fine quartz mosaic. Bedding is not seen in hand specimens, and in many places in the field it cannot be made out either, the rocks having a massive character, but much traversed by cracks and faults, shattering them into irregular masses.

In other places, again, the bedding is distinct, or even marked. Where this is the case the individual beds vary in thickness from a few inches to several feet. Very often the bedding has a contorted, or rather, wavy character, more conspicuous when viewed from some distance off.

On some of the cliffs faulting is very marked, which has probably given rise to the general shattered condition of the rocks. Most of the faults noted are strike-faults. When the faults are not so much in evidence, the rock shows in places well-marked jointing, often very difficult to distinguish from bedding planes.

Varieties of the greywacke occur. These are of very local occurrence, and are not usually sharply defined, but shade off imperceptibly into the common type. The following are the principal varieties:—

1. Greywacke conglomerate. Contains rounded quartz pebbles, not usually larger than  $\frac{1}{4}$  in. in diameter, and pieces of dark slate or shale, rounded or flattened angular laminae, up to  $\frac{3}{4}$  in. in length. This is an extremely hard, tough rock, intimately pervaded by the siliceous matrix, so that the grains seem to fade into each other and into the cementing material, instead of having sharp outlines. When fractured, the component pebbles break across, but on natural weathered surface the matrix gives way sooner, leaving the individual pebbles sticking out as in a conglomerate. A microscopic section shows that the allothigenic or derived materials are practically

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the same as in the normal greywacke, *i.e.* pebbles of quartz and chalcedony, pieces of shale, small crystals of plagioclase, a few grains of sphene and zircon, and biotite flakes. Of the larger quartz pebbles, some at least are typical plutonic quartz, with lines of fluid inclusions, but showing strain shadows: the majority seem to be derived from some metamorphic rock—pebbles which in ordinary transmitted light appear quite uniform, between crossed Nicols are seen to be composed of a mosaic of different crystallographic individuals. The cementing material is not so obvious as one would expect from a naked-eye examination, as the interstices between the larger pebbles are filled up by smaller fragments, chiefly of quartz. It has the same chalcedonic appearance as in the typical greywacke, but green chloritic flakes are more abundant. There are numerous small veins of both calcite and quartz: one of the latter, about 0.03 mm. in width, was observed running right through some of the large quartz pebbles.

2. Greywacke-slate. Has a fine laminar structure parallel to the places of deposition, is of a lighter grey colour, and splits up readily into thin laminæ. There is no true slaty cleavage developed, however.

3. Greywacke, showing gneissic banding and folding. This was only got in one patch of very limited extent.

Shaly rocks also occur. In one situation only were regular beds of shale found alternating with layers of greywacke. Commonly the shale occurs simply as patches in the greywacke, seemingly irregularly mixed up with it, or with ill-defined borders shading off into the greywacke. The shale is much cleaved and broken, the individual pieces being bent and curved, and showing numerous slickensided faces, the result of the crushing and faulting to which it has been subjected. Microscopically it shows much brownish-grey amorphous material and black carbonaceous matter in the lines of stratification—forming a sort of network in the silica matrix. Interstratified lenticular-shaped patches occur, which are much freer from amorphous matter. With crossed Nicols these resolve themselves into a crypto-crystalline chalcedony, identical in character with the cementing material of the greywackes.

The largest development of the shale occurs on a small islet off the south coast of Laurie Island, near Cape Dundas—its eastern

end—and which has been called Graptolite Island. Here three fossils were got. One of these is a graptolite, which has been examined by Miss Elles, who considers it to be part of a *Pleurograptus*. This would make the bed correspond in age with the Hartfell shales—almost the uppermost beds of the Ordovician system. The others have been kindly examined for me by Dr Peach. As is seen in his Note, he considers them to be parts of a Phyllocarid crustacean, probably nearly allied to *Discinocaris*, a form typical in this country of the Lower Birkhill shales, at the base of the Upper Silurian.

If this is the case, then there is here an association in one bed of two forms which, in the South of Scotland, are characteristic of two different but at the same time closely contiguous zones.

As regards the structure of the island as a whole, it is unfortunate that the data regarding the dip and strike of the rocks are rather meagre. This is due partly to the fact that so much of the area is covered by ice, and partly because in so many places the dip could not be made out. The most common strike of the rocks is north-westerly, varying from N.N.W. to W.N.W., the dip being in most cases at a high angle north-easterly or south-westerly. One definite anticlinal axis was observed, running in a N.N.W. and S.S.E. direction. In a few localities other directions of strike were noted, but these were nowhere of large extent, and are probably only local contortions.

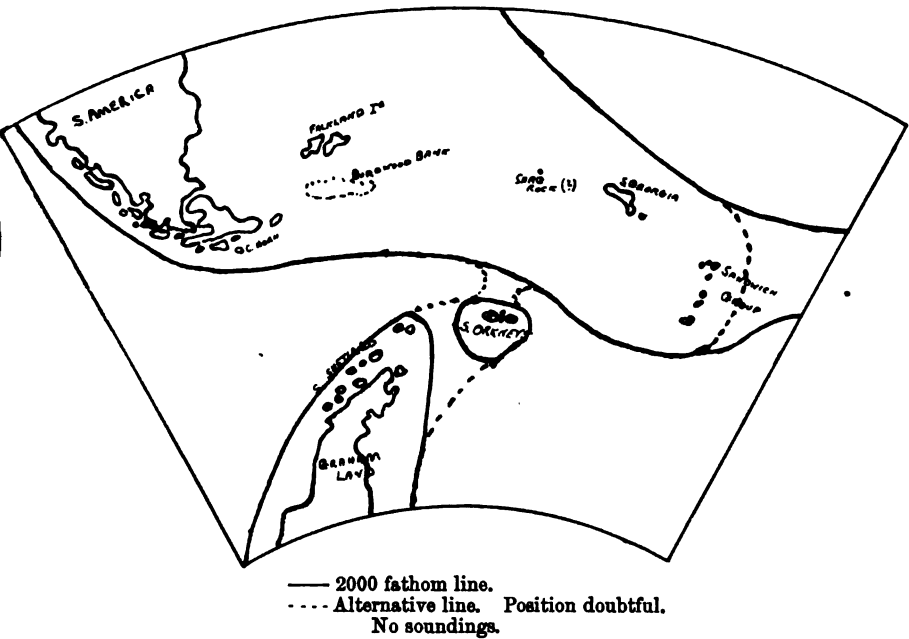
Laurie Island itself, although its greatest length is in an E.N.E. and W.S.W. direction, consists of a series of peninsulas and hill ridges, running in a general N.W. and S.E. direction, with deep bays between adjacent peninsulas, and usually low cols crossing the island from the head of a bay on the north side to the head of another on the south side.

The same structure is repeated in the group as a whole, which, though it extends furthest in an east and west direction, is cut up by two large straits, which cross it in about a N.N.W. and S.S.E. direction.

These two sets of facts—the strike of the rocks and the general alignment of the hill ridges—lead one to believe we have here to deal with a series of plications whose axes run in a general N.W. and S.E. direction—probably rather nearer N.N.W. and S.S.E.

In the only previous reference to the structure of these islands that I have been able to find, viz., the reports of M. Dumont D'Urville's voyage,\* their only landing seems to have been on a small islet about half a mile from Saddle Island, where they report greyish limestone and phyllitic shales, with a N.N.W. and S.S.E. strike, and inclined at over 60°.

Although geographically situated nearer the South Shetlands and Graham Land, the strike of the rocks leads one to consider whether these islands are not more intimately connected with



South America. In this connection it is important to consider some geological facts from areas further afield. In the Falkland Islands the Silurian or Devonian rocks there are folded along an east and west axis. South Georgia, composed entirely of clay slates, in which one fossil shell has been found—of Upper Palæozoic or Lower Mesozoic Age, according to Professor Koken—is stated † by Dr Andersson, of the recent Swedish Antarctic

\* "Voyage au Pole Sud, sous le commandement de M. Dumont D'Urville," *Géologie*, par M. J. Grange.

† Andersson, *Geog. Jour.*, Oct. 1902.



Expedition, to consist of a series of folds along an axis nearly parallel to the long axis of the island, *i.e.* a north-west and south-east axis. Then the soundings taken by the "Scotia" indicate that the deep water between Cape Horn and the South Shetlands narrows as we go eastwards into a trough-like depression of over 2000 fathoms, passing north of the South Orkneys, then probably turning south-eastwards, to become continuous with the deep area of the Weddell Sea.

It may be, therefore, that the Andean axis, already turning eastwards in Southern Patagonia and Tierra del Fuego, is continued in this direction south of the Burdwood bank, and then curves south-eastwards between the South Orkneys and South Georgia.

If this is the case, then there is a relationship established between these Silurian rocks of the South Orkneys and the Silurian rocks occurring on both sides of the main Andean chain in Bolivia and Northern Argentina,\* and in the province of Buenos Aires, in the Sierra Tandil and Sierra de la Ventana.

More soundings in the area between the South Orkneys, Cape Horn, and South Georgia would probably shed further light on this problem; and they are also much to be desired between the South Orkneys and Graham Land, where rocks of an entirely different type occur, *viz.*, plutonic and metamorphic rocks on the Pacific side, and on the eastern side Lower Tertiary rocks, similar to those of Patagonia.

At all events, the presence of isolated islands such as the South Orkneys and South Georgia, composed of sedimentary rocks, mostly inclined at high angles, and surrounded by deep water, proves a former much greater extension of land in this area. If they formed part of the Tertiary Antarctica postulated by Professor H. F. Osborn and many others,† to explain the floral and faunal relationships of S. America, S. Africa, and Australia, it is evident from the recent soundings ‡ that the changes of level in sea and land in this region have been very considerable: it would now require an elevation of nearer 20,000 feet than the 10,000 assumed by Professor Osborn as necessary to unite S. America with Antarctica.

\* Cf. Suess, *La Face de la Terre*, vol. i. pp. 684-686.

† H. F. Osborn, *Science*, 1900, vol. xi. p. 566.

‡ Andersson, *loc. cit.*, and "Second Voyage of 'Scotia,'" *Scot. Geog. Mag.*, Jan. 1904.

*Note by Dr Peach on Specimens from the South Orkneys.*

Two specimens of black shale, Nos. O14 and O15, from the South Orkneys, have been submitted to me by Dr Pirie for examination.

No. O15.—In addition to some stipes of graptolite, determined by Miss Elles to belong to the genus *Pleurograptus*, there occurs a fragment of another organism, showing a web of dark carbonaceous matter, with a succession of sub-parallel ridges which appears to belong to a Phyllocarid crustacean, probably nearly allied to *Discinocaris*.

No. O14 shows the remains of what appears to have been another form of Phyllocarid crustacean, preserved in a dark shining anthracitic substance. What seems to be the carapace is broad and smooth, with faint indications of raised lines directed outwards and forwards on the left side. Where the supposed carapace has broken away in splitting the shale, a succession of bands about  $\frac{1}{2}$  inch broad, and numbering six within about the same breadth backwards, may be observed. These are each ornamented with sub-parallel lines and with broadened posterior margins. Both the carapace and the apparent body segments are abruptly truncated anteriorly in the breaking of the shale.

A wide experience of the black graptolite-shales of the Southern Uplands of Scotland and North Wales, of all horizons, from the Lowest Arenig up to the Wenlock and Ludlow rocks, has shown that, with the exception of a few small hingeless brachiopods and some glass-rope sponges, only the tests of chitinous Phyllocarid crustaceans have been met with. Of these, the genus *Caryocaris* characterises the Arenig, *Pinnocaris* the Lowest Hartfell shales (Caradoc), *Discinocaris* and *Peltocaris* the Lower Birkhill shales (Lower Llandovery), and *Aptychopsis* and *Ceratiocaris* (the Wenlock) dark graptolitic shales.

The general style of ornament found in the test of most of the above genera is that of the sub-parallel raised lines, which may be arranged on the carapaces almost concentrically to rudely simulate lines of growth in some forms; but in *Ceratiocaris* they run longitudinally backwards. On specimen O15 there appears to be a slight curve in the raised lines similar to what occurs in *Discinocaris gigas*, Jones and Woodward, and figured in their monograph.

This form has only been found in the Birkhill shales (Llandovery) of Moffat, while the graptolite *Pleurograptus* found on specimen O15 shows that this specimen belongs to a lower horizon (Caradoc). *Pleurograptus linearis*, Carruthers, is the zonal form of the Uppermost zone of the Lower Hartfell shales (Caradoc) of Moffat. I do not, therefore, consider that any of the specimens could be determined either specifically or generically; but if these organic remains belong, as they appear to do, to Phyllocarid crustaceans, their occurrence along with graptolites in black shales in both the northern and southern hemispheres would signify more than a near coincidence.

(Issued separately March 30, 1905.)

**A Possible Explanation of the Formation of the Moon.**By **George Romanes, C.E.**

(Read November 21, 1904.)

The subject of the moon's development has been dealt with by Professor G. H. Darwin by means of a highly abstruse mathematical analysis, which the present writer cannot pretend to be able to discuss. He wishes to point out, however, that Professor Darwin's theory requires the assumption that earth and moon formed, at one time, a single highly-heated fluid mass; the theory being that the moon was thrown off by centrifugal force aided by the sun's tidal influence and synchronous vibratory motion of the fluid mass.

There is another possible explanation of the formation of the moon, that gets over many difficulties in explaining its features.

It is to suppose that earth and moon were separately formed out of different parts of the same nebula, or crowd of small parts which were at one time circulating round their common centre of mass at great varieties of distances, in every plane and with every degree of eccentricity, the whole having a balance of moment in the plane and direction in which earth and moon are now revolving. The portions near the centre would tend to collect there to form the earth, while the outer portions gradually collected into larger and larger masses to form the moon, and in doing so built up its mass in such a way as to leave a record, which it is the purpose of this paper to endeavour to interpret.

Before considering the markings on the moon's surface, the writer wishes to show, as clearly as he can, how such a result as the building up of the moon in this way is possible. All bodies circulating round the earth are subject not only to the influence of the earth, but also that of the sun and of each other; which must have caused great irregularities in their motions, and increased the chances of collisions among each other, and thus gradually reduced

the number and increased the average size ; and the largest body, the moon, would capture most matter in this way.

There is a very important difference between the collisions of bodies moving in the *same* direction of revolution and of those moving in *opposite* directions, which must be kept in view. The former are caused principally by bodies attracting each other ; they are not destructive ; and while they cause the mean distances of the orbits to be diminished, they tend to make these orbits *less* eccentric. The latter occur at high speeds ; they are highly destructive, and cause the orbits to become *more* eccentric. The moon's moment of momentum round the earth proves that it has been built up principally of bodies having the same direction of revolution.

The several portions which now form the moon must have long had independent orbits round the earth, and many may have grown to a considerable size before being caught by the moon. The moon's mass is now an eighty-first part of that of the earth, and at distances of 23,800 miles (more or less, according to circumstances) from the moon its influence is equal to that of the earth. Hence, when a small body having an independent orbit round the earth came near the moon, it would be drawn into a subsidiary orbit with the moon's centre as focus, which, with reference to the moon, would be a hyperbola ; and the body might strike or graze the moon's surface, or escape and keep on an orbit round the earth, much modified by the encounter, till some other close approach, when it might be captured.

With regard to bodies being captured by the earth, if two equal masses circulating at the same mean distance in *opposite* directions were to collide, their moments of momentum would be mutually destroyed, they would be highly heated and driven to pieces, and they would fall direct to the earth. So exact a balance as this is against all probability, and the most usual result of such collisions would be to render the resultant orbits more eccentric, and thus give increased chances of further collisions, because they would cross other orbits to a greater extent. Finally, many orbits would be rendered so eccentric as to cause the bodies to graze the earth's atmosphere at each revolution, which would thus reduce the orbit till the earth captured the whole in small pieces, this effect being

aided by the disintegrating influence of the atmosphere and of the earth's tidal attraction. The earth's atmosphere would thus be the first and principal recipient of the heat caused in this way. Direct impacts on the earth would be rare, and their marks would in time be effaced by the various geological influences.

Impacts on the moon, of bodies having independent orbits round the earth, would be of a very different nature; these would often be very direct, and the bodies themselves might be of considerable size, possibly up to 20 miles or more in diameter. Such bodies being built up of many parts loosely held together by their own feeble gravity, would be more like masses of sand and dust than solid stone; hence a grazing impact of such a body on the moon would be like a sand-blast which would liquefy the rock and plough out a straight groove. The utmost velocity the moon can produce by its attraction is 1·476 mile per second, and bodies having orbits round the earth at the same mean distance in the opposite direction would, if they collided, strike it with the velocity of 1·946 mile per second, and it would be struck by bodies having orbits within its own, as well as by others beyond it; thus velocities of impact might range from 1·4 mile to even 2 miles per second on rare occasions. These velocities represent energies capable of raising the temperature of the bodies striking by 5200° Fahr. to 10600° Fahr., or rather of raising the temperature not only of the bodies themselves, but also of much of the moon's surface, to an extent sufficient to liquefy them; while the mechanical force of the impact would cause much of the surrounding surface to be forced up into irregular mountain ranges all round, and cause great splashings of liquid rock from the hollows thus formed, and great surgings to and fro of the liquid rock within them; and no doubt gases would be formed and fly off, till the liquid rock had time to cool.

Besides being struck by single bodies, the moon may often have been struck and grazed by nebulae—that is to say, swarms of small bodies which had sufficient moments of momentum about their centres of mass to keep them from aggregating more closely.

Impacts of large bodies having independent orbits round the sun would be very rare, and it is doubtful if any would leave marks large enough to be seen from the earth.

The writer has been referred to Professor N. S. Shaler's great essay in the Smithsonian *Contributions to Knowledge* to study his views, and to discuss them herein.

Professor Shaler does not discuss the manner of the moon's growth except by a reference to Professor G. H. Darwin's theory, a modified form of which he apparently accepts (page 3 of his essay), and he makes the following assumption on pp. 31-32: "The most reasonable view of the interior condition of the moon when its vulcanoids (craters) were in activity is that it was in a state of essential fluidity with a relatively thin crust." This is making use of a popular idea that the moon, like all other cosmic bodies, must at one time have been so hot as to be fluid. This is not a scientific view, as no proof of it is possible. Professor Shaler makes no attempt to show how the moon became so hot as to be fluid, and on page 48, under "Adjustments of the Surface to Contraction," he gives the following strong evidence that leads to a contrary inference: "On the earth he (the geologist) sees in the ample folds of the sea-basins and of the continents, as well as in many folded mountain chains, what he takes to be evidence of a long-continued accommodation of an anciently cooled crust to a central mass which is ever losing heat. On the moon he finds what, in proportion to the size of that sphere, is surely not the hundredth part of such action. . . . What then is the meaning of this startling diversity in the orogenic history of the two spheres?" Also on page 4 Professor Shaler states the relative densities of the moon and earth as six to ten; but he does not draw the inference that the moon has been less subjected to gravitational compression, and therefore has had less internal heating than the earth; indeed, the influence of mass in causing the heating of cosmic bodies seems not to have been sufficiently present to his mind. Professor Shaler requires the presence of fluid lava a short way below the surface of the moon to explain the formation of vulcanoids (craters) by a rise and fall of liquid lava through holes in the crust, which he supposes have been formed by the help of gases like slow boiling; and he accounts for the formation of terraces on the inside of the surrounding walls of the vulcanoids by the different levels at which the lava successively stood. These terraces are very irregular, and by no means continuously horizontal, and they

occur outside the crater walls as well as inside ; but the writer does not find that Professor Shaler accounts for those on the outside. In discussing G. K. Gilbert's hypothesis, that the craters were due to impacts, he rejects it, because, he says in a footnote (page 12), the masses or bolides would have struck with velocities that would have raised their temperature more than 150,000 degrees (scale not stated). He (probably after Gilbert) is thinking of velocities of  $7\frac{1}{2}$  miles per second or more. The possibility of such masses (bolides) having always been in company with the earth and moon has not occurred to him ; and he objects (page 12) that such impacts would have caused much cracking of the moon's surface—thinking, no doubt, of hard masses striking stone, but not considering that the bodies striking might have been more like heaps of loose material moving generally with the velocity of only  $1\frac{1}{2}$  mile per second.

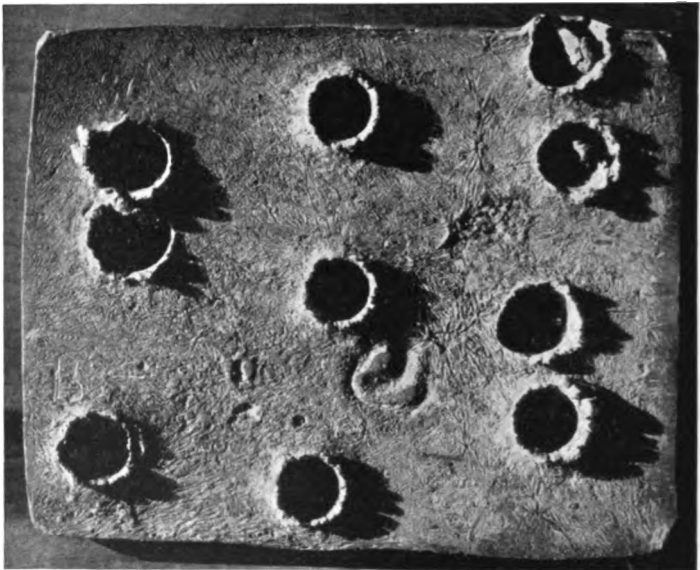
Again, Professor Shaler thinks that the maria must have been formed, each by the impact of one or more bolides with planetary velocities (page 17), and he considers the great amount of melting of rock they could produce ; but he does not sufficiently consider that a mass moving at such velocity, instead of melting a great quantity of rock, would melt only a moderate quantity, and spend much of its energy in driving the melted rock right away from the moon in a great splash.

Professor Shaler has taken an immense amount of care, and given many years of labour to accumulate facts as to the moon, and he has stated those facts with great impartiality for the benefit of science ; but in explaining the causes at work in producing them, the writer thinks he has started from wrong premises, and found difficulties that disappear when the true causes become known.

The writer will now state his views as to the cause of some of the principal lunar formations. He thinks that the circular or slightly elliptical craters have been formed by the impacts of bodies belonging to the earth's system, of all sizes up to 20 miles or more in diameter. The floors of these craters are in general much depressed below the surrounding surface, and the crater walls are sometimes of such great elevations as 17,000 feet or more above the floors, while the diameter of the craters varies from the



smallest size that can be seen up to at least 140 miles. Some of the crater floors are above the general level, such as Gassendi close to Mare Humorum, and some more nearly at the same level, when they are situated in or near the maria. The general characteristic, however, of those that are not near the maria is to have their floors much depressed, even to the extent of thousands of feet in some cases. The forms of these craters can be fairly well imitated by firing bullets into a mass of lead. The cavity thus formed has always a raised burr round it, is much larger in diameter than the



bullet, and is generally fairly round even when the bullet strikes obliquely, if not so obliquely as to glance off altogether. There is always a small cone left in the cavity, and the surface of the whole cavity can be seen to glow red-hot immediately after the shot is fired. In the case here illustrated the bullets were elongated leaden ones .22 inch in diameter, and the cavities were .44 inch diameter. The three shots down the middle were fired perpendicular and the others obliquely, but not at measured angles; however, the mark on the right of the centre was roughly estimated to be at about  $45^{\circ}$ .

The velocity of impact in these experiments is not known, but might have been about 1200 feet per second; and, no doubt, bullets fired at higher velocities would form cavities wider in proportion to the bullet. A velocity of 1200 feet per second is from one-sixth to one-ninth of the velocities we are dealing with in the case of the moon, and the body striking is a compact one; whereas, as has been shown above, the bodies striking the moon were by no means compact; and the circumstances are so different that the analogy between the bullet marks and the lunar craters will not be very close. However, the experiments make it clear that cavities so formed on the moon's surface may be expected to be greatly larger in diameter than the body that caused them, and generally fairly round.

The great radial streaks, notably those from Tycho, are probably caused by splashes of liquid rock comminuted and blown out by the gas formed at the same time. Their great brilliancy at full moon is probably due to the surface being rough—that is, covered with small particles, and not appearing vitrified like the rest of the moon's surface. As no shadows can be seen at full moon, rough surfaces must then appear brighter than under indirect illumination. Although these streaks extend to great distances, such as 1000 miles, it is obvious that the initial velocity, necessary to project them from their source to any other part of the moon's surface, is much less than the moon caused by its attraction on the bodies that produced them; and therefore this cause of them is quite within the limits of possibility.

The irregular terraces or wrinkles, seen on both the inner and the outer slopes of the circular mountain rings, and particularly well seen in Copernicus, are probably caused by the powerful side thrust that raised them up.

The cones inside the craters are evidence that part of the body striking was unmelted, and was piled up in a heap or heaps near the centre, and cemented together by the liquid rock surging to and fro. The absence of cones in some craters shows that the whole has been melted, either at first or by lava from other sources, such as molten lava being thrown in by the violent surgings of the maria when they were formed.

The Valley of the Alps has all the appearance of having been

ploughed out by the grazing impact of a moonlet. Its floor is level with Mare Imbrium, and its sides are nearly vertical; hence it may have been scoured through by white-hot lava from the Mare Imbrium when that mare was violently surging on its formation. There are numerous features of the nature of the Valley of the Alps on the moon's surface, notably in the region of craters Albategnius and Ptolemæus, and also in the region south of Mare Serenitatis. These are arranged in series of parallel lines, and may be due to the grazing impact of swarms.

A large portion of the moon's surface is covered with the maria, some of which have a roughly circular outline, such as Mare Imbrium, Mare Serenitatis, and Mare Crisium, which seems to indicate that each is the result of some single great catastrophe. These may have been formed by the impact of a nebula or swarm of bodies; and the mountain ranges bordering them, such as the Alps and Apennines bordering Mare Imbrium, may have been the result of the same catastrophe which formed the sea they are associated with. These mountain ranges have all the appearance of masses of matter thrown down in a sidelong heap and splashed over with liquid rock. There is much appearance on the surface of the maria of their having been in commotion, and indeed they must have been in violent commotion when they were formed. Many long ridges on their surfaces show that they have not quite come to a level surface till they were too viscous to do so. These ridges seem to indicate a creeping together of the lava from opposite sides when it was nearly solid. The surfaces of the maria are generally darker than the rest of the moon's surface, owing, no doubt, to their comparative smoothness rather than to any difference in the kind of rock; obviously, a polished surface would look black at full moon, if not at the centre of its disc.

A very interesting feature, that may be noticed more or less on all parts of the moon's surface, is the immense number of old craters and mountain ranges that have been overwhelmed by the lava of the maria, or battered down by more recent formations; which shows that the formation of those craters and maria is no casual occurrence, depending on the chance meeting of meteors from outer space, but the natural process by which the moon's mass has been built up.

It may finally be suggested that the sudden accession of large quantities of matter, such as that of a mare, to the moon's surface, might slightly alter its balance, and cause it to turn a somewhat different face to the earth. The frequent occurrence of such changes would be in favour of its assuming the true form of equilibrium even although it has never been fluid; and all influences to which it has been subjected would have the same tendency.

The writer has heard, since this paper was read, that former attempts have been made to illustrate the formation of lunar craters by firing bullets; but he has heard of no former attempt to explain the whole formation of the moon's mass as due to impacts of bodies which have always been part of the earth's system, in the manner explained above.

He wishes to state that he is greatly indebted to Mr Heath of the Royal Observatory for help of every kind in gaining information, and for the slides which were shown in illustration of this paper.

(*Issued separately March 30, 1905.*)

**On Pennella : a Crustacean parasitic on the Finner Whale**  
(*Balænoptera musculus*). By Sir William Turner, K.C.B.,  
LL.D.

In this memoir the author described a *Pennella* found attached to the back of a *Balænoptera musculus*, specimens of which were given to him in 1903 by Mr Chr. Castberg. The specimens were of the same species as the *Pennella balænopteræ* described by Koren and Danielssen in 1857, and found infesting *B. rostrata*. The species is a giant Copepod, and the longest examples measured about  $12\frac{1}{2}$  inches.

The description included a short historical introduction to the genus, an account of the external characters and internal anatomy of the species, its comparison with other species, and the attachment to one of the specimens of *Conchoderma Virgata*. The memoir, with illustrations, will appear in the *Transactions* of the Society.

(Issued separately March 30, 1905.)

**The Diameters of Twisted Threads, with an Account of the History of the Mathematical Setting of Cloths.**  
 By Thomas Oliver, B.Sc. (Lond. & Edin.). *Communicated*  
 by Dr C. G. Knorr.

(MS. received January 27, 1905. Read March 20, 1905.)

During the last generation the idea of reducing the "setting" of cloths to mathematical accuracy has been gradually taking hold of the minds of thinking men in the various textile trades. That this end is perfectly attainable is perhaps an open question, but there can be no doubt that the investigation of such problems must lead to a more satisfactory knowledge of the factors which determine the construction of fabrics.

The base from which these "setting" theories begin is naturally the diameter of the thread, since the "set" of a cloth, *i.e.* the number of threads in some unit distance, usually the inch, made in any one weave or scheme of interlacing, is inversely proportional to the diameter of the thread employed in the construction of the cloth. Clearly, then, the first step in this investigation must be the determination of the diameters of the numerous "counts" or numbers of yarns in the various materials which are in use in the textile industries. But this is by no means such an easy task as it may seem at first sight. The diameter of a thread is neither easily measured at any one section, nor a constant quantity throughout its length. Especially is this the case with woollen yarns, in which the fibres projecting from the body of the thread in every conceivable direction renders the averaging up of the section a tedious and often unsatisfactory operation.

The history of the mathematical setting of cloths is, however, not confined to the last generation. The earliest record of a systematic attempt to attain this end is preserved in the British Museum in a copy of *Mathematical Sleaving Tables*, calculated by Mr Joseph Beaumont, a writer on the Irish linen trade in 1712. He recognised that the setting of cloths should be based

on the diameter of the thread, although he erroneously applied this term, not to the actual diameter, but to what was really the pitch of the threads in the warp, *i.e.* the diameter of the thread plus the space between the threads. We find another stepping-stone in the evolution of this subject in comparative setting or caaming tables included in Murphy's classical *Art of Weaving*, published about the beginning of last century. It is, however, not too much to say that "rule of thumb" held practically undisputed sway in this field until thirty years ago.

About 1875 the late Mr Robert Johnstone, of Galashiels, a shrewd Scotch designer, possessed of remarkable powers of observation, put out a little work entitled *Designer's Handbook*, in which he gave a rule to set webs in the reed. After stating the rule, he appends the following note:—"I have often been asked why the square root of the size weight of a yarn multiplied by the numbers stated in this rule gives the number of the reed which should be used. I answer the question in this way:  $\frac{1}{2}$  of an inch divided by the square root of any weight of yarn is equal to the diameter of it. Now if that is so, the diameter of 1 cut yarn will be  $\frac{1}{2}$  of an inch, and that of 25 cut will be  $\frac{1}{4\sqrt{5}}$  of an inch." The yarns were numbered on the Galashiels system. The above statement, though rather loosely worded, is the first instance, so far as the present writer is aware, in which the diameter of a yarn was employed in its proper sense as a basis on which the "set" for a given yarn might be determined. The conclusions arrived at are all the more remarkable since Mr Johnstone must have deduced them by observation on cloths alone, as he had no means of making micro-measurements. Besides, neither he nor his fellow-workmen could have been burdened with much education, nor had he the advantage of consulting literature on the subject, since there was none. Johnstone's rule is held in high repute amongst Scotch designers, and it is safe to say that it gives very good results for the average Scotch woollen cloths, for which the rule was intended.

The great epoch in this subject, however, occurred in 1880, when the late Mr Thos. R. Ashenurst, then head of the textile department of Bradford Technical College, gave out the results of his experiments and deductions to the textile public. Mr Ashenurst's experiments consisted of measuring the diameters of

a large number of threads in different sizes and materials with a micrometer, and taking the average for each yarn number. These numbers are tabulated in his work on *Textile Calculations*, published in 1884.

Subsequently he found that the following empirical formula gave results closely approaching to the number tabulated from his experiments. The diameter, expressed as a fraction of an inch, is equal to the reciprocal of the square root of the number of yards per lb., with a deduction of 10 per cent. from the square root for worsted, cotton, linen, and silk yarns, while for woollen yarns a deduction of 16 per cent. should be made. This deduction is sometimes spoken of as the allowance for surface fibre, which is, however, quite erroneous, as the surface fibre is far too variable a quantity to be reckoned as proportional to the diameter or any other attribute of the thread. It has really no physical meaning whatever. The reason that there should be a deduction is purely a mathematical one, *i.e.* to make one number correspond with another. Ashenhurst was helped towards the explanation of his diameter rule by Mr T. F. Bell, of Belfast, in 1889. The full correspondence on this matter will be found in the *Textile Educator*, February 1889, of which Mr Ashenhurst was the editor. There is little doubt that it is a very useful formula, and gives very good results when applied, in conjunction with his other setting formulæ dealing with variations in weave (a subject, however, outside the scope of this paper), to the average run of cloths made in Yorkshire, where the practice is to set cloths much closer than is customary in the Scotch trade. There has been very little done in this field of research since the time of Mr Ashenhurst's experiments. The statements enunciated by him have been repeated by lecturers, and have figured in text-books and examination papers for over twenty years, until textile students are beginning to consider these statements as absolute as the inverse square law of gravitation, while practical men rock over to the other extreme, treating the whole matter as theoretical humbug, and people generally do not trouble to investigate the subject further. This course is clearly not in accordance with the scientific spirit of inquiry permeating other branches of industry at the present time. While all honour is



due to the memory of Mr Ashenhurst in connection with his pioneer labour in this field of research, to recognise that it was only a forward step in the evolution of a difficult subject in no way detracts from that honour. Textile students would do well to consider the foundation on which Ashenhurst's assumptions rest, and to investigate the limitations to which they are subject, as set forth in his own words in the second section of his *Textile Calculations*; so that by the aid of experiment and reasoning the next twenty years may be more fruitful in results than the same period which has just passed.

As the author's experiments on the absolute diameters of threads do not admit of generalisation at the present stage, we shall pass on to consider what is the main subject of this paper, viz., the diameter of a twisted thread compared with the diameter of its component singles. The subject is admittedly a difficult one both on the analytical and experimental sides, which may doubtless have deterred textile writers from discussing it. But it is, nevertheless, a logical consequence of Ashenhurst's teaching.

Single threads for purposes of calculation may be assumed to be flexible cylinders if not subjected to lateral stress, since to this form single threads approximate according as they approach perfection in structure. Writers on textile calculation have always tacitly reckoned twisted threads to have the same form also, in order to avoid the mathematical difficulties which more complex forms must introduce. If the thread is twofold, i.e. consists of two threads twisted together, then its diameter is considered to be the same as the diameter of a single thread of twice the weight and volume per unit length, or twice the sectional area. A little consideration, however, will show that this is an erroneous idea, and sufficient in many cases to vitiate the results arrived at. It is very evident from fig. 1 that a twofold twist consists of two spirals interlocking each other, a form differing very markedly from that of the cylindrical single thread.

The dimension of a thread which is of practical importance in the theory of cloth-setting is its horizontal projection, since in all ordinary cases cloth is constructed by the interlacing of two series of threads which cross each other at right angles. The series which is stretched lengthways in the loom is called the "warp,"

while the other series, which interlaces the warp transversely according to some definite scheme or weave, is called the "weft." Therefore the number of threads which can be crowded into a given distance in a horizontal plane, *i.e.* into cloth, must be dependent upon the horizontal dimensions of the threads. If a single thread is stretched horizontally, it is evident that its



FIG. 1.—Horizontal Plan of Thread.

horizontal projection is a rectangle if perfectly even spun, but in the case of a twofold twist the outline of the projection consists of two overlapping curves, each of which will be readily recognised as a curve of sines.

At section A of fig. 1 the maximum width = two diameters of the single thread; at section B, the minimum width = one diameter only; while between A and B the projection width assumes every value from two diameters to one diameter as we pass from A to B.

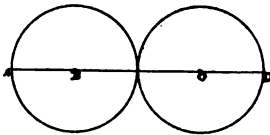


FIG. 2.—Section A.

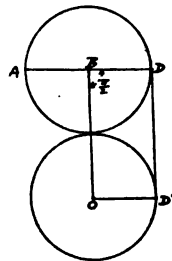


FIG. 3.—Section B.

In passing beyond B on to D it is evident that the same values will be reached, but in the reverse order, until at D the projection width is again  $2d$ , where  $d$  = the diameter of the single thread. The next part of the problem is to find the average horizontal projection, because if we warp a large number of threads or weave a large number of picks (as the weft threads are technically termed) side by side, the probability is that the broad parts of

some of the threads will come against the narrow parts of others in such a way that they will average up and fill the same space as an equal number of threads of a hypothetical yarn uniform throughout its length and with a diameter equal to the mean projection width of the real yarn. To find this mean value we may proceed in one or other of two ways. (1) The most expeditious method is to employ the integral calculus. We may consider, for purposes of calculation, that the twist is generated by keeping one thread stationary and rotating the other about the axis of the first as centre. Proceeding from section A to section B, the angle of twist grows from  $0^\circ$  to  $90^\circ$ , i.e. through  $\frac{1}{4}$  turn of twist.

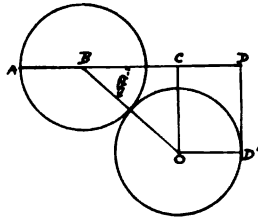


FIG. 4.—Section C.

If we call the angle of twist  $\theta$  and consider any intermediate section C, the horizontal projection is AD or AB + CD + BC, but AB + CD =  $d$ , the diameter of the single thread, and BO =  $d$

$$\therefore BC = d \cos \theta$$

$$\therefore AD = d (1 + \cos \theta)$$

And the sum of all the sections =  $d \int (1 + \cos \theta) d\theta$  between the limits  $\theta = 0$  and  $\theta = 90^\circ$  or  $\frac{\pi}{2}$  radians,

$$\text{or } d \int_0^{\frac{\pi}{2}} (1 + \cos \theta) d\theta = d \left[ \theta + \sin \theta \right]_0^{\frac{\pi}{2}} = d \left( \frac{\pi}{2} + 1 \right)$$

$$\begin{aligned} \therefore \text{the mean width of projection} &= \frac{\text{Integral}}{\frac{\pi}{2}} = \frac{\frac{\pi}{2} + 1}{\frac{\pi}{2}} d = \left( 1 + \frac{\pi}{2} \right) d \\ &= 1.637d \end{aligned}$$

*A graphical method of solving the problem.*—The following graphical method will be intelligible to those who are not familiar

with the calculus. Plot to a large scale on squared paper the values of  $\theta$  as abscissae and the corresponding values of AD as ordinates, and draw a curve through the tops of the ordinates in the usual way. The values of AD may be found by drawing figures for the ten values of  $\theta$ , i.e.  $0^\circ, 10^\circ, 20^\circ \dots 90^\circ$ , and

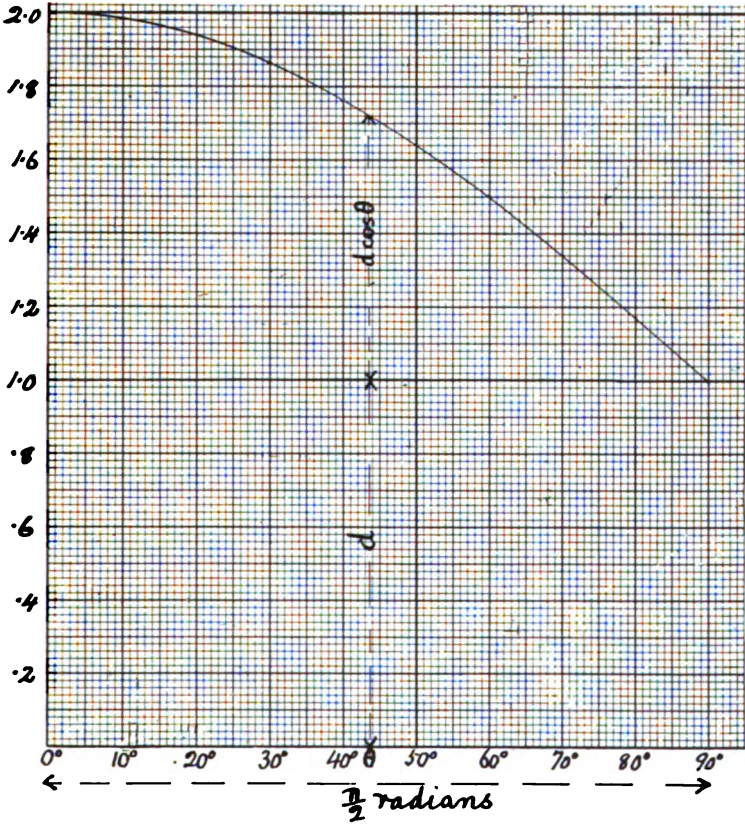


FIG. 5.

measure off the lengths for each case, or the values of  $\cos \theta$  may be taken from a four-figure table of cosines.

The area inclosed by the base line, the curve, and the two end ordinates may be found by the planimeter, or any of the rules for summing areas in mensuration. Of the latter, the mid-ordinate rule, being the simplest and sufficiently accurate, might be used.

The mean projection width = the mean value of the mid-ordinates of the nine strips into which the diagram is conveniently divided.

$$\begin{aligned}
 &= \frac{d}{9}(1.996 + 1.966 + 1.906 + 1.819 + 1.707 + 1.574 + 1.423 \\
 &\hspace{15em} + 1.259 + 1.087) \\
 &= 1.637d.
 \end{aligned}$$

Now, if the twist had been taken as equivalent to a single thread of twice the sectional area of one of the component singles, the conclusion would have been arrived at that the projection width =  $\sqrt{2}d$  or  $1.414d$ . Thus an error of about 14 per cent. would have been made, following the usual assumption.

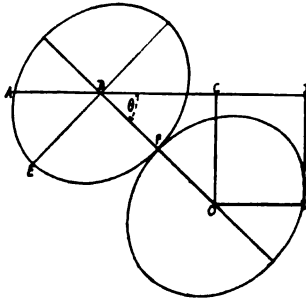


FIG. 8.

In practice, however, it will be found that the discrepancy is not so great as shown above, because, for the sake of simplicity in introducing the subject, a hypothetical case has been considered which would never arise in practice, *i.e.* an unstretched thread. When yarn is formed into a warp it is necessary that it should be subjected to a relatively large longitudinal stress in order to secure uniformity in weaving. The result of this is that the spirals in the twist tend to become straight, and consequently each single thread exerts a transverse pressure on the other along the spiral line of contact: in practice, contact takes place along, not a line, but a surface, the extent of which depends upon the compressibility of the material of which the thread is composed. A thread also presents this deformation to a lesser degree, even when not subjected to longitudinal stress. Because, in the process of form-

ing the twist on the throstle frame, the threads are under considerable tension, which strains the cylindrical singles. When the stress is relieved, after the thread passes away from the throstle, the friction between the rough surfaces of the singles prevent to some extent the natural elasticity of the material from bringing the thread back to its original form. The single threads no longer present a circular cross section, but elliptical, with the minor axes of the ellipses everywhere at right angles to the line or surface of contact. The mean projection width is now more difficult to find, since the integral is of a higher order. Section C is now as shown in fig. 6.

Let  $BE = a$ ,  $BF = b$ ,  $BA = r$ .

The polar equation to the ellipse when  $\theta$  is the angle CBO or angle of twist is

$$\begin{aligned} \frac{1}{r^2} &= \frac{\sin^2 \theta}{a^2} + \frac{\cos^2 \theta}{b^2} \\ \therefore r &= \frac{ab}{\sqrt{b^2 \sin^2 \theta + a^2 \cos^2 \theta}} \\ &= \frac{b}{\sqrt{\cos^2 \theta + \frac{b^2}{a^2} \sin^2 \theta}} \\ &= \frac{b}{\sqrt{1 - \left(1 - \frac{b^2}{a^2}\right) \sin^2 \theta}} \\ &= \frac{b}{\sqrt{1 - e^2 \sin^2 \theta}} \quad \text{where } e^2 = 1 - \frac{b^2}{a^2} \end{aligned}$$

Therefore  $AB$  or  $CD = \frac{b}{\sqrt{1 - e^2 \sin^2 \theta}}$

and  $BC = BO \cos \theta = 2 b \cos \theta$

But the projection width =  $AD = AB + CD + BC$   
 $= 2r + 2b \cos \theta$

$$= 2b \left( \frac{1}{\sqrt{1 - e^2 \sin^2 \theta}} + \cos \theta \right)$$

Then the sum of all the sections between the limits  $\theta = 0$  and

$\theta = 90^\circ$  or  $\frac{\pi}{2}$  radians  $= 2b \int_0^{\frac{\pi}{2}} \left( \frac{1}{\sqrt{1 - e^2 \sin^2 \theta}} + \cos \theta \right) d\theta$

$$= 2b \left( \int_0^{\frac{\pi}{2}} \frac{d\theta}{\sqrt{1 - e^2 \sin^2 \theta}} + \int_0^{\frac{\pi}{2}} \cos \theta d\theta \right)$$

The first integral is evidently a complete elliptic function of the first order, and therefore not expressible in terms of elementary transcendents. For convenience this function will be referred to, as is usual, by  $F_1$  and its value taken from tables or determined by quadrature for any value of  $e$  (or  $e^2$  preferably).

$$\int_0^{\frac{\pi}{2}} \cos \theta \, d\theta = \left[ \sin \theta \right]_0^{\frac{\pi}{2}} = 1$$

$\therefore$  the sum of all the sections =  $2b(F_1 + 1)$

$$\text{and the mean projection width} = \frac{2b(F_1 + 1)}{\frac{\pi}{2}} = \frac{4b}{\pi}(F_1 + 1)$$

Instead of using tables of elliptic functions, it is instructive to use approximate methods of solution.

(1) Expanding the radical  $\frac{1}{\sqrt{1 - e^2 \sin^2 \theta}}$  by the Binomial Theorem, the series  $1 + \frac{1}{2}e^2 \sin^2 \theta + \frac{3}{8}e^4 \sin^4 \theta + \dots$  is obtained which is uniformly convergent from  $\theta = 0$  to  $\theta = \frac{\pi}{2}$  radians, since  $e^2 < 1$ . Integrating this series term by term and using the formula

$$\int_0^{\frac{\pi}{2}} \sin^n \theta \, d\theta = \frac{(n-1)(n-3)\dots 1}{n(n-2)\dots 2} \frac{\pi}{2}$$

the value of the function is obtained as

$$\int_0^{\frac{\pi}{2}} \frac{d\theta}{\sqrt{1 - e^2 \sin^2 \theta}} = \frac{\pi}{2} \left( 1 + \frac{e^2}{4} + \frac{9e^4}{64} + \dots \right)$$

which can be easily evaluated for all values of  $e^2$  and to any degree of approximation by taking sufficient terms of the series. From the nature of the problem, it is, however, not only unnecessary but misleading to use more than three or four significant figures.

(2) *The graphical solution.*—Calculate the value of the expression  $2b \left( \frac{1}{\sqrt{1 - e^2 \sin^2 \theta}} + \cos \theta \right)$  for 10 values of  $\theta$ , viz.,  $0^\circ$ ,  $10^\circ$ ,  $20^\circ$ ,  $\dots$ ,  $90^\circ$ , keeping  $e^2$  constant, say  $\cdot 1$ . Plot these values as ordinates and  $\theta$  as abscissæ. Draw a curve through the plotted points. The mean height of the diagram gives, as before, the mean projection width for  $e^2 = \cdot 1$ . Plot out the results on the same sheet for  $e^2 = \cdot 2, \cdot 3 \dots$  and the different curves on the same diagram will render evident to the eye at a glance how the

projection width varies with the square of the eccentricity of the elliptical section.

These curves are shown in fig. 7.

The comparison of these results with that obtained by considering the thread in its unstrained condition is beset with difficulties. The volume of the thread must necessarily be less

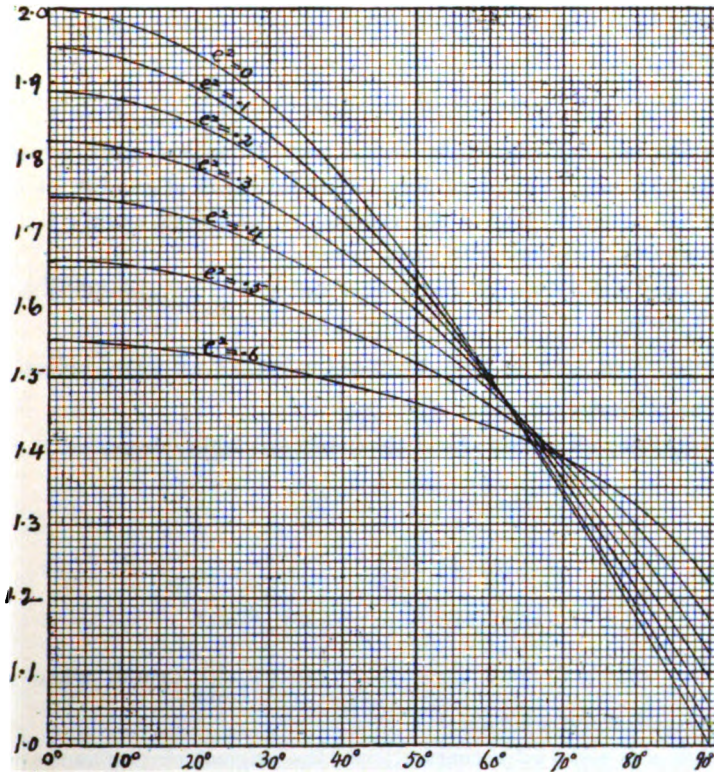


FIG. 7.

in the strained than in the unstrained condition, because (1) the yarn will stretch and thus decrease its sectional area; (2) each single thread is subjected to lateral compression. The latter cause, however, will not greatly affect the volume unless the twist is hard, as the fibres are free to a considerable extent to move away from the surface of compression. The amount of this compression cannot be arrived at by *a priori* reasoning, but must be



the subject of experiment. The results of the author's experiments give reasonable ground for the belief that the law of compression is such that  $a + b = d$  to a first approximation if  $e^2$  is not  $> .6$ , where  $a$  and  $b$  are the semi-major and semi-minor axes respectively of the elliptical section, and  $d$  the original diameter of the unstrained single thread. In any case it is instructive to work out the results for this hypothetical case. This is practically equivalent to reckoning the perimeter constant if  $e$  is not large.

*Proof.*—The perimeter of an ellipse =  $4a \int_0^{\frac{\pi}{2}} \sqrt{1 - e^2 \sin^2 \theta} d\theta$  which is a complete elliptic function of the second order, values of which may be obtained from tables for values of  $e$  and  $\theta$ . But as the compressibility of the material is not known exactly, it is unnecessary to work with exact values.

$$\sqrt{1 - e^2 \sin^2 \theta} = 1 - \frac{1}{2}e^2 \sin^2 \theta - \frac{1}{8}e^4 \sin^4 \theta \dots \text{(by Binomial Theorem).}$$

Integrating term by term between the limits  $\theta = 0 + \theta = \frac{\pi}{2}$  radians.

$$\text{The perimeter} = 2\pi a \left(1 - \frac{1}{4}e^2 - \frac{3}{8}e^4 \dots\right)$$

Neglecting all powers of  $e$  of the fourth and higher degree

$$\begin{aligned} &= 2\pi a \left(1 - \frac{e^2}{4}\right) \\ &= \pi a + \pi a \left(1 - \frac{e^2}{2}\right) \\ &= \pi(a + b) \qquad \because b = a\sqrt{1 - e^2} \\ &= a \left(1 - \frac{e^2}{2}\right) \text{ approx. when } e \end{aligned}$$

is small, and if  $a + b = d$

then  $\pi(a + b) = \pi d$  a constant, viz., the original circumference of the single thread.

Substituting for  $a$  in  $a + b = d$

$$b = \frac{\sqrt{1 - e^2}}{1 + \sqrt{1 - e^2}} d \qquad \because a = \frac{b}{\sqrt{1 - e^2}}$$

$$\therefore \text{mean projection width of strained thread} = \frac{4}{\pi} (F_1 + 1)b$$

$$= \frac{4}{\pi} (F_1 + 1) \frac{\sqrt{1 - e^2}}{1 + \sqrt{1 - e^2}} d$$

TABLES OF FUNCTIONS.

$e^2$	0	.1	.2	.3	.4	.5	.6	.7	.8
$F_1$	1.571	1.612	1.660	1.713	1.777	1.854	1.950	2.076	2.257
$F_{1+1}$	2.571	2.612	2.660	2.713	2.777	2.854	2.950	3.076	3.257
$\frac{4}{\pi}(F_{1+1})$	3.274	3.326	3.398	3.455	3.537	3.634	3.757	3.917	4.148
$1 - e^2$	1.	.9	.8	.7	.6	.5	.4	.3	.2
$\sqrt{1 - e^2}$	1.	.9487	.8944	.8367	.7746	.7071	.6325	.5477	.4472
$1 + \sqrt{1 - e^2}$	2.	1.9487	1.8944	1.8367	1.7746	1.7071	1.6325	1.5477	1.4472
$\sqrt{1 - e^2} / (1 + \sqrt{1 - e^2})$	.5	.4967	.4722	.4555	.4364	.4142	.3875	.3538	.3091
Mean projection width	1.637 <i>d</i>	1.619 <i>d</i>	1.600 <i>d</i>	1.574 <i>d</i>	1.546 <i>d</i>	1.505 <i>d</i>	1.456 <i>d</i>	1.387 <i>d</i>	1.282 <i>d</i>
$(1 + \sqrt{1 - e^2})^2$	4.	3.799	3.587	3.375	3.150	2.914	2.662	2.397	2.094
$\sqrt{1 - e^2} / (1 + \sqrt{1 - e^2})^2$	.2500	.2498	.2493	.2479	.2459	.2427	.2375	.2285	.2186
Sectional area of thd. $\pi d^2$	.2500	.2498	.2493	.2479	.2459	.2427	.2375	.2285	.2186

$$\text{The sectional area of thread} = \pi ab = \frac{\pi b^2}{\sqrt{1 - e^2}} = \pi d^2 \frac{\sqrt{1 - e^2}}{(1 + \sqrt{1 - e^2})^2}$$

TABLE showing the variation in the width of projection from  $e^2 = 0$  to  $e^2 = .6$  through  $\frac{1}{4}$  turn of twist (when  $a + b = d$ ).

Values of $\theta$	Values of $e^2$						
	0	.1	.2	.3	.4	.5	.6
0°	2.000 <i>d</i>	1.947 <i>d</i>	1.889 <i>d</i>	1.822 <i>d</i>	1.746 <i>d</i>	1.657 <i>d</i>	1.550 <i>d</i>
10°	1.985 <i>d</i>	1.934 <i>d</i>	1.877 <i>d</i>	1.815 <i>d</i>	1.739 <i>d</i>	1.652 <i>d</i>	1.545 <i>d</i>
20°	1.940 <i>d</i>	1.894 <i>d</i>	1.843 <i>d</i>	1.784 <i>d</i>	1.716 <i>d</i>	1.633 <i>d</i>	1.532 <i>d</i>
30°	1.866 <i>d</i>	1.830 <i>d</i>	1.786 <i>d</i>	1.737 <i>d</i>	1.676 <i>d</i>	1.604 <i>d</i>	1.512 <i>d</i>
40°	1.766 <i>d</i>	1.740 <i>d</i>	1.710 <i>d</i>	1.671 <i>d</i>	1.624 <i>d</i>	1.565 <i>d</i>	1.486 <i>d</i>
50°	1.643 <i>d</i>	1.629 <i>d</i>	1.613 <i>d</i>	1.590 <i>d</i>	1.559 <i>d</i>	1.518 <i>d</i>	1.460 <i>d</i>
60°	1.500 <i>d</i>	1.499 <i>d</i>	1.497 <i>d</i>	1.490 <i>d</i>	1.480 <i>d</i>	1.463 <i>d</i>	1.432 <i>d</i>
70°	1.342 <i>d</i>	1.351 <i>d</i>	1.362 <i>d</i>	1.375 <i>d</i>	1.384 <i>d</i>	1.393 <i>d</i>	1.395 <i>d</i>
80°	1.174 <i>d</i>	1.194 <i>d</i>	1.217 <i>d</i>	1.241 <i>d</i>	1.268 <i>d</i>	1.299 <i>d</i>	1.333 <i>d</i>
90°	1.000 <i>d</i>	1.026 <i>d</i>	1.055 <i>d</i>	1.091 <i>d</i>	1.127 <i>d</i>	1.172 <i>d</i>	1.225 <i>d</i>
Mean Values	1.637 <i>d</i>	1.619 <i>d</i>	1.600 <i>d</i>	1.574 <i>d</i>	1.546 <i>d</i>	1.505 <i>d</i>	1.456 <i>d</i>
Maximum Value	2.00	1.90	1.79	1.67	1.55	1.41	1.26
Minimum Value							

The experimental work of this subject has been greatly facilitated by accessories invented and added to the microscope by Mr George R. Smith, of Bradford, about three years ago. The complete

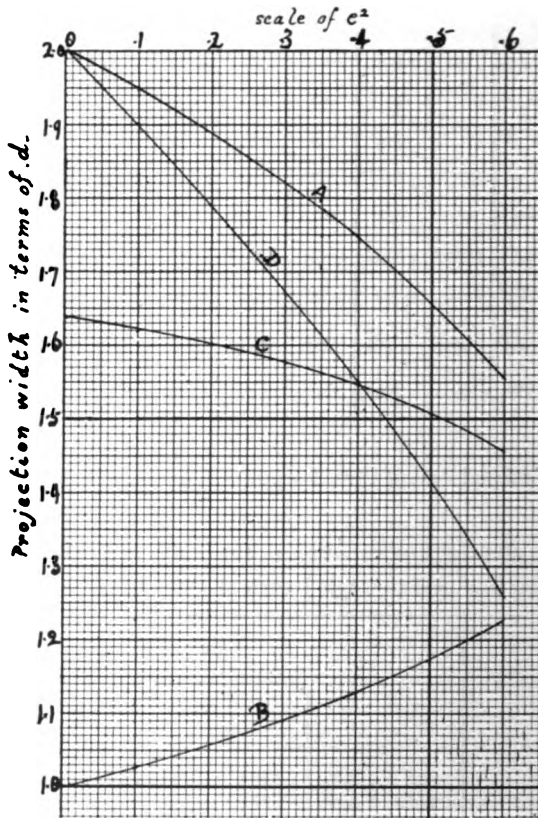


FIG. 8.

Curve A shows the maximum values of the projection width as  $e^2$  changes.  
 " B " minimum " " "  
 " C " mean " " "  
 " D " ratio of the maximum to the minimum.

apparatus is shown in fig. 9. A frame is fixed in grooves under the stage of the microscope, and it can be moved to and fro by a rack and pinion. One end of the frame carries a bell crank lever neatly pivoted, the upright arm of which carries a jaw for securing

one end of the thread, while the other consists of a notched lever on which a weight can be moved along to produce the required tension. The other end of the frame carries a sliding jaw, which can also be rotated by a handle, and the rotations indicated by a counter. Any length of thread from half an inch to four inches can be operated on, the sliding jaw being drawn back to any of the numbers on the base under the stage. The number of turns

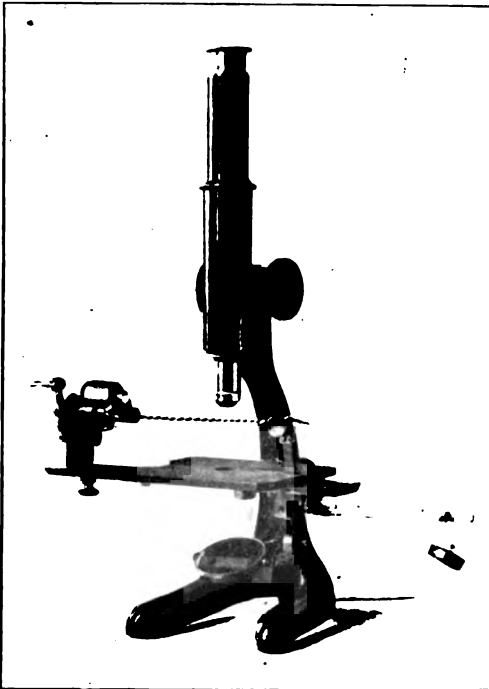


FIG. 9.

of twist is indicated by the counter when all the twist is taken out by turning the sliding jaw. The twist can also be varied at will by the same arrangement. The diameter of the thread is measured by means of an eye-piece micrometer, which is much better for this purpose than a stage micrometer, as with the latter it is impossible to bring the image of the widest part of the thread to coincide with the image of the scale if the thread is moderately thick. Another advantage of this instrument is that

the whole length of thread may be moved across the field of view of the microscope by the rack and pinion underneath the stage.

The following tables show the results of micro-measurements on three representative yarns selected from a large collection, the general tendency of which is to confirm the theory discussed in this paper. The numbers are in micrometer divisions, each of which = .00618 inch. But as the subject is only relative, i.e. the comparison of a twist thread with a single thread, it is unnecessary to translate the readings into absolute measure. The three yarns selected are, (1) a 2/36s worsted with 16 turns per inch, (2) a 50-cut 2-ply woollen yarn with 9 turns per inch, (3) a 2/20s cotton with 9 turns per inch.

(1) 2/36s Worsted.		(2) 50-cut 2-ply Woollen.		(3) 2/20s Cotton.	
Minimum width of Projection.	Maximum width of Projection.	Minimum width of Projection.	Maximum width of Projection.	Minimum width of Projection.	Maximum width of Projection.
1·72	2·32	1·98	2·92	1·54	2·51
1·87	2·48	2·12	3·02	1·88	2·68
2·00	2·85	2·08	3·10	2·05	2·68
2·05	2·47	2·15	3·25	2·00	2·60
1·81	2·20	2·20	3·35	1·95	2·55
1·80	2·24	2·15	3·30	1·72	2·50
1·59	2·32	2·10	3·25	1·85	2·55
1·65	2·45	2·10	3·25	1·82	2·55
1·61	2·10	2·15	3·25	1·72	2·50
1·64	2·05	2·13	3·15	1·65	2·50
10)17·74	10)22·98	10)21·16	10)31·84	10)18·18	10)25·62
1·77 average	2·30 average	2·12 average	3·18 average	1·82 average	2·56 average

	(1)		(2)		(3)	
	From experiments.	From graphs of fig. 8.	From experiments.	From graphs of fig. 8.	From experiments.	From graphs of fig. 8.
<b>Maximum</b> . . .	$\frac{2.30}{1.77} = 1.30$	...	$\frac{3.18}{2.12} = 1.50$	...	$\frac{2.56}{1.82} = 1.41$	...
<b>Minimum</b> . . .	$\frac{2.30}{1.48} = 1.56d$	1.58d	$\frac{3.18}{1.88} = 1.69d$	1.71d	$\frac{2.56}{1.52} = 1.69d$	1.66d
<b>Maximum</b> . . .	$\frac{1.77}{1.48} = 1.20d$	1.21d	$\frac{2.12}{1.88} = 1.13d$	1.14d	$\frac{1.82}{1.52} = 1.20d$	1.17d
<b>Minimum</b> . . .	1.48 divisions	...	1.88 divisions	...	1.52 d.visions	...
<b>Average diameter of single</b> . . .						

The author is indebted to the Carnegie Trust for the Universities of Scotland for a grant to meet the expenses of this research.

(*Issued separately April 8, 1905.*)

**A Study of Three Vegetarian Diets.** By D. Noël Paton  
and J. C. Dunlop.

(From the Research Laboratory of the Royal College of  
Physicians, Edinburgh.)

(MS. received February 24th, 1905. Read March 6th, 1905.)

The recent publication of Prof. Chittenden's *Physiological Economy in Nutrition* tends to establish a new standard of dietary requirements, if not for the labouring classes, at least for men, middle-aged and young, who are not undergoing continued and sustained muscular work.

He records a prolonged series of observations upon himself and on his colleagues, representing professional men, on soldiers and upon student athletes. In the first class, health and undiminished working capacity were sustained for 7 to 9 months on a diet containing only about 46 grms. of proteid per diem, and yielding only from 1550 to 2530 Calories of energy. In the group of soldiers, 44 to 50 grms. of proteid and from 2500 to 2800 Calories of energy were sufficient to maintain their working power; and in the case of the students 55 grms. of proteid and under 3000 Calories of energy were found to be sufficient to meet the dietary requirements of men in training.

From the fact that most of the diets of those able to select their food contain at least 100 grms. of proteid, it has been, perhaps too readily, assumed that this amount of proteid is essential for the maintenance of health and a good state of muscular activity. Chittenden has certainly shown that adult men not subjected to sustained muscular exertion can maintain themselves in a state of good muscular development on less than half this amount. He does not, however, touch the question of whether, in growing children, pregnant women, and labouring men, it is advantageous or, indeed, possible to reduce the proportion of proteids in the diet to anything like this extent.

It is not our purpose here to consider this aspect of the question, but we think that the new light thrown upon dietetics by Chittenden's book makes the study of what might be considered atypical diets of considerable interest.

In the diets recorded by him, vegetables, as might be expected, figure very largely, and while in all of them the amount of animal food is lower than is usual, in some of the diets vegetables almost entirely replace animal products.

As a result of the publication of our *Dietary Studies of the Labouring Classes in Edinburgh* in 1898, the opportunity has been presented to us of studying three very atypical vegetarian diets, which had been selected by their consumers for what appeared to them reasons of health and economy, and they seem to us to present features of sufficient interest to warrant their publication.

The first illustrates the danger of a refusal to accept the very evident fact that the food must supply the necessary energy for work; the second records what, in the light of Chittenden's work, might be considered a very liberal diet, but illustrates one of the difficulties of vegetarianism; while the third reveals the diet of a vegetarian glutton, and shows how the *res angusta domi* have produced a reformation.

#### *Study I.*

The subject of this study was a retired professional man. His theory is that most men overeat themselves, and that the less a man eats the better and the stronger he is. His physical condition does not support his theory. He is in a state of emaciation, and his appearance is more that of a man suffering from some wasting disease than that of a man in robust health. His height is 5 feet 10½ inches; his weight at the commencement of the week's observation was only 52 kilos.—about 40 per cent. less than the normal for his height.

The food which he selected for himself during the period of observation, as suitable for the maintenance of health, was banana and hot water. The quantity of banana he consumed during the five days was 9½ lbs.; on four of the observation days he ate one pound of the bananas at about 8.30 a.m., and a second pound at



about 3 p.m., taking a little hot water twice or thrice in the 24 hours. During the observation period he reported that he was feeling well and satisfied, but on the last day allowed that he had not slept well, that he was feeling hungry, and that he would appreciate a change to a diet containing some bread and butter. After five days of the banana diet his weight was 50 kilos.—a loss of 2 kilos.

The food-value of his diet amounted for the five days to: proteid, 37·5 grammes; fat, 3·5 grammes; and carbohydrates, 999 grammes, the equivalent per man per day being:—

Proteids . . . . .	7·5 grms.
Fats . . . . .	0·7 „
Carbohydrates . . . . .	199·8 „
Calories . . . . .	856

His excretions were carefully analysed during the period, and the results of the analyses are shown in the following table:—

URINE.							
	1st Day.	2nd Day.	3rd Day.	4th Day.	5th Day.	Average.	Per cent. of Total N.
Quantity, c.c. . . . .	1040	950	500	460	860	762	...
Specific gravity . . . . .	1014	1014	1020	1022	1012	...	...
Reaction, on each day alkaline . . . . .	...	...	...	...	...	...	...
Total nitrogen, grammes . . . . .	3·92	4·25	2·96	2·85	2·98	3·23	...
Urea nitrogen, grammes . . . . .	3·30	3·58	2·26	2·35	2·68	2·83*	80
Ammonia nitrogen, grammes . . . . .	·112	·095	·081	·084	·095	·09	2·5
Uric acid, grammes . . . . .	·008	·004	·045	·033	·028	·02†	3·0
Non-urea nitrogen, grammes . . . . .	·62	·67	·70	·78	·76	·70	20
Phosphoric acid, grammes . . . . .	·88	·88	·80	1·00	·84	·88	25
FÆCES.							
Dry weight, grammes . . . . .	23·2	29·2	29	29·5	30·5	22·5	...
Total nitrogen, grammes . . . . .	·81	1·02	1·56	1·12	1·21	1·14	...
NITROGEN BALANCE.							
	Intake.			Output.			
Food . . . . .	1·21			Urine . . . . .		3·23	
				Fæces . . . . .		1·14	
						4·37	
FOOD ANALYSIS.							
Bananas, Proteid, 0·67; Fat, 0·08; Carbohydrate, 23·17 per cent.							

\* Average urea, 6·2.

† Or 0·106 gm. N.

The more noteworthy points about this study were:—

1. The extreme smallness of the diet. The caloric value of it was only about one-fourth of the normal diet for moderate labour, and the proteid value was only about one-twentieth of the normal.

2. The urine was alkaline throughout the entire period. It was more strongly so on the third, fourth, and fifth days than on the first two days. This alkalinity was due to the food being purely vegetable.

3. The excretion of nitrogen was very similar to that found in total starvation. Of the total nitrogen, only 80 per cent. was excreted as urea, a proportion less than the normal. The total amount of non-urea nitrogen was less than the normal, but was relatively not so much reduced as was the excretion of nitrogen in urea.

4. The excretion of preformed ammonia was very small. This may be ascribed to the presence of excess of alkali and to the comparative absence of organic sulphur in the food.

5. The nitrogen balance was decidedly negative, and indicated a daily average loss of 19·8 grms. of tissue proteid, or about 100 grms. of flesh.

### *Study II.*

The subject was a woman aged forty-two, a typist, who had for a long time been a modified vegetarian. The study was made at the same time and in the same way as our studies of the diets of the labouring classes of Edinburgh. She stated that she was strong and well, and able for a large amount of exercise, that she habitually bicycled and walked long distances. She always sat with the window of her room open, and did not feel cold. The study extended over a period of one week.

The food she used during the period was as follows:—Butter, 20 oz. ; milk, 60 oz. ; eggs, 8 ; cream, 10 oz. ; cheese, 3 oz. ; bread, 32 oz. ; brown bread, 22 oz. ; cakes and pastry, 50 oz. ; chocolate cream, 2 oz. ; sugar, 13 oz. ; jam 11 oz. ; potatoes, 46 oz. ; fresh vegetables, 24 oz. ; prunes, 16 oz. ; bananas, 21 oz. ; oranges, 29 oz. ; and apples, 8 oz.

The food principles in such a diet are estimated by us to amount per week to: proteid, 406·5 grammes; fat, 896·1

grammes; carbohydrates, 2923 grammes. The equivalent diet per man per day is:—

Proteids . . . . .	73·6 grms.
Fats . . . . .	160·0 „
Carbohydrates . . . . .	522·0 „
Calories . . . . .	3926

Here a fair energy value is yielded by a large supply of fats and carbohydrates, while the proportion of proteids is unusually low.

The point of special interest in this diet is the very large amount of fat and carbohydrate food taken to get the necessary energy, an amount which many persons would find it difficult to digest.

The cost of the week's diet was 12s. 4d., or equivalent to 14s. 10d. per man per week, or 25½ pence per day. The ordinary labourer's family in Edinburgh gets a larger supply of proteid and a fair supply of energy for about 7d. per day.

### *Study III.*

Along with a cutting concerning our *Dietary Studies* from the *South-Eastern Advertiser* of 24th February 1900, we received a letter from a Mr H., of which the following is an extract:—

15th October 1900.

DEAR SIR,—After reading the above, it occurred to me that I might as well send you a copy of my half-year's expenditure. . . . I cannot possibly be called a typical person; but there are so few people who do keep exact records of what they eat, drink, and spend, that I suppose scientific men are glad to get such records from almost anybody."

With this letter was a very full and detailed budget of C. H.'s income and expenditure, and a detailed statement of the food consumed during the six months from 1st April to 30th September 1900. It is unnecessary to publish this at length. The following list contains the articles of importance, and the quantity of each used, the quantities being expressed in kilogrammes:—Apples, 15·88; cherries, 0·45; bilberries, 0·45; strawberries, 0·45; melons, 8·00; red currants, 1·00; gooseberries, 2·50; oranges, 20·00; lemons,

4·00 ; tomatoes, 3·37 ; monkey nuts, 94·00 ; hovis bread, 84·00 ; other breads, 10·00 ; nucoline, 9·50 ; quaker oats, 20·00 ; sugar, 41·00 ; nut butter, 1·00 ; jam, 15·00 ; golden syrup, 0·90 ; cocoa, 0·50 ; coffee, 0·15 ; peas, 10·00 ; lentils, 4·70 ; onions, 3·20 ; carrots, 0·20 ; radishes, 4·00 ; rhubarb, 4·00 ; biscuits, 3·00 ; chocolate, 1·10 ; peppermints, 0·10 ; eggs, 0·30 ; condensed milk, 1·50 ; lemon squash, 1·00 ; nutta, 0·50 ; plasmon, 0·20 ; yeast, 0·07 ; bananas (dried), 0·40.

The food-value of such a diet has been estimated by us, and it is found that its value is per man per diem :—

Proteids . . . . .	230·3 grms.
Fats . . . . .	275·3 „
Carbohydrates . . . . .	734·2 „
Calories . . . . .	6514

Of the total energy, 8·5 per cent. is derived from animal food, and 91·5 from vegetable food. The cost of the diet for the six months was £8, 18s. 11d., which is equal to 6s. 10d. per man per week, or 11·7d. per day.

Even supposing that this diet is over-estimated by 10 per cent., it is still Gargantuan, yielding over 200 grs. of proteid and 5800 Calories of energy. From the observations of Avsititkiski, of Dunlop upon prisoners, and of Noël Paton on dogs, it is almost certain that a great part of this enormous diet was not digested and absorbed, and was therefore not available.

When putting together our results, we wrote to Mr H. as to the enormous amount of food consumed, and he writes, under date 5th February 1904 :—

“I must own, however, that I am a larger eater than most, indeed, a glutton. Everyone has his own physical vice, and I make up for abstinence from alcohol, tobacco, tea, coffee, meat, and breakfasts, and for devotion to the morning cold tub, by overeating myself three or four evenings a week. I always read at meals, and this tends to make one go on feeding mechanically.”

With this letter he sends details of the diet of himself and of his wife and three children from 1st April to 30th September 1903. He says :—“I do not think I eat quite so much now as in 1900. I cannot say I have ever suffered much in

health from overfeeding (though I suppose all physical sins must be paid for in the long run), but a growing family and growing debts exert on me a highly beneficial pressure. I enclose a list of the food I ate April–September '03. The table was made primarily with a view to cost, but weights can be deduced from it within probably 5 per cent. of the truth."

The diet here recorded is a much more normal one, and considering the non-availability of the proteid in many vegetable foods, and the fact that many of the vegetables used contain a large proportion of non-proteid nitrogen which is here recorded as proteid, the food consumption is by no means above the average. The growing family and growing debts have certainly been beneficial so far as his diet is concerned.

The food consumed during this second six-months period was of essentially the same kind as during the first period, but differed from the latter in quantity. He had reduced his six-monthly consumption of monkey nuts from 94 kilogrammes to 13·1, of hovis bread from 84 kilogrammes to 30; but had increased his supplies of other, more ordinary, breads from 10 to 65. Another notable change was that he had much increased his supply of fresh vegetables, using no less than 22 kilogrammes of carrots, while during the first period he only used 0·02 of that vegetable. Here is a list of the food used during the second period, expressed in kilos:—Monkey nuts, 13·10; roasted peanuts, 0·90; apples, 9·80; oranges, 1·50; lemons, 2·00; tomatoes, 1·40; melons, 5·00; red currants, 1·00; cucumbers, 1·50; stoned raisins, 1·80; hovis bread, 30·613; whole-meal bread, 44·73; malt bread, 16·00; white bread, 5·00; biscuits, 4·00; bannocks, 12·00; cake, 0·30; quaker oats, 0·90; force, 1·80; sugar, 10·5; carrots, 22·20; onions, 3·00; scallions, 1·20; turnips, 8·00; green peas, 2·00; rhubarb, 14·00; radishes, 1·50; jam, 6·30; honey, 0·40; syrup, 9·00; nucoline, 6·30; walnut butter, 0·40; peanut butter, 0·40; cow (*sic*) butter, 7·7; cocoa, 0·2; coffee, 0·2; chocolate, 4·0; sweets, 0·6; eggs, 1·40; Briggs' food, 0·40; orange wine; plasmon, 0·4; Maggi's soup powder, 0·1.

The total food principles in these six months' rations, as estimated by us, amount to: proteid, 19,054·4 grammes; fat, 18,981·9;

carbohydrates, 93,689·0; and from that estimate the diet per day per man is found to be :—

Proteids . . . . .	104·1 grms.
Fats . . . . .	103·7 „
Carbohydrates . . . . .	512·3 „
Calories . . . . .	3497

The cost of the diet for six months was £7, 12s. 1d.; the equivalent cost per man per week was 5s. 10d., or 10d. per day.

Considered in the light of the older standards, the diet is here a very liberal one, while in the light of Chittenden's observations it may be considered as still excessive.

The diet of this man's wife and children for the period of six months included the following, quantities being expressed as kilogrammes :—Flour, 126; butter, 23·1; 236 eggs; sugar, 27·1; potatoes, 144; milk, 204; lentils, 10; bacon, 9·5; and smaller quantities of bread, lard, cheese, onions, peas, turnips, cabbages, carrots, rhubarb, radishes, tea, coffee, cocoa, oranges, lemons, tomatoes, cucumbers, apples, bananas, plums, currants, monkey nuts, quaker oats, cake, biscuits, jam, sweets, peel, corn-flour, nut butter, meat, ham, sausages, sardines, and tinned salmon. The food principles in the six months' rations amount to: proteid, 35,324·6; fat, 34,568·9; carbohydrates, 172,506·0. Using Atwater's estimate of the proportional requirements of a man, and of women and children, we estimate that the requirements of his wife and family, three children, aged six, four, and two, would amount to 2·1 times that of a man. On that basis we estimate that the diet submitted is equivalent to a diet per man per diem :—

Proteids . . . . .	65·7 grms.
Fats . . . . .	90·2 „
Carbohydrates . . . . .	444·1 „
Calories . . . . .	2929

The cost of the diet was for the six months £12, 19s. 10d.; this is equal to a cost of 4s. 9d. per man per week, or 8·1d. per day.

It is a diet, largely vegetarian, which meets the requirements laid down by Chittenden, but which by an older standard would be considered deficient in proteid and in energy.

*Conclusions.*

On two of the diets studied the subject was able to maintain health and muscular vigour because the amount of proteid and energy yielded was sufficient, but in both the cost was considerably in excess of that for which the labouring classes in town or country are able to procure an equally satisfactory diet. They are both essentially wasteful diets, and are not to be recommended for general adoption.

The study of the ordinary diets of the labouring classes in all countries seems to show that whenever possible a diet is secured which will yield something over 3000 Calories of energy and over 100 grms. of proteids per man per diem. It is improbable that so many different races should have made the same mistakes in the essential elements of their very varied diets, and we think that the evidence afforded by these diets cannot be set aside even by so careful a set of experiments as those conducted by Chittenden.

*(Issued separately April 8, 1905.)*

**Continuants whose Main Diagonal is Univariial.**

By **Thomas Muir, LL.D.**

(MS. received December 12, 1904. Read January 23, 1905.)

(1) In a recently written paper \* dealing with a continuant first considered by Cayley, it was pointed out that the function in question owed its complicated law of development to peculiarities of specialisation, there being a much more general continuant governed by a simpler law. The theorem enunciated regarding the latter was:—*If  $A_r$  be written for the sum of all the r-ary products formed from  $b_1, b_2, \dots$  with the restriction that no two consecutive  $b$ 's shall appear in any single product, then*

$$\begin{vmatrix} \theta & b_1 & . & \dots & \dots \\ -1 & \theta & b_2 & \dots & \dots \\ . & -1 & \theta & \dots & \dots \\ \dots & \dots & \dots & \dots & \dots \end{vmatrix}_n = \theta^n + A_1 \theta^{n-2} + A_2 \theta^{n-4} + \dots$$

For example, when  $n = 6$  the expansion is

$$\theta^6 + (b_1 + b_2 + \dots + b_5) \theta^4 + (b_1 b_3 + b_1 b_4 + b_1 b_5 + b_2 b_4 + b_2 b_5 + b_3 b_5) \theta^2 + b_1 b_3 b_5.$$

(2) The curious fact has now to be noted that this theorem itself can be generalised with a minimum of change in the mode of expression by altering the 2nd, 4th, 6th, . . . diagonal-elements on the left into  $\phi$  and writing  $\theta\phi$  for  $\theta^2$  on the right, the resulting theorem being then formulated as follows:—

$$\begin{vmatrix} \theta & b_1 & . & . & \dots \\ -1 & \phi & b_2 & . & \dots \\ . & -1 & \theta & b_3 & \dots \\ . & . & -1 & \phi & \dots \\ \dots & \dots & \dots & \dots & \dots \end{vmatrix}_n \tag{I}$$

$$= (\theta\phi)^m + A_1(\theta\phi)^{m-1} + A_2(\theta\phi)^{m-2} + \dots \quad \text{when } n = 2m,$$

$$= \theta \left\{ (\theta\phi)^{m-1} + A_1(\theta\phi)^{m-2} + \dots \right\} \quad \text{when } n = 2m - 1.$$

\* See *Messenger of Math.*, xxxiv. p. 126.



That  $\theta$  is a factor in the latter case is evident from a consideration of the fundamental identity

$$(\theta^{b_1}\phi^{b_2}\theta^{b_3}\phi^{b_4}\theta \dots) = \theta(\phi^{b_2}\theta^{b_3}\phi^{b_4}\theta \dots) + b_1(\theta^{b_2}\phi^{b_4}\theta \dots),$$

which shows that if, as is easily seen to be the case, the continuant of the 3rd order has  $\theta$  for a factor, so also must the continuant of the 5th order, and therefore also the continuant of the 7th order, and so on.

(3) The fact that the change from a univariial to a bivariial diagonal necessitates no change in the coefficients on the right-hand side of the identity prepares one for an analogous widening of other theorems in which continuants with a univariial diagonal are involved. Thus, denoting the continuant in (I) by  $\Phi_n$  we have the important condensation theorem—

$$\Phi_{2m} = \begin{vmatrix} \theta\phi + b_1 & b_1 & . & \dots & . \\ b_2 & \theta\phi + b_2 + b_3 & b_3 & \dots & . \\ . & b_4 & \theta\phi + b_4 + b_5 & \dots & . \\ \dots & \dots & \dots & \dots & \dots \\ . & . & . & \dots & \theta\phi + b_{2m-2} + b_{2m-1} \end{vmatrix} \quad \text{(II)}$$

$$\Phi_{m-1} = \theta \begin{vmatrix} \theta\phi + b_1 + b_2 & b_2 & . & \dots & . \\ b_3 & \theta\phi + b_3 + b_4 & b_4 & \dots & . \\ . & b_5 & \theta\phi + b_5 + b_6 & \dots & . \\ \dots & \dots & \dots & \dots & \dots \\ . & . & . & \dots & \theta\phi + b_{2m-3} + b_{2m-2} \end{vmatrix} \quad \text{(II')}$$

Dividing  $\Phi_n$  as it appears in (II) by the cofactor of its first element we obtain a continued fraction, and dividing the equivalent continuant in (I) by the cofactor of its first element we obtain another continued fraction: and as, when  $n$  is even, the two divisors differ only by the factor  $\phi$ , the two continued fractions differ to the same extent. We thus have

$$\theta\phi + b_1 - \frac{b_1 b_2}{\theta\phi + b_2 + b_3} - \frac{b_3 b_4}{\theta\phi + b_4 + b_5} - \dots = \phi \left\{ \theta + \frac{b_1}{\phi} + \frac{b_2}{\theta} + \frac{b_3}{\phi} + \dots \right\}$$

$$= \theta\phi + \frac{b_1}{1} + \frac{b_2}{\theta\phi + 1} + \frac{b_3}{1} + \dots$$

Consideration of the case where  $n$  is odd leads to the same result, —a result given, probably for the first time, by Heilermnan.\*

(4) Similarly we have the theorem

$\theta$	1	.	.	.....	
$n-1$	$\phi$	2	.	.....	
.	$n-2$	$\theta$	3	.....	(III)
.	.	$n-3$	$\phi$	.....	
.....					$n$

$= (\theta\phi - 2^2) (\theta\phi - 4^2) (\theta\phi - 6^2) \dots$  when  $n$  is even,

and  $= \theta(\theta\phi - 1^2) (\theta\phi - 3^2) (\theta\phi - 5^2) \dots$  when  $n$  is odd,

—a theorem which degenerates into Sylvester’s (*Nouv. Ann. de Math.*, xiii. p. 305) when  $\phi$  is put equal to  $\theta$ . It has to be noted, however, that the mode of proof followed in the case of Sylvester’s theorem, viz., removing the linear factors separately, is now unsuitable. A mode of removing the quadratic factors will be found in the *Proc. Roy. Soc. Edin.*, xxiv. pp. 105–112.

(5) Thirdly, if we denote the preceding generalisation of Sylvester’s continuant by  $\sigma_n$  we obtain

$\theta$	1	.	.	.....	$= \sigma_n - \frac{n(n-1)}{2} (x-n+1)\sigma_{n-2}$ $+ \frac{n(n-1)(n-2)(n-3)}{2 \cdot 4} (x-n+1)(x-n+2)\sigma_{n-4}$ $- \frac{n(n-1) \dots (n-5)}{2 \cdot 4 \cdot 6} (x-n+1)(x-n+2)(x-n+3)\sigma_{n-6}$ $+ \dots$	(IV)
$x$	$\phi$	2	.	.....		
.	$x-1$	$\theta$	3	.....		
.	.	$x-2$	$\phi$	.....		
.....						

\* See *Zeitschrift f. Math. u. Phys.*, v. (1860), pp. 362–363 ; also Günther’s *Darstellung der Näherungswerte der Kettenbrüchen*, p. 75, Leipzig, 1878.

—a theorem which degenerates into Cayley's (*Quart. Journ. of Math.*, ii. pp. 163–166) when  $\phi$  is put equal to  $\theta$ .

(6) Fourthly, all the theorems given in the paper referred to in § 3 are capable of the same extension as Sylvester's. Only one of them need be quoted in its generalised form, viz.:—*If in the continuant of the  $n^{\text{th}}$  order*

$$\begin{vmatrix} \theta & b_1 & . & . & \dots\dots \\ \beta_{n-1} & \phi & b_2 & . & \dots\dots \\ . & \beta_{n-2} & \theta & b_3 & \dots\dots \\ . & . & \beta_{n-3} & \phi & \dots\dots \\ \dots\dots\dots\dots\dots\dots & \dots\dots\dots\dots\dots\dots & \dots\dots\dots\dots\dots\dots & \dots\dots\dots\dots\dots\dots & n \end{vmatrix} \quad (V)$$

*the difference between the element following any  $\theta$  and the element preceding the same be constant, equal to  $b_1$  say; and the corresponding difference in the rows containing  $\phi$  be also constant, equal to  $\beta_1$  say; then*

$$\theta\phi - b_1\beta_1$$

*is a factor of the continuant, the cofactor being the similar continuant of the  $(n - 2)^{\text{th}}$  order whose minor diagonals are got from those of the original by striking out  $b_1, b_2$  from the one and  $\beta_1, \beta_2$  from the other.*

(7) Fifthly, with the notation of § 4 we find

$$\begin{vmatrix} \theta & x-n+2 & . & . & \dots\dots \\ . & \phi & x-n+3 & . & \dots\dots \\ . & x-1 & . & x-n+4 & \dots\dots \\ . & . & x-2 & \phi & \dots\dots \\ \dots\dots\dots\dots\dots\dots & \dots\dots\dots\dots\dots\dots & \dots\dots\dots\dots\dots\dots & \dots\dots\dots\dots\dots\dots & n \end{vmatrix}$$

$$= \sigma_n - \binom{n-1}{1} \cdot (x-n+1)(x+1) \cdot \sigma_{n-2}$$

$$+ \binom{n-2}{2} \cdot (x-n+1)(x+1) \cdot (x-n+2)x \cdot \sigma_{n-4}$$

$$- \dots\dots\dots \quad (VI)$$

where the putting of  $\phi = \theta$  gives a theorem first published in the paper referred to in § 1.

(8) Proofs of the foregoing six theorems have been purposely omitted, because the modes of procedure followed in the case of the original ungeneralised theorems are applicable without alteration to the new theorems. In only one instance, that of (II), does previous work stand markedly in need of being supplemented. The first part of it, viz., where  $n$  is even, is best dealt with as follows:—

$$\begin{aligned} \Phi_6 \times \phi^3 &= \begin{vmatrix} \theta & b_1 & . & . & . & . & \phi & 1 & . & . & . & . \\ -1 & \phi & b_2 & . & . & . & . & 1 & . & . & . & . \\ . & -1 & \theta & b_3 & . & . & . & -b_2 & \phi & 1 & . & . \\ . & . & -1 & \phi & b_4 & . & . & . & . & 1 & . & . \\ . & . & . & -1 & \theta & b_5 & . & . & . & -b_4 & \phi & 1 \\ . & . & . & . & -1 & \phi & . & . & . & . & . & 1 \end{vmatrix}, \\ &= \begin{vmatrix} \theta\phi + b_1 & b_1 & -b_1b_2 & . & . & . & . & . & . & . & . & . \\ . & \phi & . & . & . & . & . & . & . & . & . & . \\ -1 & -1 & \theta\phi + b_2 + b_3 & b_3 & -b_3b_4 & . & . & . & . & . & . & . \\ . & . & . & \phi & . & . & . & . & . & . & . & . \\ . & . & -1 & -1 & \theta\phi + b_4 + b_5 & b_5 & . & . & . & . & . & . \\ . & . & . & . & . & \phi & . & . & . & . & . & . \end{vmatrix}, \\ &= \phi^3 \begin{vmatrix} \theta\phi + b_1 & -b_1b_2 & . & . & . & . \\ -1 & \theta\phi + b_2 + b_3 & -b_3b_4 & . & . & . \\ . & -1 & \theta\phi + b_4 + b_5 & . & . & . \end{vmatrix}, \\ \therefore \Phi_6 &= \begin{vmatrix} \theta\phi + b_1 & b_1 & . & . & . & . \\ b_2 & \theta\phi + b_2 + b_3 & b_3 & . & . & . \\ . & b_4 & \theta\phi + b_4 + b_5 & . & . & . \end{vmatrix}. \end{aligned}$$

Applying the same treatment to  $\Phi$  when of odd order we obtain

$$\Phi_7 = \begin{vmatrix} \theta\phi + b_1 & b_1 & . & . & . & . & +\phi, \\ b_2 & \theta\phi + b_2 + b_3 & b_3 & . & . & . & \\ . & b_4 & \theta\phi + b_4 + b_5 & b_5 & . & . & \\ . & . & b_6 & \theta\phi + b_6 & . & . & \end{vmatrix}$$

—a result interesting in itself, although not the form desired. Increasing each column by the column which immediately follows it, we have

$$\Phi_7 = \begin{vmatrix} \theta\phi + b_1 + b_2 & \theta\phi + b_1 + b_2 + b_3 & b_3 & . & . \\ b_2 & \theta\phi + b_2 + b_3 + b_4 & \theta\phi + b_3 + b_4 + b_5 & b_5 & . \\ . & b_4 & \theta\phi + b_4 + b_5 + b_6 & \theta\phi + b_5 + b_6 & . \\ . & . & b_6 & \theta\phi + b_6 & . \end{vmatrix} \div \phi$$

and now diminishing the second column by the first, the third by the new second, and so on, we obtain

$$\begin{aligned} \Phi_7 &= \begin{vmatrix} \theta\phi + b_1 + b_2 & b_3 & . & . \\ b_2 & \theta\phi + b_3 + b_4 & b_5 & . \\ . & b_4 & \theta\phi + b_5 + b_6 & . \\ . & . & b_6 & \theta\phi \end{vmatrix} \div \phi, \\ &= \theta \begin{vmatrix} \theta\phi + b_1 + b_2 & b_2 & . \\ b_3 & \theta\phi + b_3 + b_4 & b_4 \\ . & b_5 & \theta\phi + b_5 + b_6 \end{vmatrix}, \end{aligned}$$

given in § 2.

(Issued separately April 8, 1905.)

**On Professor Seeliger's Theory of Temporary Stars.**

By J. Halm, Ph.D., Lecturer on Astronomy in the University of Edinburgh, and Assistant Astronomer at the Royal Observatory.

(Read November 7, 1904. MS. received November 28, 1904.)

Professor Becker's paper "On the Spectrum of Nova Persei and the Structure of its Bands," recently published in the *Transactions* of this Society, contains an interesting confirmation of some results already pointed out by Messrs Campbell and Wright of the Lick Observatory,\* which seem to be of considerable importance for the theory of temporary stars. By most careful micrometric measurements of the positions of the bright and dark bands in the photographic spectrum of Nova Persei, Professor Becker arrives at the conclusion that all the bands are similar in type, and that the distances of corresponding maxima and minima from the centres of the bands are proportional to the wave-lengths. The results derived from the Lick photographs point to exactly the same conclusion. It appears, therefore, from these two carefully and independently executed series of observations, that the chemical nature of the elements, whose light-vibrations gave rise to the selective radiations and absorptions noticed in Nova Persei, had no influence on the appearance of the bands. According to the Lick observers, there is no evidence that the structure and character of these bands were affected by other considerations than that of wave-length.

This important result appears to necessitate now the exclusion from our view of those theories in which chemical or physical properties of the incandescent gases and vapours figure as determining factors. It seems, for instance, incompatible with the high-pressure theory advocated by Professor Wilsing of Potsdam, because those effects of pressure on the displacements of spectral lines which form the basis of Wilsing's theory are by no means the

\* Eighth Bulletin of the Lick Observatory.

same for all gases and vapours.\* The identical behaviour of lines in the spectrum of Nova Persei pertaining to different chemical elements must be considered to contradict this explanation. For the same and also some other reasons we cannot perhaps accept a new theory advanced by Dr Ebert of Munich,† in which abnormal refraction is claimed as the principal cause of the peculiar duplex character of the Nova lines. Dr Ebert's considerations are based on the fact that in a medium, the spectrum of which shows distinct absorption maxima, the index of refraction changes abruptly in the immediate neighbourhood of such a maximum, being greater on the less refrangible, and smaller on the more refrangible side. In his opinion, the light-radiation perceived in the bands of the Nova is not due to the radiative energy of the gas itself, but to light originally emanating from the incandescent surface of the star, which is abnormally refracted in the gaseous envelopes outside in such a manner that bright and dark bands are formed lying on the red and violet edges of the lines peculiar to the traversed gases. Three objections may be raised against this theory. *Firstly*, Dr Ebert's theoretical intensity-curve of the bands, as we shall see, differs materially from that observed in Nova Persei. *Secondly*, his theory gives insufficient account of the presence of the bright bands after the continuous spectrum had disappeared. For obviously, if the continuous spectrum is, in Dr Ebert's opinion, the *conditio sine qua non* for the bands, we are at a loss to explain how these bands could have possibly outlived, as they have actually done, the incandescence of the star's surface. *Thirdly*, the effect of abnormal dispersion is by no means the same for all gaseous media. According to the electro-magnetic theory, it depends on the elastic resistance of the ions, a force which cannot be supposed to be the same for different atoms. According to observation, even lines of one and the same metallic vapour, *e.g.* sodium, behave quite differently. Hence the theory of abnormal dispersion seems to offer no explanation of that similarity in structure and character of the Nova bands, which, according to the observations, appears to be a fundamental feature of the Nova spectrum.

\* See H. Kayser, *Handbuch der Spectroscopie*, ii. p. 325.

† *Ueber die Spectren der neuen Sterne*, A.N. 3917.

If the results of the Glasgow and Mount Hamilton observations thus limit our field of search by excluding those explanations in which the internal properties of the vibrating atoms must be considered to play an important part, there remains, in my opinion, only one explanation which is in *a priori* agreement with the observed facts, viz., that based on Doppler's principle. Indeed, if motions in the line of sight are the cause of the peculiar emission and absorption bands in the spectra of the Novae, the similarity of their structure, independent of the chemical nature of the elements, and the strict proportionality of all the displacements to the wave-length are necessary desiderata. The crucial point seems to be, therefore, this: On the one hand, observation has demonstrated that the structure of the bands is governed by no other conditions than that of wave-length; on the other hand, of all explanations, only that based on Doppler's principle accounts for this fact: hence motions of matter in the line of sight must be considered as the probable cause of the remarkable spectrum of temporary stars.

This conclusion has increased my confidence in some theoretical views published two years ago, by which I attempted to explain the Nova spectrum. The new facts brought to light by subsequent observations, especially by those referred to, make it now desirable to again publish these tentative speculations in a somewhat modified form, and at the same time to compare the results of theory with our present empirical knowledge.

Before entering upon the subject, I beg to pass a few general remarks of an historical character. In the earlier days of star spectroscopy explanations of the Nova spectrum were pre-eminently based on Doppler's principle. Theoretical views focussed more or less round the one conception that motions of radiating and absorbing matter must be responsible for the observed displacements of the spectral lines. Later, however, doubts began to be felt as to the correctness of this view. Apart from the fact that the velocities of matter in the Novae exceeded by far the average motions in the line of sight commonly dealt with in stellar spectroscopy, a serious objection to this view was thought to be found in the striking similarities between the spectra of all the Novae hitherto accessible to spectroscopic investigation. A universal



feature of all these spectra is the appearance of broad bright lines, bordered on their more refrangible edges by diffuse absorption-bands. Now, assuming that these composite bands were caused by two bodies moving in different directions, why should the bright bands invariably appear on the less refrangible and the dark bands on the more refrangible side? In other words, why should the body or bodies giving the bright line spectrum always move from us, while those showing the absorption-lines should be invariably directed towards us? It was the seeming inexplicability, on the ground of Doppler's principle, of this universal phenomenon which led astrophysicists to search for other explanations, such as high pressure and abnormal dispersion.

But if we look more closely into the question, the reasons for abandoning Doppler's principle seem by no means so convincing as they were thought to be. The position was abandoned before the field was thoroughly reconnoitred. Indeed, I shall endeavour to show in the subsequent remarks that an explanation of the extremely complicated spectrum of new stars based on Doppler's principle is not only possible, but also sufficiently probable, on account of the simplicity of the underlying hypotheses on the one hand, and the satisfactory agreement between theory and observation on the other.

The leading idea upon which these considerations are based is well known to astronomers through Professor Seeliger's ingenious investigations: my present contribution is indeed merely an extension of the celebrated theory which we owe to this distinguished astronomer. Seeliger's hypothesis, which ascribes the outburst of a new star to the collision between a dark solid body and matter of a nebular constituency, has so far not been worked out in detail, so that no definite conclusions have been formed as to the motions of the matter involved in the catastrophe. In a general way, however, Professor Seeliger draws attention to the important rôle performed by the star's gravitational attraction on the approaching nebulous matter, a consideration we often find seriously neglected in subsequent investigations. He remarks that, as the star approaches the nebulous cloud, the latter, through the action of gravitation, will extend out to meet it. The attracted

cloud particles, in obedience to gravitational laws, describe hyperbolic orbits round the star's centre as focus, in exactly the same manner as do those meteoric swarms of our own system which have been launched upon us from the remote recesses of space. The idea occasionally met with in papers on this subject, that the star penetrates into the cloud as a bullet pierces the air, is quite erroneous. Its fallacy is so obvious that I need not dwell upon it.

The hyperbolic paths described by the attracted particles are of course extremely different in shape and position, forming a chaos of motions which to unravel seems at first sight a hopeless task. But, fortunately, at least one definite conclusion may be drawn which is of vital importance for our problem. We know that the character of the conic section described by a body round a centre of attraction is perfectly defined by its velocity  $V$  at any point of the orbit. The body describes

$$\begin{aligned} \text{an ellipse,} & \quad \text{if } V^2 < \frac{2\mu}{r} \\ \text{a parabola,} & \quad \text{if } V^2 = \frac{2\mu}{r} \quad \mu = k^2 (M + m) \\ \text{a hyperbola,} & \quad \text{if } V^2 > \frac{2\mu}{r} \end{aligned}$$

where  $r$  is the radius vector at that point (expressed in units of the mean distance  $\odot - \delta$ ),  $k$  the Gaussian constant, and  $M, m$  the masses of the attracting and attracted body in units of the solar mass. Now, in our case a collision between the star and a meteoric particle must occur in all instances where the perihelion distances are less than the radius of the star. Such particles will impinge upon the surface. But impact means loss of energy of motion (molar energy), which is converted into kinetic (molecular) energy, *i.e.* heat. Hence  $V$ , the orbital velocity, must be smaller after the impact than it had been before. In other words, the impact-friction on and near the star's surface, by converting a more or less considerable portion of energy of motion into energy of heat, acts as a resisting medium, with the effect that in many cases  $V$  becomes less than  $\frac{2\mu}{r}$ , *i.e.* that the hyperbolae are transformed into ellipses.

An example may illustrate this simple reasoning. Suppose a swarm of meteors approaches the star O from A in hyperbolic orbits. The perihelion distance of the inner particles is assumed to be less than the star's radius R. These particles must impinge upon the star's surface, where their further career will be checked; *i.e.*  $V^2$ , which was greater than  $\frac{2\mu}{R}$  immediately before the impact, will be zero after the catastrophe, supposing that the whole orbital motion has been transformed into heat. On the other hand, the orbital velocities of particles grazing the surface, though impeded by surface friction, will undergo much

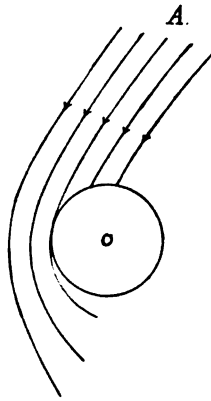


Fig 1.

smaller reductions, while bodies sufficiently removed from the star may pursue their hyperbolic paths practically undisturbed. Hence we notice a gradual transition in the values of  $V$  from zero to hyperbolic velocities, so that the swarm, although arriving at the star with practically uniform velocity, exhibits after the impact the most heterogeneous motions of its individual members. These new motions determine the character of the orbits described by those particles which are at all capable of escaping the star after impact. Since  $V$  may have all possible values, the new orbits contain all possible conic sections, from the circle to the hyperbola. The important point is, that many of these new orbits are closed, the particles becoming permanently attached to the system of the

star. In other words, as a consequence of the collision, the star becomes permanently surrounded with a ring of luminous meteoric matter, revolving in ellipses with eccentricities probably ranging from zero to unity. The transformation of molar into molecular energy must lead to incandescence, which will be in proportion to the amount of converted energy. But this latter is evidently greatest in the case of circular orbits, because here the reduction of  $V$  from its original hyperbolic value is most considerable. Hence the brightest parts of the ring are composed of particles moving round the star in ellipses of small eccentricities.

Now, we cannot avoid the conclusion that the kind of collision here described must occur in the case of a new star, provided that Seeliger's fundamental assumptions be true. I can imagine only one exceptional instance to which the above reasoning would seem inapplicable, viz., that the cloud particles move towards the star exactly in the direction of its centre, but I think the scarcity of such a phenomenon will at once be admitted. The most probable assumption is that of a more or less one-sided collision, such as is represented in fig. 1. Granting the reasoning so far, we conclude that after the catastrophe the star is surrounded by radiating nebular (meteoric) matter revolving in closed elliptical paths round the star's centre as focus, the brightest nebular particles describing orbits of small eccentricities.

The result in this general form is sufficient to assist us later on in the interpretation of the Nova spectrum. With regard to the constituency of the luminous ring, the most general assumption is that it consists of a mixture of bodies in all three states of aggregation—solid, liquid, and gaseous. But owing to their high power of radiation, the liquids and solids will cool down much sooner than the gases, so that in a more advanced state the spectral appearance of the ring will be that of an incandescent gaseous body emitting a line spectrum.

The problem, in its main principle, is seen to be closely related to Encke's celebrated theory of a resisting medium. A force acting near perihelion in the direction of the tangent against the orbital motion of a body causes a progressive (secular) diminution of the major axis and eccentricity of the orbit, and therefore tends to incorporate the body into the system of the attracting centre,

which otherwise might not be capable of bringing the cosmic invader under its permanent gravitational sway.

Still another consequence of importance, however, must be drawn from those fundamental considerations by which Professor Seeliger was guided in the framing of his theory. We admit that the development of heat at the surface of the star must be enormous, even granting the most unfavourable assumptions as to the tenuity of the impinging cloud. We may safely assume that the amount of heat developed during the bombardment may have exceeded many times that expended by the sun during a corresponding time. This fact seems to warrant the conclusion, not only that the surface of the star is rapidly liquefied, but also that from this surface of molten lava an incessant escape takes place of molecules with extremely high velocities, leading to the formation of an expanding incandescent atmosphere of vapours and gases. This unquestionable fact of an expanding atmosphere has, so far, not been considered in theories of temporary stars. Is there reason for neglecting the influence of its motions on the appearance of the lines in the Nova spectrum? There can be little doubt that the gaseous molecules escaping from the liquid surface of the star would tend towards a state of equilibrium such as is presented in the gaseous envelopes surrounding the photospheres of ordinary stars. The height of this 'atmosphere' is determined by gravitation on the one hand, and by the surface temperature on the other. If, for instance, we assume the mass and radius of the star to be equal to that of the sun, and its surface temperature to that of the solar photosphere, then the atmosphere would most probably assume the dimensions of the solar chromosphere, provided that it contains the same gaseous materials. If, however, the surface temperature of the new star equals that of the photospheres of so-called 'white' stars, which, as we know, possess very extensive atmospheres, its gaseous molecules would tend to form an envelope of similar dimensions. Now, I have shown in a paper in the *Astronomische Nachrichten*, Nos. 3822-3, that the extension of stellar atmospheres must be supposed to increase very rapidly if the surface temperature is raised. On the other hand, as shown in the same paper, our assumptions as to the temperature of new stars immediately after the collision are practically

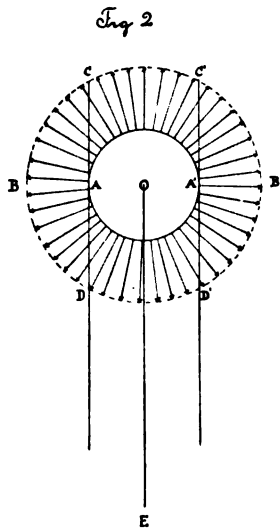
unlimited. I have mentioned already that the surface temperature of a Nova may exceed many times that of the solar photosphere.\* Hence there is no reason to contradict the assertion that the atmosphere of a new star, after the catastrophe, may assume dimensions surpassing considerably even those presented in the white stars. Indeed, this atmosphere may even extend infinitely, for it is well known that when the temperature of the surface exceeds a certain critical value, the height of the atmosphere above the surface must become infinite, *i.e.* gravitation then proves insufficient to counteract the continuous dissipation of the gases into space. As is shown in the paper referred to, this state of matters may happen already at a comparatively low temperature, exceeding not many times that of stars of the Sirian class (*i.e.*, 118). Now, in this peculiar case of infinite expansion, the initial velocities of the gaseous molecules at the surface must have been greater than the so-called critical velocity of the star (*i.e.* 610 km. per second if the sun's mass and dimensions be assumed).

\* Some estimate of the amount of heat developed by the impact may be gained from the following consideration. Suppose the materials of a cosmic cloud to fall from infinity upon our sun. The velocity  $V$  with which the cloud particles arrive at the sun's surface is hyperbolic, and therefore greater than 600 km. per second. Now we know that 1 kgr. matter moving with a velocity of  $V$  metres per second, if completely stopped, develops a quantity of heat which equals  $\frac{1}{8330} \cdot v^2$  calories. If, then, a quantity of cosmic matter weighing 1 kgr. at the surface of the earth would impinge upon the sun with parabolic velocity (about 600,000 metres per second), ca. 45 millions of calories would be developed by the collision. Suppose that during every second 1 kgr. matter impinges upon the area of 1 square metre, then the heat developed would be about 2400 times the amount of heat actually radiated by our sun during the same time. Now it is easy to see that this kgr. of matter is distributed within a parallelepipedon whose basis is 1 square metre and whose height is 600 km., because when the first particle of the kilogram arrives at the surface, the last particle which impinges exactly one second later will be, roughly speaking, at a distance of 600,000 metres from the surface. But the density of such a cloud is only about 1 : 800,000 of the density of air at ordinary temperature and pressure. Hence we conclude that an all-round impact of cosmic matter whose density is only the 1 : 2,000,000,000th part of that of our atmosphere would still produce an amount of heat equivalent to the energy radiated into space during the same time by our sun under normal circumstances. This rough calculation appears to justify the remark in the text, that the amount of heat supplied by the collision may indeed be assumed to be practically unlimited.

Such velocities are of the order of magnitude which would correspond to the displacements in the spectra of new stars if they were to be explained on the basis of Doppler's principle. Hence, if the expansion of the Nova atmosphere is associated with enormous surface temperature, the velocities involved must have a most profound bearing on the structure of the spectrum. This conclusion necessitates an examination of the spectral character of lines emitted by gases which form a rapidly expanding atmosphere round the incandescent nucleus of the star. It may be well to remember at this stage that we are by no means unfamiliar with the phenomenon of rapid gaseous expansion at the surfaces of celestial bodies. Notable instances are afforded in the solar eruptions, where the motions sometimes recorded fall little short of the sun's critical velocity. My opinion on these phenomena, more fully expressed in the paper referred to, is that they are the inevitable consequences of local changes of temperature in the interior layers of the sun. If, for some reason or other, upon which I will not enter at present, the temperature of the photosphere at a certain locality should be raised to that, say, of a Sirian star, the conditions of equilibrium over this particular area would require that the hydrogen atmosphere of the sun should expand to the dimensions of the hydrogen atmosphere of Sirius. What we perceive in a solar eruption is therefore, according to this view, the violent transition from a state of atmospheric equilibrium at solar temperature to that corresponding to the higher Sirian temperature. If the sun were suddenly bombarded by a shower of meteors, raising the temperature of the photosphere, an inevitable consequence would be the rapid development of a protuberance over the place of impact, simply because the atmosphere would tend to assume that form of thermal and mechanical equilibrium which corresponds to the higher temperature of the layers underneath. I conclude that the expansion in solar eruptions and that of the atmospheres on new stars are analogous phenomena, in both cases due to the tendency on the part of the gaseous molecules to assume that state of equilibrium which corresponds to the temperature at the surface. If this analogy be accepted, and if we take note of the high velocities so often revealed in the solar gases, we see probably no further difficulty in

admitting enormous atmospheric expansion in temporary stars. The correctness of this view will be more fully evidenced if we now investigate the effects of such a rapidly expanding gaseous envelope on the appearance of the spectrum. Almost at a glance we notice that a satisfactory explanation of one of the most enigmatic features of the Nova spectrum is here offered.

To show this in a few words, let us consider the star immediately after the collision, when its surface is in a state of high incandescence, and when the gaseous matter evaporating from the surface expands in radial directions outwards. Let the circle  $A A'$  repre-



sent the boundary of the star nucleus in a plane passing through the observer,  $O E$  being the line of sight. We suppose the outside boundary of the expanding atmosphere at this particular moment to be at  $B C C' B' D' D$ . We may also assume the star to be so far removed that the light of its photosphere (=incandescent star surface) and of the surrounding atmosphere is thrown simultaneously upon the slit of the spectroscope. Now, obviously, all the rays leaving the photosphere in the direction  $O E$ , *i.e.* towards us, have to pass through that part of the atmosphere which lies within the area  $A D D' A'$ . The natural assumption being that the gases of the outside layers at  $D D'$ , in consequence



of cooling by expansion, are at a lower temperature than the photosphere, absorption-lines, characteristic of the substances of the atmosphere, appear in the otherwise continuous photospheric spectrum. But since all these atmospheric particles move *towards* us, their lines must be displaced towards the more refrangible side of the spectrum, in accordance with Doppler's principle. Now, it will be noticed that, in whatever direction we may look at the star, *i.e.* in whatever part of space the observer may be stationed, the phenomenon will always be the same. The displacement of the absorption-lines towards the more refrangible side of the spectrum is therefore a *general* feature peculiar to all stars possessing expanding atmospheres.

There are two reasons why these absorption-lines, instead of being narrow and sharply defined, as in normal star spectra, should be broad and hazy. Firstly, the motions of the gaseous particles towards us are not uniform. We may take it for certain that considerable differences must exist in the amount and direction of these motions which would tend to broaden the lines. Secondly, the density of the atmosphere near the surface may be considerable, especially during the first stages of the star's development. We know that from this cause, too, a broadening of the lines may be expected. Considering the doubtless violent character of the catastrophe, we may also safely conclude that the broadening due to the causes mentioned must have been considerable.

We now turn our attention to the radiations emanating from those parts of the expanding atmosphere lying inside the segments  $DBC$  and  $D'B'C'$ . Obviously the spectrum produced by these radiations must show *bright* lines, characteristic of the same substances which cause the absorption spectrum in front of the star. But since in this case there are as many motions *towards* as *from* us, the centres of these lines—which are also broad and hazy, owing to the effects of density and divergence of directions—must appear in their *normal* positions.

In consequence of the great distance of the star, the two spectra are superimposed upon one another in the spectroscope. We see, therefore, a double spectrum, consisting of broad bright lines in approximately normal positions, edged on their more refrangible

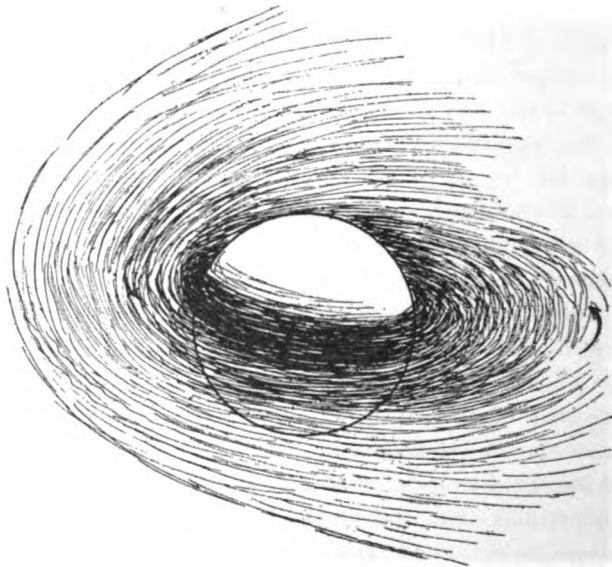
sides by broad and hazy absorption-lines, these duplex lines being projected on the continuous photospheric spectrum.

This consideration of the conditions prevailing on a star whose atmosphere is rapidly expanding leads already to conclusions with regard to the character of its spectrum which are in satisfactory agreement with the principal and most important feature of the Nova spectrum. Our conclusions require only some further modification through the existence of the rotating ring of luminous matter we have considered before. Great significance must be attached to the fact that the same type of spectrum must appear under all circumstances, whatever may be the relative positions of star and observer. Hence the remarkable uniformity of the spectra of all the Novae appears to be capable of a simple explanation.

According to the views here expressed, the described phenomena should occur in a certain sequence which deserves careful attention. The immediate effect of the collision being incandescence of the star's surface, the spectrum of the star, at the moment of the catastrophe, will be purely continuous. Subsequent to this stage, which is probably of short duration, we have the development of the expanding atmosphere, which impresses its existence on the spectrum only after the expanding gases have cooled below the temperature of the surface. At this stage broad and diffuse dark lines, strongly displaced towards the violet, make their appearance. Some further time will elapse, however, before the atmospheric halo round the star has sufficiently expanded to render its bright lines visible against the luminous background of the continuous spectrum. Now this order of events deduced from the theory seems to be confirmed by certain observations. In the case of Nova Persei, thanks to its timely discovery by Dr Anderson, we were fortunately permitted to watch the celestial catastrophe almost from its very commencement. When the spectrum was first viewed here in Edinburgh by the Astronomer-Royal for Scotland on the early evening of the 22nd February 1901, it certainly appeared to be purely continuous. A few hours later, however, I noticed distinctly faint dark bands, one of which agreed in position with the absorption-band afterwards noted on the violet edge of the bright F-line. On that night emission-bands

were not conspicuous. A few days afterwards these bright bands appeared strongly developed, and were then the most prominent feature of the spectrum.

I have already mentioned that the rotating ring of luminous nebular matter modifies to a certain extent the appearance of the spectral lines. Its effect will be to produce two additional maxima of brightness, the one displaced towards the red, the other towards the violet. For in whatever direction we may view the

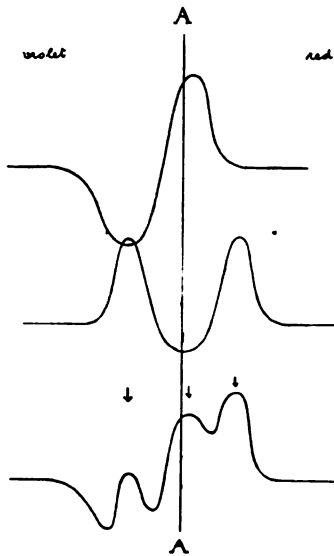


*Fig. 3.*

star—unless the line of sight be at right angles to the plane of rotation—we will always have some substance of the ring moving *towards* us on the one side of the star, and matter moving from us on the other. Between these maxima a more or less hazy absorption-line appears at approximately normal wave-length (leaving, of course, out of consideration the relative motion of the whole system: star + ring). This absorption-line is due to the gaseous particles of the ring travelling in front of the star, as seen from the standpoint of the observer. (Compare the sketch given in fig. 3.)

The complete structure of the bands is determined by these

considerations. We obtain an at least approximate idea of the appearance of such a band, so far as it is due to the radiations of the photosphere and the expanding atmosphere, by combining the intensity-curves of (1) the continuous spectrum in the neighbourhood of the line, (2) the absorption-band displaced towards the violet, and (3) the emission-band at normal wave-length. A combined band of this character is schematically represented in fig. 4, A A representing the normal position of the special line.



*Fig. 4, 5 and 6*

On the other hand, the radiations contributed to the band by the luminous ring may be roughly represented by the intensity-curve in fig. 5. If, in combining the two curves 4 and 5, we apply the simple additive rule, we obtain the total intensity-curve of the band in fig. 6. Since our assumptions as to the relative shift and intensity of the various components must of necessity be vague, there are, of course, many ways of drawing these curves, and the diagrams therefore represent only one special case out of a great number of possible combinations. But in constructing the curves I have aimed at adapting their relative dimensions to the phenomena actually observed in one particular case of new stars, viz., in Nova

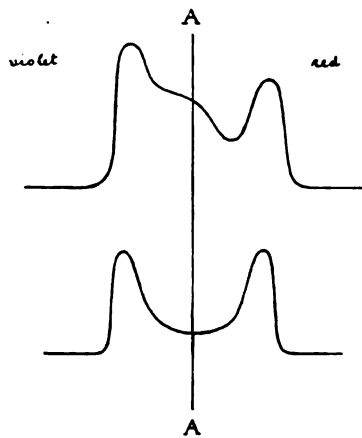
Aurigae. Afterwards I shall have occasion to exhibit other curves representative of the conditions present in Nova Persei, and I shall then be in a position to show how the observed differences in the character of the bands of these two stars can be accounted for. Fixing our attention for the present on fig. 6, we notice a broad bright band strongly displaced towards the red, and a broad diffuse absorption-band considerably shifted towards the violet. A remarkable feature is the distinct appearance of seeming 'reversals' in both the emission- and absorption-bands. Now, this same phenomenon was noticed in the observed spectrum. I may quote the following remark from Scheiner-Frost's *Astronomical Spectroscopy*, pp. 288-9:—"The microscopical examination of the photographic spectra showed the individual lines, both dark and bright, to be quite complex. A fine bright line could be seen extending down through the middle of many of the dark lines, and many of the bright lines had two or more points of maximum intensity . . . . From measurements on nine plates obtained at Potsdam between 14th February and 4th March (1892) Vogel deduces the following results, to which are added those calculated by Vogel from Campbell's measurements on six plates taken between 8th February and 6th March, and those published by Belopolsky from measurements on six plates taken from 24th February to 3rd March 1892. The velocities have been corrected for the motion of the earth, and are therefore relative to the sun. A + velocity denotes recession from the sun, a - velocity approach toward the sun.

*Displacements in Tenth-Metres.*

Line employed.	Bright line within dark.	First Max. of intensity.	Second Max. of intensity.	Third Max. of intensity.
H $\gamma$	-10.3	-0.1	+8.2	...
H $\delta$	-9.3	-0.8	+5.4	...
H	-10.1	-2.1	+3.5	...
K	-9.4	-1.7	+3.3	...
Vogel Mean	-9.8	-1.2	+5.1	...
F	-10.4	...	+8.3	...
H $\gamma$	-9.3	...	+7.2	+14.4
H $\delta$	-7.	...	+6.	...
Campbell Mean	-8.9	...	+7.2	...
Belopolsky H $\gamma$	-9.8	-0.7	+8.6	+15.7

The bright bands showed two maxima, except  $H\gamma$ , where three were noticed. We know, however, from the observations of the spectrum of Nova Persei, that the  $H\gamma$  band has a specially complex structure, being overlapped by another band on the less refrangible side. The third maximum noticed in Nova Aurigae may therefore not belong to the  $H\gamma$  radiations at all. Accepting this not improbable supposition, we conclude that in Nova Aurigae the bright bands showed two maxima with displacement of  $-0.6$  and  $+6.3$  tenth-metres, whereas the absorption-band exhibited a maximum in form of a bright line at  $-9.6$  tenth-metres. If we assume a common motion of the whole system of about  $-1.6$  t.m., the reduced motions of the maxima of the bright band would be  $+1.0$  and  $+7.9$  t.m., and that of the maximum in the absorption-band  $-8.0$  t.m. Now if we draw the observed intensity-curve corresponding to the arbitrary scale of fig. 6, the positions of the three maxima will be indicated by the three arrows above the curve. We notice, therefore, that the relative distances between these maxima are very closely represented by the theoretical curve in our diagram. At this stage we may submit the theory to a further test, which in my opinion goes far to show its probability. Doubtless the presence of the continuous spectrum has a decisive influence on the appearance of the bands, whose character on the more refrangible side is mainly determined by the absorption-band which is caused by gaseous matter moving between the incandescent star and the observer. But we know from observations that in Nova Aurigae, as well as in Nova Persei, the continuous spectrum has gradually faded away in such a degree that the star in its last stages of luminosity was almost entirely reduced to its gaseous emissions. Seeliger's hypothesis explains this course of events quite naturally. We have only to consider that an incandescent solid or liquid radiates heat more freely than a gas, and also that the brilliance of the star nucleus is confined to a shallow surface layer whose energy will be rapidly dissipated. We may ask: What becomes of the band shown in fig. 6 after the continuous spectrum has disappeared? Considering that the former absorptions will now have become radiations, the combined spectrum of the bands will be represented by an intensity-curve such as is shown in fig. 7. Here, again, our want of knowledge

of the actual intensities of all the components implicated in the formation of the band renders it impossible to select from the infinite number of possible cases the one which corresponds to the actual conditions. But the fact I want to point out here will be shown under all circumstances. It becomes at once apparent if we compare the bright band of fig. 7 with that of fig. 6. While the continuous spectrum was present, the band appeared shifted towards the red (fig. 6); after the continuous spectrum had vanished, the band appears in approximately normal position, but the maximum of light lies on the violet side (fig. 7).



*Fig 7 and 8*

It is readily noticed that the excess of brightness on the more refrangible side is due to the expanding atmosphere between star and observer. Since the density of this atmosphere must be supposed to diminish in the course of time, the same quantity of gas occupying more and more extended spaces on its outward journey, and since at the same time its temperature will be reduced by expansion, its contribution to the light of the bands will gradually lessen, and we may finally imagine a state in which the light of the star is mainly due to the gaseous radiations of the ring, whose temperature may be maintained for a more considerable time by the doubtless frequent collisions between its individual meteoric members. At this stage the intensity-curve of

the band will somewhat resemble that given in fig. 8, the light being now distributed symmetrically to the normal position. The sequence of phenomena, as theory would require it, may therefore be described as follows. While the continuous spectrum was brilliant, the observer must have noticed a strong displacement of the bright bands towards the red. We saw already that this conclusion is borne out by the observations (fig. 6). After the vanishing of the continuous spectrum, however, the same bands must have appeared shifted towards the violet, since then the maximum on which the observer would make his measurements, on account of the breadth and indistinctness of the band, lies on the violet side (fig. 7). The observer would therefore gain the impression that the star's motion in the line of sight had been considerably changed during the interval between his two observations.

This apparent shift towards the more refrangible side would gradually lessen, and finally the bright bands would appear in their normal positions (provided that the common motion of the whole system has been accounted for). Now, the student of the spectroscopic evolution of Nova Aurigae will at once recognise an agreement between these theoretical conclusions and the facts actually observed. The agreement is sufficiently demonstrated by the following data. It is well known that the spectrum of Nova Aurigae during its last stages of luminosity, from August 1902 to the end of 1903, was almost purely gaseous, and resembled that of a planetary nebula. There is also the possibly strongest evidence that the hydrogen-lines were represented in this later spectrum as well as in that of the former period when continuous radiation was powerful. In the following table I give the measured wave-lengths of these lines in both cases,\* and also their normal wave-lengths:—

	First Period (Feb. 1892). Continuous Spectrum, strong. Two Maxima in bright H-bands.	Second Period (Aug. 1892). Continuous Spectrum, feeble or absent (?). Maxima of bright H-bands.	Normal wave-length of H-lines.
H $\beta$	4869.9 } 4861.6 }	4857	4861.5
H $\gamma$	4347.8 } 4340.6 }	4336	4340.6
H $\delta$	4108 } 4102 }	4098	4101.9

\* Cf. Scheiner-Frost, pp. 287 and 291.



While we notice a displacement of the centres of the lines towards the red in the first period, we also see clearly their shift towards the violet in the second period, therefore confirming the conclusions drawn from figs. 6 and 7. Now, Professor Campbell of the Lick Observatory has given special attention to this shift of the bands during the second period, and has found evidence of a tendency of these bands to approach their normal positions. His measurements were made on the chief nebular line  $\lambda = 5007\cdot15$  (normal). They are exhibited in the following table taken from p. 293 of Scheiner-Frost's *Astronomical Spectroscopy*.

1892. Aug. 20-30.	$\lambda = 5003\cdot3$	$\Delta\lambda = -3\cdot8$ t.m.
Sept. 3-22.	5002·2	- 4·9
Oct. 12-19.	5003·7	- 3·4
Nov. 2-24.	5004·6	- 2·5
1893. Feb. 10-27.	5006·0	- 1·1
Mar. 26-May 9.	5005·3	- 1·8
Aug. 6-Oct. 10.	5005·9	- 1·2

The decrease in  $\Delta\lambda$  is quite apparent. In 1892 the displacement amounted to about  $-4$  t.m., a value which agrees very well with that of the H-lines of the preceding table. In 1893, on the other hand, the displacement was only about  $-1\cdot5$  t.m. Since we had found before from other considerations that this was probably in amount and sign the common displacement of the whole system, we conclude that at this stage the intensity-curve of the bands must have approximately agreed with that given in fig. 8.

Thus the theory here discussed seems to offer a simple and probable explanation of an otherwise extremely puzzling phenomenon, viz., of the enormous shift of the bands from red towards violet during the time between the first and second period of the history of Nova Aurigae.

So far, our attempt to explain the character of the spectrum of Novae on the basis of Seeliger's theory has been of a purely qualitative character. It is important to show now its possibility also from the quantitative point of view. In order to explain the displacements of the absorption-bands towards the violet by motions in the line of sight, we have to assume average velocities of the expanding gases amounting to about 600 km. per second.

This figure exceeds the average velocity of the outrushing gases in solar eruptions, but it may nevertheless be assumed to be at least of the order of these velocities. If, therefore, we grant the reality of the motions in the case of the sun, we should find no difficulty in accepting the explanation of the absorption-bands in new stars which I have proposed in this paper. I fully admit that a physical explanation of such exorbitant velocities in expanding gases has still to be framed, and that our present thermodynamical views, by accepting Boyle's law, offer no clue whatever. But these views, it must be remembered, are based on conceptions of molecular matter which we now admit to be imperfect. The new physics of the molecule and atom is quite different from that which led formerly to the kinetic explanation of Boyle's law. We are no longer permitted to conceive of the motions of gaseous molecules at solar temperatures as being exclusively governed by the frequency and intensity of their mutual impacts, and uninfluenced by any other forces acting between them. A number of facts point to the conclusion that gases emitting line-spectra are ionised. If this view is accepted, we have to take into consideration the electric agencies which are brought into play in cases of moving electric charges, and which, as Professor J. J. Thomson and others have shown, influence profoundly our conceptions of mechanical mass and energy. The kinetic theory of an ionised gas is therefore different from that of an electrically neutral gas, because in the former internal forces, viz. electric agencies, are operating which are not present in the latter. But the existence of these electric forces demands the introduction of an internal virial in Clausius' fundamental equation, which means, in other words, that Boyle's law is inapplicable, since the definition of a so-called 'perfect' gas excludes the presence of an internal virial. Now, our difficulty in understanding the greatness of motions in solar eruptions arises mainly from the fact that we have hitherto considered the gases on the sun as being in this 'perfect' state, and therefore have accepted Boyle's law as the basis from which we formed our opinions of the greatest possible speeds in expanding gases at solar temperatures. We have reasoned in the following way: The velocity with which a certain disturbance of equilibrium within the gas can be propa-

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gated by internal mechanical agencies can under no circumstances exceed that with which sound would travel through the gas. Now, we have a fairly warranted estimate of the surface temperature of the sun, and with this temperature we can compute the speed of sound in the solar chromosphere. But we find that the computed velocity falls considerably short of that usually noticed in solar prominences. Hence we argue that the phenomenon of solar eruptions cannot be explained on the basis of thermodynamical reasoning. The argumentation seems strong enough, only we must not forget that our computation of the velocity of sound is essentially founded on Boyle's law. If, for instance, we assumed that between the molecules of the solar gases powerful repulsive forces were acting, the computed speed of sound would become considerably greater, and hence our conclusion as to the maximum speed of propagation would also differ from that we hold at present. We are, I think, clearly placed before the alternative: either Boyle's law is correct, then it is difficult to see how solar eruptions can be real displacements of matter; or Boyle's law does not express the true kinetic conditions existing in solar gases, then the high velocities in solar eruptions become conceivable if we assume powerful repulsive forces acting between the molecules. As I mentioned before, there are reasons which seem to favour the second alternative. If, for instance, we accept the modern view that radiation is due to motions of the electrons within the atom or molecule, are we not bound to look upon the molecules of an incandescent gas as moving electric currents? And suppose, under this condition, two molecules to approach each other, will not the induced electric force tend to drive them apart, *i.e.* act as a repulsive force? We are quite certain that this will happen in the case of ordinary currents and conductors, such as we are able to produce in laboratories: why not also in currents of molecular dimensions? What difference is there between a current produced by electrons moving along a conducting wire, and one caused by electrons moving round the nucleus of the atom or molecule? I think questions of this kind may at least shake the hitherto implicit confidence in the so-called 'perfect' state of incandescent gases, and may also make us aware that the kinetic theory of matter endowed with distinct inherent electric properties must

differ essentially from that with which we are at present familiar.

The object of this short transgression upon the field of molecular physics is to show that the reality of enormous velocities in expanding gases, such as we see in solar eruptions, cannot well be refuted by a reasoning based on conceptions of molecular matter in which electric agencies are ignored. There is no cogent reason, either on the part of theory or observation, which would force us to pronounce the displacements of gaseous matter on the sun as 'appearances' only. Hence, from the point of view here advocated, the stupendous rate of expansion of the 'atmosphere' of a new star may also be brought within the range of mental comprehension. It must be considered as a decided advantage of this theory that the asserted displacements of the spectral lines by motions of expanding gases in the line of sight are phenomena clearly noticeable in solar spectroscopic observations, whereas we have no recorded instance in cosmic evolution which might support, in a similar convincing manner, the assumption of exorbitant pressure or of abnormal refraction.

We will now turn to the quantitative test of the displacements caused by the rotating ring. The motions in the line of sight are here, according to the theory, of the order of the orbital velocities of bodies revolving round the nucleus of the attracting star near its surface. If we assume the star of the mass and dimensions of our sun, and if we remember that the brightest part of the ring is formed by substance revolving in *circular* orbits whose radius is practically that of the star, we find displacements of the two maxima in the bands corresponding to approximately 4-500 km. per second, which would be equivalent to a distance of about 11-14 tenth-metres between the two maxima. These figures are in close agreement with the observations which showed a distance of about 15 tenth-metres. Hence there is no difficulty in comprehending these displacements, and therefore also the enormous breadth of the Nova lines, on the assumption that they are caused by the orbital motions of particles revolving in the immediate neighbourhood of the star's surface.

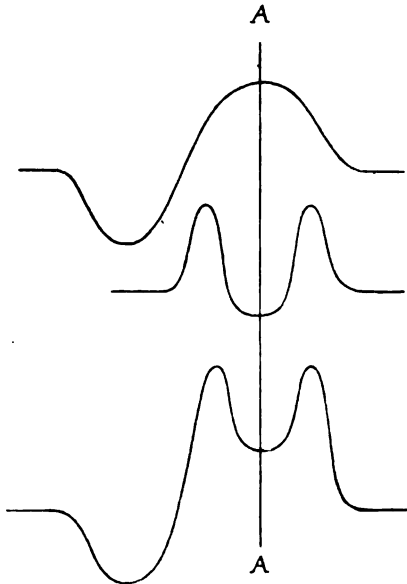
It will doubtless be noticed that the theory requires no assumption as regards the magnitude of the original relative motion

of star and nebula. Certainly the two objects must have approached each other, otherwise a collision would, of course, have been impossible. But the rate of approach is here a question of no relevancy. In this point the theory may be clearly distinguished from the earlier attempts, in which two or more bodies were assumed to move through space in different directions, with speeds far transcending the average proper motions of celestial bodies.

In illustrating these views on the physical processes connected with the phenomenon of temporary stars, I have discussed some of the more important facts brought to light by the observations of Nova Aurigae. I beg now to enter upon a brief discussion of the observational records of Nova Persei. Broadly speaking, the spectral phenomena noticed in this specially remarkable new star were in fair accordance with those of its predecessor of 1892. There are, however, some peculiar differences in the structure of the bands which seem to require an explanation. Most noticeable among these is the fact that during the time when the continuous spectrum was strong, the bright band, which in Nova Aurigae was strongly displaced towards the red (fig. 6), appeared in its normal position in Nova Persei. Fortunately, our theory is sufficiently flexible to explain this peculiar difference. We have seen before that the absorption-band on the violet side is caused by the rapid development of an expanding atmosphere at the moment of the collision. Now, obviously, the rate of expansion will depend on the temperature developed during the impact. If, therefore, on account of greater density of the impinging cloud, we suppose the catastrophe of Nova Persei to have been considerably more violent than that of Nova Aurigae—an assumption which is perhaps supported by the relative brightness of the two stars—then the displacement of the absorption-band would also be more considerable in Nova Persei. On the other hand, if the masses of the two stars have been nearly the same, the two maxima of the bright bands which are due to gravitational effects would appear in the same positions. Thus, while the curve in fig. 4 would have to be extended in the horizontal direction (fig. 9), fig. 5 would remain unaltered (fig. 10). By combining the two curves in the same way as before we obtain the intensity-curve in fig. 11. Hence, as the total effect of the combined radiations and absorptions, we find

in this case a broad bright line with two maxima, the centre of which lies at the normal position, and a hazy absorption-band on the violet side of the bright band. We notice that the assumption of a more energetic expansion at once explains why the bright Perseus-lines should have been found in normal positions, contrary to what had been seen in the former fainter Novae, where these bands were displaced towards the red.

The fact that the bright bands in Nova Persei were not dis-



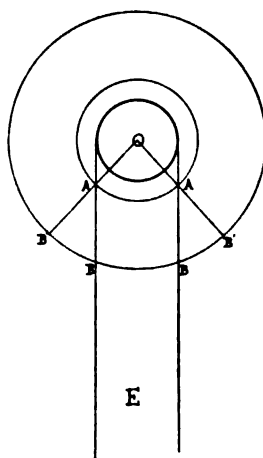
*Fig. 9, 10 and 11.*

placed renders it difficult, on the other hand, to accept either the high-pressure or the abnormal-refraction theory. According to the former, we must expect, under all circumstances, displacement of the bright bands towards the red, while the absorption-bands should appear in normal positions. The observations show that in Nova Persei just the opposite phenomenon occurred. The *a priori* improbable assumption which might save the theory, viz., that the star may have possessed an enormous proper motion towards us, is clearly contradicted by other observed facts. In the case of the refraction theory the same difficulty is experienced, even in

a more pronounced form, because it is inconceivable that any photospheric light can have been abnormally refracted on wave-lengths shorter than the normal. The whole of the bright band should have developed on the less refrangible side. This is clearly shown by the theoretical intensity-curve in Dr Ebert's paper.

I have mentioned before that the density of the expanding atmosphere, which may have been considerable at the moment of impact, must be assumed to decrease in course of time, and I have pointed out the effect which this must have on the appearance of the absorption-bands. These bands, being very broad and hazy at first,

*Fig 12*



will gradually shrink into narrow lines. Suppose fig. 12 to represent a central section through the star and its atmosphere, A B indicating the line of sight. It is clear that atmospheric particles, at one time distributed along the arc A A, will, by radial expansion, in course of time be distributed over the greater arc B' B'. Now all the particles within A A have contributed to the absorption-band at the first moment, but of these only those lying within the arc B B will absorb the photospheric light at the second moment. Hence the total number of gaseous molecules passed through by photospheric rays in the direction of the line of sight will be less at the second moment. Now, since the breadth and haziness of

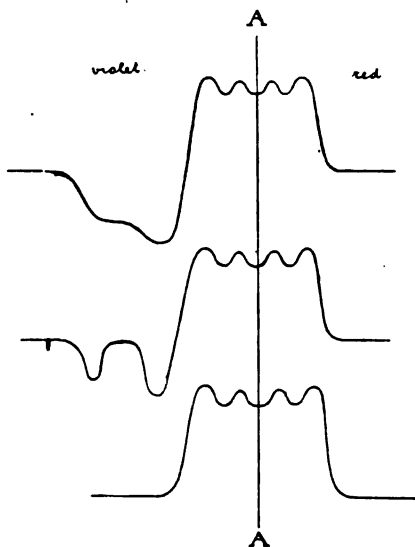
the spectral lines, as experiments show, increase with the density, and perhaps also with the temperature, of the emitting gas; and since both density and temperature are more considerable at A than at B, we must conclude that the broad and hazy absorption-band is gradually reduced to a narrow line, and finally fades away altogether.

This peculiar shrinkage of the absorption-bands has indeed been noticed during the spectral evolution of Nova Persei. But, curiously, the band resolved into *two* lines instead of *one*. To explain this duplicity we have to make a further assumption, but fortunately one which seems not improbable. We have indeed only to suppose that in this special case the dark body was a *double* star. We are quite familiar with double-star systems in which one of the components is invisible (stars of the Algol type). There is, however, no reason that might debar us from assuming double stars in which the surfaces of both components have cooled below the range of visibility. Now, in such a case it is very unlikely that both stars should have the same mass. But if the masses are different, then the gravitational effects on the cloud particles should also be different, and hence the heat-development at the surfaces and the orbital velocities of the encircling rings. In other words, we should then obtain an intensity-curve of the bands which is found by combining two curves of the shape of fig. 11 drawn on different scales. The resultant curve is shown in fig. 13, which is indeed typical of the first stage of development in Nova Persei. The following stage is characterised by fig. 14, where the broad hazy absorption-band has already been resolved into two comparatively distinct absorption-lines. At a still further stage, when the density of the expanding atmosphere has become extremely small, the absorption has practically disappeared, and there remains only the radiation of the two rings, giving rise to a bright band with four more or less pronounced maxima, its centre lying at normal wave-length (fig. 15). All these conclusions are well borne out by the observed facts.

I may be allowed here to quote the following remark from a paper by Father Sidgreaves on the spectrum of Nova Persei in *Monthly Notices*, vol. xii. p. 141, descriptive of the gradual changes in the dark hydrogen-bands:—"At the beginning these



dark lines appeared to grow in strength between 28th February and 8th March . . . . But after 8th March their decline was regular and uninterrupted; they slowly disappeared, together with the bright calcium line K. On 12th March they had lost their centres and appeared as well-defined double lines, separated by a thin clear reversal. The more refracted components were much the weaker, and were the first to disappear. They had lost much on the 16th, and were quite extinct on the 20th, when the red side components



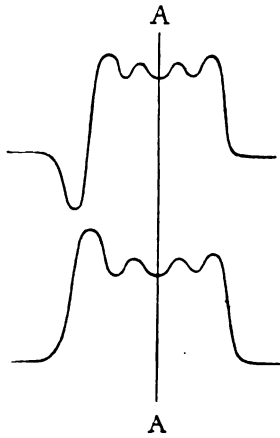
*Fig. 13, 14 and 15.*

formed the series of sharp thin lines, which were seen for the last time on the 21st."

It appears from this quotation that in Nova Persei the two absorption-lines in fig. 14 have been of different intensity, the one less refracted being decidedly the stronger of the two. The consequence was that this line outlived its more refracted feebler neighbour, and that there was a stage when the intensity-curve of the bands showed the structure exhibited in fig. 16. Suppose now this state of matters to have lasted for some time, during which the continuous spectrum has more and more decreased in brightness. Under these circumstances the absorption-line would

gradually become an emission-line, and, as such, might enhance the intensity of the violet edge of the emission-band. We should then notice those peculiar finger-post structures (fig. 17) which are so prominent features in the later spectrograms of the Lick Observatory (see *L.O. Bulletin*, No. 8).

There is a good reason for the longer persistence of the *less* refracted absorption-line. The more rapidly the atmosphere expands, the more quickly will the absorption-band thin out and disappear. But since the more refracted band is due to the more rapidly expanding atmosphere, we may naturally infer that its existence



*Fig 16 and 17*

must be of shorter duration than that of its neighbour, which is caused by the less expanding gases.

The double-star hypothesis, which apparently explains in a satisfactory way some of the peculiar spectral features of Nova Persei, may also assist us in understanding more fully the peculiar variability of the star's light, specially noticed during the first stages of development. In an earlier paper (*Astronomische Nachrichten*, Nos. 3822-3) I have attempted to show that the principal features of this variability may be explained by a rotation of the star round an axis. I have there emphasised the fact that by the more or less one-sided collision the star's superficial layers must be melted unequally, the liquefaction reaching down into

lower levels at the place of maximum impact. A central section through the star immediately after the catastrophe may therefore be represented by No. 1 of fig. 18, the ring  $ABA'B'$  showing the incandescent surface layers, and A being the locality of maximum impact. After the collision has passed over, the surface begins to cool, and the star will gradually arrive at the stage No. 2, where the surface at B has cooled down to darkness, while the surface at A, through more vigorous conduction, and perhaps convection of heat from the interior, may still be in a state of incandescence. Some time afterwards the stage No. 3 will be reached, where the incandescence is now limited to a small lenticular segment at A. In this way the star would gradually pass from a state of all-round incandescence to total obscurity. If, now, we suppose the

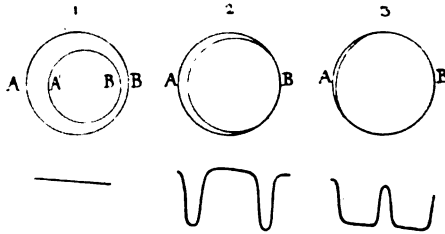


Fig 18

star to possess a rotatory motion, by which the points A and B are successively brought into the line of sight, we would notice the following features of variability: At No. 1 a uniform gradual decrease of brightness; at No. 2 the same, but in addition a periodic recurrence of pronounced maxima and minima, the former being much extended and covering the greater part of the period, the latter being indicated by abrupt and short inflections of the light curve; at No. 3, protracted minima covering the greater part of the period and maxima of short duration, hence the reverse of No. 2. These three theoretical light-curves are also represented in fig. 18. They are in fair accordance with the observed phenomena.

This assumption of an axial rotation advocated in my former paper is by no means improbable, since the impacts will doubtless impart a certain moment of momentum to the star nucleus. But it may perhaps seem unnecessary in the case of a double star,

where the observed phenomena may as well be explained by the revolution of the two stars round their common centre of gravity.

In the introductory remarks to this paper I have laid considerable stress on the fact that the observed displacements of the spectral lines are proportional to their wave-lengths, and independent of the chemical nature of the emitting gas. I pointed out that this remarkable fact supports the view that the displacements are due to motions in the line of sight. Indeed, if an incandescent gaseous body is moved with a velocity  $v$ , its lines are displaced by an amount  $\pm d\lambda$ , so that

$$\pm d\lambda = \frac{v}{V} \cdot \lambda,$$

where  $V$  is the velocity of light ( $= 300,000$  km. per second) and  $\lambda$  the wave-length of the line. This equation holds also if the body consists of a mixture of gases moving in the line of sight with a common velocity  $v$ . Hence any line of the spectrum emitted by these various gases will be displaced by an amount

$$\pm d\lambda = \text{const.} \times \lambda,$$

*i.e.* the displacement depends solely on the wave-length. Professor Becker's elaborate measurements confirm this statement in every respect. I should like here to supplement his important conclusions, which bear out so admirably the theoretical results of this communication, by a few similar measurements published by Messrs Campbell and Wright in the 8th Bulletin of the Lick Observatory. I begin with the displacements of the absorption-bands. The following table contains in the first column the lines measured and the elements to which they belong, in the second and third columns the observed and computed displacements towards the violet. The values of the third column have been computed from the formula:—

$$-d\lambda = 0.0046 \times \lambda$$

		Displacement	
		Observed.	Computed.
K	} Calcium	{ - 17 t.m.	- 18
H			- 18
H $\delta$	} Hydrogen	{ - 19	- 19
H $\gamma$			- 19
H $\beta$			- 22
D	Sodium	- 27	- 27

The agreement between observation and computation is so extraordinary that the observers felt justified to remark: "There is, then, no evidence that the position of the band is affected by other considerations than that of wave-length."

A similar result is obtained from the investigation of the bright bands. According to theory, the enormous width of these bands, as well as the appearance of maxima within them, are also to be explained by motions of gaseous matter in the line of sight. Hence we conclude that the width and the displacements of corresponding maxima should be linear functions of the wave-lengths, but independent of the chemical nature of the emitting substances. The correctness of this conclusion is shown in the following table. Here the measurements given in the second column refer to the chief (violet) maximum of the bright bands, while the displacements in the third column have been computed from the formula

$$-d\lambda = 0.00212 \times \lambda.$$

The fourth column contains the observed widths of the bright bands. Naturally these measurements are far less reliable, but nevertheless the alleged proportionality to the wave-length is quite evident.

Displacement of chief maximum  
of bright bands

Wave-length.	Displacement of chief maximum of bright bands		Width.
	Obs.	Comp.	
3868.9 t.m.	- 8.7 t.m.	- 8.2 t.m.	31
3967.6	- 8.6	- 8.4	31
4101.9 H	- 8.2	- 8.7	36
4340.6 H	- 9.4	- 9.1	...
4363.3 Neb.	- 9.7	- 9.2	33
4471.6 He	- 9.6	- 9.5	34
4643.	- 11	- 10	...
4685.9 Neb.	- 10	- 10	...
4713.2 He	- 10	- 10	...
4861.5 H	- 9	- 10	33
4959.0 Neb.	- 11	- 11	34
5007.0 Neb	- 11	- 11	38
5752. Neb.	- 11	- 12	43
5875.9 He	- 13	- 13	40

I cannot believe that these results, combined with the corroborating evidence of Professor Becker's observations, leave any doubt as to the fact that the displacements in the spectra of new stars depend exclusively on the wave-length, and are not caused by agencies which depend on the atomic structure of the emitting substances. This fact must be considered as the touch-stone of theories on temporary stars; so much so, indeed, that we may at once dismiss any explanation, however plausible in other respects, which is not in entire accordance with it.

We are now in a position to form, step by step, a mental picture of the evolution of a new star, and to compare our deductive conclusions with the observed facts. The more important events in the star's history as a radiating body may be thus summarised:—

(1) The immediate consequence of the impact between star and cosmic cloud is a more or less one-sided incandescence of the star's surface, causing a purely continuous spectrum. This stage was noticed here in Edinburgh about sixteen hours after the outburst.

(2) In consequence of the sudden and enormous heating a gaseous envelope is formed, which expands very rapidly in radial directions. The velocity of expansion may be assumed to exceed that noticed in solar eruptions. The expanding gases now begin to influence the spectrum. At first absorption predominates, and is shown by broad absorption-lines, displaced towards the more refrangible side. The lines must be broad and hazy, on account of the density and the divergent motions of the gases. This stage was observed here in Edinburgh about twenty hours after the discovery, when the visual spectrum was strongly continuous, but interrupted at various places by faint broad absorption-bands. As the density must have decreased while the atmosphere was more and more expanding, the broad and hazy absorption-bands in course of time reduced to sharp dark lines, which ultimately thinned out and faded away. This peculiar feature, too, has been noticed by observers (see F. Sidgreaves' note quoted above). At the same time the star's atmosphere becoming more extensive, its radiation outside the star's disc grows more and more prominent, giving rise to broad emission-bands in normal positions. Hence, after a time, the spectrum shows bright bands, bordered on their violet edges by absorption-bands. This constitutes the typical new star

spectrum. It may be specially noticed that, from the theoretical point of view, the absorption-line must *under all circumstances* be on the violet side of the emission-line.

(3) The expanding atmosphere, formed from the volatilised substances of the star matter, and being at temperatures comparable to those prevalent in star-atmospheres, will spectroscopically resemble the chromosphere. This conclusion is confirmed by the table on pp. 286-7 of Scheiner-Frost's *Spectroscopy*, in which a comparison is made between the lines seen in Nova Aurigae and those most frequently and most intensely noticed in the solar chromosphere.

(4) Besides the expanding atmosphere, account must be taken of the revolving ring of nebular matter which, after the collision, has been brought under the permanent gravitational sway of the star. The presence of motions of this character explains not only the enormous width of the bright bands, but also the appearance of symmetrically grouped maxima within them. We are further enabled to understand the strong displacement of the bright band towards the red in one case (Nova Aurigae), and the absence of such a shift in another (Nova Persei).

(5) When once this gyrating ring of matter has been established, further direct impact of meteoric matter upon the star will be impeded, since a considerable number of nebular particles may collide already inside the ring without reaching the surface of the star. This enhances, on the one hand, the luminosity of the ring, and reduces, on the other hand, the incandescence of the nucleus. Consequently we notice a decrease of the continuous spectrum coupled with an increase of those gaseous radiations which are caused by the incandescence or luminosity of the gyrating nebular matter. The expanding atmosphere having gradually faded away, the chromospheric spectrum has also disappeared, and has been superseded by those lines which are peculiar to the spectrum of gaseous nebulae.\* This is, briefly, the course of events which theory would lead us to expect. At the same time, it is also in many respects the sequence of phenomena shown by observation. The gradual disappearance of the continuous spectrum together with the lines which belong to the chromospheric radiations, and the simultaneous intensification of the nebular lines,—the peculiar process of

\* See note at end of paper.

“backwardation,” as Sir Norman Lockyer appropriately calls it, because it reveals a sequence of phenomena exactly opposite to what we comprehend as the natural evolution of a cosmic body,—are indeed features well known to students of this problem.

We notice, then; that Seeliger's ingenious hypothesis can be made to respond to a number of observed facts if the circumstances are duly considered under which the supposed collision between a dark body and a cosmic cloud may occur. An effort has been made in this communication to emphasise the important rôle played by the star's gravitational force, and to show that the motions of incandescent matter generated by the star's attraction are probably sufficient, from a qualitative as well as a quantitative point of view, to explain the peculiarities of the Nova spectrum, and also to account for the extraordinary process of evolution noticed in temporary stars.

One of the conclusions reached in this paper is that, as an effect of one-sided collision, the cosmic body may become surrounded by a revolving ring of nebular matter. Before the collision, neither the star nor the nebula were supposed to possess a rotational momentum. But the mere fact of a meteoric swarm impinging upon the star leads to the conclusion that a permanent ring of meteoric matter may be formed, the constituents of which revolve with orbital velocities round the star nucleus. May not this conclusion perhaps assist us in explaining the origin of the rotation of our own solar system? It is well known that Laplace, in his celebrated hypothesis, assumed rotation as a pre-existing quality of the solar nebula. He clearly recognised, what had escaped the less mathematical genius of Kant, that rotation could not have been generated by the internal motions of the contracting matter; that only an external agency could have introduced it into our system. Laplace made no attempt to define this agency: he boldly assumed its primeval operation, and started his hypothesis from the moment when rotation had been impressed upon the vast cosmic cloud from which our present system has gradually been formed. No doubt, our attempts to grasp the evolution of the natural world can only begin from a certain stage; unconceivable creation stands at the beginning of the cosmos. Laplace's assumption of original rotation is therefore certainly justified, and must be



preferred to the Kantian attempt to explain this quality of solar matter from an untenable mathematical and physical point of view. If the Laplaceian hypothesis would otherwise satisfactorily account for the development of our system, we might well grant his assumption that rotation was due to an external impulse beyond the grasp of our intelligence. But a recent criticism of the hypothesis has shown that our minds cannot readily accept all the conclusions drawn in this great poem of cosmic evolution. In a paper contributed to the *Astrophysical Journal*, vol. xi., Mr Moulton, a mathematical astronomer of high repute, attacks the hypothesis from various mathematical and physical points of view. His negative conclusions appear in many respects sufficiently sound and vigorous to convey the impression that the evolution of our system must have differed very largely from the ideal picture of Laplace. Since a brief review of Mr Moulton's arguments seems necessary in order to understand more clearly the bearing of our own hypothesis upon solar evolution, I beg to quote a few passages from his work which may give an idea of the nature and extent of the difficulties encountered in the nebular hypothesis.

(P. 104.) "The methods of testing the theory will be divided into three categories:—(i.) Comparison of observed phenomena with those which result from the expressed or implied conditions maintained by the hypothesis; (ii.) Answers to the question whether the supposed initial conditions could have developed into the existing system; (iii.) Comparison of those properties of the supposed initial system with the one now existing, which are invariant under all changes resulting from the action of internal forces."

(P. 129.) "Under the methods of the first category certain phenomena are enumerated which contradict the hypothesis so flatly that candid minds must admit that its validity in the form considered is open to serious question. In less exact sciences such objections would overthrow a theory or lead to its reconstruction. The objections are, that the planes of the planets' orbits present considerable deviations, while four satellites revolve in planes making practically right angles with the average of the system; that the distribution of mass in the planets is unaccount-

ably and suspiciously irregular; and that there is an unexplainable anomaly in the motion of the inner ring of Saturn.

"Under the methods of the second category, it is shown that the development of a system of planets and satellites from an extended nebula is by no means a simple matter, and that in the system under consideration the conclusions which it was possible to make were invariably adverse to the theory. In subjects where perfectly rigorous mathematical processes cannot be employed, such a uniform agreement of conclusions, when so various methods of attack are employed, is sufficient to establish a proposition. The objections are, that the lighter elements would have escaped; that matter would have been left off continually, instead of in rings at rare intervals; that if a ring were all contracted into a planet except an infinitesimal remainder distributed in its path, the process of aggregation could not complete itself; that the gravitation between the masses occurring in the rare media would be so feeble that they would seldom come in contact, and that Roche's limit and a similar new criterion show that fluid masses of the density which must have existed would be disintegrated by the disturbing action of the sun.

"The one objection which is advanced in the methods of the third category \* is of great simplicity, and leads to certain conclusions. It is of such a character, and the numerical discrepancies are so great, that it seems to render the nebular hypothesis, in the simple form in which it has usually been accepted, absolutely untenable, unless some fundamental postulates, now generally

\* (P. 126.) "It is known from the elementary principles of dynamics that the moment of momentum of a system which is subject to no external forces is constant." Mr Moulton demonstrates, however, that when the solar nebula extended to Neptune's orbit, the moment of momentum was 32.176, while in the system at present it is only 0.151. Hence, "instead of being a constant, the moment of momentum is found to vary in a remarkable manner. . . . It follows from these figures that if the mass of the solar system filled a spheroid extending to Neptune's orbit, and rotated with a velocity sufficient to make its moment of momentum equal to that of the present system, and if it then contracted . . . the centrifugal force would not equal the centripetal until it had shrunk far within Mercury's orbit. Such an enormous difference cannot be ascribed to uncertainties in the law of density, or to the approximations in the mechanical quadratures; but it points to a mode of development quite different from, and much more complicated than, that postulated in the nebular theory under discussion."

accepted, are radically erroneous. It seems a necessary inference from the results of the discussion that the solar nebula was heterogeneous to a degree not heretofore considered as being probable. . . .”

Now, it seems to me that some of these difficulties are avoided if we ascribe the formation of the planets to the rotating ring engendered by the collision between the solar body and a dense cosmic cloud. I would still assume the original solar body to have been formed from a nebula by the process of contraction. But this nebula had no inherent property of rotation. Consequently the resulting liquid body had neither a tendency to rotate nor was it surrounded by a revolving planetary system. Now let us suppose this body, on its journey through space, to approach a cosmic cloud of considerable density. As a consequence of the collision, which in all probability will be one-sided, not only a revolving ring of matter will become permanently attached to the star, but also those particles which impinge upon the body will impart a rotation to it in the same direction as that of the ring. The result is a slowly rotating central nucleus surrounded by a ring of quickly revolving matter. I have pointed out that the orbits of the ring particles, immediately after the catastrophe, have all possible eccentricities ranging between zero and unity, those near the star describing circles, those farther removed elongated ellipses. But this aspect will gradually change. On each return to periastron the particles will encounter fresh collisions, by which the major axes and the eccentricities of their orbits are lessened, the ring thereby becoming denser, and at the same time more and more circular. For we must keep in mind that, in consequence of the enormous heat communicated to the star by the impacts, there will be a dense and extensive atmosphere around it, through which the ring particles have to force their way every time they return to periastron. The tendency would therefore be to establish a circular ring. The density of matter within this ring may be quite heterogeneous. It is indeed to be expected that matter may be more concentrated in some of its parts than in others. From the beginning distinct nuclei may be present, around which matter is more or less densely grouped. These nuclei would form centres of attraction, and, as such, would mark the initial steps towards the formation of planets. From this

point of view, however crude it may appear in its present form,\* the difficulties of Mr Moulton's first and third category are at once removed, and those of the second certainly reduced. We understand better why the distribution of matter in the solar system should be so heterogeneous, and why there is not that constancy of the moment of momentum which would have to be expected if the Laplacean hypothesis were correct. Besides, we are here for the first time confronted with a possible explanation of how rotation may have been introduced into the solar system. In the problem of cosmic evolution, this question has always proved an insurmountable difficulty to those philosophers who attempted to trace the natural development of our world from the primordial chaos. That matter endowed with gravitational force may have contracted from nebulae into spherical bodies, and that these latter may have originally been impressed with chance motions through space—such conclusions are quite compatible with our conception of the chaos where chance has ruled supreme. But how, from this anarchy of forces and directions, a system of cosmic bodies could have been moulded, in which one particular tendency of motion prevails to the exclusion of all others—this question has so far been considered as pertaining to the domain of metaphysics rather than of natural philosophy. The difficulty seems now to be somewhat lessened, inasmuch as it can be shown that the chance approach of a star towards a nebular or meteoric agglomeration of matter may entail the formation of a rotating ring surrounding the star, and also the impression of an equally directed moment of momentum upon the body itself. It seems not unlikely, therefore, that in the phenomenon of a new star we notice the initial stage of the fabric of a solar system, and that Nature presents here to our eyes—although, perhaps, on a less gigantic scale—a sequence of events which had taken place in our own system in the remote past.

*Note added on 31st January 1905.*—It has been pointed out to me that I do not explain the noteworthy fact that the nebular lines have appeared a considerable time after the outburst, and were not present during the initial stages, whereas the theory demands the existence of nebular matter round the star from the

\* See my paper, "Some Suggestions on the Nebular Hypothesis."

very beginning of the catastrophe. I admit that my exposition contains no direct allusion to this point, which, however, seemed to me too obvious to require a special explanation. We must grant, I think without hesitation, that the appearance of the nebular spectrum presupposes not only the presence of nebulous matter, but also those special conditions of temperature under which alone this matter can emit the peculiar lines of gaseous nebulae. Nobody denies now that the materials of which the stars are composed once formed nebular clouds, and that under such conditions they emitted the typical nebular spectrum, of which at present, with a few exceptions, we see no traces in their atmospheres. It is one of the great achievements of modern spectroscopy to have shown that the same substance emits essentially different spectra under different conditions (*e.g.* the spectrum of hydrogen at low and high temperature). Hence we are clearly not permitted to think that nebular matter—an infinitely more complex structure than the simple hydrogen atom—will betray its existence by one and the same typical spectrum under all circumstances. The spectrum of nebular matter at a high temperature will most likely be essentially different from that at a low temperature. If our ideas of cosmic evolution be correct, the former must resemble that of incandescent cosmic matter in the star atmospheres, *i.e.* it must be chromospheric, while the latter is typical of the conditions in nebulae which our modern views suppose to be at very low temperatures, and luminous rather than incandescent. Doubtless the nebular matter round a temporary star is under the former conditions immediately after the outburst. It is only after the subsidence of impacts that the star and the nebulous matter round it gradually cool down and approach those conditions of low temperature which finally lead to the appearance of the typical nebular spectrum. In the ordinary process of evolution, therefore, cosmic matter begins its spectroscopic existence by showing the low temperature nebular spectrum, and thence develops its high temperature or chromospheric character; in temporary stars we notice the inverse process—so to speak, a negative evolution. These remarks will suffice to explain why the nebular spectrum should be absent at first, and should gradually develop with the cooling of the star.

(*Issued separately April 15, 1905.*)

**Some Suggestions on the Nebular Hypothesis.**

By J. Halm, Ph.D.

(MS. received March 6, 1905. Read March 20, 1905.)

The hypothesis of Laplace on the genesis of the solar system from an extensive nebula presents difficulties of so serious a character that important modifications appear to be required in order to make it conformable with the laws of dynamics. The objection most frequently brought forward refers to the mode in which Laplace assumes the separation of the planets from the contracting nebula to have taken place. It is urged that the intermittent shedding-off of rings is a somewhat unintelligible process considering the physical constitution of the nebula; that we should rather expect a *continuous* separation of particles at the equator, where the centripetal force is overbalanced by the centrifugal force, and hence that no fissure of a large ring from the main bulk is to be expected. Much hope is now entertained that the brilliant researches of M. Poincaré and Professor Darwin on the form of equilibrium of rotating fluids may eventually remove this difficulty, and teach us something about the evolution of the solar fluid when its axial rotation was quickening through contraction. It is conceivable that even in a heterogeneous body, as the solar nebula doubtless was, a course of events might take place which would lead from the sphere through the series of spheroids and Jacobi ellipsoids to Poincaré's well-known pear-shape; and ultimately, by increasing constriction of the waist of the pear, to the division of the body into two or more. But even granting such a possibility, some difficulty is felt in approaching an explanation of solar evolution from this groove of thought, because, as Mr Moulton has pointed out, the solar nebula has not fulfilled the law of constant moment of momentum. There can be no doubt that the present sum of rotary moments is considerably less than it should be if the planets had been

formed by the contraction of a rotating nebula. But the reduction of rotary momentum in a system cannot be explained in absence of external forces; and since such forces, acting with sufficient power, cannot be claimed, we must conclude that the formation of the planets must have been due to a cause different from that assumed by Laplace. In the following communication, which is of a merely suggestive character and is based on some conclusions arrived at in my previous paper on temporary stars, I have tried to avoid this difficulty by proposing a possible mode of development, in some respects different from Laplace's view, but ultimately leading to the same conclusions. I assume that the conditions necessary for the formation of planets were introduced after the solar body had condensed from a *non-rotating* nebula into a spherical body of a diameter probably less than the distance of Mercury. I suppose that at this stage the solar body, on its course through space, had approached a cosmic cloud of meteoric constitution, and had passed through a series of events such as have been described in my previous paper, leading—as was shown there—to the formation of a ring of meteors rotating with orbital velocities round the solar nucleus. The question to be discussed is whether we may explain the formation of planets and their rotation round an axis simply from the heterogeneity of the ring and the mutual perturbing action of its constituents. The conclusion, although reached by somewhat general and admittedly crude considerations, seems yet to be that these perturbations would introduce motions in the particles round a point of the ring where matter was denser than on the average, such as would impart a rotation to the condensing planet in the required direction. There seems also reason to suppose that Professor Darwin's ingenious conception of fluid-pressure in a meteoric swarm would sufficiently account for a gradual evacuation of the ring by the gravitational action of the planets. Lastly, I propose to show that the suggested view offers an explanation of the origin of comets compatible with observed facts, and may thus perhaps supplement the nebular hypothesis with regard to a point as to which Laplace's theory gives no satisfactory account.

In my paper, "On Professor Seeliger's Theory of Temporary Stars," an attempt was formerly made to explain the genesis of

rotation in the solar system by one-sided impacts of a meteoric cloud upon the solar nucleus. We must admit, on dynamical grounds, that the partial destruction of the orbital velocities of the meteors involves the generation of closed orbits round the star as focus, and also that one-sidedness of the impacts leads to the preponderance of a distinct direction of rotation. It may, however, seem difficult at first sight to understand how a system, in which the outer orbits must have possessed large eccentricities, should have developed into one in which all the bodies move now sensibly in circles. But on closer examination this difficulty seems to be lessened. Professor Darwin, in his essay on "The Mechanical Conditions of a Swarm of Meteorites and on Theories of Cosmogony," in the Transactions and Proceedings of the Royal Society for 1888, has proposed an ingenious thermodynamical theory of meteoric matter based on the laws of the kinetic theory as ordinarily applied to gases. One point of his investigation refers to the viscosity of such an agglomeration of meteoric substance, which he finds to be remarkably great. His conclusion suggests that friction must have largely influenced the orbital motions of the ring-particles. The passage in Professor Darwin's paper which has a direct bearing on this point may here be quoted:—

"The very essence of the nebular hypothesis is the conception of fluid-pressure, since without it the idea of a figure of equilibrium becomes inapplicable. Now, at first sight, the meteoric condition of matter seems absolutely inconsistent with a fluid-pressure exercised by one part of the system on another. We thus seem driven either to the absolute rejection of the nebular hypothesis, or to deny that the meteoric condition was the immediate antecedent of the sun and the planets. The object of this paper [*Proc. Roy. Soc.*, vol. 45, p. 4] is to point out that by a certain interpretation of the meteoric theory we may obtain a reconciliation of these two orders of ideas, and may hold that the origin of stellar and planetary systems is meteoric, whilst retaining the conception of fluid-pressure. According to the kinetic theory of gases, fluid-pressure is the average result of the impacts of molecules. If we imagine the molecules magnified until of the size of meteorites, their impacts will still, on a coarser scale, give a quasi-fluid-pressure. I suggest, then, that the fluid-pressure essential to the



nebular hypothesis is in fact the resultant of countless impacts of meteorites."

In applying this idea of a kinetic theory of meteors to the present problem, we have to consider the conditions prevailing in a system which consists of a non-rotating nucleus surrounded by a gyrating ring of meteoric substance. The conception of fluid-pressure, as proposed by Professor Darwin, involves the assumption of friction between star and ring. The star's surface being continually bombarded by neighbouring ring-particles, rotary momentum is imparted to the star, and is consequently lost by the ring. The motion of the inner ring is thus gradually reduced, in much the same way as that of an air-current passing along the earth's surface. The friction being propagated throughout the whole ring in accordance with laws similar to those of the internal friction in gaseous media, the materials of the ring will be constantly submitted to resisting forces acting in the direction of their motion. Hence, in course of time, the eccentricity of the ring, as a whole, must be lessened, and the system will tend towards a figure of equilibrium consistent with fluid-pressure.

This reasoning has brought us to the state of matters from which Laplace started his hypothesis. We see now some possibility, at least, how, by accepting this new hypothesis, we may explain the introduction of rotation into our system without abandoning any of the Laplaceian conclusions. So far the present view may therefore be considered merely as an extension of Laplace's cosmogonic conceptions. But in consideration of the grave objections raised against the nebular hypothesis in its present form, it may seem advisable to trace also the further development of the rotating fluid, and to see whether the difficulties expressed by Mr Moulton and others are indeed so insurmountable as they appear. One of the most serious objections refers to the formation of the planetary rings. The intermittent shedding-off of annular aggregates, which Laplace assumes, is a process not easily adaptable to our conception of the physical properties of meteoric matter. But I think this hiatus may be avoided. Little doubt can be felt regarding the assumption that the original ring must have been heterogeneous. Granting this, we must admit the existence of nuclei of condensation within the ring attracting the

smaller particles around them. Now let us consider the effect of these mutual attractions. All the particles in front of the nucleus (counting in the direction of the rotation of the ring) are pulled towards the condensation by forces, the tangential components of which are acting *against* their orbital motions round the central star. It is evident that these particles must fall *towards* the sun; they acquire radial velocities in the *inward* direction. Exactly the opposite course of events must happen with particles in the rear. Here the tangential pull is *in* the direction of orbital motion; they must move from the sun, and hence acquire radial velocities in the *outward* direction. On the other hand, the attraction of the particles on the nucleus acting equally in all directions, the latter suffers no deflection from its original motion. Now it is easy to picture what will happen when the attracted particles coalesce with the nucleus. The conclusion is that neither the front nor the rear particles fall directly towards the centre of the condensation: all the front particles must show a tendency to swing round on the inner side, *i.e.* between nucleus and sun, and all the rear particles on the outer side. Hence the accreting meteors must impart a rotary motion to the condensing nucleus, and the direction of this rotation must necessarily be that of the ring itself. Here, then, we have the conditions of rotary motions actually existing in our system. Whereas Laplace explains planetary rotation by the difference of speed between the outer and inner parts of the ring, which he must therefore assume to rotate with uniform angular velocity, we find now that the detachment of a Laplaceian ring and its subsequent coalescence into a planet is not necessarily required to account for the rotary motions of the planets.

The next point I desire to illustrate may be inferred from the following consideration. Let us imagine two bodies of equal masses to revolve in the same circle round the sun. Suppose, also, that the distance between the two bodies is sufficiently great to permit us to neglect their mutual attractions. Obviously the time of their revolution will also be exactly the same, and hence their distance from each other will remain unaltered. But let us assume their masses to be unequal. The period of revolution of the heavier body being shorter than that of the lighter body, the

former must gradually overtake the latter. If, for instance, one of the bodies possesses the mass of Jupiter, while the other body has only half this mass, their periods would be in the ratio 1:1.000237; and hence if the bodies had been 180° apart in the beginning, they would be at identical points of the common orbit in about 14,000 years. This reasoning shows clearly that the planet, after having attained a portion of its mass through accretion, must gradually bring under its gravitational influence the smaller masses revolving in its orbit. The planet would therefore evacuate its own ring. But if we accept Professor Darwin's conception of fluid-pressure, the idea of a vacuum cannot be maintained. The gap round the planet would be constantly filled up by meteors rushing into the planet's orbit from the outer and inner parts of the ring. The planet would act somewhat like a powerful air-pump, sucking in the meteoric molecules thrown into its sphere of gravitational attraction by the outside collisions. We may also gather from the mode in which the planet acts on the particles in its front and rear that the motions of those meteors which escape amalgamation with the attracting nucleus are deflected either towards or from the sun. This, no doubt, must increase the chance of collisions with the inner and outer portions of the ring. It is also understood that the increasing diversity of motions of the smaller meteors may assist the planets in their function of incorporating the small fragments thrown into their paths. There should be a gradual approach towards conditions such as we notice at the present moment when we find meteors crossing the earth's orbit in all possible directions. I am far from saying, however, that all the present meteors should be considered in this way as the last remnant of the original ring.

A point of extreme difficulty in Laplace's hypothesis is the explanation of the present slow rotation of the sun. Mr Moulton\* has demonstrated that if the solar nebula had contracted in the way Laplace assumed, the moment of momentum of the solar system should be more than two hundred times what it actually is; but, on the other hand, if the nebula had always possessed its present

\* F. R. Moulton, "An Attempt to Test the Nebular Hypothesis by an Appeal to the Laws of Dynamics," *Astrophysical Journal*, vol. xi., 1900, p. 103.

moment of momentum, the centrifugal force could not have over-balanced the centripetal force until the solar body had shrunk far within the orbit of Mercury. This argument against the Laplacean view seems to me unanswerable, and I agree with Mr Moulton when he contends that it "points to a mode of development quite different from, and much more complicated than, that postulated in the nebular theory." The present view is not exposed to this difficulty; on the contrary, the slow rotation of the sun follows of necessity from the mode in which rotation is supposed to have been brought into the system.

A further point in favour of the hypothesis seems to be the reasoning by which the existence of comets and the peculiarities of their orbits may be explained. Laplace, as is well known, considered the comets as bodies *not* belonging to our system. He arrived at this conclusion by investigating the question what form of cometary orbits should be the most probable if they are bodies launched upon us from outside space. He found that the most probable orbit must be the parabola; and since this is indeed the typical form of cometary orbits, he concluded that his supposition on their origin was correct. Subsequently, however, Schiaparelli has proved that Laplace committed an error in his analysis, and that the result to be expected from Laplace's supposition should be exactly opposite to his conjecture. Schiaparelli showed that the parabola is in fact the least probable curve in which a foreign body may intrude upon our system, and that under Laplace's supposition the great majority of orbits should be hyperbolic. His researches leave scarcely any doubt that the comets are members of our own system; that at practically infinite distance there exists a cosmic cloud travelling with our sun through space with practically the same speed and in the same direction; and that all the comets originate from this mysterious appendage. To explain these facts by the Laplacean hypothesis seems to me extremely difficult; but they are rendered almost obvious by the present theory. I have shown in my paper on temporary stars that through the catastrophe the star becomes surrounded with an expanding atmosphere of gases and vapours. We have the strongest possible evidence of the presence of this atmosphere in the absorption-bands of the spectra of new stars, which by their

displacements towards the violet indicate that the ejected gases move with enormous velocities, the greatest exceeding the critical velocity of a star such as our sun. Matter moving with such speeds is most probably for ever lost. But we may admit that there must be a considerable range of velocities among the various parts of this expanding cloud. Some move quicker, others more slowly, and all those particles whose initial velocities were less than the critical will sooner or later come to a point of rest whence they begin their return journey towards the star. Obviously the slowest particles must have returned soonest; they have either impinged upon the solar body long ago, or, in consequence of perturbations, have been drawn into elliptic orbits. They may have formed systems of periodic comets in the past, which now, through the continued disintegrating action of the sun and planets, have degenerated into meteoric swarms. Some of the present periodic swarms probably had this origin. If this view is correct, then the comets falling upon our system at the present moment must move in ellipses not distinguishable from parabolaë, since their return points must have been at practically infinite distances from the sun. That the fall of these bodies is not central, may be explained by the doubtless inevitable perturbations experienced, not only during their outward journey, but perhaps also at the outer limit, where the cloud may at times have been under the gravitational influence of neighbouring stars.\* Accepting this view, we understand why comets describe parabolic and elliptic orbits, why all inclinations are possible, and why there is the well-known physical resemblance between the members of this cosmic family. The expanding atmosphere of a new star would thus be a cometary cloud *in statu nascendi*.

The assumption of previous solar condensation, which is clearly necessary in this theory, may appear as a disadvantage, because it involves the creation and expenditure of solar energy *before* the planets were formed, and thereby seems to limit the time at our disposal for explaining the evolution of the planetary system. But, on the one hand, we must keep in mind that the generation of heat by contraction is at first a slow process. Indeed, the amount of

\* This is also Schiaparelli's view.

caloric energy produced by the sun through contraction from infinity to the orbit of Mercury is little more than one per cent. of what he acquired afterwards through shrinkage to the present diameter. On the other hand, this loss may have been fully compensated by the impact of the meteoric cloud. Considering the enormous rise of temperature when this happened, it is not unlikely that the ring which probably first developed near the sun's surface, where the destruction of orbital motion was greatest, through the heat developed by the collisions, expanded and afterwards filled the whole space of the planetary system.

But these are perhaps futile speculations which I will not pursue further, fearing that in this general outline already the hypothesis has stretched too far into the regions of uncontrolled imagination. Considered by itself, the theory would be of little value. But the fact, acknowledged by common consent, that collisions between stars and nebulae occur even now before our eyes in temporary stars, and that they are accompanied by phenomena which, judging from the spectroscopic evidence, point to the genesis of a rotating ring of nebular matter round the attracting body, is so suggestive of a similar course of events having been the cause of the rotation in our system, that I could not resist the temptation to venture upon speculative ground. Certainly no extraordinary gift of imagination is required to picture to ourselves the spectrum of the solar system under the initial conditions here assumed, with its expanding atmosphere of embryonal comets and the luminous ring of meteoric substance, the protoplasm of the future planets, and then to realise that this spectrum must have appeared to a distant observer in space as the typical spectrum of a new star. The "experimental" proof of the theory, afforded by the preceding examination of the spectroscopic evidence of temporary stars, is therefore encouraging, whatever may otherwise be urged against the superficial and highly incomplete treatment of so important a question in this communication.

*(Issued separately April 15, 1905.)*

**Deep Water Ship-Waves.\*** (*Continued from Proc. R.S.E.,  
June 20th, 1904.*) By Lord Kelvin.

(MS. received January 23, 1905. Read same date.)

§§ 32–64. Canal Ship-Waves.

§ 32. To avoid the somewhat cumbrous title “Two-dimensional,” I now use the designation “Canal † Waves” to denote waves in a canal with horizontal bottom and vertical sides, which, if not two-dimensional in their source, become more and more approximately two-dimensional at greater and greater distances from the source. In the present communication the source is such as to render the motion two-dimensional throughout; the two dimensions being respectively perpendicular to the bottom, and parallel to the length of the canal: the canal being straight.

§ 33. The word “deep” in the present communication and its two predecessors (§§ 1–31) is used for brevity to mean infinitely deep; or so deep that the motion does not differ sensibly from what it would be if the water, being incompressible, were infinitely deep. This condition is practically fulfilled in water of finite depth if the distance between every crest (point of maximum elevation), and neighbouring crest on either side, is more than two or three times its distance from the bottom.

§ 34. By “ship-waves” I mean any waves produced in open sea or in a canal by a moving generator; and for simplicity I suppose the motion of the generator to be rectilinear and uniform.

The generator may be a ship floating on the water, or a submarine ship or a fish moving at uniform speed below the surface; or, as suggested by Rayleigh, an electrified body moving above the surface. For canal ship-waves, if the motion of the water close to the source is to be two-dimensional, the ship or submarine must be a pontoon having its sides (or a submerged bar having its ends) plane and fitting to the sides of the canal, with freedom to move horizontally. The submerged surface must be cylindrical with generating lines perpendicular to the sides.

§ 35. The case of a circular cylindrical bar of diameter small compared with its depth below the surface, moving horizontally at a constant speed, is a mathematical problem which presents interesting difficulties, worthy of serious work for anyone who may care to undertake it. The case of a floating pontoon is much more difficult, because of the discontinuity between free surface of water and water-surface pressed by a rigid body of given shape, displacing the water.

§ 36. Choosing a much easier problem than either of those, I take as wave generator a forcive\* consisting of a given continuous distribution of pressure at the surface, travelling over the surface at a given speed. To understand the relation of this to the pontoon problem, imagine the rigid surface of the pontoon to become flexible; and imagine applied to it, a given distribution  $\Pi$  of pressure, everywhere perpendicular to it. Take  $O$ , any point at a distance  $h$  above the undisturbed water-level, draw  $OX$  parallel to the length of the canal and  $OZ$  vertically downwards. Let  $\xi$ ,  $\zeta$  be the displacement-components of any particle of the water whose undisturbed position is  $(x, z)$ . We suppose the disturbance infinitesimal; by which we mean that the change of distance between any two particles of water is infinitely small in comparison with their undisturbed distance; and that the line joining them experiences changes of direction which are infinitely small in comparison with the radian. For liberal interpretation of this condition see § 61 below. Water being assumed frictionless, its motion, started primarily from rest by pressure applied to the

\* "Forcive" is a very useful word introduced, after careful consultation with literary authorities, by my brother the late Prof. James Thomson, to denote any system of force.



free surface, is essentially irrotational. But we need not assume this at present: we see immediately that it is proved by our equations of motion, when in them we suppose the motion to be infinitesimal. The equations of motion, when the density of the liquid is taken as unity, are:—

$$\left. \begin{aligned} \frac{d^2\xi}{dt^2} + \xi \frac{d\xi}{dx} + \zeta \frac{d\xi}{dz} &= -\frac{dp}{dx} \\ \frac{d^2\zeta}{dt^2} + \xi \frac{d\zeta}{dx} + \zeta \frac{d\zeta}{dz} &= g - \frac{dp}{dz} \end{aligned} \right\} \dots \dots \dots (59),$$

where  $g$  denotes the force of gravity and  $p$  the pressure at  $(x, z, t)$ . Assuming now the liquid to be incompressible, we have

$$\frac{d\xi}{dx} + \frac{d\zeta}{dz} = 0 \dots \dots \dots (60).$$

§ 37. The motion being assumed to be infinitesimal, the second and third terms of the first members of (59) are negligible, and the equations of motion become:—

$$\left. \begin{aligned} \frac{d^2\xi}{dt^2} &= -\frac{dp}{dx} \\ \frac{d^2\zeta}{dt^2} &= g - \frac{dp}{dz} \end{aligned} \right\} \dots \dots \dots (61).$$

This, by taking the difference of two differentiations, gives:—

$$\frac{d}{dt} \left( \frac{d\xi}{dz} - \frac{d\zeta}{dx} \right) = 0 \dots \dots \dots (62),$$

which shows that if at any time the motion is zero or irrotational, it remains irrotational for ever.

§ 38. If at any time there is rotational motion in any part of the liquid, it is interesting to know what becomes of it. Leaving for a moment our present restriction to canal waves, imagine ourselves on a very smooth sea in a ship, kept moving uniformly at a good speed by a tow-rope above the water. Looking over the ship's side we see a layer of disturbed motion, showing by dimples in the surface innumerable little whirlpools. The thickness of this layer increases from nothing perceptible near the bow to perhaps 10 or 20 cms. near the stern; more or less according to the length and speed of the ship. If now the water suddenly loses viscosity and becomes a perfect fluid, the dynamics of vortex

motion tells us that the rotationally moving water gets left behind by the ship, and spreads out in the more and more distant wake and becomes lost; \* without, however, losing its kinetic energy, which becomes reduced to infinitely small velocities in an infinitely large portion of liquid. The ship now goes on through the calm sea without producing any more eddies along its sides and stern, but leaving within an acute angle on each side of its wake, smooth ship-waves with no eddies or turbulence of any kind. The ideal annulment of the water's viscosity diminishes considerably the tension of the tow-rope, but by no means annuls it; it has still work to do on an ever increasing assemblage of regular waves extending farther and farther right astern, and over an area of  $19^{\circ} 28'$  ( $\tan^{-1} \sqrt{\frac{1}{8}}$ ) on each side of mid-wake, as we shall see in about § 80 below. Returning now to two-dimensional motion and canal waves: we, in virtue of (62), put

$$\xi = \frac{d\phi}{dx}, \quad \zeta = \frac{d\phi}{dz} \quad \dots \dots \dots (63),$$

where  $\phi$  denotes what is commonly called the "velocity-potential"; which, when convenient, we shall write in full  $\phi(x, z, t)$ . With this notation (61) gives by integration with respect to  $x$  and  $z$ ,

$$\frac{d\phi}{dt} = -p + g(z + C) \quad \dots \dots \dots (64).$$

And (60) gives

$$\frac{\partial^2 \phi}{\partial x^2} + \frac{\partial^2 \phi}{\partial z^2} = 0 \quad \dots \dots \dots (65).$$

Following Fourier's method, take now

$$\phi(x, z, t) = -k\epsilon^{-mz} \sin m(x - vt) \quad \dots \dots \dots (66),$$

\* It now seems to me certain that if any motion be given within a finite portion of an infinite incompressible liquid originally at rest, its fate is necessarily dissipation to infinite distances with infinitely small velocities everywhere; while the total kinetic energy remains constant. After many years of failure to prove that the motion in the ordinary Helmholtz circular ring is stable, I came to the conclusion that it is essentially unstable, and that its fate must be to become dissipated as now described. I came to this conclusion by extensions not hitherto published of the considerations described in a short paper entitled: "On the stability of steady and periodic fluid motion," in the *Phil. Mag.* for May 1887.

which satisfies (65) and expresses a sinusoidal wave-disturbance, of wave-length  $2\pi/m$ , travelling  $x$ -wards with velocity  $v$ .

§ 39. To find the boundary-pressure  $\Pi$ , which must act on the water-surface to get the motion represented by (66), when  $m$ ,  $\tau$ ,  $k$  are given, we must apply (64) to the boundary. Let  $z=0$  be the undisturbed surface; and let  $d$  denote its depression, at  $(x, o, t)$ , below undisturbed level; that is to say,

$$d = \xi(x, o, t) = \frac{d}{dz} \phi(x, z, t)_{z=0} = mk \sin m(x - vt) \quad (67),$$

whence by integration with respect to  $t$ ,

$$d = \frac{k}{v} \cos m(x - vt) \quad (68).$$

To apply (64) to the surface, we must, in  $gz$ , put  $z=d$ ; and in  $d\phi/dt$  we may put  $z=0$ , because  $d$ ,  $k$ , are infinitely small quantities of the first order, and their product is neglected in our problem of infinitesimal displacements. Hence with (66) and (68), and with  $\Pi$  taken to denote surface-pressure, (64) becomes

$$kmv \cos m(x - vt) = \frac{g}{v} k \cos m(x - vt) - \Pi + gC \quad (69);$$

whence, with the arbitrary constant  $C$  taken = 0,

$$\Pi = kv \left( \frac{g}{v^2} - m \right) \cos m(x - vt) \quad (70);$$

and, eliminating  $k$  by (68), we have finally,

$$\Pi = (g - mv^2)d \quad (71).$$

Thus we see that if  $v = \sqrt{g/m}$ , we have  $\Pi = 0$ , and therefore we have a train of free sinusoidal waves having wave-length equal to  $2\pi/m$ . This is the well-known law of relation between velocity and length of free deep-sea waves. But if  $v$  is not equal to  $\sqrt{g/m}$ , we have forced waves with a surface-pressure  $(g - mv^2)d$  which is directed with or against the displacement according as  $v <$  or  $> \sqrt{g/m}$ .

§ 40. Let now our problem be:—given  $\Pi$ , a sum of sinusoidal functions, instead of a single one, as in (70);—required  $d$  the resulting displacement of the water-surface. We have by (71) and (70), with properly altered notation,

$$\Pi = \Sigma B \cos m(x - vt + \beta) \dots \dots \dots (72),$$

$$d = \Sigma \frac{B}{g - mv^2} \cos m(x - vt + \beta) + A \cos \frac{g}{v^2}(x - vt + \gamma) \dots (73),$$

where  $B, m, \beta$  are given constants having different values in the different terms of the sums; and  $v$  is a given constant velocity. The last term of (73) expresses, with two arbitrary constants ( $A, \gamma$ ), a train of free waves which we may superimpose on any solution of our problem.

§ 41. It is very interesting and instructive in respect to the dynamics of water-waves, to apply (72) to a particular case of Fourier's expansion of periodic arbitrary functions such as a distribution of alternate constant pressures, and zeros, on equal successive spaces, travelling with velocity  $v$ . But this must be left undone for the present, to let us get on with ship-waves; and for this purpose we may take as a case of (72), (73),

$$\Pi = gc \left\{ \frac{1}{2} + e \cos \theta + e^2 \cos 2\theta + \text{etc.} \right\} = gc \frac{\frac{1}{2}(1 - e^2)}{1 - 2e \cos \theta + e^2} \quad (74),$$

$$d = Jc \left\{ \frac{1}{2J} + \frac{e}{J-1} \cos \theta + \frac{e^2}{J-2} \cos 2\theta + \text{etc.} \right\} \dots \dots (75);$$

where

$$\theta = \frac{2\pi}{a}(x - vt + \beta) \dots \dots \dots (76);$$

$$v^2 = \frac{g\lambda}{2\pi}; J = \frac{a}{\lambda} = \frac{ga}{2\pi v^2} \dots \dots \dots (77);$$

and  $e$  may be any numeric  $< 1$ . Remark that when  $v = 0, J = \infty$ , and we have by (75) and (74),  $d = \Pi/g$ , which explains our unit of pressure.

§ 42. To understand the dynamical conditions thus prescribed, and the resulting motion:—remark first that (74), with (76), represents a space-periodic distribution of pressure on the surface, travelling with velocity  $v$ ; and (75) represents the displacement of the water-surface in the resulting motion, when space-periodic of the same space-period as the surface-pressure. Any motion whatever; consequent on any initial disturbance and no subsequent application of surface-pressure; may be superimposed on the solution represented by (75), to constitute the complete solution

of the problem of finding the motion in which the surface-pressure is that given in (74).

§ 43. To understand thoroughly the constitution of the forcive-datum (74) for  $\Pi$ , it is helpful to know that,  $n$  denoting any positive or negative integer, we have

$$2\pi(\frac{1}{2} + e \cos \theta + e^2 \cos 2\theta + \text{etc.}) = \sum_{n=-\infty}^{n=\infty} \frac{ba}{b^2 + (x - na)^2} \dots (78),$$

if

$$\left. \begin{aligned} b &= \frac{\alpha}{2\pi} \log(1/e) \\ x &= \frac{\alpha}{2\pi} \theta \end{aligned} \right\} \dots \dots \dots (79).$$

This we find by applying § 15 above to the periodic function represented by the second member of (78).

The equality of the two members of (78) is illustrated by fig. 11;

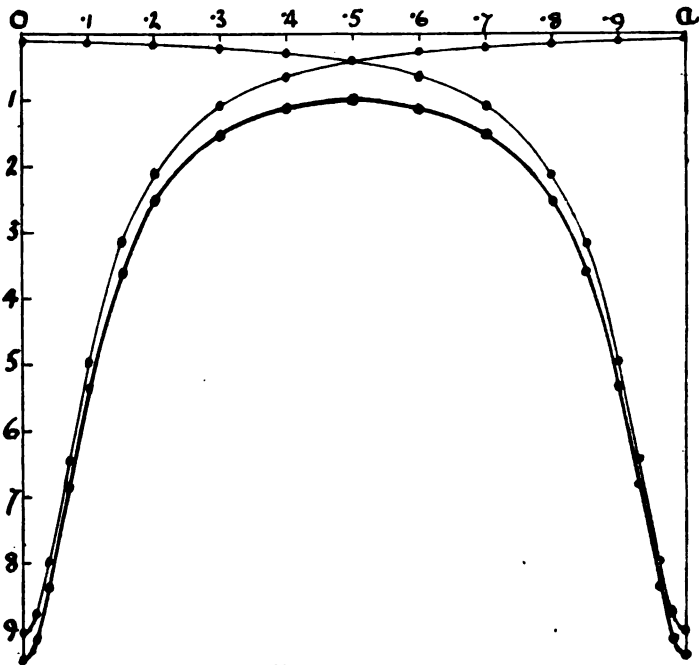


FIG. 11;  $e = \cdot 5$ .

in which; for the case  $e = \cdot 5$  and consequently, by (79),  $b/\alpha = \cdot 1103$ ; the heavy curve represents the first member, and the two light curves represent two terms of the second member; which are as

many as the scale of the diagram allows to be seen on it. There is a somewhat close agreement between each of the light curves, and the part of the heavy curve between a maximum and the minimum on each side of it. Thus we see that even with  $e$  so small as  $\cdot 5$ , we have a *not very rough* approximation to equality

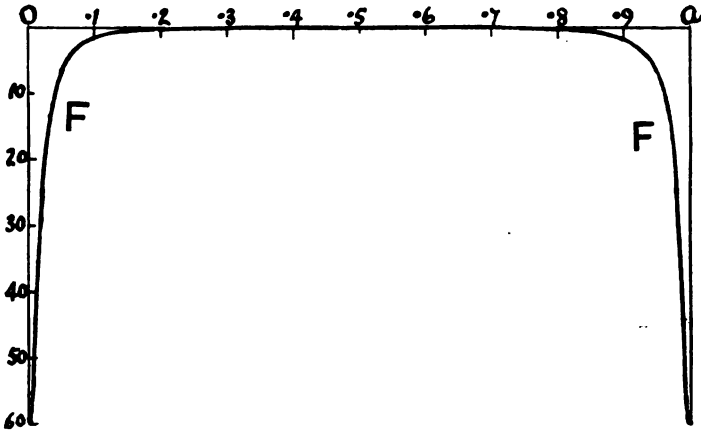


FIG. 12 ;  $e = \cdot 9$ .

between successive half periods of the first member of (78) and a single term of its second member. If  $e$  is  $< 1$  by an infinitely small difference this approximation is infinitely nearly perfect. It is so nearly perfect for  $e = \cdot 9$  that fig. 12 cannot show any deviation from it, on a scale of ordinates  $1/10$  of that of fig. 11. The tendency to agreement between the first member of (78) and a single term of its second member with values of  $e$  approaching to 1, is well shown by the following modification of the last member of (74) :—

$$\Pi = gc \frac{\frac{1}{2}(1 - e^2)}{1 - 2e \cos \theta + e^2} = gc \frac{\frac{1}{2}(1 - e^2)}{(1 - e)^2 + 4e \sin^2 \frac{1}{2}\theta} \dots (80).$$

Thus we see that if  $e \doteq 1$ ,  $\Pi$  is very great when  $\theta$  is very small; and  $\Pi$  is very small *unless*  $\theta$  is very small (or very nearly  $= 2i\pi$ ). Thus when  $e \doteq 1$ , we have

$$\frac{1}{gc} \Pi \doteq \frac{\frac{1}{2}(1 - e^2)}{(1 - e)^2 + e\theta^2} \dots \dots \dots (81);$$

which means expressing  $\Pi$  approximately by a single term of the second member of (78).

§ 44. Return to our dynamical solution (75); and remark that if  $J$  is an integer, one term of (75) is infinite, of which the dynamical meaning is clear in (70). Hence to have every term of (75) finite we must have  $J = j + \delta$ , where  $j$  is an integer and  $\delta$  is  $< 1$ ; and we may conveniently write (75) as follows :

$$d = c(\delta + j) \left\{ \frac{1}{\delta + j} + \frac{e \cos \theta}{\delta + j - 1} + \frac{e^2 \cos 2\theta}{\delta + j - 2} + \dots + \frac{e^j \cos j\theta}{\delta} - \frac{e^{j+1} \cos (j+1)\theta}{1 - \delta} - \frac{e^{j+2} \cos (j+2)\theta}{2 - \delta} - \text{ad inf.} \right\} \quad (82);$$

or

$$d = \mathcal{F} + \mathcal{J} . . . . . (83),$$

where  $\mathcal{F}$  and  $\mathcal{J}$  denote finite and infinite series shown in (82).

§ 45. We are going to make  $\delta = \frac{1}{2}$ ; and in this case  $\mathcal{J}$  can be summed, in finite terms, as follows. First multiply each term by  $e^{j+\delta} e^{-j-\delta}$ ; and we find

$$\begin{aligned} \mathcal{J} &= -c(\delta + j)e^{j+\delta} \left[ \frac{e^{1-\delta}}{1-\delta} \cos (j+1)\theta + \frac{e^{2-\delta}}{2-\delta} \cos (j+2)\theta + \text{etc.} \right] \\ &= -c(\delta + j)e^{j+\delta} \int de \left[ e^{-\delta} \cos (j+1)\theta + e^{1-\delta} \cos (j+2)\theta + \text{etc.} \right] \\ &= -c(\delta + j)e^{j+\delta} \int de e^{-\delta} \{RS\} q^{j+1} (1 + eq + e^2 q^2 + \text{etc.}); \end{aligned}$$

where  $q$  denotes  $e^\theta$ ; and, as in § 3 above, {RS} denotes realisation by taking half sum for  $\pm \iota$ . Summing the infinite series, and performing  $\int de$ , for the case  $\delta = \frac{1}{2}$ , we find

$$\begin{aligned} \mathcal{J} &= -c(j + \frac{1}{2})e^{j+\frac{1}{2}} \{RS\} q^{j+\frac{1}{2}} \log \frac{1 + \sqrt{qe}}{1 - \sqrt{qe}} \quad . . . . . (84), \\ &= -c(j + \frac{1}{2})e^{j+\frac{1}{2}} \{RS\} q^{j+\frac{1}{2}} \log \frac{1 + \sqrt{e} \cos \frac{1}{2}\theta + \iota \sqrt{e} \sin \frac{1}{2}\theta}{1 - \sqrt{e} \cos \frac{1}{2}\theta - \iota \sqrt{e} \sin \frac{1}{2}\theta} \\ &= -c(j + \frac{1}{2})e^{j+\frac{1}{2}} \{RS\} q^{j+\frac{1}{2}} \left[ \log \sqrt{\frac{1 + 2\sqrt{e} \cos \frac{1}{2}\theta + e}{1 - 2\sqrt{e} \cos \frac{1}{2}\theta + e}} + \iota(\psi - \psi') \right] \end{aligned}$$

where

$$\psi = \tan^{-1} \frac{\sqrt{e} \sin \frac{1}{2}\theta}{1 + \sqrt{e} \cos \frac{1}{2}\theta}, \quad \psi' = \tan^{-1} \frac{-\sqrt{e} \sin \frac{1}{2}\theta}{1 - \sqrt{e} \cos \frac{1}{2}\theta} . . (85),$$

and therefore

$$\psi - \psi' = \tan^{-1} \frac{2\sqrt{e} \sin \frac{1}{2}\theta}{1 - e} .$$

Hence finally

$$\mathcal{F} = c(j + \frac{1}{2})e^{j+\frac{1}{2}} \left\{ -\cos(j + \frac{1}{2})\theta \log \sqrt{\frac{1 + 2\sqrt{e} \cos \frac{1}{2}\theta + e}{1 - 2\sqrt{e} \cos \frac{1}{2}\theta + e}} \right. \\ \left. + \sin(j + \frac{1}{2})\theta \tan^{-1} \frac{2\sqrt{e} \sin \frac{1}{2}\theta}{1 - e} \right\}. \quad (86).$$

For our present case, of  $\delta = \frac{1}{2}$ , (82) gives

$$\mathcal{F} = c(j + \frac{1}{2}) \left\{ \frac{1}{2} \frac{1}{j + \frac{1}{2}} + \frac{e \cos \theta}{j - \frac{1}{2}} + \frac{e^2 \cos 2\theta}{j - \frac{3}{2}} + \dots + \frac{e^j \cos j\theta}{\frac{1}{2}} \right\} \quad (87).$$

With  $\mathcal{F}$  and  $\mathcal{G}$  thus expressed, (83) gives the solution of our problem.

§ 46. In all the calculations of §§ 46-61 I have taken  $e = \cdot 9$ , as suggested for hydrokinetic illustrations in Lecture X. of my Baltimore Lectures, pp. 113, 114, from which fig. 12, and part of fig. 11 above, are taken. Results calculated from (83), (86), (87), are represented in figs. 13-16, all for the same forcive, (74) with  $e = \cdot 9$ , and for the four different velocities of its travel, which correspond to the values 20, 9, 4, 0, of  $j$ . The wave-lengths of free waves having these velocities are [(77) above]  $2a/41$ ,  $2a/19$ ,  $2a/9$ , and  $2a$ . The velocities are inversely proportional to  $\sqrt{41}$ ,  $\sqrt{19}$ ,  $\sqrt{9}$ ,  $\sqrt{2}$ . Each diagram shows the forcive by one curve, a repetition of fig. 12; and shows by another curve the depression,  $d$ , of the water-surface produced by it, when travelling at one or other of the four speeds.

§ 47. Taking first the last, being the highest, of those speeds, we see by fig. 16 that the forcive travelling at that speed produces maximum displacement *upwards* where the *downward* pressure is greatest; and maximum *downward* displacement where the pressure (everywhere downward) is least. Judging dynamically it is easy to see that greater and greater speeds of the forcive would still give displacements above the mean level where the downward pressure of the forcive is greatest, and below the mean level where it is least; but with diminishing magnitudes down to zero for infinite speed.

And in (75) we have, for all positive values of  $J < 1$ , a series always convergent, (though sluggishly when  $e \doteq 1$ .) by which the displacement can be exactly calculated for every value of  $\theta$ .

§ 48. Take next fig. 15, for which  $J = 4\frac{1}{2}$ , and therefore, by



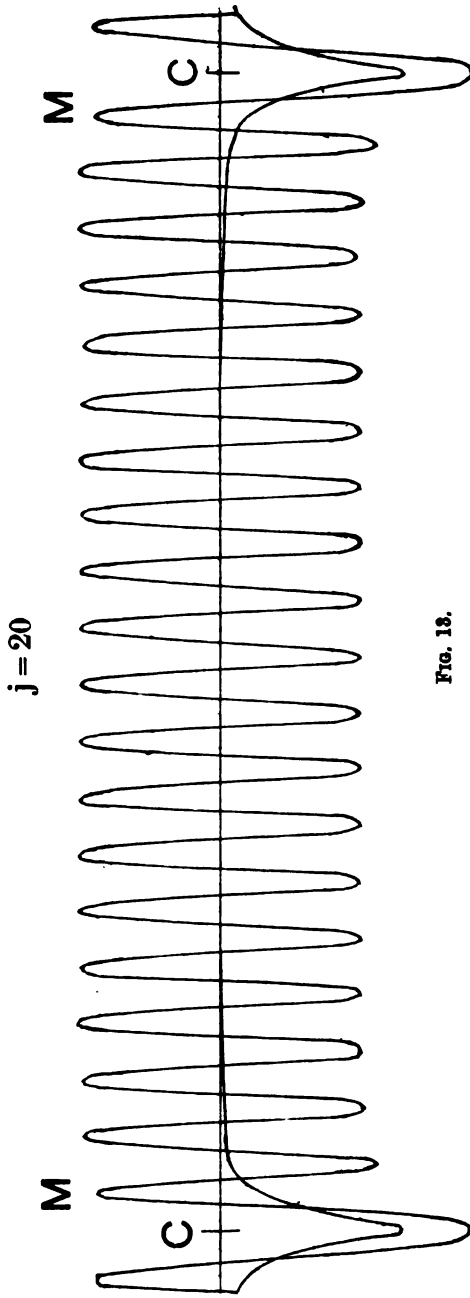


FIG. 18.

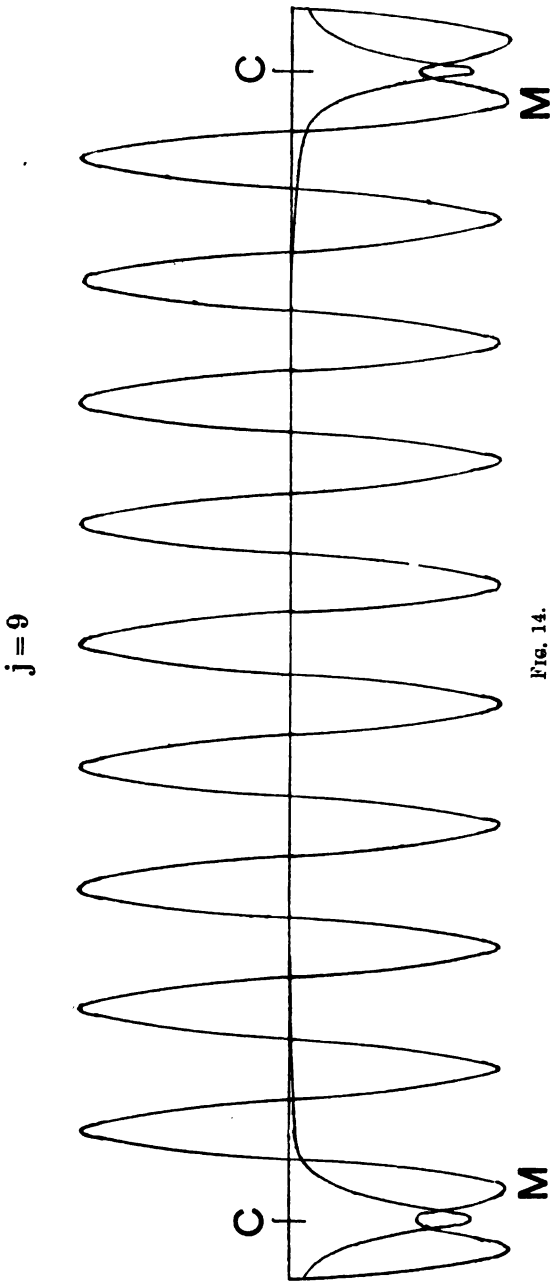


Fig. 14.

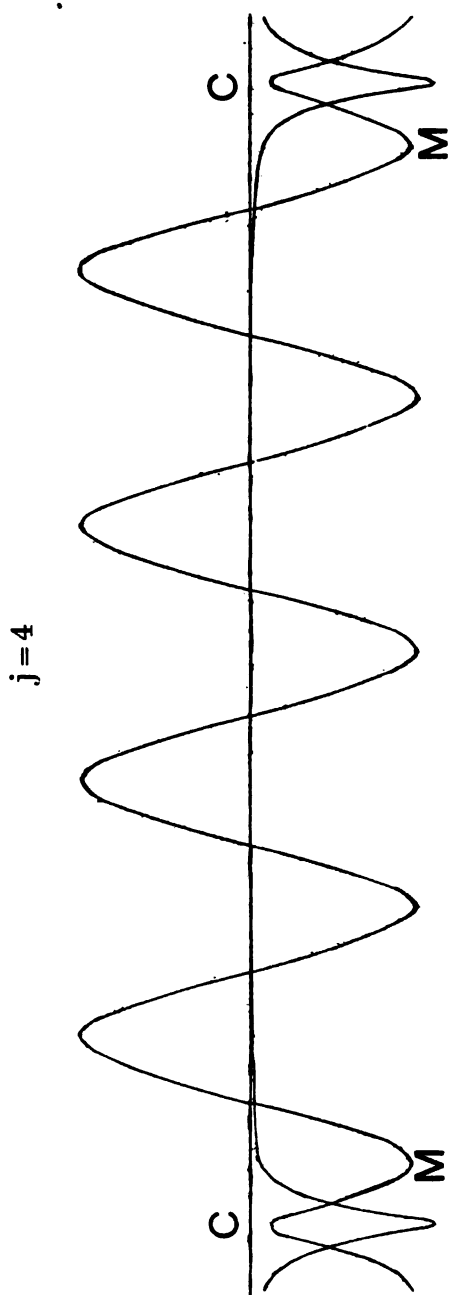


FIG. 15.

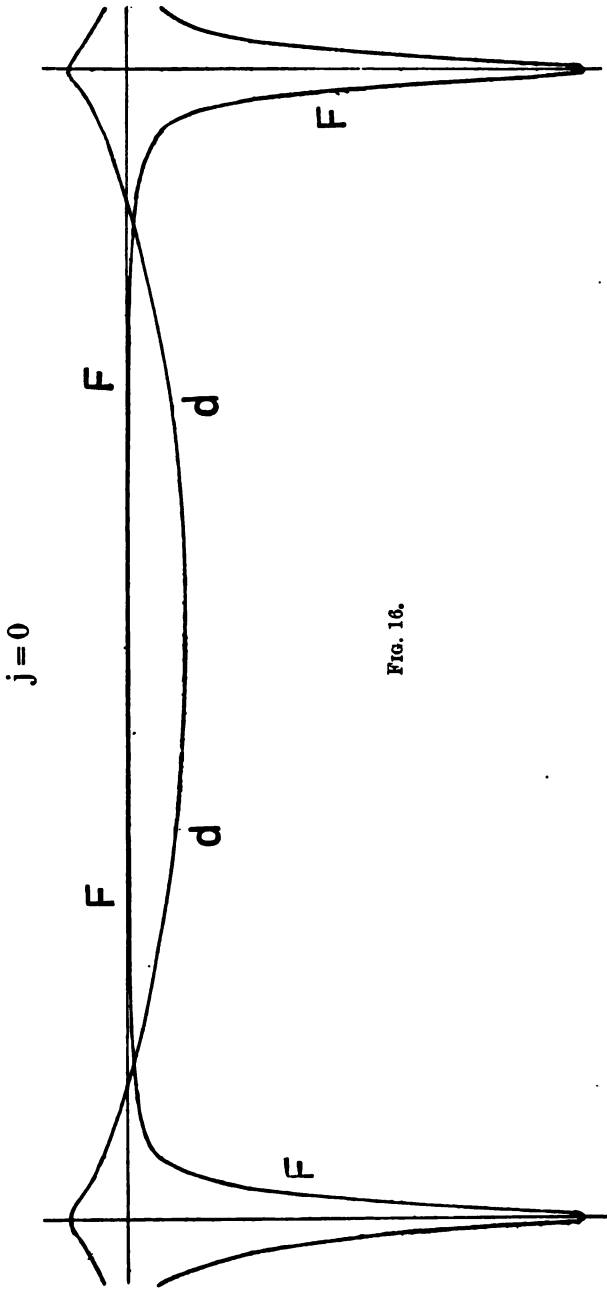


FIG. 16.

(77),  $v = \sqrt{ga/9\pi}$ , and  $\lambda = a/4.5$ . Remark that the scale of ordinates is, in fig. 15, only  $1/2.5$  of the scale in fig. 16; and see how enormously great is the water-disturbance now in comparison with what we had with the same forcive, but three times greater speed and nine times greater wave-length ( $v = \sqrt{ga/\pi}$ ,  $\lambda = 2a$ ). Within the space-period of fig. 15 we see four complete waves, very approximately sinusoidal, between M, M, two maximums of depression which are *almost exactly* (but very slightly less than) quarter wave-lengths between C and C. Imagine the curve to be exactly sinusoidal throughout, and continued sinusoidally to cut the zero line at CC.

We should thus have in CC a train of  $4\frac{1}{2}$  sinusoidal waves; and if the same is continued throughout the infinite procession . . . . CCC . . . . we have a discontinuous periodic curve made up of continuous portions each  $4\frac{1}{2}$  periods of sinusoidal curve beginning and ending with zero. The change at each point of discontinuity C is merely a half-period change of phase. A slight alteration of this discontinuous curve within  $60^\circ$  on each side of each C, converts it into the continuous wavy curve of fig. 15, which represents the water-surface due to motion at speed  $\sqrt{ga/9\pi}$  of the pressural forcive represented by the other continuous curve of fig. 15.

§ 49. Every word of § 48 is applicable to figs. 14 and 13 except references to *speed* of the forcive, which is  $\sqrt{ga/19\pi}$  for fig. 14 and  $\sqrt{ga/41\pi}$  for fig. 13; and other statements requiring modification as follows:—

For  $4\frac{1}{2}$  "periods" or "waves," in respect to fig. 15; substitute  $9\frac{1}{2}$  in respect to fig. 14, and  $20\frac{1}{2}$  in respect to fig. 13.

For "depression" in defining M M in respect to figs. 15, 14; substitute *elevation* in the case of fig. 13.

§ 50. How do we know that, as said in § 48, the formula  $\{(83), (86), (87)\}$  gives for a wide range of about  $120^\circ$  on each side of  $\theta = 180^\circ$ ,

$$d(\theta) \doteq (-1)^j d(180^\circ) \cdot \sin(j + \frac{1}{2})\theta \quad \dots \quad (88),$$

which is merely §§ 48, 49 in symbols? *it being understood that j is any integer not  $< 4$ ; and that e is .9, or any numeric between .9 and 1?* I wish I could give a *short* answer to this question

without help of hydrokinetic ideas! Here is the only answer I can give at present.

§ 51. Look at figs. 13-16, and see how, in the forcive defined by  $e = \cdot 9$ , the pressure is almost wholly confined to the spaces  $\theta < 60^\circ$  on each side of each of its maximums, and is very nearly null from  $\theta = 60^\circ$  to  $\theta = 300^\circ$ . It is obvious that if the pressure were perfectly annulled in these last-mentioned spaces, while in the spaces within  $60^\circ$  on each side of each maximum the pressure is that expressed by (74), the resulting motion would be sensibly the same as if the pressure were throughout the whole space CC ( $\theta = 0^\circ$  to  $\theta = 360^\circ$ ), exactly that given by (74). Hence we must expect to find through nearly the whole space of  $240^\circ$ , from  $60^\circ$  to  $300^\circ$ , an almost exactly sinusoidal displacement of water-surface, having the wave-length  $360^\circ / (j + \frac{1}{2})$  due to the translational speed of the forcive.

§ 52. I confess that I did *not* expect so small a difference from sinusoidality through the *whole*  $240^\circ$ , as calculation by {(83), (86), (87)} has proved; and as is shown in figs. 18, 19, 20, by the D-curve on the right-hand side of C, which represents in each case the value of

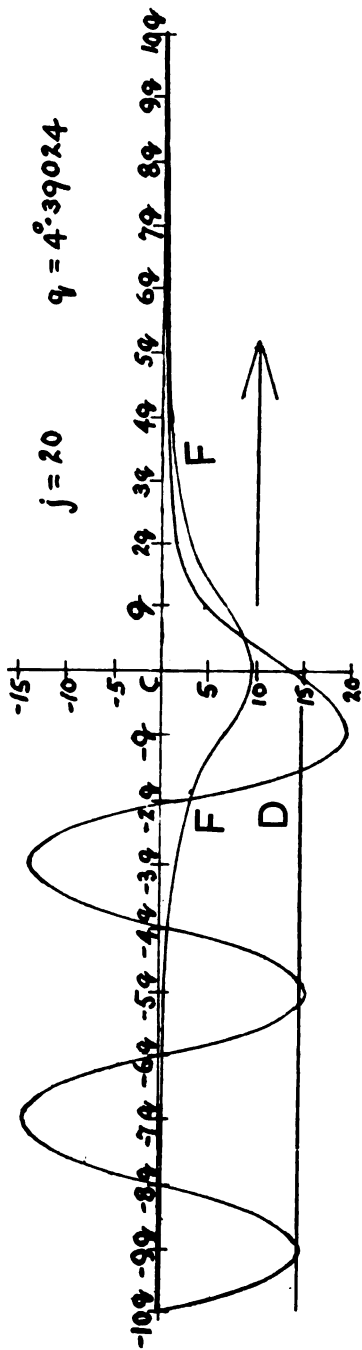
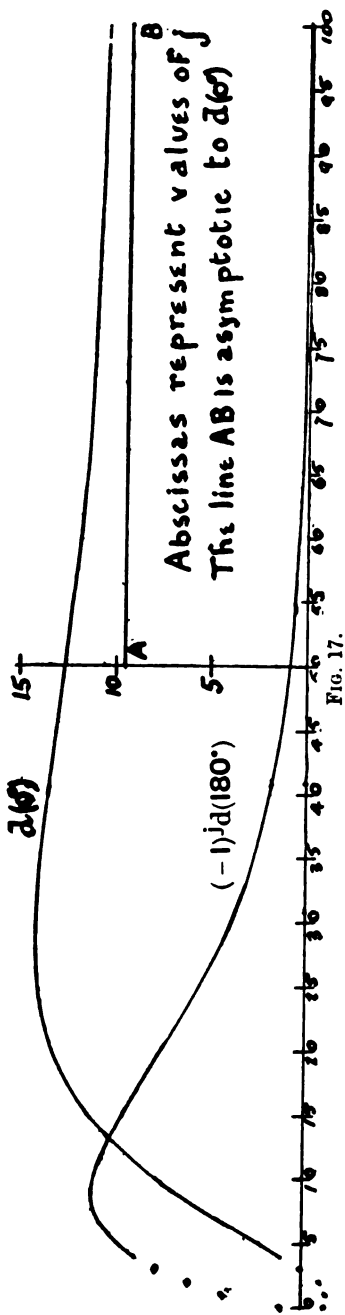
$$D(\theta) = d(\theta) - (-1)^j d(180^\circ) \cdot \sin(j + \frac{1}{2})\theta \dots (89),$$

being the difference of  $d(\theta)$  from one continuous sinusoidal curve. The exceeding smallness of this difference for distances from C exceeding  $20^\circ$  or  $30^\circ$ , and therefore through a range between CC of  $320^\circ$ , or  $300^\circ$ , is very remarkable in each case.

§ 53. The dynamical interpretation of (88), and figs. 18, 19, 20, is this:—Superimpose on the solution {(83), (86), (87)} a “free wave” solution according to (73), taken as

$$-(-1)^j d(180^\circ) \cdot \sin(j + \frac{1}{2})\theta \dots (90).$$

This approximately annuls the approximately sinusoidal portion between C and C shown in figs. (13), (14), (15); and approximately doubles the approximately sinusoidal displacement in the corresponding portions of the spaces CC, and CC on the two sides of C. This is a very interesting solution of our problem § 41; and, though it is curiously artificial, it leads direct and short to *the* determinate solution of the following general problem of canal ship-waves:—



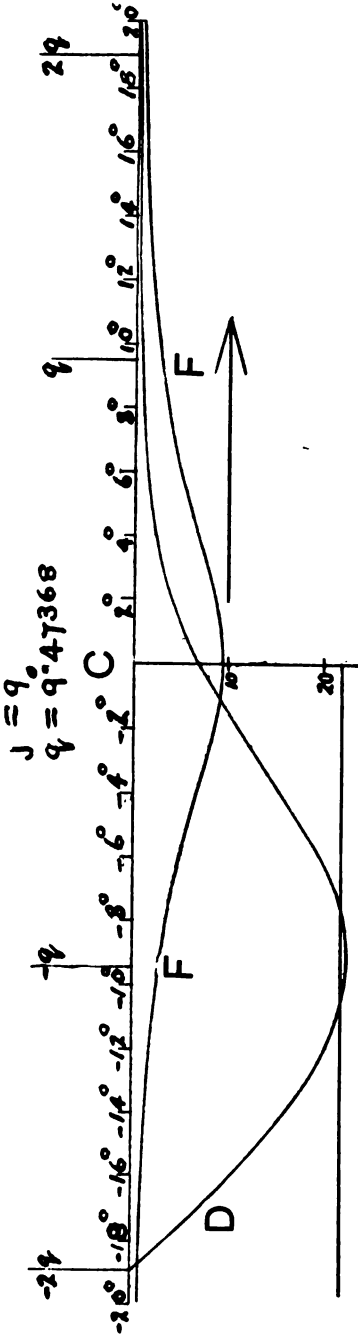


FIG. 19.

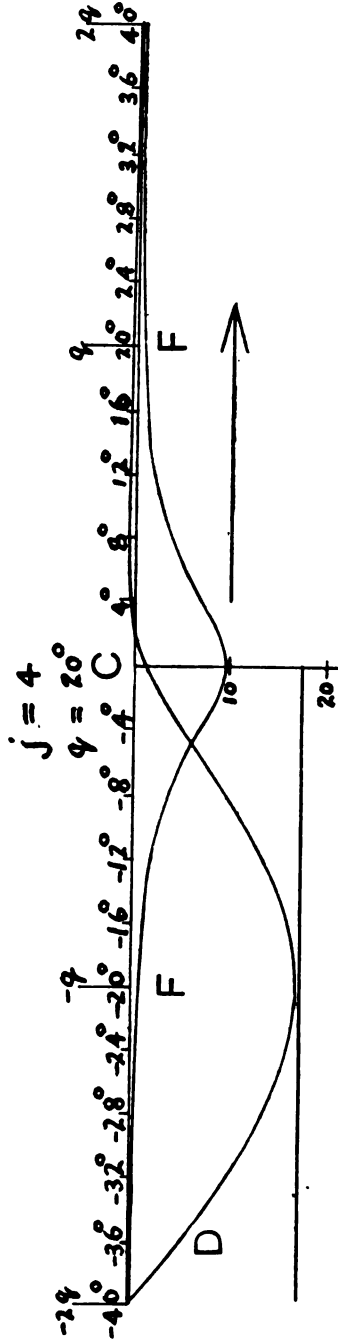


FIG. 20.



§ 54. Given, as forcive, the isolated distribution of pressure defined in fig. 12, travelling at a given constant speed; required *the steady distribution of displacement of the water in the place of the forcive, and before it and behind it; which becomes established after the motion of the forcive has been kept steady for a sufficiently long time.* Pure synthesis of the special solution given in §§ 1-10 above, solves not only the problem now proposed, but gives the whole motion from the instant of the application of the moving forcive. This synthesis, though easily put into formula, is not easily worked out to any practical conclusion. On the other hand, here is my present short but complete solution of the problem of steady motion for which we have been preparing, and working out illustrations in §§ 32-53.

Continue leftward, indefinitely, as a curve of sines, the D curve of each of figs. 18, 19, 20; leaving the forcive curve, F, isolated, as shown already in these diagrams. Or, analytically stated:— in (89) calculate the equal values of  $d(\theta)$  for equal positive and negative values of  $\theta$  from  $0^\circ$  to  $40^\circ$  or  $50^\circ$  by {(83), (86), (87)}; and for all larger values of  $\theta$  take

$$d(\theta) \doteq (-1)^j d(180^\circ) \sin(j + \frac{1}{2})\theta \dots \dots (91),$$

where  $d(180^\circ)$  is calculated by {(83), (86), (87)}. This used in (89), makes  $D(\theta) \doteq 0$  for all positive values of  $\theta$  greater than  $40^\circ$  or  $50^\circ$ ; and makes it the double of (91) for all negative values of  $\theta$  beyond  $-40^\circ$  or  $-50^\circ$ .

§§ 55, 56. *Rigid Covers or Pontoons, introduced to apply the given forcive (pressure on the water-surface).*

§ 55. In any one of our diagrams showing a water-surface imagine a rigid cover to be fixed, fitting close to the whole water-surface. Now look at the forcive curve, F, on the same diagram, and wherever it shows no sensible pressure remove the cover. The motion (non-motion in some parts) of the whole water remains unchanged. Thus, for example, in figs. 13, 14, 15, 16, let the water be covered by stiff covers fitting it to  $60^\circ$  on each side of each C; and let the surface be free from  $60^\circ$  to  $300^\circ$  in each of the spaces between these covers. The motion remains unchanged

under the covers, and under the free portions of the surface. The pressure  $\Pi$  constituting the given forcive, and represented by the F curve in each case, is now automatically applied by the covers.

§ 56. Do the same in figs. 18, 19, 20 with reference to the isolated forcives which they show. Thus we have three different cases in which a single rigid cover, which we may construct as the bottom of a floating pontoon, kept moving at a stated velocity relatively to the still water before it, leaves a train of sinusoidal waves in its rear. The D curve represents the bottom of the pontoon in each case. The arrow shows the direction of the motion of the pontoon. The F curve shows the pressure on the bottom of the pontoon. In fig. 20 this pressure is so small at  $-2q$  that the pontoon may be supposed to end there; and it will leave the water with free surface almost exactly sinusoidal to an indefinite distance behind it (infinite distance if the motion has been uniform for an infinite time). The F curve shows that in fig. 19 the water wants guidance as far back as  $-3q$ , and in fig. 18 as far back as  $-8q$  to keep it sinusoidal when left free;  $q$  being in each case the quarter wave-length.

§§ 57-60. *Shapes for Waveless Pontoons, and their Forcives.*

§ 57. Taking any case such as those represented in figs. 18, 19, 20; we see obviously that if any two equal and similar forcives are applied, with a distance  $\frac{1}{2}\lambda$  between corresponding points, and if the forcive thus constituted is caused to travel at speed equal to  $\sqrt{g\lambda/2\pi}$ , being, according to (77) above, the velocity of free waves of length  $\lambda$ , the water will be left waveless (at rest) behind the travelling forcive.

§ 58. Taking for example the forcives and speeds of figs. 18, 19, 20, and duplicating each forcive in the manner defined in § 57, we find, (by proper additions of two numbers, taken from our tables of numbers calculated for figs. 18, 19, 20,) the numbers which give the depressions of the water in the three corresponding waveless motions. These results are shown graphically in fig. 21, on scales arranged for a common velocity. The free wave-length for this velocity is shown as  $4q$  in the diagram.

§ 59. The three forcives, and the three waveless water-shapes

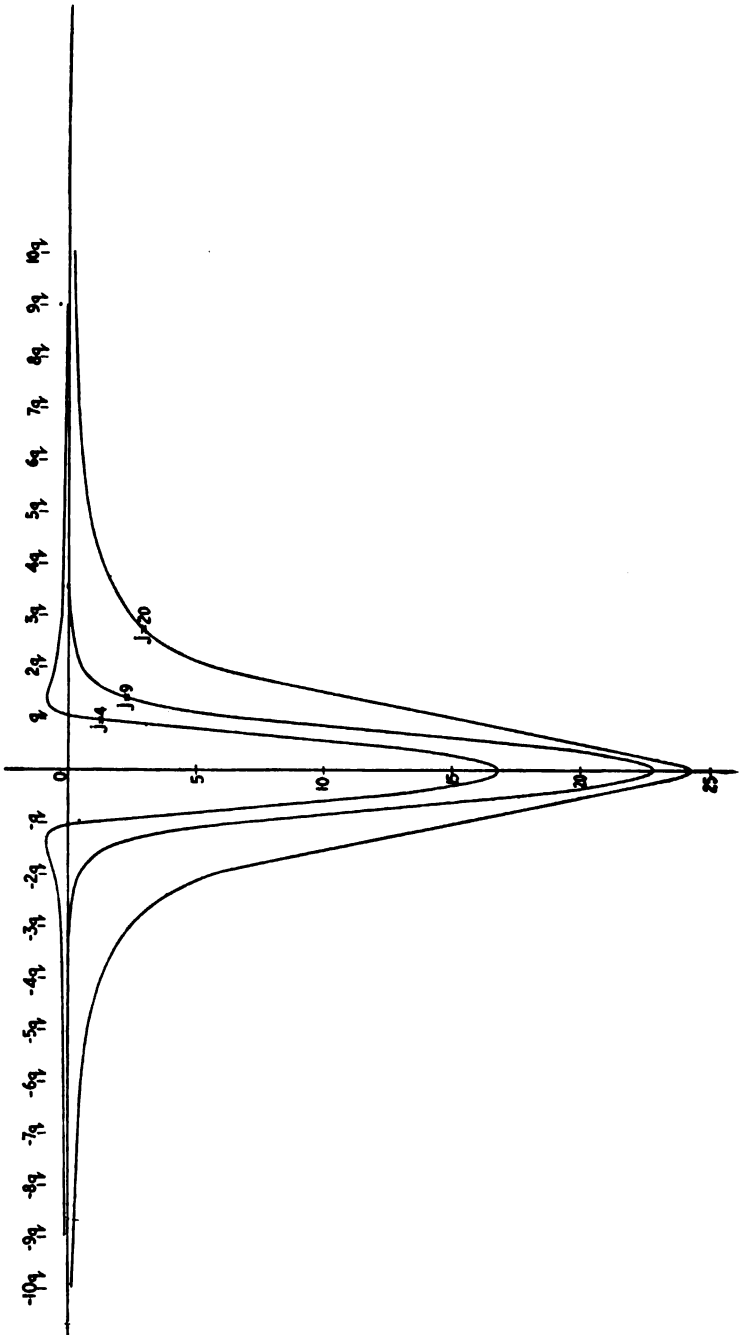


FIG. 21.

produced by them, are shown in figs. 22, 23, 24 on different scales, of wave-length, and pressure, chosen for the convenience of each case.

§ 60. As most interesting of the three cases take that derived from  $j=9$  of our original investigation. By looking at fig. 23 we see that a pontoon having its bottom shaped according to the D curve from  $-3q$  to  $+3q$ ,  $1\frac{1}{2}$  free wave-lengths, will leave the water sensibly flat and at rest if it moves along the canal at the velocity for which the free-wave-length is  $4q$ . And the pressure of the water on the bottom of the pontoon is that represented hydrostatically by the F curve.

§ 61. Imagine the scale of abscissas in each of the four diagrams, figs. 21-24, to be enlarged tenfold. The greatest steepnesses of the D curve in each case are rendered sufficiently moderate to allow it to fairly represent a real water-surface under the given forcive. The same may be said of figs. 15, 16, 18, 19, 20; and of figs. 13, 14 with abscissas enlarged twentyfold. In respect to mathematical hydrokinetics generally; it is interesting to remark that a very liberal interpretation of the condition of infinitesimality (§ 36 above) is practically allowable. Inclinations to the horizon of as much as  $1/10$  of a radian ( $5^{\circ}7'$ ; or, say,  $6^{\circ}$ ), in any real case of water-waves or disturbances, will not seriously vitiate the mathematical result.

§ 62. Fig. 17 represents the calculations of  $d(0^{\circ})$  and  $(-1)d(180^{\circ})$  for twenty-nine integral values of  $j$ ; 0, 1, 2, 3, . . . . 19, 20, 30, 40, . . . . 90, 100; from the following formulas, found by putting  $\theta=0^{\circ}$  and  $\theta=180^{\circ}$ ; and with  $e=.9$  in each case, and  $c=1$

$$d_j(0^{\circ}) = (2j+1)e^j \left[ -\frac{1}{2}e^{\theta} \log \frac{1+\sqrt{e}}{1-\sqrt{e}} + 1 + \frac{e^{-1}}{3} + \frac{e^{-2}}{5} + \dots + \frac{e^{-j+1}}{2j-1} + \frac{1}{2} \frac{e^{-j}}{2j+1} \right] \quad (92),$$

$$d_j(180^{\circ}) = (-1)^j(2j+1)e^j \left[ \frac{1}{2}e^{\theta} \tan^{-1} \frac{2\sqrt{e}}{1-e} + 1 - \frac{e^{-1}}{3} + \frac{e^{-2}}{5} + \dots + (-1)^{j-1} \frac{e^{-j+1}}{2j-1} + (-1)^j \frac{1}{2} \frac{e^{-j}}{2j+1} \right] \quad (93).$$

The asymptote of  $d(0^{\circ})$  shown in the diagram is explained by remarking that when  $j$  is infinitely great, the travelling velocity of

the forcive is infinitely small ; and therefore, by end of § 41, the depression is that hydrostatically due to the forcive pressure. This, at  $\theta = 0^\circ$ , is equal to

$$\frac{1}{2} \frac{1+e}{1-e} c = \frac{1.9}{2} c = 9.5.c.$$

§ 63. The interpretation of the curves of fig. 17 for points between those corresponding to integral values of  $j$  is exceedingly interesting. We shall be led by it into an investigation of the disturbance produced by the motion of a single forcive, expressed by

$$\Pi = \frac{gcb}{b^2 + x^2} \dots \dots (94);$$

but this must be left for a future communication, when it will be taken up as a preliminary to sea ship-waves.

§ 64. The plan of solving by aid of periodic functions the two-dimensional ship-wave problem for infinitely deep water, adopted in the present communication, was given in Part IV. of a series of papers on Stationary Waves in Flowing Water, published in the *Philosophical Magazine*, October 1886 to January 1887, with analytical methods suited for water of finite depths. The annulment of sinusoidal waves in front of the source of disturbance (a bar across the bottom of the canal), by the superposition of a train of free sinusoidal waves which double the sinusoidal waves in the rear, was illustrated (December 1886) by a diagram on a scale too small to show the residual disturbance of the water in front, described in § 53 above, and represented in figs. 18, 19, 20.

In conclusion, I desire to thank Mr J. de Graaff Hunter for his interested and zealous co-operation with me in all the work of the present communication, and for the great labour he has given in the calculation of results, and their representation by diagrams.

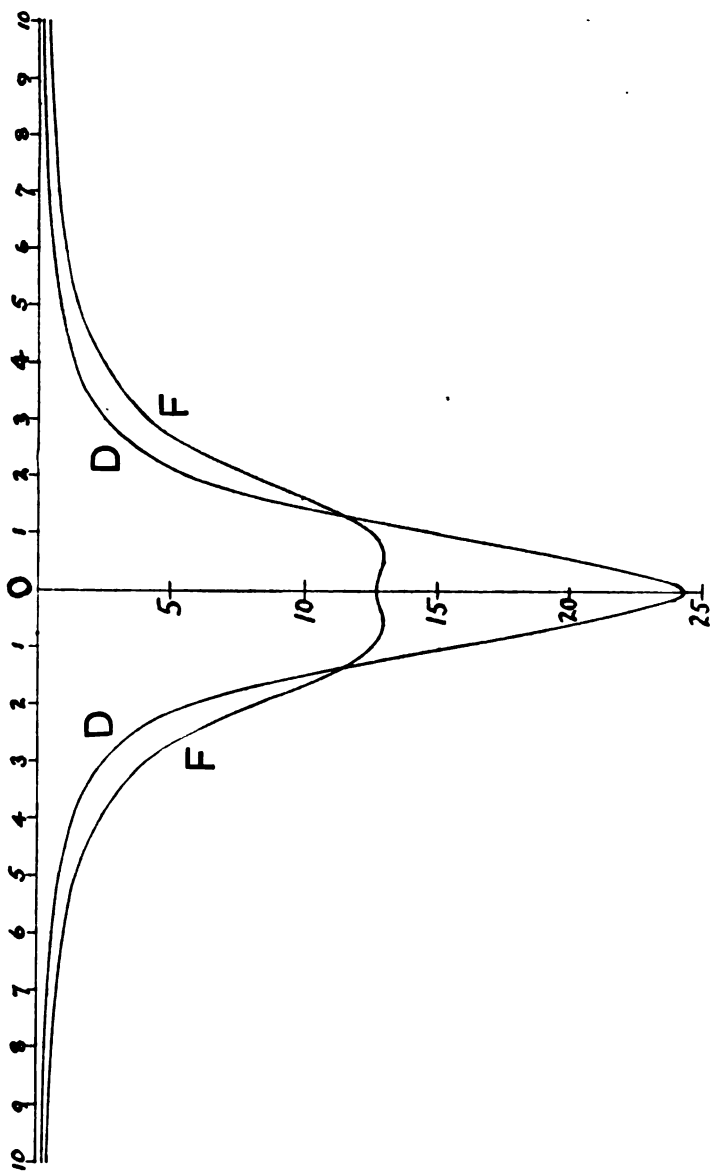


FIG. 22 ;  $j = 20$ .—Scale of abscissas is quarter-wave-lengths.

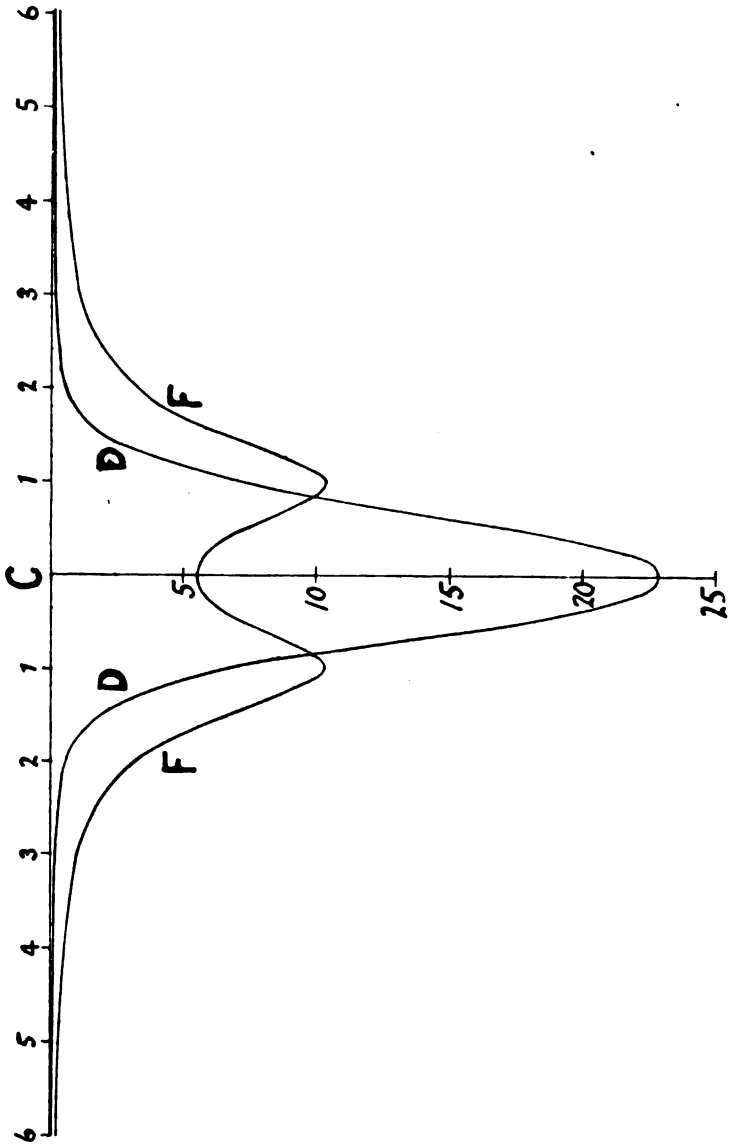


FIG. 28 ;  $j=9$ .

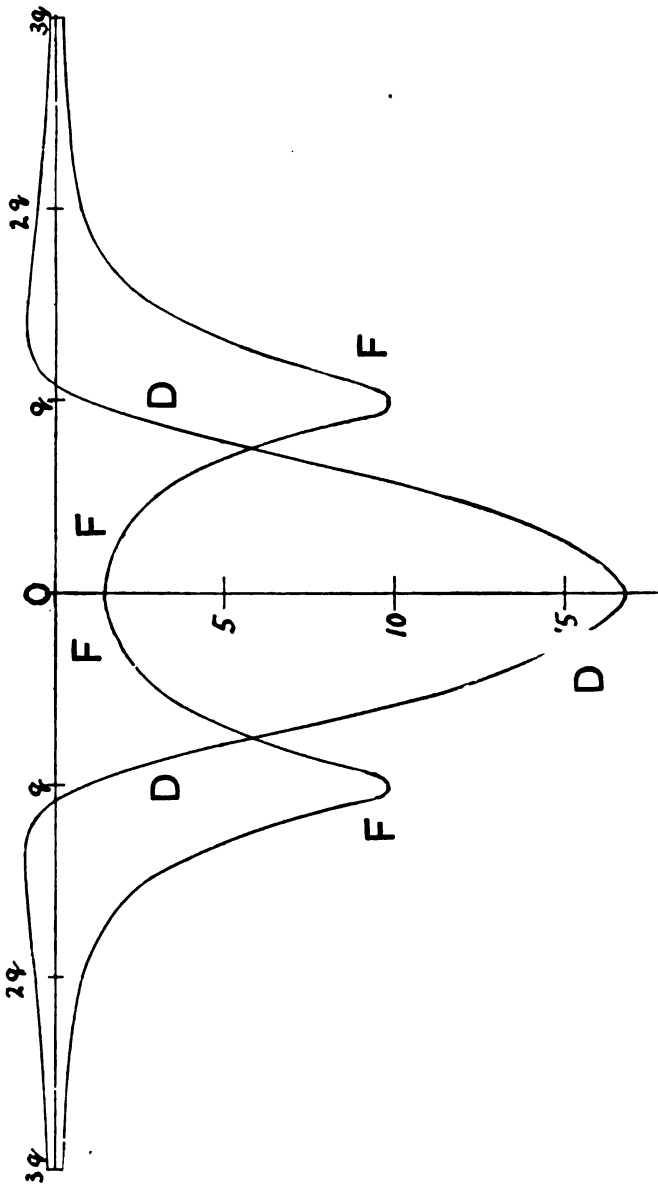


FIG. 24 ;  $j=4$ . —Scale of abscissas is quarter-wave-lengths.

(Issued separately April 18, 1905.)



**On Two Liquid States of Sulphur  $S_\lambda$  and  $S_\mu$  and their Transition Point. By Alexander Smith.**

(MS. received February 17, 1905. Read March 20, 1905.)

(*Abstract.*)

It is well known that melted sulphur when heated becomes suddenly dark brown and viscous in the neighbourhood of  $160^\circ$ . These and other facts rendered it probable that there was a transition point in liquid sulphur, and that two liquid states could be proved to exist, one of them being stable below the transition point, and the other above it. According to the phase rule, a single substance can exist in three phases (two liquid and one vapour phase) only as a non-variant system at a single temperature and pressure. Thus, if the two liquid forms were not completely miscible, the lower one might form the greater part of the material until, as the temperature rose, it became saturated with the upper one and a new phase separated out. This phenomenon would mark the transition point, and the smallest further rise in temperature would cause the complete disappearance of the first phase. The substance would then contain a small proportion of the lower form in solution in the upper, and this proportion would diminish with rising temperature. No case of an exactly parallel nature is known; but the transition from "liquid crystals" to an isotropic liquid in the case of certain organic compounds is to a certain extent analogous.

For the discovery of a transition point of this kind the study of the progressive change in any physical property as the temperature rises is available. The author examined successively the change in viscosity, the change in solubility, the variation in the coefficient of dilatation, and the rather marked absorption of heat which is observed to accompany the sudden onset of viscosity in the fluid. The results were as follows:—

1. In melted sulphur, easily perceptible viscosity appears first at  $159.5^\circ$ . At  $160^\circ$  the viscosity is already very great.
2. When sulphur is held at  $162.5^\circ$  or any higher temperature a sudden absorption of heat and simultaneous sudden access of viscosity occur, and the temperature falls to  $162^\circ$ . The transition point is therefore not above the latter temperature.
3. Distilled sulphur does not show either of these phenomena

so sharply as does recrystallised sulphur, and seems to be subject to superheating.

4. It was shown in a previous paper (*Proc. Roy. Soc. Edin.*, vol. xxiv. p. 342) that ordinary sulphur owes to the presence of sulphur dioxide its tendency to give amorphous sulphur when chilled, and that sulphur which while melted has been treated with ammonia, gives when quenched nothing but soluble crystalline sulphur. The phenomena described in 1 and 2 above take place in the same way and at precisely the same temperatures, whether the sulphur concerned is such as by chilling gives insoluble sulphur, or, having been treated with ammonia, does not.

5. The existence of two independent curves of solubility for the two kinds of liquid sulphur in triphenylmethane and other solvents is demonstrated. The solubility of yellow mobile sulphur ( $S_\lambda$ ) increases, that of brown viscous sulphur ( $S_\mu$ ) decreases, with rise in temperature.

6. The expansion of yellow mobile sulphur ( $S_\lambda$ ) diminishes rapidly from  $154^\circ$  to  $160^\circ$ ; that of brown viscous sulphur ( $S_\mu$ ) increases rapidly from  $160^\circ$  upwards. The statement under 4 holds in this case also.

7. The dilatometric method gives no evidence of the existence of Frankenheim's transition point ( $250-260^\circ$ ).

8. It is shown that the point of minimum dilatation is displaced upwards when triphenylmethane is dissolved in the sulphur. The displacement averages  $2.8^\circ$  for 1 per cent. of this foreign body.

9. The production of the new phase is easily to be seen when strongly heated brown viscous sulphur is allowed to cool in a test-tube. The radiation from the greater surface at the bottom causes the formation of the mobile yellow liquid first in that region. The interface between the two varieties is quite distinct, and recedes slowly up the tube as the transition proceeds. Owing, however, to the progress of the change *within* the upper brown layer, the interface gradually becomes indistinct.

10. It is thus shown conclusively that there are two liquid states of sulphur, which are partially, but only partially, miscible. These are  $S_\lambda$ , which predominates from the melting point to  $160^\circ$ , and  $S_\mu$ , which prevails above  $160^\circ$ . As the temperature ascends, saturation of the former with the latter determines the separation of the new phase, and conversely when the temperature falls.

(Issued separately April 18, 1905.)

**The Nature of Amorphous Sulphur, and Contributions to the Study of the Influence of Foreign Bodies on the Phenomena of Supercooling observed with Melted Sulphur is suddenly Chilled. By Alexander Smith.**

(MS. received February 17, 1905. Read March 20, 1905.)

(*Abstract.*)

1. The hardening of plastic sulphur was investigated, and it was found that partial reversion to soluble sulphur prevents the securing in quasi-solid form of the whole of the amorphous sulphur present. It was discovered, however, that sulphur formed by precipitation in presence of concentrated acids does yield 100 per cent. of insoluble sulphur, and that only the impossibility of realising the requisite condition of very fine subdivision is therefore responsible for the smaller yields from melted sulphur which has reached the highest temperatures previous to being chilled.

2. A new series of measurements of the proportions of insoluble sulphur formed when common sulphur is chilled from various temperatures was made. The amounts vary from 4.2 per cent. at 130° to 34 per cent. at 448°. In this, and in all other cases described below, only the insoluble sulphur which remains after the viscous material has completely hardened was estimated.

3. It was found that when sulphur was subjected to prolonged heating at 448°, or was heated for a shorter time *in vacuo*, or was used immediately after recrystallisation, or was washed with water before being heated, the amount of insoluble sulphur obtainable by chilling was greatly reduced. The effects of these modes of treatment seemed to be to remove a trace of sulphuric acid which sulphur acquires by exposure to the air.

4. It appeared that gases like carbon dioxide, and particularly ammonia and hydrogen sulphide, when led through melted sulphur,

destroyed the ability to give insoluble sulphur. Their use did not however, affect the viscosity above  $160^{\circ}$ .

5. It was shown that air and sulphur dioxide restored the ability to give insoluble sulphur. The halogens, the halogen hydrides, and even dry phosphoric acid, had the same effect.

6. It was found that sulphur which had been treated with ammonia while melted, and had afterwards been recrystallised, if used at once, froze at  $119.17^{\circ}$  and contained no insoluble sulphur. In a previous investigation (*Proc. R.S.E.*, vol. xxiv. p. 299) it had been shown that insoluble sulphur, when present, depressed the freezing point, in accordance with Raoult's law.

7. Sulphur containing iodine (100:2) gave when heated and chilled large amounts of insoluble sulphur. These ranged from 4 per cent. at  $110^{\circ}$  to 62.7 per cent. at  $448^{\circ}$ .

8. The amount of insoluble sulphur obtained at  $150^{\circ}$  was proportional to the quantity of iodine present when the quantity of the latter was 1 per cent. or more.

9. Sulphur prepared by distilling the element and quenching the burning stream in ice-water gave 51 per cent. of insoluble sulphur. Chilling boiling sulphur in ether gave 44.1 per cent. of the insoluble form.

10. It was demonstrated, by identity in boiling points under ordinary and reduced pressures, and by identity in specific gravities, that sulphur which will give the insoluble form when chilled is identical in constitution near the boiling point with that which will not.

11. It was shown by identity in solubility between  $120^{\circ}$  and  $160^{\circ}$  that the two kinds of sulphur mentioned in 10 are identical in constitution also below the transition point of  $S_{\mu}$  to  $S_{\lambda}$  ( $160^{\circ}$ ).

12. The facts referred to in 10 and 11, together with the conclusions of the preceding paper showing the identity of the two kinds of sulphur at the transition point ( $160^{\circ}$ ) itself, demonstrate that the insoluble form is present in all specimens of melted sulphur in proportions depending upon the temperature alone, whether, by treatment with ammonia or otherwise, they have lost the capacity to give insoluble sulphur by chilling or not.

13. The conclusion is reached that amorphous sulphur is supercooled  $S_{\mu}$ ,—the form stable above  $160^{\circ}$ .

14. With pure sulphur, freed from sulphur dioxide by recrystallisation or by treatment with carbon dioxide above  $310^{\circ}$ , or by treatment with ammonia or hydrogen sulphide at any temperature at which it is fluid, the  $S_{\mu}$  reverts so rapidly to the soluble form that it cannot be supercooled. When traces of sulphur dioxide, iodine, and other substances are present,  $S_{\mu}$  is more or less completely supercooled and gives amorphous sulphur. The way which the latter class of foreign substances produces this effect is still being investigated.

15. There is a close analogy of these phenomena to that observed in the cooling of cast-iron and steel.

*(Issued separately April 18, 1905.)*

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