Nature

A WEEKLY

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" To the solid ground
Of Nature trusts the mind which builds for aye."—WORDSWORTH

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THURSDAY, NOVEMBER 4, 1886

EXPLOSIONS IN COAL-MINES

Explosions in Coal-Mines. By W. N. and J. B. Atkinson, H.M. Inspectors of Mines. (London: Longmans, 1886.)

EVERYBODY in the least degree conversant with matters connected with coal-mining will at once admit that our knowledge of the remote causes of colliery explosions has increased enormously during the last few years. Whether, however, the practical application of this knowledge has kept pace with the rate of increase in the knowledge itself is another matter. Since 1851, when the first Mines Inspection Act was in force, the number of fatal explosions in collieries has steadily diminished, but the annual loss of life from these catastrophes is as great as ever. During the ten years ending 1860 there were 820 fatal explosions, resulting in 2441 deaths, or an average of 2·98 deaths per fatal explosion; during this decade there was an average of 3000 persons employed in and about the mines for every fatal explosion, and 1008 persons for each resulting death. During the ten years ending 1870 the number of fatal explosions fell to 565; the deaths were 2267, or an average of 4·01 per fatal explosion; and the ratio of persons employed to each fatal explosion was 5650, and hence to each resulting death 1408. During the ten years ending 1880 the number of fatal explosions was 424; the resulting deaths were 2686, or an average of 6·33 per fatal explosion; the ratio of persons employed to each fatal explosion was 11,372, and to each resulting death 1795. During the five years ending 1885 we have had 146 fatal explosions, with a loss of 906 persons, or an average of 6·20 deaths from each explosion; the ratio of persons employed to each fatal explosion was 17,503, and to each resulting death 2820. These figures are in the highest degree significant, but they are not capable of telling everything. They do not, for example, bring out the fact that the actual violence of colliery explosions when they do occur is nowadays greater than formerly. This may seem to be indicated by the increase in the average number of deaths from each fatal explosion, but then, on the other hand, there are far more men employed in pits now than formerly. The diminished number of explosions is probably due, in the first instance, to the more general employment of safety-lamps, and, during late years, to the restrictions which have been placed upon the use of explosives. The increase in the average number of deaths to each explosion is doubtless owing to the gradual deepening of the pits and to differences in the mode of origin and character of the explosion. Thirty years ago the pits as a rule were comparatively shallow and damp. Such a sinking as that of the Ashton Moss pit at Audenshaw, which is upwards of half a mile deep, was unknown. Explosions in these damp shallow pits were usually caused by the ignition of gas, most frequently by naked lights; they were very local in their action, and the loss of life was small. Nowadays an explosion in a deep and dry mine not unfrequently penetrates throughout the whole pit; it is often extremely violent, and the number of deaths, mainly from after-damp, is correspondingly great.

There can be very little doubt that such explosions are, in the main, caused by dust. The fact that fire-damp is not the only explosive agent which may be present in coal-mines is now generally recognised. It is, however, a moot point with many practical men whether coal-dust alone, in the entire absence of gas, can bring about an explosion of any magnitude. It is generally conceded that a very small amount of gas, an amount, indeed, too small to be recognised by the elongation of the flame of a safety-lamp, or the formation of a "cap," is sufficient in the presence of coal-dust to form a dangerously explosive atmosphere, but colliery managers and many mining engineers have, apparently, been slow to believe that dust itself may, under certain conditions, effect an explosion quite as violent in its character as the most formidable gas explosion of which we have any record. The Royal Commissioners appointed to inquire into accidents in mines reported that in their opinion it was well established that even when the air is quite free from fire-damp, an exceptionally inflammable coal-dust, in a very finely-divided and dry condition, and existing in abundance in the immediate vicinity of a blown-out shot, may when raised by the shot be ignited so readily and carry on the flame so rapidly that it may produce explosive effects of a similar character to those caused by a gas explosion. The flame as it rushes along, if fed by freshly raised dust, may extend under these circumstances to very considerable distances, with results resembling, in their disastrous nature, those of explosions originating with, and mainly due to, fire-damp. This conclusion is very greatly strengthened by the evidence which the Messrs. Atkinson have brought together in the book before us. Their work indeed constitutes the most formidable indictment against coal-dust as a cause of colliery explosions which has yet been drawn up. In their capacity of Inspectors they have investigated with the most patient care the circumstances connected with what we may call six typical explosions. These were—

Date	Name of colliery	Deaths	Time of explosion	Seams affected
1880				
Sept. 8	Seaham	164	2.20 a.m.	Maudlin and Hutton
1882				
Feb. 16	Trimdon Grange	74	2.30 p.m.	Harvey
April 18	Tudhoe	37	1.15 a.m.	Brockwell
April 19	West Stanley	13	1.0 a.m.	Basty
April 25	Whitehaven	4	11.15 p.m.	Main Band
1885				
March 2	Usworth	42	8.58 p.m.	Maudlin

All the explosions with the exception of that at Whitehaven were in the county of Durham. It would be quite impossible in the space at our disposal to follow the successive steps in the minute analysis to which the authors have subjected each of these explosions. We should require, moreover, many of the numerous plans of the colliery workings with which the book is illustrated were we to attempt such a task. All that can now be done is to point out the characteristic features of the several explosions, and to indicate the general conclusions which the authors draw from the consideration of the various circumstances connected with them. We are conscious that in some respects this method of treating their work hardly does justice to the authors. It fails to convey any idea of the thoroughly scientific manner in which the Messrs. Atkinson's investigations have been conducted; of the minute and painstaking mode of their observation; or of the care and skill with which their deductions have been made. The authors, even in the earlier pages of the book, make their position in regard to the question of Gas *versus* Dust perfectly clear, but not even the most prejudiced opponent of the dust hypothesis can complain of the manner in which the evidence is presented.

The Durham explosions presented many features in common. In the first place no accumulations of gas were known to exist in quantity sufficient to cause the widespread destruction which happened, nor were such accumulations considered possible. In all these explosions the downcast shafts were more or less damaged. At Trimdon Grange, Tudhoe, West Stanley, and Usworth the explosions did not cross the downcast shafts; these were wet, and the roadways near them were damp. At Seaham the shaft was dry, and the explosion crossed it and extended far beyond it. In all cases the violence and ame of the explosions were confined to roads on which there was much coal-dust. The explosions were most violent in the intake and haulage roads, or between the downcast shafts and lamp-stations, *i.e.* in places where practically no gas was to be expected, and where naked lights were in constant use. The path of the explosion was in all cases that of the fresh air traversing the pit: in no case did it extend by means of the return air-way The return air-ways carry off the gases evolved in the pit, but are practically free from dust. In certain of the intake air-ways at Seaham and Usworth no coals were led, and they were consequently comparatively free from coal-dust: no traces of the explosions were observed in these roads. The explosions were in many cases arrested where the haulage roads were wet. In no instance did the explosion ascend or descend vertically through staples or shafts communicating with other planes of workings. If the explosions were due to gas, their extension would not be influenced by the direction of a communicating passage; on the other hand, very little coal-dust collects in vertical passages. In almost every case of an explosion which could with certainty be attributed to fire-damp, there is evidence that men have been alarmed and have attempted to escape from the workings before the actual occurrence of the disaster: in all the five Durham explosions there was no indication that any movements had taken place amongst the men suggestive of alarm; their bodies were found in the places where their work required them to be, close to their tools and lamps.

At Seaham, Tudhoe, West Stanley, and Usworth the explosions were simultaneous with the firing of shots in stone; in these cases the explosions occurred when the pits were occupied by stonemen and repairers and at the only time when the operations of the mines allowed the firing of shots. At Seaham, Tudhoe, and Usworth the shots were fired on a main intake air-road and at points where currents of air of between 20,000 and 30,000 cubic feet per minute were passing. At West Stanley the shot was fired, in stone, at a working place by a naked flame, and the air in the vicinity would probably contain a small quantity of fire-damp, but not sufficient in amount to show its presence in the safety-lamp or by itself to be explosive. In the other cases it is almost impossible to conceive that the air could contain any sensible quantity of gas. At Seaham it would be necessary to assume that the gas came down the shaft, or that there were three separate and simultaneous outbursts of it on the three main roads diverging from the shaft. At Tudhoe, where the air came direct from the surface by two shafts, it would be necessary to assume two separate and simultaneous outbursts. At Usworth the air had passed no working place, and could hardly have contained even a trace of fire-damp. At West Stanley no appreciable quantity of gas could be present in the main intakes, although a small quantity might be contained in the air near the place where the shot was fired.

There remains the Trimdon Grange explosion, which, was unconnected with shot-firing. There was distinct evidence that it originated with the ignition of gas at the light of a boy engaged at a pump in connection with some drowned workings from which gas was found to issue and that it extended with great force to parts of the pit more than a mile distant from its origin along the main intake air-ways.

Now all the circumstances connected with the Durham disasters make it almost certain that the main agent in the propagation of the explosion was dust, and in three out of the five cases it was dust alone. In four out of the five cases the immediate cause was shot-firing, *but in no instance was the shot blown out.* It is not at all necessary that the shot should be blown out to cause the ignition of the dust-cloud which the concussion raises in a dusty road. Properly fired shots show flame even when they dislodge the stone or coal; and the flame is often considerable if there has been an overcharge of powder, or if small coal or earth mixed with coal-dust has been used, as frequently happens, in the tamping. At Seaham, Tudhoe, West Stanley, and Usworth the flame of the shot ignited the dry inflammable dust dislodged from the roof or raised from the floor by the concussion of air which followed, and the explosion was propagated by fresh dust-clouds raised in the manner described by the Royal Commissioners. At Trimdon Grange an explosion of fire-damp operated in the same way: the violent movement of air resulting from the ignition of fire-damp and air raised a cloud of coal-dust into which the flame from the fire-damp passed, and the ignition of the coal-dust propagated itself as in the other cases, and, as in these, continued so long as it was fed by fresh fuel. This rapid ignition of dust containing upwards of 80 per cent. of carbon would result in the formation of large quantities of carbonic acid, and possibly even of the more poisonous carbonic oxide: when it is considered that it is impossible to live in air containing even 3½ per cent. of carbonic acid, the deadly character of the after-damp so formed is readily conceivable.

In striking contrast to the Durham explosions was that at Whitehaven. This was in a wet pit; the coal being worked was wet, and all the surroundings were damp, and free from dust. The cause of the explosion was gas, which was known to be in the pit, and frequently present in large quantities. Although it is probable that some 30,000 cubic feet of an inflammable mixture of air and fire-damp were ignited, the explosion was confined to a limited area of the workings, which extend to nearly three miles from the shafts. Seven men were within the district of the explosion, of whom three escaped. The survivors stated that all the men were alarmed by the appearance of gas immediately before the explosion, and hurried away. In the act of retreating the gas ignited at a lamp which was afterwards proved to have been defective and to allow of the passage of the flame. This the authors say was the most considerable explosion of fire-damp and air that they are acquainted with. They have personally investigated during the last twelve years almost all the explosions occurring in the North of England, and they cannot point to a case where there was direct evidence of so large a quantity of fire-damp and air exploding.

The moral of all this is obvious. It can scarcely be gainsaid that some of the most disastrous explosions of the last thirty years are primarily to be attributed to the practice of firing gunpowder in dusty mines. That under certain circumstances gunpowder can be used with safety is allowed. But the Royal Commissioners have issued a warning in no uncertain terms. They have convinced themselves that the abolition of the use of powder in dry and dusty mines will not generally involve any formidable inconvenience, inasmuch as the work which is accomplished by its employment both in coal and in stone can now be performed with equal efficiency, and at very little if any greater outlay, by other means. Unless, therefore, mining engineers, or those immediately responsible for the working of collieries, can devise some satisfactory method of minimising the danger due to dust, they will be compelled before very long, in deference to public opinion, to renounce the practice of blasting by means of gunpowder, or by any other agent which causes a flame.

T. E. THORPE

McLENNAN'S "STUDIES IN ANCIENT HISTORY"

Studies in Ancient History: comprising a Reprint of "Primitive Marriage." By the late John Ferguson McLennan. A New Edition. (London: Macmillan and Co, 1886.)

THE first edition of "Primitive Marriage" appeared in 1865, and the book was already extremely rare when, in 1876, it was reprinted as the first part of the "Studies in Ancient History." The reprint also soon became scarce, and while the influence of the author has been steadily growing, and almost all students of early society have come to attach great importance to his speculations, his principal writings have for some years been almost inaccessible. This new edition therefore supplies a real want, and it is doubly welcome for the sparing, but judicious, notes and appendixes which the editor, Mr. D. McLennan, has attached to his brother's book. "Primitive Marriage" broke ground in a new field of research, and, as the point of view was wholly novel, the collection, sifting, and marshalling of the evidence on which the argument was based was entirely pioneer's work. At the close of his life, McLennan was in possession of a much larger material; he had pursued his argument in new directions and to further conclusions, and on one or two points he had come to change his views. But new research had only confirmed the main lines of the argument sketched with so firm a hand in his original essay; and read with the *caveats* which his brother has introduced at one or two points—chiefly as regards the interpretation of the Levirate, and the prevalence of Agnation—the present reprint may be taken as generally representing, so far as it goes, the author's final conclusions on the subjects discussed. I say *so far as it goes*, for in many directions his conclusions had been added to and his views developed. The editor promises us a second volume, to consist for the most part of writings hitherto unpublished, which will throw a good deal of light on these new developments; meanwhile he has restricted himself in the notes "to certain matters on which the author had announced a change of view, and to certain others where circumstances had made an additional statement imperative." Of the additional statements, the most important is contained in two long notes appended to the essay on Morgan's "classificatory system" of relationships, in which it is clearly made out that Morgan's theory rests on misconception of the facts, and that the supposed classificatory system of relationship is not a system of relationship at all, but a system of terms of ceremonial or friendly address, used in conversation even between persons who are not related to one another in any way. This comes out so clearly in the cases about which we are best informed, that it is very questionable whether the facts so laboriously collected by Mr. Morgan can be used to throw light on the early history of the family.

From his plan of reprinting the book as it stood, with no more annotation than was absolutely necessary, the editor has departed only in one point. The appendix containing "additional examples of the form of capture" has been re-cast and enlarged upon the basis of a paper of J. F. McLennan published in the *Argosy* in 1866, but with additions from other and more accurate sources. The reasons for adopting this course are obvious: the new matter in this appendix could not conveniently have been reserved for the promised second volume, and the facts are so arranged and explained as to confirm the author's argument, and effectually dispose of the notion that the form of capture in marriage is to be explained by maidenly bashfulness.

It will be seen from this brief account that, sparing as the editor's additions are, they make the new edition of the "Studies" well worthy of the attention of those who already possess the book in its older form. And to the not small class of students of early society who know McLennan's work only at second hand or by one hasty perusal, it may not be unprofitable to say that this is emphatically a book of which a general knowledge is not sufficient, inasmuch as some of the most important and interesting points are precisely those which are almost sure to be missed on a first reading. For this, perhaps, McLennan himself is partly responsible, for in giving to "Primitive Marriage" the subordinate title "an inquiry into the origin of the form of capture in marriage ceremonies," he seems to fix attention on what is only the starting-point of a far-reaching research. In print and in conversation one often meets with the notion that the doctrines of marriage by capture and kinship through women only are mere archæological *curiosa*, and that for the study of later law and custom it is quite indifferent whether these things are true, or whether, on the contrary, mankind started from the first with male kinship. But the importance of McLennan's researches lies largely in the demonstration that the structure of society under a system of kinship in the male line which has been preceded by kinship through women cannot be the same as would be reached by a race which has had male kinship from the first. Other writers have taught a doctrine of the priority of kinship through women, but no one except McLennan has accurately developed the consequences of the doctrine, and shown how it solves a problem which, though ignored by most writers, is of the highest importance, namely, the origin of *gentes* within a nation. Like all really original thinkers, McLennan has for one of his chief merits that he recognised the existence of difficult problems in matters which ordinary people pass over without seeing any difficulty at all. And therefore precisely those passages in his writings which on a hasty reading seem needlessly laboured and proper to be skipped are found upon re-perusal to be particularly useful and stimulating.

A word may be said in conclusion on what is promised for the second volume. It is satisfactory to know (p. 75) that it will include a short essay on the origin of exogamy. And from a note at p. 176 it may be inferred that in this essay the origin of exogamy will be sought in a state of society where marriage by capture was an established custom. We are also promised (p. 63) an essay on the marriage law of the Australian Kamiraloi, one of those highly complex problems in which McLennan's powers of analysis ought to appear at their best. From notes on pp. 109 and 228 it appears that part at least of McLennan's hitherto uncollected essays in the *Fortnightly Review*, including the papers on Totemism, or "On the Worship of Plants and Animals" (1869-70), will also be republished. It is to be hoped that in these reprints the editor will allow himself, in one direction, greater freedom of annotation than in the present volume. The Totem papers are in some respects the least finished of McLennan's writings, the evidence of totemism in the nations of ancient civilisation being much too largely drawn from second-hand sources. This gives an appearance of weakness to the whole structure of the argument, which has been very prejudicial to the influence of a most original and striking investigation. In point of fact a few of the detailed pieces of evidence ought to be abandoned altogether, but enough remains to leave the substance of the argument unaffected, and this ought to be clearly brought out by notes, referring to original authorities of unquestioned reputation, or giving up statements that cannot be authenticated. Even in the present volume one misses some notes of this kind. The polyandria of the Athenians (p. 235) rests on better evidence than the story which Augustine cites from Varro (Clearchus *ap.* Athen. xiii. p. 556 d.). Again, the note at p. 47, in which an attempt is made to prove the existence of the form of capture among the Hebrews from the phrase "to take a wife," ought rather to have been withdrawn than again built upon by the editor at p. 181; and what is said of the marriages of the Persians at p. 219 *sq.* requires careful revision.

W. ROBERTSON SMITH

BRITISH HYMENOMYCETES

British Fungi, Hymenomycetes. By Rev. John Stevenson. With Illustrations. Vol. II. Cortinarius—Dacrymyces. Pp. 336. 8vo. (Edinburgh: William Blackwood and Sons, 1886.)

WE are glad to welcome this second volume so speedily after the first, although we fear that expedition has been secured by some sacrifice of efficiency. It is a misfortune when the reader is impressed at once with the feeling that a volume has been hurried out to meet certain exigencies. That feeling is by no means absent in scanning these pages. As soon as p. 165 is reached, and there is no longer Fries's "Monographia" to fall back upon, *descriptions* give place to *diagnoses*, notwithstanding the remarks in the preface, which would seem to regard diagnoses with something of contempt. From p. 166 to the end the *student* must be content with the diagnoses from Fries's "Hymenomycetes Europæi," although there might have been collected together valuable notes from Fries's "Systema," Observationes," and "Elenchus." Nevertheless some advantage has been taken of the few descriptions published in the letterpress to Fries's "Icones."

It is of considerable importance to students that a work which professes to include all British species, up to date, should satisfy all reasonable expectations. The first volume omitted some forty species, and the present is by no means perfect. We open at p. 232, and find under the genus *Solenia* one solitary British species recorded, that of *Solenia ochracea*. Surely our author could not have been ignorant of the fact that *Solenia anomala*, P., is still more common, and was recorded by Berkeley in the "English Flora" (p. 199) fully fifty years ago. Neither could he have forgotten that another species was included in Cooke's "Hand-book" (p. 329) under the name of *S. candida*, since corrected to *S. fasciculata*. As these specimens were collected near Batheaston, by no other than Mr. C. E. Broome, and confirmed by the Rev. M. J. Berkeley, no doubt can be entertained of their being authentic. Furthermore, the name was corrected and the species figured by Berkeley and Broome in the *Annals of Natural History*, December 1870, No. 1301. The fourth species is *Solenia stipitata*, Fuckel, of which there are specimens in the Kew Herbarium. It cannot be conceded that a "Flora" satisfies all reasonable expectations when in one genus only one of four species is recorded.

Turning to an allied genus, that of *Cyphella*, we seek in vain for *C. Curreyi* or *C. albo-violascens* (which may be identical), *C. cyclas*, Cke. and Phil., *C. punctiformis*, Fries, *C. villosa*, Pers., all but one of which are well-known and widely-distributed species.

Whether the species under the genera *Stereum* and *Corticium* might have been arranged in a manner more in accordance with modern ideas, and far more useful to the student in their identification, may be left an open question. Those who are not facile in the use of the microscope may find it convenient to follow Fries, who paid little attention to microscopical characters, but surely in a large and difficult genus, such as *Corticium*, no assistance should be despised.

We observe, with some surprise, the genus *Microcera*, of Desmazières, included in a work devoted to British Hymenomycetes (p. 308) with the intimation "no British species." The fact is that *Microcera coccophila*, Desm., which is the type of the genus, has been found in Britain, and is recorded on p. 556 of Cooke's "Hand-book," and furthermore it is also true that it is not a Hymenomycete at all, but the conidia of one of the *Sphæriacei*, and is included as such in Saccardo's "Sylloge Fungorum" (vol. ii. p. 513). This singular double error might have been avoided had some mycologist been consulted who had not confined his attention exclusively to the Hymenomycetes.

The limits of species is another open question, and it is scarcely advisable to make too much of the insertion of what some may regard as doubtful species in a "Flora" wherein the author is not free to give reasons in their favour; nevertheless, we venture to hint that *Polyporus armeniacus*, Berk. (p. 215), is generally admitted to be only a resupinate condition of *P. amorphus*, Fries, and should not be continued as a distinct species. *P. Herbergii*, Rost (p. 195), is placed as an ally of *P. sulphureus* in the section "Caseosi," whereas *P. cuticularis* is found (at p. 202) in "Spongiosi." Unfortunately for this arrangement, the two species (*P. Herbergii* and *P. cuticularis*) are so closely allied that sometimes it is difficult to distinguish the one from the other, except by the difference in size of the pores, and hence some regard them as varieties of one species. At any rate, there is no good reason why such closely-allied forms should be separated by four-and-twenty intermediate species.

The mention of localities for species throughout the work is so vague, that some explanation should have been offered. When only one locality is given, the inference which would be drawn by the majority of readers would be that no other British locality was known at the time for that particular species. That this conclusion would be wrong is manifest from *Hydnum Weinmanni* (p. 242), which may be taken as an example. The locality cited is "Bristol," but Bristol is not the only, or the most important station for this species in Britain, because it occurs plentifully in the neighbourhood of Carlisle. If the intention was simply to indicate the locality where the species was first found in these islands, then again we fancy it is inaccurate, because, as we believe, it was first discovered by the late Rev. A. Bloxam, at Gopsall. The only solution we could suggest is that "Bristol" is the locality mentioned in Berkeley's "Outlines," and it was accepted as the only authentic record, without inquiry. Some species are stated to be "common," others "frequent," and others "rare," and when, in the absence of any one of these terms, a single locality is given, it is a fair inference that only one locality was known to our author, and that was the reason why it was given. Assuming this to be the case, we fancy that a very large number of these single localities could be challenged as not unique.

In addition to a "Glossary" of five pages, we are glad to find a good index of genera and species, but we search in vain for any clue to the contractions, in some cases only a single letter, employed in quoting authorities. Under nearly every species follows a line or two, sometimes five or six lines, of hieroglyphics, to which figures are appended. It may be all clear enough to the Rev. John Stevenson what is intended to be conveyed by "Quel. t. 11, f. 1," or "Viv. t. 27," or "C. Illust., pl. 276," but who these illustrious persons are, or what they have done, to be curtailed in such wise, is nowhere indicated. Surely the author must have determined upon giving a key to these mysteries when he first commenced to employ them, and, in the hurry to issue the second volume, quite forgot the "students," even if he remembered the "scholars," and closed the book before he had finished his work.

A summary of the contents of these volumes, as they stand, exhibits the following results as compared with the last preceding work on the same subject:—

"Hand-book of British Fungi" ...	1044
Stevenson's "British Fungi"	1675

or, an addition of 631 species of Hymenomycetes since the year 1871. The majority of the additions have been made in the Agaricini, which stand thus:—

"Hand-book of British Fungi" ...	699
Stevenson's "British Fungi"	1183

or, an addition of 484 species, leaving only 147 species to be distributed over the residue of the genera of Hymenomycetes. These results are at any rate a justification, if any were needed, for the publication of a new work, especially when the older one is entirely out of print.

There can be no doubt that all that portion of the work which contains translations from the "Monographia" of Fries will be exceedingly valuable to British mycologists, and this extends through the whole of the first volume and 165 pages into the second; the only regret being that the few remaining species, which have not as yet been recorded in these Isles, were not inserted in brackets, or published as an appendix, so that the whole of Fries's excellent work might have been in the hands of every mycologist in this country. Perhaps even now such an appendix might be published, and no doubt it would meet with a hearty welcome.

Despite of such strictures as we have been impelled to make, we venture to hope that the present edition will soon be exhausted, and that its author will be called upon to prepare a new and revised edition, with a key to all the mysteries of the old one. M. C. C.

THE OCEAN

Der Ozean. Von Otto Krümmel. (Leipzig und Prag: Freytag-Lempsky, 1886.)

THE great interest which oceanographical studies have aroused within the last few years is shown in a marked manner by the publications destined to popularise the notions acquired respecting this vast and important chapter of physical geography. Not long after the appearance of the "Lehrbuch der Ozeanographie" by Boguslawski, whose untimely death has interrupted the publication of the second volume, we have a new and small manual by Dr. Otto Krümmel, whose name is already known to oceanographers.

This little treatise is clearly written, and the most important general notions concerning the physical geography of the sea are well stated, and discussed with ability. The author has succeeded in expressing briefly the essential notions about the ocean, which have been recently acquired by the *Challenger* and other deep-sea expeditions.

The author describes, in the first place, the ocean's surface and its subdivisions ("Die Meeresflächen und ihre Gliederung"); discusses the relation of oceanic and terrestrial areas from the point of view of their respective size; indicates the distribution according to hemispheres; and points out the classification he has adopted into *oceans*, properly so called, with their general systems of ocean currents, and *secondary seas*, which are more or less cut off from the great oceans. The *secondary seas* are again subdivided into *interior*, or *inter-continental*, and *border seas*, situated on the outer edges of the continents. The volume of oceanic water is then estimated. In the second chapter the interesting questions connected with the deformation of the level and surface of the ocean, owing to the attraction of the continental masses, are examined The depths and contours of the ocean basins are next pointed out, and the work of the *Challenger* and other deep-sea expeditions, together with the apparatus employed, is described. The observations of the *Challenger* upon the nature and distribution of deep-sea deposits are summarised. The physical and chemical properties of sea-water are set forth in a special chapter—the salinity of the ocean, its distribution and origin; the gas contents; the transparency and colour, are, in turn, treated of. After having made known the principal phenomena regarding the temperature of the ocean and its distribution, Krümmel treats of the glacial phenomena of the Arctic and Antarctic Oceans, pointing out the limits of the floating ice and icebergs in each region, and the influence of these regions on the questions of general oceanic circulation. The last chapter is reserved for a consideration of the movements of water, such as currents, waves, and tides.

Such is the general order and method of this manual. There is no attempt to give any general notion of the life of the shores, deep sea, and surface of the ocean, or of any of the phenomena due to organisms. The author shows himself to be everywhere *au courant* with the most recent discoveries in his subject. It would appear, however, that he has not had an opportunity of consulting the "Narrative of the Cruise of the *Challenger*," published last year, or he would have embraced in his descriptions some additional interesting details and general views. The work is illustrated by many woodcuts and small charts, some of which are instructive, others conveying little information to the reader, but when the low price of the book (one shilling) is remembered it would be unfair to criticise closely these illustrations. Dr. Krümmel has attained the object he had in view—to popularise in a scientific manner our knowledge relative to the physical geography of the sea, a subject full of interesting questions for all cultured minds. J. M.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to insure the appearance even of communications containing interesting and novel facts.*]

On the Connection between Chemical Constitution and Physiological Action

As regards Dr. Brunton's letter in last week's NATURE (p. 617), I would express myself as more than satisfied with the personal explanation, but Dr. Brunton has not noticed the most important point to which I wished to call attention, viz. that whatever may be the value of my experiments, as showing a connection between physiological action and chemical constitution, the researches of Crum Brown and Fraser have really no bearing on the subject, for the simple reason that they had no knowledge of the chemical constitution of the re-agents they employed. There is an old receipt for cooking a hare which commences "*First catch your hare,*" and in attempting to show the influence of change in chemical constitution on physiological action, it is well first to get a constitution. In the last edition of Watts's "Organic Chemistry" (1886) it is stated, "All these bases (the alkaloids), like the amines, are derivatives of ammonia, but their molecular structure is for the most part unknown." Even as regards inorganic compounds, our knowledge of their chemical constitution is not the most definite, but I believe that the arrangement of the elements in isomorphous groups expresses most clearly the resemblance in the chemical constitution of their compounds.

After again reading carefully Dr. Brunton's paper, I must confess that I cannot find anything showing the connection between chemical constitution and physiological action, except, perhaps, in the case of the alcohols. Here we have a class of bodies in which the different members of the series have probably the same relation to each other as the elements in the same isomorphous group, and it is an interesting fact that not only do they resemble each other in their physiological action, but that their toxic action increases with their molecular weight, as I have shown to be the case with the inorganic elements, where, in each isomorphous group, the toxic action increases with the atomic weight of the elements.

In conclusion, I would reprint an extract from a paper published forty years ago:—"A moment's reflection on the problems to be solved will suffice to show that experiments conducted with this class (inorganic) of substances are more likely to furnish useful results than those made with bodies derived from the animal or vegetable kingdom, although, owing to the striking effects caused by some of these substances, physiologists have mostly directed their attention to them. By so doing, however, we are employing re-agents with the properties and composition of which we are imperfectly acquainted, to the neglect of those on the nature of which chemistry has already thrown much light, for not only are we better acquainted with the more purely chemical properties of inorganic compounds, but their relation to heat, electricity, and molecular polarity has been to a considerable extent made out." JAMES BLAKE

Disinfection by Heat

IN Dr. Parsons's Report on Disinfection by Heat (NATURE, vol. xxxiv. p. 583) occurs the statement: "It appears that there are no tables or formulæ in existence by which the degree of humidity of the air corresponding to a given difference between the wet and dry bulb thermometers at these high temperatures can be ascertained." There are both tables and formulæ; but the tables are the numerical values for the formulæ, and such tables are to be found in Balfour Stewart on "Heat," Dixon's "Treatise on Heat," Blanford's "Meteorologist's Vade-mecum," and numerous works on the steam-engine.

Let the degree of humidity be represented by h; vapour-tension at dew-point by x; wet-bulb temperature by t, its vapour-tension by f; dry-bulb temperature by T, its vapour-tension by F; barometric pressure by b. Then, the theory of the dew-point gives

$$h = \frac{x}{F};$$

and

$$x = f - \frac{0{\cdot}382\,(T-t)b}{1115 - 0{\cdot}7t};$$

hence

$$h = \frac{f}{F} - \frac{0{\cdot}382\,(T-t)b}{(1115 - 0{\cdot}7t)F}.$$

The tables give the vapour-tension, consequently if T, t, and b are known, h can be found. At these high temperatures the degree of humidity would probably not be required very accurately. If within 1 or 2 per cent. of accuracy would suffice, the second term may be omitted. This results from the fact that the higher the temperature of the air, the nearer is the dew-point to the wet-bulb temperature.

The quantity sought then is $h = \frac{f}{F}$. Given $T = 299$, 299, and 249; $t = 146$, 165, 190; and taking F and f in pounds from a table in "Lardner on the Steam-Engine":—

(1) $$h = \frac{3{\cdot}5}{63} = {\cdot}05;$$

(2) $$h = \frac{5{\cdot}5}{63} = {\cdot}08;$$

(3) $$h = \frac{9{\cdot}5}{29} = {\cdot}32.$$

Here saturation is represented by unity. This is at once a short and simple method of calculating the degree of humidity at these very high temperatures. If the barometrical pressure were observed, and the long second term worked out, the results would not be materially different, but would be something less.

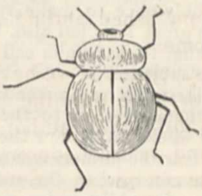
R. STRACHAN

11, Offord Road, London, N., October 25

The Beetle in Motion

MUCH has been written on "the horse in motion." Can any readers of NATURE supply me with references to published matter on the subject of hexapod progression?

The few observations I have made may be summed up in a few words. I use the letters *r* and *l* to signify the right and left legs respectively, and number the limbs from before backwards. When walking rapidly the appearance is as if *l* 1, *r* 2, and *l* 3 moved forward together simultaneously, alternating with *r* 1, *l* 2, and *r* 3. When the pace is slower it is seen that *l* 1 and *r* 2 start together and come down at about the same time, some-



times one sometimes the other being a little the first. Then, lifted almost but apparently not quite at the same time, *l* 3 starts. The motion of this leg being somewhat slower, and the limb having further to travel, the foot generally comes to the ground appreciably later than *l* 1 or *r* 2. The general effect is to produce, at the moments of pause between the strides, the position indicated in the figure, which differs considerably from the conventional position delineated by artists who seek to represent the beetle in motion. C. LLOYD MORGAN

University College, Bristol

The Astronomical Theory of the Great Ice Age

IN Sir Robert Ball's paper on this subject, which appears in your last number (p. 607), that author states that the calculation given "has convinced him that Mr. Croll's theory affords an adequate explanation of the Ice age." It is more in the hope of obtaining from Sir Robert a statement of the grounds of this conviction than for the purpose of controversy that I write this letter.

It will of course be conceded that the frost and snow of a single winter, melted off during the following summer, would not produce an Ice age. But, on Sir Robert Ball's figures, the increase of winter cold at the period in question was accompanied by a corresponding and equal increase of summer heat. Why, then, should the latter prove insufficient to melt the winter accumulation of snow and ice in any locality where it now suffices to melt it?

The question is one of the joint result of two opposing forces. Both, under the supposed conditions, are intensified and equally intensified. How does this affect the result? More snow and ice is doubtless formed in the winter, but then more heat is employed in melting it during the ensuing summer. Why, then, was it not melted in any place where it is now melted? A kind of answer to this question may be extracted from the writings of Mr. Croll, but not, I think, a satisfactory one. I am therefore anxious (in common, I am sure, with many others of your readers) to hear the reply of Sir Robert Ball.

Llandudno, October 25 W. H. S. MONCK

The Enormous Loss from Ox-Warble

I VENTURE to solicit your co-operation in making some points better known in order that farmers may be better able to protect themselves from the enormous loss from warbles on cattle from the bot-fly, positive proof having been furnished that it largely exceeds 2,000,000*l*. to 3,000,000*l*. yearly! To begin: I appeal to those farmers who have somewhat studied the question to make it clear to those who have not done so that *each warble lump has a large maggot under it*, feeding on the juices of the hide or flesh. These lumps many call "health lumps" or "thriving bumps," and *seem to prefer that their cattle should have them*. It is readily seen how this serious fallacy has arisen, viz. from the fact that the warble lumps begin to show about Christmas (from the growth of the maggot under them), which also happens to be the time that the cattle receive their most nourishing food, and are then warmly housed or sheltered. But there could be no greater mistake than to think that the swellings from the ravages of these horrid maggots are proof of a thriving condition! A correspondent writes me: "Since reading recent issues on the ox-bot or warble-fly, I have visited several cattle markets and slaughter-houses to see for myself if the ravages of the maggots are so serious as the statements led one to believe. I must frankly state that what I have seen convinces me that the statements are much under the mark rather than over it. The first beast I handled showed 42 warbles, some only 3 to 6, whilst many others showed 30 to 70; and on examining hides at slaughter-houses this state of things was again confirmed (the warbles are more readily seen upon the *under*-side of the skin, and many are small ones that would not show as a lump. I am certain a farmer has only once to make such a visit to be not only convinced of the great loss, but also, if he has any neighbourly feeling about him, to make him call the attention of his brother-farmers to the subject."

I am anxious to indorse this recommendation, for the farmers should now satisfy themselves as to the actual state of the matter, as in a few weeks from now the warble lumps will have vanished, and I fear the farmers will hardly take protective measures during the summer, when the warbles are not visible, unless they are convinced; whilst seeing would be believing. I may remark that the following simple remedies are all efficacious to destroy the maggots: mercurial ointment and carbolised oil, to be applied with caution by a careful man; or, better still, quoting from the Report of the Royal Agricultural Society, "As a general application, safe in all hands, McDougall's preparation has proved excellently useful," and I have convinced myself it is the best and safest remedy that can be applied, not only for destroying the maggots, but, later on, as a wash to prevent the attacks of the flies. I would not have occupied so much of your space, but I am convinced this is a subject of national importance. JOHN WALKER

Southport

P.S.—Farmers wishing for further information should read "Observations on Ox-Warble or Bot-Fly," 1884, and a second Report on "Ox-Warble or Bot-Fly," 1885, by Eleanor A. Ormerod, F.R.Met.Soc., &c. (London: Simpkin, Marshall, and Co.), and a new pamphlet called "The Bot-Fly," just issued by J. C. Jack, Grange Publishing Works, Edinburgh. This work fully defines every minute detail of the history, life, prevention, and losses sustained by the dreaded pest.

Aurora

THE remarkable aurora borealis observed by Prof. Piazzi Smyth at Edinburgh on July 27 (NATURE, vol. xxxiv. p. 312) seems to have been visible over a very great area. In my meteorological journal it is remarked on July 27 that the bright silver-clouds appeared beautiful between 9.30 and 11 p.m. "The colour of the northern sky above the silver-clouds was misty and brownish, though not cloudy." I had never seen such a tint in the sky. I have no hesitation in saying that the unusual darkness was the same as observed at Edinburgh. The fair white arc I did not see; clouds came up at midnight. It may be interesting to state that I also saw, on July 26 at 9.30 p.m., an aurora-like white cloud in the north-west. This cloud was very different from the well-known silver-clouds so often described in 1885 and 1886. On the 28th and 29th nothing extraordinary is mentioned in my journal, but on the 30th faint traces of the silver-clouds and again "a very strange yellow-brownish colour of the north and north-west sky" are remarked. The great aurora on March 30 we also observed very well at Königsberg. F. HAHN,

Professor of Geography at the Königsberg University

Königsberg, Prussia, October 25

Earthquakes

IT is always interesting to look for coincidences in the earthquakes in different parts of the world. In NATURE, vol. xxxiv. p. 627, you announce that a violent earthquake was felt at Charleston and many other places in the United States of North America, on the 22nd inst. at 3 o'clock in the afternoon, *i.e.* 20h. 20m. Greenwich time. On the same day a very slight shock is recorded as having occurred at Neuchâtel, Switzerland, at 9h. 20m. evening, Berne time, *i.e.* 20h. 50m. Greenwich time. It is not impossible, but I must confess scarcely probable, that the faint shock at Neuchâtel was the re-percussion of the severe earthquake of North America. F. A. FOREL

Morges, Switzerland, October 31

IN connection with Prof. O'Reilly's letters in NATURE of October 14 and 28 (pp. 570, 618), and your notice of October 21 (p. 599), I supply a few data, which at first I thought of too little interest for your columns. At 6.12 p.m. local time (17h. 41m. universal time), on October 16, two shocks occurred with a short interval, the direction being approximately that of the meridian. The intensity was such as might be produced by very heavy carts passing. H. DU BOIS

Strasburg, October 31

Meteor

THIS evening, at about 8.25, I saw a magnificent meteor, of a blue colour, falling a little to the left of the Pleiades.

Belfast, October 31 JOSEPH JOHN MURPHY

FREDERICK GUTHRIE

FREDERICK GUTHRIE was born in Bayswater on October 15, 1833, and was the youngest of six children. His father, Alexander Guthrie, was a tailor, carrying on business in New Bond Street, and is said to have been a man of literary taste and ability; that he was a man of cultivation is shown by the education he provided for his children, one of whom, Francis, early distinguished himself at University College, London, and at the London University, as a mathematician, and is now Principal of the South African College, Cape Town. As a boy, Frederick Guthrie was taught privately until his twelfth year by the late Henry Watts, F.R.S.; afterwards he was sent to University College School, then under the head-mastership of Prof. Key, whence he passed into University College, London. There he remained three years, the last two of which were devoted mainly to the study of chemistry, under Profs. Graham and Williamson, and of mathematics under De Morgan, a teacher with whom it was impossible for a young man of Guthrie's power to come into contact without receiving a life-long impress. There also he again came into contact with Watts, who was then principal assistant in Prof. Williamson's laboratory, and an intimate friendship was cemented with his old tutor that remained unbroken till the death of the latter. In the spring of 1854 Guthrie went to Germany to continue his chemical studies, and worked first at Heidelberg, under Bunsen, and then at Marburg, under Kolbe, where he took the degree of Doctor of Philosophy ("*summa cum laude*") in 1855, having previously graduated as Bachelor of Arts of the University of London. After returning to England he was appointed, in 1856, assistant to Dr. Frankland, then Professor of Chemistry in Owens College, Manchester. In 1859 he went to Edinburgh as assistant to the late Vice-President of the Council, who had just succeeded Dr. William Gregory as Professor of Chemistry in the Edinburgh University.

Two years later Guthrie accepted the Professorship of Chemistry and Physics in the Royal College, Mauritius. He arrived in the island in May 1861, and for six years he devoted himself to endeavouring to introduce and establish on a durable basis scientific instruction in the colony. Here one of his colleagues was Mr. Walter Besant, the eminent novelist, with whom he formed a friendship that remained intimate and uninterrupted through life. He returned to London on leave in 1867, and in 1869 he was elected Lecturer on Physics in the Royal School of Mines, a post which, with extended duties and modified title, he retained till his death.

In the spring and early summer of this year many of Guthrie's friends remarked upon his looking ill and seeming to be in low spirits. After a while he complained of a difficulty in swallowing, which presently became so serious that he was unable to take solid food. When at last he was prevailed upon to consult a physician, it was discovered that he was suffering from cancer of the throat. He sank rapidly during the last two or three months, and the inevitable end of his disease came on October 21. He was buried in Kensal Green Cemetery on the 26th.

Such were some of the chief outward and visible stages in Frederick Guthrie's career. Perhaps the first thing to strike any one on making his acquaintance was his strongly-marked individuality. His opinions were, much more than most men's, of his own forming, not simply picked up as they floated about in talk or in print. And his conduct followed his opinions: he did what he thought right, with very little regard to the consequences to himself, or to what might be thought of him by others. His scientific knowledge, too, was, much more than most men's, of his own getting, the result of his own observation and experiment. In others, also, he valued even a small scrap of self-gotten knowledge more than a large store of secondhand erudition. In this respect he sometimes went to excess, and, though not without mathematical knowledge, he was somewhat apt to underrate the scientific importance of the work of mathematical physicists in comparison with that of pure experimentalists. But even this mistake had root in the thoroughly sound conviction that it is the duty of a man of science to be a strictly faithful interpreter of the observed facts of Nature, and that, the further he ventures in the field of theoretical deduction the more room is there for self-deception. He seemed, however, sometimes to forget that phenomena do not present themselves to the natural philosopher ready clothed in words, and that all that can be expressed in human language is the conception formed in the mind of the observer. The true function of the mathematical physicist is in reality, as Kirchhoff has pointed out, nothing more than to find out the simplest statements that are consistent with observation.

Guthrie's devotion to science was complete and single-minded. He had a deep conviction of the value and dignity of any kind of genuine, self-forgetful, scientific work, and he knew how, if necessity arose, to claim the dignity due to a sharer in such work. But from affectation or vanity he seemed entirely free. His wonderful gift of humour and power of terse and telling speech made it easier for him, than for most men, to put down any approach to impertinence or presumption; but, except where he felt that a lesson was needed, he was most considerate of others, both in speech and action. He delighted in playful mystifications (see, for example, Prof. von Nudeln's letter in NATURE, vol. xxi. p. 185, on the "Potential Dimensions of Differentiated Energy"), but his drollery was never ill-natured. He was generous and kind-hearted in the extreme; as a friend he was steady and faithful. Although essentially a man of science, he had considerable literary attainments, and had an excellent knowledge of both German and French, while his powers of literary expression were remarkable. It will not astonish those who knew his ability in this direction to learn that as a young man he published (under the *nom-de-plume* of Frederick Cerny) a poem called "The Jew," and a metrical drama called "Logroño."

With regard to Guthrie's scientific position and achievements it may be remarked, in the first place, that he belonged to a class that was probably commoner in his generation, and in that which preceded it, than it is likely to be in the future—that, namely, of physicists who served their time as chemists. Until within the last twenty years or so the only accessible school of experimental science was a chemical laboratory, and consequently, for the last two generations, a large proportion of the most prominent physicists have been men who began their scientific career as chemists. Among many others, it may suffice to mention Faraday and Regnault. Guthrie's first published investigation seems to have been his dissertation on taking his Ph.D. degree; it was entitled "Ueber die chemische Constitution der ätherschwefelsauren Salze und über Amyloxydphosphorsaure." In the six years between taking his degree and going to Mauritius, he published eight or ten papers, mostly on points of organic chemistry—one of them, on the amyl group, contains the discovery of the therapeutic action of nitrite of amyl, and suggestions for its introduction into the pharmacopœia. His first physical investigations were published while he was in Mauritius, and included two researches into the formation of drops and one into the properties of bubbles. It is striking evidence of the reality of Guthrie's love of science and of his force of character that, under circumstances in almost all respects adverse to scientific work beyond what was required by his official position, he should have persevered steadily with his experiments and produced papers of great value. While in Mauritius he also published a paper on the iodide of iodammonium, and a pamphlet on "The Sugar-Cane and Cane-Sugar," and made complete analyses of the waters of the principal rivers of the island. After his return to England his scientific work was almost wholly confined to physics, but it is perhaps significant of the side from which he approached the study that the subjects that occupied him principally had relation to what is usually called in the text-books "molecular physics." Among many other researches the following may be specially mentioned: on the thermal conductivity of liquids; on approach caused by vibration; on stationary vibrations of liquids in rectangular and circular troughs; on salt-solutions and attached water (the results of this investigation were contained in a series of eight papers, and included the discovery of the substances named by Guthrie "cryohydrates," a class of solid hydrated salts which melt without change of composition, in most cases below 0° C.); on "Eutexia," an investigation into the properties, especially the melting-points, of metallic alloys and mixtures of salts.

As a teacher, it has been well said of Guthrie by one who knew him well, that "he did not desire merely to fill his pupils' heads, but to make them use them"—a far more valuable but more difficult result to attain. A large proportion of his pupils consisted of "certificated science teachers," and for these he introduced a system of instruction, consisting largely in making them construct with their own hands the apparatus required for their experiments, which was probably more fruitful (especially in the case of this particular class of pupils) than any other that he could have adopted.

In 1873 Guthrie issued to his scientific friends a characteristically worded little circular, which resulted in the formation, early in the following year, of the Physical Society of London, a Society which now includes, with very few exceptions, all the leading physicists of the United Kingdom. Through his intervention, permission was obtained from the Lords of the Committee of Council on Education for the meetings of the Society to be held in the Physical Laboratory of the Science Schools at South Kensington. He chose for himself the somewhat onerous post of "Demonstrator" to the Society, and in this capacity placed his time and the resources of his laboratory freely at the disposal of those who wished to exhibit experiments or apparatus at the Society's meetings. It was not till 1884 that he consented to become President.

In the early part of the present year he gave a course of three lectures before the Society of Arts on "Science Teaching," in which he advocated with equal vigour and humour the advantages of a training in experimental science.

Besides the poetical works already mentioned, and his numerous papers on scientific subjects, Guthrie was the author of the following books:—"Elements of Heat and Non-Metallic Chemistry," "Electricity and Magnetism," "Molecular Physics and Sound," and "The First Book of Knowledge."

He was elected a Fellow of the Royal Society of Edinburgh in 1859, and a Fellow of the Royal Society of London in 1873. G. C. F.

THE LONGEVITY OF GREAT MEN[1]

THE conclusion that the intellectual giants of the race are favoured by an abundance of years on the scene of their heroic activity, and are thus further differentiated from their more common fellow-men, seems natural, and has been accepted upon evidence which, in a less pleasing conclusion, would be considered ridiculously insufficient, and even false. The usual method of attempting to answer the question whether great men are longer-lived than others, is to prepare a list of the ages, at death, of a number of eminent men, take the average age, and compare it with a similar average of a number of ordinary men, or even with the average lifetime of the race, and in this way to make the results speak decidedly in favour of the superior longevity of great men. All that such a method can prove (and this it does prove) is that it takes long to become great. It neglects to consider that a select class of men is dealt with, and that, to be even potentially included in this class, one must have lived a certain number of years.

For example: in an article translated in the *Popular Science Monthly* for May 1884, it is argued that astronomers are a long-lived race because the average life-period of 1741 astronomers is 64 years and 3 months. An average human life is only 33 years; but as one cannot be an astronomer before adult life, the author takes the expectation of life at 18 years, which is 61 years, and thus makes an excess of over 3 years in favour of astronomers. He also divides his astronomers into four degrees of eminence, and finds that those of the first rank live longer than those of the second, and they in turn longer than those of the third, and so on, thus implying that the best astronomers are most favoured with years. The true conclusion is, that it takes longer to become a first-rank astronomer than it does to become a less eminent one.[2]

If great men were great from their infancy, and we had the means of ascertaining this fact, the method would be correct. But, as it is, we must define in some way or other what we mean by greatness, and then fix the average age at which it becomes possible to distinguish an amount of talent sufficient to enable its possessor to be enrolled in the ranks of the great as already defined. What is known as the "expectation of life" at any number of years tells the most probable age at death of one who has attained the years under consideration: a comparison of this age with the age at death of great men will decide whether they are longer-lived or not.

The attempt was made to select about 280 to 300 of the greatest men that ever lived.[3] Throwing out about 30 of the doubtful names, there remain 250 men, about whom the statement is hazarded that a list of the 250 greatest men, prepared by another set of persons, will not materially differ from our list, as far as all the purposes for which it is to be used are concerned. From this list I have selected at random a set of men of whom it was probably easy to fix the age at which they had done work which would entitle them to a place on this list, or work which almost inevitably led to such distinction: it is a date about midway between the first important work and the greatest work. The average of over 60 such ages is 37 years; which means, that, on the average, a man must be 37 years old in order to be a candidate for a place on this list. The real question, then, is, How does the longevity of this select class of 37-year-old men compare with that of more ordinary individuals? The answer is given by the expectation of life at 37 years, which is 29 years, making the average age at death 66 years. And this is precisely the age at death of these 60 great men; showing, that, as a class (for these 60 may be considered a fair sample), great men are not distinguished by their longevity from other men.

Further interesting conclusions can be drawn if we divide the men into classes, according to real psychological and physiological differences in the ways of manifestation of the several kinds of genius. It is almost surprising how well the ordinary trinity of faculties—intellect, emotions, and will—accomplishes this purpose. Greatness seems to appear either in a brilliant thought, a deep feeling, or a powerful will. Under men of thought would be included philosophers, scientists, historians, &c.; under men of feeling, poets, musicians, religionists, &c.; under men of action, rulers, commanders, statesmen, &c. Before comparing the relative longevity of these three classes of men, I assure myself that the period at which greatness begins to be possible does not materially differ[1] in the three classes, and, as was done in the former case, I exclude all cases of unnatural death. I find that men of thought live 69·5 years, or 3·5 years longer than ordinary men; while the lives of men of feeling are 3 years, those of men of action 5 years, shorter than those of average men,—a conclusion that agrees with the commonly accepted view on the subject. If we subdivide these three classes, we find, that, while all classes of men of thought live longer than ordinary men, the moralists live longest, scientists coming next; that among the men of feeling the religionists alone live the full period of life, while poets' lives are 5 years, and musicians' lives 8 years, too short; that, of men of action, rulers and commanders both fail to complete the full term of life by 4 years. One sees from these statements (which, however, in their detail at least, must be accepted with hesitation, owing to the fewness of examples) that the kind of psychical and physical activity pursued influences the life-period; that certain types of genius are apt to die young, while others are particularly favoured with a full allowance of years.

The question of longevity becomes important when we consider that through it the leaders of civilisation are allowed to exercise their important function a few years longer, thus enabling more great men to be alive at the same time; and that, by its tendency to be inherited by the offspring, the children of great men will begin life with a better chance of reaching maturity, and, in turn, of becoming important to the world, if, as we have reason to believe it would, the genius of their ancestors has left its traces in them. JOSEPH JASTROW

[1] From *Science*.

[2] Mr. Galton ("Hereditary Genius," p. 34) has allowed himself to neglect a similar consideration. In giving the number of men in each class that the population of the United Kingdom would have between certain ages, he gives 35 as the number of men of class *G* (a very high degree of eminence) between the ages 20 and 30, and only 21 such men between 40 and 50 years. But this cannot be true, because only a very small proportion of men could possibly attain the eminence requisite to be classed among the *G*'s in 20 to 30 years, while almost all (of those who will attain it at all) will have attained it before the end of their fiftieth year. And this consideration far outbalances the excess in absolute number of men between the former ages over those between the latter. Similarly the falling-off in the number of men of class *g*, *i.e.* idiots, from decade to decade, would be more rapid than in ordinary men,—a fact which the tables fail to show.

[3] The names were selected by three others and myself, while engaged in a study of what might be called the natural history of great men. The process of selection was most rigid and careful, by a system which it would take too long to describe.

[1] Mr. Sully (*Nineteenth Century*, June 1886) has shown that men of feeling are more precocious than men of thought; but the difference in the age at which their first great work is done, though in favour of men of feeling, is very slight indeed.

THE GEOLOGY OF THE LEBANON

WE are indebted to Dr. Carl Diener, of the University of Vienna, for an able monograph on the geological and physical formation of the Lebanon and surrounding districts, accompanied by maps, sections, and illustrations reproduced from photographs.[1] Notwithstanding the observations of Russegger, Fraas, and others, on the physical features and structure of this region, a complete monograph on its geology has long been a desideratum, and the work of Dr. C. Diener forms a fitting continuation of the survey of Lartet in Palestine, and of the Palestine Exploration Society in Arabia Petræa and the Jordan Valley.

Down to a comparatively recent period, the ranges of the Lebanon and Anti-Lebanon were supposed to be formed of Jurassic limestones, but the observations of Oscar Fraas showed that this was an error, and that they are mainly formed of Cretaceous and Eocene limestones. It is only within the limits of a narrow belt at the western base of Mount Hermon that Jurassic beds really occur; this being their first appearance on proceeding northwards from Arabia Petræa. The formations overlying the Jurassic strata are referable to the "Neocomian" (?), Cenomanian, Turonian, Senonian, Eocene, and newer Tertiary periods; while great sheets of basaltic lava of late Tertiary age occur both to the north and to the south of the region embraced by the memoir.

Dr. Diener has worked out with great success the numerous lines of faulting and flexuring which the strata have undergone since their deposition, and which have been produced mainly during the Miocene epoch. Mount Hermon itself owes its position in a great degree to the elevation of its mass along the line of a great fault which coincides with its western base. Its beds of limestone, belonging to the age of the Lower Chalk of Europe, are disposed in the form of a low arch, the axis of which passes under the summit, and ranges in a north-north-east direction along the line of the heights of Anti-Lebanon. Other faults range along the southern and eastern flanks of the great dome-shaped mount which has thus been bodily upheaved in respect of the bordering strata. There can be no question that the system of terrestrial disturbances along which the Syrian mountains have been fractured and dislocated is the same as that which has given origin to the Jordan-Arabah depression; and amongst the lines of displacement traced out by Dr. Diener, we can have no difficulty in recognising that which is the actual prolongation of the leading fault of the Jordan Valley. This great line of fracture and displacement appears to enter the valley of the Leontes (Litany) at the western base of Hermon, where a complete change of the stratification takes place on either side, and the "Lebanon Limestone," with the subordinate Lower Cretaceous beds, are thrown into a nearly vertical position, and brought into contact with horizontal strata of the Upper Chalk (Senonkreide). It may therefore be inferred that the great valley of Cœle-Syria (El Bekâ'a), separating the range of the Lebanon from that of Anti-Lebanon, owes its origin, in the first instance, to the same system of faults which has caused the depression of the Jordan Valley, the original features having been modified by extensive denudation; and if we suppose that the primary line of fault reaches as far north as the Lake of Homs, in the valley of the Orontes, and as far south as the Gulf of Akabah, the distance through which this great line of fracture of the earth's crust will have been traced will amount to about 350 English miles.

Dr. Diener expresses some doubts regarding the former existence of glaciers in the Lebanon, notwithstanding the opinions of such observers as Hooker, Fraas, Girard, and others. Hooker especially identifies the mound upon which the grove of ancient cedars is planted as an ancient moraine. The author throws some doubt upon this view, because he was unable, after three hours of search, to find scratched or striated boulders, although he admits that, viewed in certain directions, the mounds do present the appearance of a terminal moraine. In reference to this subject, it may be observed that the position and altitude of the Lebanon Range makes it extremely probable that perennial snow, giving origin to glaciers, occupied the higher regions during the Glacial epoch. Amongst the Caucasus, which are only a few degrees further north, though somewhat higher, glaciers occur at the present day, and during the Glacial epoch the valleys were brimful of ice. Hence it would be strange if in the Lebanon it were proved that they had been entirely absent. The scarcity or absence of glacial striations, on which Dr. Diener founds his objection, is easily accounted for when we recollect that the blocks and stones consist of rather friable limestone which has been exposed through thousands of years to the effects of frost, heat, and rain. It is only when the surface of a rock, or of a boulder, has been protected by a coat of stiff glacial clay, that we can expect the striæ and scars to be preserved throughout a long period of time.

On another point Dr. Diener expresses his dissent from the views of previous observers, arising, as it seems to the writer, from his want of appreciation of the full effect of eroding agencies. The neck of land which connects the Râs Beyrût with the outer ridges of the Lebanon is formed of beds of stratified gravel or conglomerate rising from 120 to 150 feet above the sea. This is to all appearance an old sea-bed formed at a time when the land was submerged to the extent above indicated, during which Râs Beyrût was an island. The author cannot accept this view, because his observations of the coast-line of Syria, bearing on the present state of the harbours, do not appear to show a change of level of more than a few feet; less, in fact, than would be necessary to submerge the neck of land. On the other hand, he accepts the evidence offered by Lartet and the writer of a submergence of the coast of Southern Palestine and Philistia to an extent even greater than this, namely 200 feet and upwards; and he points to the evidence of great changes of level on the coast of Northern Syria and Asia Minor. May not the absence of raised beaches on the coast of Southern and Middle Syria be due to the waste caused by the wave action of the Mediterranean, which would tend to carry away such soft materials during the period of emergence where exposed and unprotected? In another case the author throws doubt on the observations of Dr. Post regarding the presence of shell-beds at levels of 150 to 250 feet near Lâdikîeh, an account of which appeared in NATURE, vol. xxx. p. 385, and which is given with much detail. It seems an instance of hypercriticism to call in question an authenticated statement merely on the ground that the author was unable to personally verify it.

The above instances will, however, go to show with what care and labour Dr. Diener has accomplished his task, and he is to be congratulated upon the production of a work which will doubtless be considered a standard of reference regarding the physical history of the Syrian mountains. I may perhaps be allowed to remark that his admirable geological map would have been improved by following the English custom of showing the dip of the strata by means of small arrows, and of distinguishing between ordinary boundaries of formations and those which are produced by faults and fractures, and the book itself would have been rendered easier for reference by an index.

EDWARD HULL

[1] "Libanon; Grundlinien der physischen Geographie und Geologie von Mittel-Syrien." (Wien, 1886.)

AUTUMNAL FLOWERING

THE "extraordinary gooseberry" season seems to have set in this year with more than usual severity. Country clergymen and amateur gardeners, who would see nothing unusual in the autumnal flowering of a hybrid perpetual rose (which reminds them, perhaps, of their old school-days, when they read of "*biferique* *rosaria Pæsti*"), are moved with astonishment at the sight of a second crop of flowers on an apple-tree or a laburnum. Common as the phenomenon is, however, not many persons, even among botanists, bestow a thought as to how it is brought about. Gardeners recognise two distinct modes in which flowers may be produced, either from the "old wood," meaning the wood formed in the previous season, or from the shoot of the present year's growth. A rhododendron with its flowers packed up in a "winter-bud" destined to unfold in spring, an apple or a laburnum with their winter-buds at the ends of short contracted shoots or "spurs," afford illustrations of the one type, while a rose, with its newly-formed shoots crowned with one or more rose-buds, supplies an example of the latter. There is the same sort of difference between these two kinds of flowers that there is between the so-called "annual" plants whose course of life is outrun in a single season, and "herbaceous perennials" which die down in winter, leaving a winter-bud to carry on the work when circumstances become propitious in spring. The second growth of flowers in autumn may, therefore, be due to two different causes. In the one case it is an anticipation of spring; the flowers being produced afore time. Conditions of growth being persistently favourable, the winter-bud, instead of remaining dormant, bursts prematurely into growth, and repeats in autumn what its predecessor had done in spring. The great difficulty in such a case is to explain why one bud, or at any rate only a small proportion of the total number of buds, acts in this way when the circumstances of the case would appear to be substantially alike in all. To talk of the individuality of buds is to denote a fact which every observer must be conversant with, but which does not supply any explanation. In the second class of cases the flowers are, as in "hybrid perpetual" roses, placed at the ends of some of the shoots of the year. In this case gardeners have availed themselves of what was originally an occasional tendency to continue the development of flowers on the end of certain shoots, and have, as it were, converted an accidental into a constant occurrence. Doubtless they might do the same in the case of the laburnum, were they so disposed. It is here that the skill of the gardener comes in, and even enables him, to some extent, to baffle adverse climatic influence and induce a plant, as a regular thing, to flower twice in a season, or even more or less continuously, when, if left to itself it would either not do so at all, or only in a fitful, uncertain manner. It is worth notice, too, that these second blooms are often (but by no means invariably) malformed. Some rhododendrons now before me are so, while the double-flowered apples that one occasionally sees are always, in my experience, formed on the midsummer shoots of the tree. So, again, with pears, the second crop of flowers is usually produced on shoots of the year, and very generally the flowers are more or less imperfect or misshapen. The "Napoleon" pear behaves in this way every year. Every year, too, I am indebted to Mr. Burbidge, of the Trinity College Botanic Garden, Dublin, for specimens of "Bishop's Thumb" pears, produced on the summer shoots. These pears are more like fingers than thumbs, and are destitute of core. The flower-stalk swells up as usual, and produces an eatable pear, but the carpels and seeds are conspicuous by their absence. The developing force has been energetic enough to produce flower- and fruit-stalk, but it has failed in the more essential process of seed- and embryo-formation. Possibly in some cases the absence of seed may be the result of want of fertilisation. It may be that in the flowers some at least of the carpels are present with their contained ovules, but, owing to the want of effective fertilisation, they have dwindled away and left no trace.

It would be a curious and important matter to ascertain whether, and to what extent, this repeated flowering process exhausts the plant. If no seed were produced the extra outlay of energy would probably not be severely felt. But every rose-grower knows how great are his losses, and how difficult it is to keep his "standards" in good form and good health. Of course there are many causes for this, but it is not unreasonable to suppose that one of them arises from exhaustion from continuous flowering, which produces a condition that predisposes to disease.

Another phenomenon of a somewhat similar character is very commonly met with this autumn, although, not unnaturally, it does not attract so much attention. I allude to the production of buds and leaf-shoots on the partially withered stems of herbaceous perennial plants, such as various species of Epilobium, Malva, &c. The branches of these plants usually dry up after flowering, leaving only a rosette of leaves or a winter-bud to carry on the growth next season; but occasionally they retain some amount of vitality, and, as at this season, produce a new generation of shoots from the old ones.

These variations show how artificial are the distinctions denoted by the terms annual, perennial, herbaceous, and the like, and they show what a wide range of physiological diversity may exist within the limits of the same species. MAXWELL T. MASTERS

ARROW-RELEASE[1]

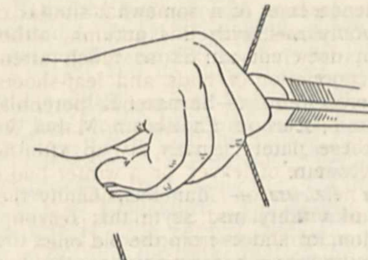
AT the commencement of this very interesting and instructive monograph, Prof. Morse tells us that when he began collecting data illustrating the various methods of releasing the arrow from the bow, as practised by different races, he was animated merely by curiosity; nor was it until he had accumulated quite a collection of sketches and other memoranda on the methods of arrow-release, not only of existing but of ancient races, as shown by frescoes and rock-sculptures, that he realised that even so trivial an art as that of releasing the arrow might possibly lead to interesting results in tracing the affinities of races. Hence he publishes in the present pamphlet the data which he has thus far collected, in the hope that further material may be secured for a more extended memoir on the subject. The great difference which Prof. Morse observed between the ordinary English and Japanese methods of using the bow first led him to investigate the subject, with the curious results to be presently narrated. The various forms of release, with their different modifications, are classified, and perhaps Prof. Morse's investigations may be most succinctly described by using his classification.

(1) *Ordinary Release.*—This is the simplest form of release, and is that which children all the world over naturally adopt in first using the bow. It consists in simply grasping the arrow between the end of the straightened thumb, and the first and second joints of the bent forefinger (Figs. 1 and 2). With a light or weak bow, says Prof. Morse, this release is the simplest and best; it makes little difference on which side of the bow the arrow rests, provided the bow is held vertically. On the other hand, however, a stiff bow cannot be drawn in this way, unless one possesses enormous strength in the fingers. This simple or primary release is that in use amongst the Ainos of Yezo, by the Demerara Indians, apparenily also by the Utes. The Navajos employ it when shooting at prairie dogs, so that the arrow will not penetrate the ground if it misses its mark; so do the Chippewas. The Micmac Indians of the Cascapedia settlement, on the north shore of the Bay of Chaleur, used it, and it is said that the other tribes in this part of Canada draw the arrow in the same way. A member of the Penobscot tribe at Moosehead Lake, seemed incredulous when Prof.

[1] "Ancient and Modern Methods of Arrow-Release." By Edward S. Morse, Director Peabody Academy of Science. Essex Institute *Bulletin*, October-December, 1885.

Morse told him that there were other methods of drawing the arrow.

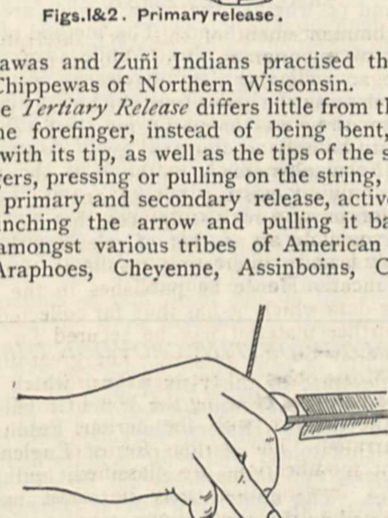
(2) *Secondary Release.*—This is a direct outgrowth from the primary release. It consists in grasping the arrow with the straightened thumb and bent forefinger, while the ends of the second and third fingers are brought to bear on the string to assist in drawing (Figs. 3 and 4).



Figs.1&2. Primary release.

The Ottawas and Zuñi Indians practised this, as also did the Chippewas of Northern Wisconsin.

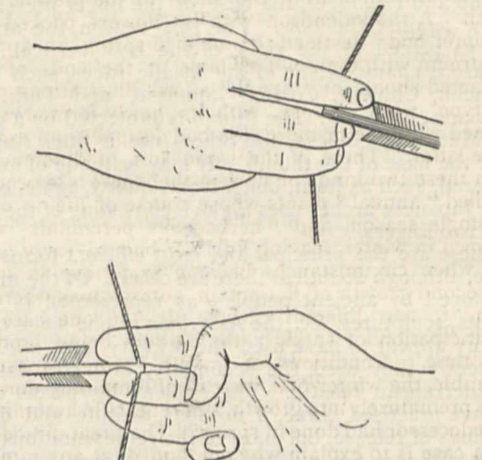
(3) The *Tertiary Release* differs little from the secondary. The forefinger, instead of being bent, is nearly straight, with its tip, as well as the tips of the second and third fingers, pressing or pulling on the string, the thumb, as in the primary and secondary release, active in assisting in pinching the arrow and pulling it back. This is used amongst various tribes of American Indians—Sioux, Araphoes, Cheyenne, Assinboins, Comanches,



Figs.3&4. Secondary release.

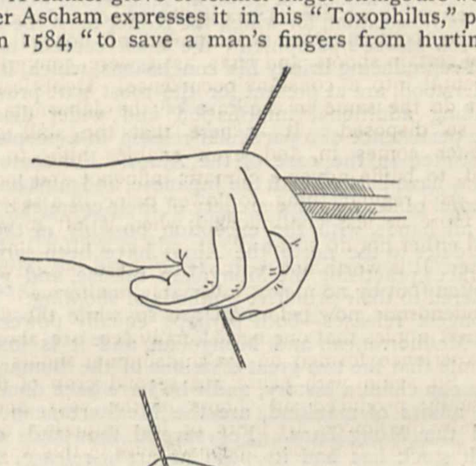
Crows, and Blackfeet. The Siamese, too, practise this release, with the difference that one finger only is used on the string instead of two. It appears, too, from Mr. Man's recent paper before the Anthropological Institute, that the Andaman Islanders use this method.

(4) *The Mediterranean Release.*—This release has been in vogue among the northern Mediterranean nations for centuries, and among those of the southern Mediterranean for tens of centuries, and is the oldest release of which we have any knowledge. It is practised to-day, continues Prof. Morse, by all modern English, French, and American archers, and is the release used by the European archers of the Middle Ages. It consists in drawing the string back with the tips of the first, second,



Figs.5&6. Mediterranean release.

and third fingers, the balls of the fingers clinging to the string, with the terminal joints of the fingers slightly flexed. The arrow is held lightly between the first and second fingers, the thumb straight and inactive (Figs. 5 and 6). A leather glove or leather finger-strings are worn, as Roger Ascham expresses it in his "Toxophilus," published in 1584, "to save a man's fingers from hurtinge,



Figs.7&8 Mongolian release.

that he may be able to beare the sharpe stringe to the uttermoste of his strengthe." In this release, the arrow must be to the left of the bow vertical. The Eskimo of Alaska employ this release, using, however, only the first and second fingers in drawing the string, and it appears to be almost universal in the Arctic regions.

These four releases may be considered, Prof. Morse thinks, as successive modifications of each other; but the next release is an entirely independent form, having no relation to the other.

(5) *The Mongolian Release.*—In this the string is drawn by the flexed thumb bent over the string, the end of the forefinger assisting in holding the thumb in position (Figs. 7 and 8). The arrow is held at the junction of the thumb and forefinger, the base of the finger pressing the arrow against the bow. For this reason the arrow is always placed to the right of the bow vertical. This release is characteristic of the Asiatic races, such as the Manchu, Chinese, Corean, Japanese, and Turk. The Persians also use it. The thumb is protected by a guard: the Manchus, Chinese, and others use a thick ring worn near the base of the thumb. It may be made of any hard material, such as horn, bone, ivory, quartz, agate, or jade. The Japanese archer uses a glove consisting of the thumb and two fingers.

These are the principal and most efficient forms of release, although doubtless there are others. Of the methods employed by ancient peoples, as represented in manuscripts, sculptures, &c., the Assyrians at one stage of their history appear to have used the primary form, while subsequently they used the secondary, and still later the Mediterranean release. The ancient Egyptians appear to have practised three, if not four, definite and distinct methods of release, but many of the representations in the old sculptures are evidently purely conventional, while some are clearly impossible. Following on these, Prof. Morse discusses the methods employed in ancient Greece, Persia, Japan, China, India, Mexico. Here he is naturally on less secure ground, for he has to endeavour to spell out a conclusion from various and conflicting positions of the hand in various ancient graphic representations of life amongst these peoples. The discussion involves a considerable amount of detail and numerous woodcuts by way of illustration, for which the reader must be referred to Prof. Morse's pamphlet. We must content ourselves with reproducing briefly his conclusions, which, it will be understood, are at present for the most part provisional, pending additional information and wider discussion. The persistence of a particular release in a people is well illustrated in the case of the Ainos. For centuries the Ainos have battled with the Japanese, and must have been mindful of the superior archery of their enemies; indeed, on all hands, with the exception possibly of the Kamchatdales to the north, the Ainos have been surrounded by races practising the Mongolian release, and yet have adhered to their primitive methods of shooting. The two strongest releases—both perhaps equally powerful—are the Mediterranean and Mongolian, and it is interesting to note that the two great divisions of the human family who can claim a history, and who have been dominant in the affairs of mankind, are the Mediterranean nations and the Mongolians. For several thousands of years each stock has had its peculiar arrow-release, and this has persisted through all the mutations of time to the present day. Language, manners, customs, religions, have in the course of centuries widely separated these two great divisions into nations. Side by side they have lived; devastating wars and wars of conquest have marked their contact; and yet the apparently trivial and simple act of releasing the arrow from the bow has remained unchanged. At the present moment the European and Asiatic archer, shooting now only for sport, practise each the release which characterised their remote ancestors. The following classified list shows in a general way that the primary, secondary, and tertiary releases are practised by savage races to-day, as well as by certain ancient civilised races, while the Mediterranean and Mongolian releases, though originating early in time, have always characterised the civilised and dominant races. The exceptions to this generalisation are curious: the Little Andaman Islanders practise the Mediterranean release, and those of the Great Andamans the Tertiary; various groups of Eskimo practise the Mediterranean release, and have designed a distinct form of arrow for this method.

Primary Release.—Savage: Ainos, Demerara Indians, various North American tribes; civilised: early Assyrian, Egyptian, and Grecian (?)

Secondary Release.—Savage: some North American tribes; civilised: later Assyrian and Indian (?)

Tertiary Release.—Savage: North American tribes, Great Andamans; civilised: Siamese, Egyptian, Grecian, and Mexican (?)

Mediterranean Release.—Savage: Eskimo, Little Andamans; civilised: European nations now, and the archers of the Middle Ages, later Assyrian, early Egyptian, Arabian, Indian, and Roman.

Mongolian Release. — Manchus, Chinese, Coreans, Japanese, Turks, Persians, Scythians, Egyptians (?)

In conclusion, Prof. Morse expresses a belief that the method of using the bow may form another point in establishing or disproving relationships, in identifying the affinities of past races. Travellers and explorers should not content themselves with observing the simple fact that such and such people use bows and arrows, but they should accurately record (1) the attitude of the shaft hand; (2) whether the bow is held horizontally or vertically; (3) whether the arrow is to the right or left of the bow vertical; and (4) whether the extra arrows are carried in the bow hand or shaft hand. The method of bracing the bow is of importance also. While anxious to get information respecting the arrow-releases of tribes and peoples, he is particularly desirous of hearing about those employed by the Veddahs of Ceylon, the hill-tribes of India, African tribes, and those of South America, especially the Fuegians. Such material, in the shape of descriptions, photographs, drawings, and if possible specimens of bows and arrows, may be sent to Prof. E. S. Morse, Peabody Academy of Science, Salem, Massachusetts, and will be acknowledged and used in a future publication on the subject.

CLIMATOLOGY OF THE CROYDON DISTRICT[1]

In a little tract of thirty-six pages, which has just appeared in the *Transactions* of the Croydon Microscopical and Natural History Club, Mr. Eaton has discussed the climatology of this part of England with a skill, clearness, and fairness seldom met with in local climatologies. The observations of temperature, which were conducted on the same systematic plan with Stevenson's screens, were made at seven stations, these being, in the order of their heights, Park Hill, Addiscombe, South Norwood, West Norwood, Waddon, Wallington, and Beddington. The periods selected for discussion are the five years 1881 to 1885 inclusive. The stations are included within an area measuring 4 miles from north-east to south-west by 2½ miles from south-east to north-west. The monthly results are given on fourteen pages with satisfactory fullness; and with them are conjoined, for the sake of comparison, the corresponding records of temperature at the Greenwich and Kew Observatories.

The heights and mean temperatures of the five stations from which observations are available for the whole of the five years are these:—Beddington, 102 feet, 48°·8; Waddon, 156 feet, 49°·0; South Norwood, 190 feet, 49°·4; Addiscombe, 202 feet, 49°·3; and Park Hill, 259 feet, 49°·4,—Park Hill, the highest station, being thus 0°·6 warmer than Beddington, the lowest station. This subversion of the general rule that the temperature diminishes with greater elevation is shown to be due to the frequency with which, on clear calm nights, the air in contact with the ground is cooled and rendered denser by radiation,

[1] "Report on the Temperature and the Rainfall of the Croydon District, 1881-85," by Henry Storks Eaton.

and thereafter descends to the low-lying grounds of the valleys, displacing the warmer air below. During the unusually dry clear months of January and July 1881 the mean temperature of Park Hill exceeded that of Beddington by 3°·5 and 2°·5 respectively. Hence the first three of the five stations which are on sloping ground have, though at greater elevations than the other two stations below, higher mean temperatures.

This peculiarity in the distribution of the night and the winter temperature becomes the more intensified as the valley is deeper and its sides steeper, and as calms and light winds prevail. Thus at Klagenfurt, situated in one of the valleys of the Tyrol, the mean temperature of January is 20°·7, whereas at the station of Obergipfel, about seven miles distant and 4270 feet higher, the mean for the same month is 19°9, being thus less than a degree lower than that of Klagenfurt. The subject is one that has seldom received the earnest attention it deserves, particularly in drawing the isothermals of the globe. The Croydon Club would make a clear addition to their observing-system if new stations were established on knolls in the valley of the Wandle for the further prosecution of this inquiry.

The means of temperature from Greenwich and Kew would have had real value in this inquiry if Mr. Eaton could have availed himself of observations made at these Observatories with thermometers exposed in the Stevenson screen. But, as pointed out, the different modes of exposing the thermometers render the results of the three systems of observing incomparable *inter se*. Thus the mean of the daily highest temperature of August for the five years is 72°·5 for Greenwich, and 69°·5 for Kew.

The rainfall has been far more extensively observed in the district, the returns of no fewer than seventy stations being available. Grouping the stations according to height, the annual amounts at stations below 200 feet show a mean of 23·27 inches; 200 to 400 feet, 25·39 inches; 400 to 600 feet, 29·12 inches; 600 to 800 feet, 31·66 inches; and above 800 feet, 31·36 inches. The largest amounts of rain occur not on the ridge of the North Downs, but some distance on the lee-side in regard to the prevalent rainy south-westerly winds; and the amount at like elevations seems also to diminish from west to east. As regards the monthly rainfall, the depth is greater in the upper groups; but the ratios of the monthly to the annual fall show that in spring, but more particularly in summer, there falls proportionally a larger amount of rain in the lower group of stations, whose average elevation is 193 feet. The relatively large increase in the summer rainfall over low-lying plains is one of the most striking facts in the geographical distribution of the rainfall, and is probably due to the physical causes concerned in the development of thunderstorms.

NOTES ON THE RECENT SWARMING OF APHIDES

THE immediate cause of the sudden appearance of clouds of insects in certain localities is not very apparent, but it may be surmised that the predominance or scarceness of their natural insect foes has much control over the phenomenon; added to which must be taken into account the effects of weather and temperature. A few days ago I had a notice from an obliging Birmingham correspondent, Mr. George Baker, who kindly furnished me with the following particulars:—

On October 5 the town of Mansfield, on the borders of Sherwood Forest, was visited by a cloud of Aphides, which swarmed in the town and over the country round, across an area of many miles. The town was visited "literally by millions; every one, as they walked along, waving their handkerchiefs or newspapers before their faces to avoid inhaling the insects. . . . Wet paint was covered by a mass of these black Aphides." This swarm continued with decreasing numbers throughout five days, and heavy rain during part of this time did not seem much to affect them. On the road to Nottingham these insects were noticed as engaged in singular gyrations and undulatory dances above the tops of the spruce-firs, there forming dense pyramidal columns.

A similar cloud, but less remarkable as to numbers, was observed about the same time at Birmingham; which, however, as the town must be at least 50 miles distant, can be scarcely considered as forming a part of this same swarm. Possibly similar causes operated to produce the like phenomenon in both places.

These insects proved on examination to be *Rhopalosiphum dianthi* of Schrank, which is identical with *Aphis persicæ* of Morren, and *A. rapæ* of Curtis, and *A. vastator* of Smee. It is a veritable pest in some years, doing considerable damage to turnip, mangel, and other crops, and in our gardens injuring our peach-trees. This present notice of its swarming is, however, by no means unprecedented.

In September and October 1834 Morren noted an immense swarm all over Belgium, and states his belief that it came across the sea from England. He says they obscured the light of day, and covered the walls of the houses so as partially to conceal them. Gilbert White notes that in August 1785 the people of Selborne were surprised by a swarm of "smother flies." Those that were walking in the street found themselves covered with these insects, which blackened the hedges and vegetables round. White thought these might be emigrations from the hop-gardens of Kent and Sussex, and from those near Farnham. If so, the species differs from the insects above noticed.

The choice of high objects to dance over is not confined to Aphides, *e.g.* many of the Tipulidæ. The singular persistent dance of *Anthomyia meteorica* over the heads of horses is familiar to all. G. B. BUCKTON

NOTES

A MOST attractive group of birds has just been placed by Prof. Flower in the great hall of the Natural History Museum at South Kensington. The case is intended to illustrate the hybridisation of species in a state of nature, and the species selected are the hooded and carrion crows (*Corvus cornix* and *C. corone*) and the European and Asiatic goldfinches (*Carduelis elegans* and *C. orientalis*). The series of these birds has been presented to the Museum by Mr. Henry Seebohm, who procured the specimens himself during his travels in Siberia. The case of the crows is one of the few instances known of actual wild hybridisation, though many more are suspected, especially among the game birds. It is certain, however, that wherever the colonies of hooded crows meet the carrion crow throughout the Palæarctic region the two species interbreed freely, and the result is shown in the young, the gray saddle-back of the hooded crow exhibiting a considerable admixture of black owing to the strain of *C. corone* in the parentage. The case of the goldfinches is not quite so completely proved, but is apparently a parallel instance of hybridisation. The British Museum has been for some time indebted to Mr. Seebohm for very valuable presents of birds, which have been mounted in the bird-galleries. Not long ago he gave a specimen of Ross's gull (*Rhodostethia rossi*), one of the rarest of the *Laridæ*, and a species which was a *desideratum* to the national collection. He presented also, last year, a fine case of Steller's sea-eagle (*Haliaëtus pelagicus*) from Kamchatka.

THE Geodetic Conference began its meetings in Berlin last week. The countries represented are Belgium, by two delegates; Denmark, by one; Germany, by fourteen, including Prof. Dr. Förster, of the Royal Observatory, Prof. Helmholtz, Dr. W. Siemens, and Colonel Golz, of the Trigonometrical Survey: France, by two, namely, MM. Faye and Tisserand; Italy, by one; the Netherlands, by one; Norway, by one; Austria, by three; Portugal, by one; Roumania, by two; Russia, by two, including Dr. von Struve, of the Observatory at Pulkowa; Sweden, Switzerland, and Spain, each by one. England, strange to say, is not represented; nor has any one come from the United States. Prof. Dr. Förster, of Berlin, was elected President, and Dr. von Struve, of Pulkowa, Vice-President of the Conference. In his opening address, Herr von Gossler, Prussian Minister of Public Worship, indulged in some general observations as to the progress and aims of geodetic science, and, in the name of the Prussian Government, thanked the various foreign deputies for their appearance in Berlin. The chief task of the present Conference has been to settle the organisation of the central geodetic bureau, which is to have its permanent seat in Berlin, in connection with the Geodetic Institute of Prussia, founded by the late Lieut.-General von Bäyer. It was at the instance of Lieut.-General Bäyer that the first constituent international meeting of geodetic experts was held in Berlin in 1864, and it is by the establishment of a central international bureau here, supported by quotas from the various countries which it represents, that it is intended to preserve to Prussia the leading part she has always taken in promoting the science of earth-measuring and all its kindred branches. The permanent Committee elected includes Prof. Hirsch, of the Neuchâtel Observatory (Secretary), Professors Förster (Prussia), Sande (Holland), Faye (France), Ferrero (Italy), Ibannez (Spain), Ragel (Saxony), Oppolzer (Austria), Stepnicki (Russia), and Zachariae (Denmark). The next Conference will be held in 1887 at Nice, on the invitation of M. Bischoffsheim, owner of the great Observatory there. Before separating, the Conference passed a resolution requesting the Prussian Government to invite other States to join the International Geodetic Society.

AT a recent meeting of the Common Council it was decided that it be referred to the Gresham Committee to consider whether the moneys now paid for lectures under the provisions of Sir Thomas Gresham's will might be devoted to the encouragement of students destined for commercial careers acquiring a useful knowledge of modern languages, with instructions to confer with the Mercers' Company, and to report thereon forthwith.

THE Professor of Physics of the University of Vienna, Dr. Victor Pierie, died suddenly of appoplexy in his laboratory on Friday last.

AT the Potato Centenary on December 2 and 3, to which we have already referred, the following subjects for conferences have been proposed:—First day, Morning: (1) historic consideration of the question, Whence came the potato to England? (2) the Incas and their cultivation of the potato; (3) distinct wild species of the potato as at present recognised; (4) the production of varieties by cultivation. Afternoon: (5) the potato disease; (*a*) historic sketch, (*b*) our present knowledge of the disease. Second day, Morning: (1) proposed methods for preventing the disease; (2) methods for using partly diseased potatoes; (3) methods for storing and preserving potatoes. Afternoon: conference of cultivators on rates for transport of potatoes.

THE French Government has granted the funds required for the completion of the Algiers Observatory, which will be in full operation next spring. Two assistant astronomers have already been sent to join M. Trépied, and two others will be selected from among the pupils of the School of Astronomy this winter. A special Congress will be held in Paris, in the month of April, for determining the part that the Algiers Observatory will take in stellar photography. The direct image of the sun will be 6 centimetres in diameter. A spectroscope by Thollon will be put into operation. The extent of the spectrum will be 10 metres. M. Trépied has organised the electrical transmission of the time to the Hôtel de Ville of Algiers and Tunis. Colonel Perrier, head of the French and Algerian Survey, is arranging the measurement of the requisite triangles for connecting the Algiers Observatory with the Colonne Voirol, the starting-point of the Algerio-Tunisian system of triangulation.

MR. W. A. CARTER, of the Colonial and Indian Exhibition, writes to us that during this last spring he placed a specimen of the Mexican axolotl in an empty (? dry) receptacle, where it has remained ever since. It is in a lively condition. The colour of the animal has become less intense, the gills have apparently disappeared, and the powers of locomotion seem quickened.

IT is worthy of note that at the establishment of the National Fish Culture Association many of the brook trout (*Salmo fontinalis*) hatched during February 1885 commenced to spawn last week, yielding about five hundred ova each. This fact is another proof of the extraordinary reproductive capacity of fishes in spite of age and artificial existence, for the fish in question have been maintained in a pond of limited dimensions. The size of the ova is small as compared with those of mature fish, therefore it is not likely that the trout when hatched will be large. The parents are in a healthy condition, and seem in no way weakened.

A CONSIGNMENT of nearly a thousand German carp of various kinds has arrived at the Colonial and Indian Exhibition. The great hardihood of the carp is evidenced by the fact that the fish in question were retained in carriers for sixty hours before being placed in tanks, when only two were found to have succumbed.

IN a paper in the October number of the *American Journal of Science* by Mr. O. W. Huntington, "On the Crystalline Structure of Iron Meteorites," the author concludes as follows:—"We have tried in this paper to establish the following points: (1) that many of the masses of meteoric iron in our collections are cleavage crystals, broken off probably by the impact of the mass against the atmosphere; (2) that these masses show cleavages parallel to the planes of all the three fundamental forms of the isometric or regular system, namely, the octahedron, the cube, and the dodecahedron; (3) that the Widmanstättian figures and Neumann lines are sections of planes of crystalline growth parallel to the same three fundamental forms of the isometric system; (4) that on different sections of meteorites Widmanstättian figures and Neumann lines can be exhibited in every gradation, from the broadest bands to the finest markings, with no break where a natural line of division can be drawn; (5) that the features of the Widmanstättian figures are due to the eliminations of incompatible material during the process of crystallisation. This investigation throws no new light upon the origin of meteorites, except so far as it strengthens the opinion that the process of crystallisation must have been extremely slow. The occurrence of large masses of native iron occluding hydrogen gas, and containing nickel, cobalt, phosphorus, sulphur, &c., implies a combination of conditions which the spectroscope indicates as actually realised in our own sun and in other suns among the fixed stars, and the most probable theory seems to be that these masses were thrown off from such a sun, and that they very slowly cooled, while revolving in a zone of intense heat. In this paper we have not taken into consideration a number of iron masses, whose meteoric origin has been generally accepted, which show no Widmanstättian figures, and not even any Neumann lines. A considerable proportion of these are certainly not meteoric. In the Harvard cabinet there are two specimens, labelled respectively Campbell County (Tennessee), and Hominy

Creek (North Carolina), which are evidently nothing but cast-iron, and a third, labelled Tarapaca Hemalga (Chili), which is probably of similar material. We could find on the specimens of this class in the Harvard collection no distinct evidences of crystallisation; but also we could find no features incompatible with that unity of structure which it has been the chief object of this paper to illustrate."

MR. HORATIO HALE has issued in pamphlet form his address "On the Origin of Languages and the Antiquity of Speaking Man," delivered before the Anthropological Section of the American Association for the Advancement of Science at Buffalo last August. The author's views were much discussed at the time, and those interested in the subject will be thankful to have them presented in this convenient form. Rejecting all the theories hitherto advanced by Lyell, Frederick Müller, and others, he endeavours to account for the vast number of *specifically distinct* languages spoken by races *not specifically distinct* by assuming that they originated from children's prattle in independent centres after the spread of speechless man over the globe. The cases are mentioned of the Boston twins born in 1860 and of some other "Geschwister," who appear to have evolved and practised for some time infantile jargons understood only amongst themselves, which it is argued might, under favourable conditions of isolation and so forth, develop into regular forms of speech consistently worked out with their own vocabularies and grammatical structure. In this way linguistic families differing absolutely one from the other need not be of any great antiquity, and in fact may have been developed from slight germs in many places and at different times since the dispersion of the "*homo alallus*" from some given centre. This *homo alallus* himself is admitted to be the lineal descendant of the men of the Stone Age, who are assumed to have been speechless, so that all forms of speech now current may be of comparatively recent date, say, not more than 8000 or 10,000 years, notwithstanding their great number and profound differences. This theory, which refers human speech in the first instance to "the language-making instinct of very young children," is presented with considerable force and plausibility, but will scarcely be taken seriously either by philologists or anthropologists. The latter especially will find it difficult to accept the conclusion that man properly so called, the *homo sapiens*, as distinguished from his precursor of the Neolithic Age, does not date further back than "somewhere between 6000 and 10,000 years ago." The theory also requires us to regard this first speaking man as already fully developed, possessing "intellectual faculties of the highest order, such as none of his descendants has surpassed," thus reversing the conclusions of modern anthropology.

IT is reported from Vienna that a great ice cavern has been discovered on the southern slope of the Dachstein, or Schneeberg, the very conspicuous lofty mountain in Lower Austria, which is visible from the ramparts of the capital. The general direction of the cavern runs from south to north, and it has been explored for a distance of 600 metres, a sharp precipice seemingly 14 metres deep having stopped for the time further progress. The cavern is from 5 to 6 metres broad, and very lofty, giving the impression that the ice is enormously thick. The explorers are of opinion that a subterranean lake will be found in the cavern.

THE additions to the Zoological Society's Gardens during the past week include a Bonnet Monkey (*Macacus sinicus* ♀) from India, presented by Miss Edith Prowse; four Common Hedgehogs (*Erinaceus europœus*), British, presented by Mr. W. Walkinshaw; a —— Buzzard (*Buteo* ——) from Mogador, North Africa, presented by Mr. P. L. Forwood; a Ring-necked Parrakeet (*Palæornis torquatus* ♀) from India, presented by Mr. W. S. Bradshaw; an Aldrovandi's Skink (*Plestiodon auratus*) from North Africa, deposited; a Rusty-spotted Cat (*Felis rubiginosa*) from Ceylon, two Diuca Finches (*Diuca grisea*) from Chili, two Wood Larks (*Alauda arborea*), British, purchased; eight Long-fronted Gerbilles (*Gerbillus longifrons*), born in the Gardens.

OUR ASTRONOMICAL COLUMN

THE BINARY STAR γ CORONÆ AUSTRALIS.—Mr. H. C. Wilson, of Cincinnati Observatory, has published elements of the orbit of this interesting southern double star in the *Sidereal Messenger* for October. These elements, which do not differ much from a set recently computed by Mr. Gore (*Monthly Notices*, vol. xlvi. p. 104), are as follows:—

$P = 78{\cdot}80$ years	$\lambda = 139^\circ{\cdot}0$
$T = 1887{\cdot}40$	$\Omega = 41^\circ{\cdot}0$
$e = 0{\cdot}324$	$a = 1''{\cdot}85$
$\gamma = 50^\circ{\cdot}5$	

Comparing observations made 1834·47 to 1883·62 with this orbit, Mr. Wilson finds that the position-angles are well represented, with the exception of those observed by Powell from 1859 to 1864, which seem to be affected by systematic error, and thinks we may conclude the period is not far from eighty years. It is to be hoped that numerous observations of this star will be obtained during the next ten years, while the distance is small and the angular motion rapid.

OPPOLZER'S ASTRONOMICAL REFRACTIONS.—Herr Oppolzer has recently published, in the *Transactions* of the Mathematical and Natural Science Section of the Imperial Academy of Sciences of Vienna, vol. liii., a paper containing a theoretical discussion of the problem of astronomical refraction, followed by numerical tables intended to facilitate the practical application of the results at which he arrives. The relation between the temperature (t) and density (ρ) of the atmosphere which Herr Oppolzer adopts is

$$\frac{\delta t}{\delta \rho} = \epsilon + \Sigma k \rho^{\sigma - 1},$$

where k and σ are quantities depending on the state of the atmosphere and on the place of observation. Whatever may be thought of the legitimacy of a relation of this form from a theoretical point of view, it at all events has the advantage, in Herr Oppolzer's skilful hands, of leading to a comparatively simple expression for the amount of refraction, deduced from a modification of the ordinary differential equation. And that it is capable, when the approximations are carried far enough, of giving results of great accuracy for large zenith distances, is shown by a comparison made between the computed values of the refraction and the well-known observations of Argelander, which form the basis of Bessel's supplementary table given in the "Tabulæ Regiomontanæ," with the following results:—

Z.D.	Observed—Computed	Z.D.	Observed—Computed
° ′	″	° ′	″
85 0	... − 1·1	88 0	... − 2·5
86 0	... + 1·2	89 0	... + 2·3
87 0	... − 1·3	89 30	... + 1·8

COMETS FINLAY AND BARNARD.—The following ephemerides for Berlin midnight are from the *Astronomische Nachrichten*, No. 2752:—

Comet Finlay (1886 *e*)

1886	R.A. h. m. s.	Decl. ° ′	Log *r*	Log Δ
Nov. 8	19 25 22	24 56·8 S.	0·0751	0·0970
10	33 49	24 36·5		
12	42 24	24 14·1	0·0718	0·0932
14	51 5	23 49·5		
16	19 59 51	23 22·7 S.	0·0697	0·0899

Comet Barnard (1886 *f*)

1886	R.A. h. m. s.	Decl. ° ′	Log *r*	Log Δ
Nov. 7	12 7 8	8 18·5 N.	0·0735	0·2031
9	15 5	9 0·3		
11	23 31	9 44·3	0·0551	0·1772
13	32 29	10 30·7		
15	12 42 1	11 19·2 N.	0·0366	0·1507

ASTRONOMICAL PHENOMENA FOR THE WEEK 1886 *NOVEMBER* 7-13

(FOR the reckoning of time the civil day, commencing at Greenwich mean midnight, counting the hours on to 24, is here employed.)

At Greenwich on November 7

Sun rises, 7h. 6m.; souths, 11h. 43m. 49·6s.; sets, 16h. 22m.; decl. on meridian, 16° 21′ S.: Sidereal Time at Sunset, 19h. 29m.

Moon (Full on November 11) rises, 15h. 4m.; souths, 20h. 58m.; sets, 3h. 2m.*; decl. on meridian, 1° 49′ S.

Planet	Rises	Souths	Sets	Decl. on meridian
	h. m.	h. m.	h. m.	° ′
Mercury	9 22	13 11	17 0	23 59 S.
Venus	6 26	11 20	16 14	13 25 S.
Mars	10 42	14 27	18 12	24 27 S.
Jupiter	4 55	10 19	15 43	7 43 S.
Saturn	20 30*	4 32	12 34	21 18 N.

* Indicates that the rising is that of the preceding evening and the setting that of the following morning.

Occultations of Stars by the Moon (visible at Greenwich)

Nov.	Star	Mag.	Disap.	Reap.	Corresponding angles from vertex to right for inverted image
			h. m.	h. m.	° °
7	4 Ceti	6	17 45	18 32	32 321
7	5 Ceti	6	18 0	19 1	48 311
7	B.A.C. 5	6	18 23	19 42	89 281
9	ν Piscium	4½	18 4	19 9	60 277
12	48 Tauri	6	19 18	20 18	61 251
12	γ Tauri	4	21 17	22 25	55 271
13	75 Tauri	6	2 38	3 37	162 275
13	θ^1 Tauri	4½	2 46	3 57	62 17
13	θ^2 Tauri	4½	3 6	near approach	39 —
13	B.A.C. 1391	5	3 39	4 46	115 332
13	Aldebaran	1	6 27	7 16	165 284

Saturn, Nov. 7.—Outer major axis of outer ring = 43″·5; outer minor axis of outer ring = 16″·8; southern surface visible.

Nov.	h.	
13	17	Mercury at greatest elongation from the Sun, 22° east.

Variable Stars

Star	R.A.	Decl.			
	h. m.	° ′			h. m.
S Cassiopeiæ	0 17·1	55 10 N.	Nov.	9,	*M*
U Cephei	0 52·2	81 16 N.	,,	8,	3 49 *m*
			,,	12,	3 29 *m*
Algol	3 0·8	40 31 N.	,,	11,	3 56 *m*
R Aurigæ	5 8·1	53 27 N.	,,	10,	*M*
S Cancri	8 37·4	19 27 N.	,,	9,	3 53 *m*
U Ophiuchi	17 10·8	1 20 N.	,,	8,	3 37 *m*
		and at intervals of			20 8
β Lyræ	18 45·9	33 14 N.	Nov.	12,	19 0 *m*
R Lyræ	18 51·9	43 48 N.	,,	13,	*m*
η Aquilæ	19 46·7	0 43 N.	,,	7,	5 0 *m*
R Vulpeculæ	20 59·3	23 22 N.	,,	9,	*M*
δ Cephei	22 24·9	57 50 N.	,,	10,	5 0 *M*

M signifies maximum; *m* minimum.

Meteor Showers

A radiant near δ Hydræ, R.A. 124°, Decl. 4° N., and one in Camelopardus, R.A. 102°, Decl. 73° N., are active in the early part of this week. Moonlight interferes with meteor observation during the greater part of the week.

THE HIGH TEMPERATURE IN OCTOBER

THE warm weather which occurred at the commencement of the month was so exceptional for the season, and extended over so large a part of Europe, that a few facts as to its general character may be of interest, and will afford opportunity of comparison with earlier records, as well as with records of any similar weather in time to come.

The highest temperatures were experienced during the first five days of the month, and were chiefly confined to Western, Central, and Southern Europe. During this time atmospheric pressure was generally high over Central Europe, and decreased towards the western or Atlantic coasts, so that the conditions of pressure were favourable to anticyclonic circulation over France and the south-east of England, and cyclonic circulation in Ireland and the northern parts of the British Islands. The barometric gradients were very slight over the Continent, but were rather steeper over Great Britain and Ireland, owing to the proximity of a barometric depression to the westward. This distribution of pressure was accompanied by southerly and south-easterly winds over Western Europe, and especially over France and our own islands, but it was only in Ireland and the more western parts of Great Britain that the wind was at all fresh.

At this season of the year our warmest weather in England is commonly experienced with south-easterly winds, as is well shown in the valuable discussion of the Greenwich observations for the years 1849 to 1868, in which the temperatures have been averaged for the several wind directions. The following are the temperatures for October:—

	N.	N.E.	E.	S.E.	S.	S.W.	W.	N.W.
Monthly means	47	50	52	55	53	53	51	51
Highest hourly means	52	55	59	61	59	58	57	55

The same discussion also shows the striking difference which exists, in October, between the temperature with a cloudless and a cloudy sky:—

	Mean	Mean max.	Mean min.
Cloudless sky	50·8	61·1	43·5
Cloudy sky	51·8	54·8	49·8

The high temperatures experienced over England in October this year occurred with an exceptionally clear sky, as well as with a remarkably steady south-easterly wind, and the air before reaching England had been subjected to very similar conditions on the continent of Europe.

The following table gives the *maximum* day temperatures at twenty stations selected from the Daily Weather Report of the Meteorological Office and from the Paris *Bulletin International* for the first five days of October:—

	Station	Day 1	2	3	4	5	Mean
		°	°	°	°	°	°
British Islands	York	64	63	66	69	63	65
	Greenwich	78	68	69	79	77	74
	Parsonstown (Ireland)	58	61	61	64	66	62
France	Dunkirk	79	72	72	81	79	77
	Cherbourg	72	73	64	72	68	70
	Paris	77	65	78	78	77	75
	Nantes	79	70	81	82	64	75
	Biarritz	82	73	81	81	68	77
	Nice	72	73	75	77	75	74
Germany	Hamburg	61	68	61	64	68	64
	Berlin	63	73	63	61	66	65
	Carlsruhe	75	70	75	72	70	72
Belgium	Brussels	76	64	71	75	77	73
Austria	Vienna	73	75	73	72	70	73
Spain and Portugal	Barcelona	88	100	95	91	85	92
	Madrid	62	72	75	70	68?	69
	Lisbon	70	68	68	70	68	69
Italy	Turin	72	73	73	73	73	73
	Rome	77	79	81	81	81	80
	Palermo (Sicily)	82	84	84	82	84	83
	Mean	73	72	73	75	72	

The stations have been selected as representative of Western, Central, and Southern Europe, and the table shows well the area over which the warm weather extended.

The more northern parts of Europe did not experience any exceptional heat, the highest temperature at Copenhagen being 63°, and at Stockholm 61°. The more western parts were also but little affected: in Ireland the highest maximum was 66° at Parsonstown on the 5th, and at no other station was the temperature above 65°. In Scotland the temperature did not reach 70°.

The Greenwich observations from 1841 show that a higher temperature has only once been registered in October, viz. 81° on the 4th in 1859; but the daily mean, which was 67°·1 on the 4th this year, is higher than any previously recorded.

The observations which were made in the apartments of the Royal Society from the year 1794, excepting the years 1811 to 1819, do not show so high a reading between 1794 and 1840. At Kew Observatory the highest temperature recorded was 77°

on the 4th, and this is the highest ever observed in the month of October; on the 5th, 76° was registered, which corresponds with the temperature observed on October 4, 1859. The returns of the Meteorological Office show that 80° was observed on the 4th in London and at Cambridge, whilst 77° was registered at several stations in the east of England and in the Midland Counties.

It is difficult to make any satisfactory comparison with previous records, except at one or two places, but these tend to show that so high a temperature at this season does not occur more than about twice in a century. CHAS. HARDING

VOLCANOES OF JAPAN

THE last number (vol. ix. part 2) of the *Transactions* of the Seismological Society of Japan is wholly occupied by a paper of Prof. Milne's, on Japanese volcanoes, which is the longest contribution that has yet appeared in the Society's *Transactions*. The paper is partly historical and partly scientific, and contains, so far as the writer has been able to collect, references to everything that is known on the subject. Very much comes from his own observations, for he has travelled over the greater part of Japan, and has ascended many of the volcanoes. The paper also contains an epitome of some thirty or forty works in Japanese. On the whole, it is a systematic account of material which has been accumulating for the last eleven years.

The following are the more important conclusions which Prof. Milne has formulated in the paper:—

1. *Number of Volcanoes.*—As Japan has not yet been completely explored, and, moreover, as there is considerable difficulty in defining the kind of mountain to be regarded as a volcano, it is impossible to give an absolute statement as to the number of volcanoes in the country. If under the term volcano be included all mountains which have been in a state of eruption within the historical period, those which have a true volcanic form, together with those which still exhibit on their flanks matter ejected from a crater, we may conclude that there are at least 100 such mountains in the Japanese Empire. If to this list be added the ruins and basalt wrecks of volcanic cones, the number would be considerably increased. These mountains are distributed as follows:—

Northern Region..	Kuriles	23
	Yezo	28
Central Region ...	Northern main island / Central " " / Oshima group	35
Southern Region..	Southern main island	1
	Kiushiu / Southern islands	13
	Total	100

Of this number about 48 are still active, or have been so during the historical period. These active volcanoes are distributed as follows:—

Northern Region..	Kuriles	16	27
	Yezo	11	
Central Region			12
Southern Region			9
	Total		48

From this it will be seen that volcanic activity in Japan decreases from the north towards the south.

2. *Number of Eruptions.*—Altogether about 232 eruptions have been recorded, and of these the greater number took place in the southern districts. This may perhaps be accounted for by the fact that Japanese civilisation advanced from the south. In consequence of this, records were made of various phenomena in the south when the northern districts were still unknown and unexplored regions. The greater number of eruptions took place in February and April. Comparing the frequency of eruptions in the different seasons, the volcanoes of Japan appear to have followed the same law as the earthquakes, a greater number having taken place during the cold months. This winter frequency of volcanic eruptions may possibly be accounted for in the same manner that Dr. Knott accounted for the winter frequency of earthquakes. During the winter months the average barometric gradient across Japan is steeper than in summer. This, coupled with the piling up of snow in the northern regions, gives rise to long-continued stresses, in consequence of which certain portions of the earth's crust are more prepared to give way during the winter months than they are in summer.

3. *Position and Relative Age of Japanese Volcanoes.*—The youngest of the Japanese volcanoes appear to be those which exist as, or on, small islands. On the islands in the Kuriles, in the Oshima group, and in the Satsuma sea, many of the volcanoes are yet young and vigorous. Moreover, many of these islands have been formed during the historical period. The island-forming period in the Satsuma sea, for example, was about the year 1780.

The volcanoes of Japan form a long chain running from N.E. towards S.W.; but a closer examination of the distribution of the volcanic vents shows that there are probably four lines:—

(*a*) The N.E.-S.W. line running from Kamchatka through the Kuriles and Northern Yezo.

(*b*) The curved line following the backbone of the main island, and terminating on the western side of the Yezo anticlinal.

(*c*) The N.N.W.-S.S.E. line of the Oshima group. This line, coming from the Ladrones, passes through Oshima and Fujisan parallel to and near to the line of a supposed fault. Here it intersects the main line running through the main island. Volcanic vents are here very numerous. As the main island line is intersected, while the Oshima line is the intersector, it may be argued that the Oshima-Fujisan line of volcanoes are younger than many of those on the main island line.

(*d*) The Satsuma line, coming from the Philippines through Sakurajima and culminating in the famous Mount Aso, which is the nucleus of Kiushiu.

4. *Lithological and Chemical Character of Lavas.*—Although Prof. Milne has made an extensive collection of the volcanic rocks of Japan, the opportunity for examining them has not yet presented itself, and therefore he can only speak of them in general terms. They are at present being carefully studied by the officers of the Geological Survey. The rocks in his possession are chiefly andesites. Those containing augite, like the rocks of Fujisan, closely approximate to basalts. True basalt is, however, rare. Another common rock is hornblende andesite, some of which contains free quartz. Quartz trachytes occur in the north of Japan. The following table shows the percentages of silica, and ferrous and ferric oxide, contained in the rocks of ten volcanoes:—

Locality.	SiO_2	FeO	Fe_2O_3
1. Norokura	61·72	1·35	3·50
2. Misake	59·97	3·27	3.86
3. Kusatsu	61·49	3·30	4·35
4. Amagi (Hakone)	65·34	2·45	3·09
5. Komagadake	56·27	2·19	6·69
6. Moriyoshi	59·17	2·65	4·15
7. Chokai	60·64	3·81	3·14
	54·55	5·19	4·42
8. Hakone (Tonosawa)	48·97	4·02	4·81
9. Fujisan	49·00	5·1	6·06
10. Oshima	52·00	13·70(?)	

One feature exhibited by the table is that the rocks of Oshima, Fujisan, and Tonosawa are basic, while those like Chokaisan and Moriyoshiyama belonging to the line of volcanoes of the main island, are relatively acidic. More extended observations of this description may show that different lines of volcanoes have thrown out different lavas, or that the lavas of different constitution are of different ages.

5. *Magnetic Character of Rocks.*—In a study of the soils in the neighbourhood of Tokio, Mr. E. Kinch refers specially to the magnetite they contain. A great portion of this comes from the disintegration of volcanic rocks. Many of the Japanese lavas have a distinct effect upon a compass needle, and many of the black lavas from the crater of Fujisan will easily turn the needle of an ordinary compass through 360°. Many of the pieces of lava are not only magnetic but polar. Dr. Naumann found a block of augite trachyte on the top of Moriyoshiyama which would deflect the needle of a compass through 155°. The most curious observation made by this investigator was that the magnetic declination near Gaujusan has during the last eighty years (when it was about 14° 30′ E.) decreased 19°, being now about 5° W. As we recede from this mountain the amount of

change is less. Assuming this result to be correct, it would seem justifiable to look to Gaujusan as connected with these local changes. Some of the volcanoes in the Kuriles are said to exert a marked influence upon the compasses of ships. When a vessel is lying near certain mountains, as, for instance, in Bear Bay at the north end of Iturup, a distant mountain will have a very different bearing to that which is indicated by the same compass when the vessel is a short distance outside Bear Bay. In both cases the ship may be lying in the same direction, and the direction of observation is practically along the same line. This leads Prof. Milne to urge, as he has already done, that a magnetic observatory should be placed on or near one of the nine active volcanoes of Japan. Changes in volcanic activity are probably accompanied by local changes in the magnetic effects produced by subterranean volcanic magmas. These changes may be due to alterations in position, alterations in chemical constitution, and changes due to the acquisition or loss of heat. If such is the case, he argues, the records of a magnetic observatory would lead up to a knowledge of the changes taking place beneath the ground. When it is remembered that volcanoes like Oshima (Vries Island), where it seems probable that there may be local and rapid changes in magnetic variation taking place, lie in the track of so many vessels, the proposed investigation has a practical as well as a scientific aspect. An investigation of earth-currents at and near volcanoes might be added to the magnetic investigations.

6. *Intensity of Eruptions.*—It appears from the accounts of eruptions which are given in the paper that the intensity of volcanic action in Japan has been as great as in any other part of the world. One period of unusual activity was between the years 1780 and 1800, a time when there was great activity elsewhere in the globe. It was during this period that part of Mount Unsen was blown up, and from 27,000 to 53,000 persons (according to different accounts) perished, that many islands were formed in the Satsuma sea, that Sakurajima threw out so much pumice material that it was possible to walk a distance of 23 miles upon the floating *débris* in the sea, and that Asama ejected so many blocks of stone—one of which is said to have been 42 feet in diameter—and a lava-stream 68 kilometres in length.

7. *The Form of Volcanoes.*—The regular so-called conical form is very noticeable in many of the Japanese mountains, especially perhaps in those of recent origin. Outlines of these volcanoes, as exhibited either by sketches or photographs, show curvatures which are similar to each other. From a collection of photographs Prof. Milne traced the profiles of a number of important mountains in Japan. These are reproduced in the paper (see Fig. 1). From an examination of these figures he found that the

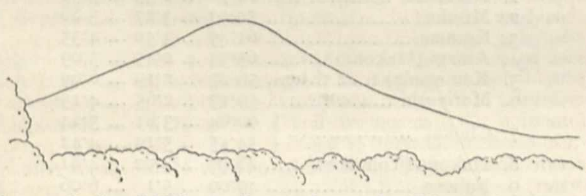


FIG. 1.—Outline of Fujiyama, from a photograph. This may be taken as typical of many Japanese volcanoes.

curvature of a typical volcano was logarithmic, or, in other words, the form of such a mountain was such as might be produced by the revolution of a logarithmic curve round its asymptote. In his original paper on the subject he said that the form agreed with that which would be produced by the piling up of loose material. He ought to have said it was the form assumed by a self-supporting mass of coherent material. Mr. George F. Becker (*American Journal of Science*, October 1885) continues these observations by an analytical investigation of the conditions of such equilibrium. If the height of a column is a, its radius y, the distance of any horizontal plane from the base x, the specific gravity of the material ρ, and the co-efficient of resistance to crushing at the elastic limit k, then the equation of the curve, which by its revolution about the x axis will generate the finite unloaded column of the "least variable resistance" is—

$$\frac{y}{c} = \frac{e^{-\frac{x}{c}} - e^{\frac{x}{c}}}{2},$$

where

$$c = \frac{2k}{\rho}.$$

This latter quantity is of course different for different materials. It can be expressed in terms of x and y—

$$\frac{2k}{\rho} = \frac{y}{(\tan^2 d - 1)^{-\frac{1}{2}}},$$

d being the angle which the tangent at any point makes with the x axis. The value of c can be obtained from photographs or drawings of a mountain, while ρ may be obtained from pendulum experiments or from specimens of volcanic material. With these data we can determine the modulus of resistance at the elastic limit of the materials which compose a mountain on a large scale for many constituents of the earth's crust. Mr. Becker concludes his observations by remarking that a study of the form and dimensions of lunar volcanoes would lead to values of $\frac{k}{\rho}$, from whence we might approximately determine whether the lunar lava is similar to that of terrestrial origin. In the table which follows, Prof. Milne has followed out Mr. Becker's suggestion, and calculated the modulus of resistance to crushing at the elastic limit in pounds per square foot for a number of Japanese mountains. The different values for $\frac{2k}{\rho}$ for the same mountain is in great measure due to the absence of an accurate scale for the various photographs which had to be investigated. Another difficulty was obtaining a value for r, or the density of the mountain. Prof. Mendenhall, who made a number of experiments with pendulums on the summit of Fujisan, says the rocks of that mountain have a density of 1·75. This is when they have air in their pores. As powder the density becomes 2·5. Wada gives the specific gravity of the rock on Fujisan as 2·6.

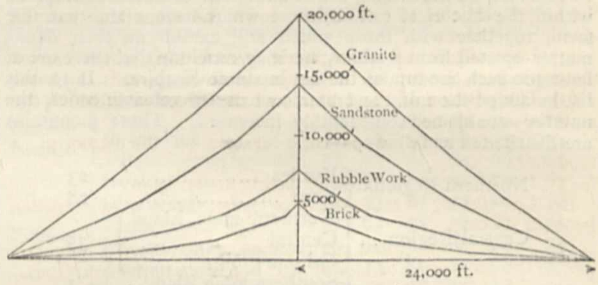


FIG. 2.—Theoretical Mountains.

Assuming the density of the earth at 5·67, then the density of Fujisan, as determined by Prof. Mendenhall's experiments, is 2·08. In the following table the density of the materials of all the mountains mentioned is taken at 2·5.

	Height in feet	$\frac{2k}{\rho}$	$\frac{k}{\rho}$	Load in lbs. per square foot	
Fujisan	12,441	4200	...	...	Photograph
	...	5000	...	...	,,
	...	4240	...	...	,,
	...	3500	...	...	,,
	...	{ 5420 / 5450 }	...	...	,,
	...	5440	...	...	,,
	...	{ 3945 / 4133 }	...	...	,,
	...	4430	...	...	Surveyed section
	...	3640	...	...	,, ,,
Average for Fujisan	...	4490	2245	350,220	,, ,,
Iwakisan	5260	2360	1180	174,080	Photograph
Nantaisan......	3800 [1]	2000	1000	156,000	,,
Alaid............	7773	{ 2195 / 2120 }	1078	163,168	,,
Krakatão	2745 / ...	{ 1310 / 1310 }	655	102,180	Surveyed section

Comparing the results given in the above table with the numbers given in the next section, which are based on experi-

[1] This is the height above Lake Chuzenji.

ments referred to in Rankine's "Civil Engineering," it may be said that the average strength of Fujisan lies between that of rubble work and sandstone; Iwakisan, Nantaisan, and Alaid are like good rubble masonry, while the strength of the ill-fated Krakatão is not much above that of ordinary brickwork.

8. *Theoretical Mountains.*—As it might be interesting to compare actual mountains with theoretical mountains constructed from the equation—

$$y = \frac{c}{2}\left(e^{\frac{n}{c}} - e^{-\frac{n}{c}}\right)$$

such mountains have been drawn, and are shown in Fig. 2. The values of *c* are given in the following table.

In drawing up the table the instantaneous breaking strength of granite and its crumbling strength, which is the largest possible value for *k*, are taken as being equal. For sandstone the crumbling strength is assumed to be three-fourths of the breaking strength, while for rubble work and brickwork it has been taken as one-half.

Material	Instantaneous breaking strength in lbs. square feet	Crumbling strength or *k* in lbs.	Weight cubic foot lbs.	$c = \frac{2k}{\rho}$
Granite	1,584,000	1,580,000	170	18,500
Sandstone............	790,000	590,000	144	8,200
Rubble masonry ...	316,000	150,000	120	2,500
Brickwork	144,000	72,000	112	1,300

The diameter of the base of each of these mountains is 48,000 feet, and the height to which mountains of the following different materials could be built upon such a base without crushing would approximately be :—

Brickwork .... 4,600 feet	Sandstone...... 14,500 feet
Rubble masonry .. 7,300 ,,	Granite .... 20,000 ,,

9. *Causes Modifying Volcanic Forms.*—Causes modifying the natural curvature of a mountain are :—

(1) The tendency during the building up of the mountain of the larger particles to roll farther down the mountain than the smaller particles.

(2) The effects of atmospheric denudation, which carries materials from the top of the mountain down towards the base.

(3) The position of the crater, and the direction in which the materials are ejected.

(4) The existence of parasitic craters on the flanks of a mountain.

(5) The direction of the wind during an eruption.

(6) The sinking of a mountain in consequence of evisceration beneath its base.

(7) The expansions and contractions at the base of a mountain due to the acquisition or loss of heat before and after eruptions.

10. *Effect of Volcanic Eruptions on the People.*—The eruptions in Japan from time to time have exerted a very marked influence upon the minds of the Japanese people. Divine interference has been sought to prevent eruptions, priests have been ordered to pray, taxes have been repealed, charities have been instituted, special prayers against volcanic disturbances have been formulated, and have remained in use for the period of 100 years, while special days for the annual offering up of these prayers have been appointed. At the present day a form of worship to mountain deities is not uncommon.

SOLUTION[1]

Opening of the Discussion by Prof. Tilden

FOR want of time, the consideration of various phenomena connected with the subject was necessarily omitted. Thus no reference could be made to the various formulæ relating to expansion or density of solutions, nor to their optical properties, magnetic rotation, nor to the subject of electrolysis. In what follows, a review is presented of the principal phenomena observed in the act of solution of solids (especially metallic salts and other comparatively simple compounds) in liquids, and the chief properties of the resulting solutions, with the object of arriving (if possible) at some conclusion as to the physical explanation of the facts. The question must at once arise whether these phenomena are to be considered as chemical or mechanical, and all the theories which have been put forward to explain the nature of solution are roughly divisible into two classes, according as, on the one hand, they represent the process as a kind of chemical combination, or, on the other, explain the phenomena by reference to the mechanical intermixture of molecules, or by the influence of the rival attractions of cohesion in the solid and liquid, and of adhesion of the solid to the liquid. The former hypothesis seems to have been universally adopted by the older writers, such as Henry and Turner, and it seems pretty clear that Berthollet also regarded solution as an act of chemical combination. Among modern chemists, Prof. Josiah P. Cooke takes a similar view, but M. Berthelot is the most consistent and powerful supporter of the same hypothesis. In his "Mécanique Chimique," tome ii. p. 160, will be found a very clear and formal statement of the views upon this subject which, it is interesting to know, are retained by M. Berthelot without modification in any essential particular.

On the other hand, there are a number of writers who, whilst referring the phenomena of solution to a molecular attraction of some kind, do not attribute solubility to the formation of chemical compounds of definite composition. Graham distinctly ranges himself on this side. Brande also appears to have taken a similar view; Daniell, Miller, Nicol, and Dossios may be more or less ranked with them. A theory differing in some important respects from those of the above writers was briefly enunciated in a paper communicated to the Royal Society by Tilden and Shenstone in 1883. In discussing the connection between fusibility and solubility of salts, the authors point out that the facts tend to "support a kinetic theory of solution, based on the mechanical theory of heat. The solution of a solid in a liquid would accordingly be analogous to the sublimation of a solid into a gas, and proceeds from the intermixture of molecules detached from the solid with those of the surrounding liquid. Such a process is promoted by rise of temperature, partly because the molecules of the still solid substance make longer excursions from their normal centre when heated, partly because they are subjected to more violent encounter with the moving molecules of liquid." This theory, however, only relates to the initial stage of the process of solution, and does not sufficiently explain saturation nor the influence of dissolved substances upon vapour-pressure, specific heat, specific volume, &c. How far is it true that evolution of heat indicates chemical combination: does the evolution of heat which often takes place on dissolving a solid in water, or on adding more water to its solution, indicate the formation of hydrates, *i.e.* compounds of the dissolved body with water in definite proportions? Thomsen answers this question in the negative ("Thermochemische Untersuch.," Band iii. p. 20).

Take the case of sulphuric anhydride (SO_3). It is evident from the diagram exhibited that more than half the total evolution of heat occurs on addition of the first molecule of water to the solid substance; yet the succeeding molecules give quite an appreciable thermal change. At what point in such a curve should we be justified in setting up a distinction between the effect due to chemical combination and that due to other causes, such as the change of volume consequent on dilution or the possible loss of energy from the adjustment of the motion of the molecules of the constituents to the conditions requisite for the formation of a homogeneous liquid, or (though not in the present case) the decomposition of the compound by the water? In the act of solution of the solids, and especially of anhydrous salts in water, the volume of the solution is always less than the sum of the volumes of the solid and its solvent, with the exception of some ammonium salts in which expansion occurs. Similarly the addition of water to a solution is followed by contraction. This contraction may be due to mere mechanical fitting of the molecules of the one liquid into the interspaces between the molecules of the other (see Mendelejeff's abstract in *Journ. Chem. Soc.*, Feb. 1885, p. 114). This would probably not be attended by loss of energy. Or the contraction may arise from the readjustment of molecular motion already referred to.

If we know the coefficient of expansion of the liquid and its specific heat, we can calculate the amount of heat evolved for a given contraction. If this is done for sulphuric acid, and many other cases, it is found that, after accounting for the thermal change due to alteration of volume alone, there is a surplus of heat evolved which may really indicate some kind or some amount of chemical combination.

Thomsen has found that as a rule the heat of solution and of dilution are both either positive or negative. Of thirty-five salts examined, only four supply well-marked exceptions. However we may ultimately explain the anomaly exhibited by these salts, the fact remains that the heat evolved or absorbed during the admixture of any substance with water is in every case a continuous function of the quantity of water added. Similarly

[1] Report of a discussion at the Birmingham meeting of the British Association.

the contraction which ensues on diluting an aqueous solution proceeds continuously, and the molecular volume of a salt in solutions of different strengths is continuously greater the larger the amount of salt present. So that in none of these thermal or volumetric phenomena is any discontinuity observed, or any indication of the formation of compounds of definite composition, distinguishable by characteristic properties.

The question we are now considering, as to whether in a solution the solvent and the substance dissolved in it—or any portion thereof—exist independently of each other, is in some degree answered by the facts known as to the specific heats and vapour-pressures. For instance, when water is added to a solution of sodium nitrate, the molecular heat of the resulting liquid seems to show that all the water added is influenced at least until a very large quantity is present. In this case one molecule of sodium nitrate can affect the movements of a hundred molecules of water, and probably more. It is also well known that the vapour-pressures of water holding in solution almost any dissolved solid is less than the vapour-pressure of pure water, and that the boiling-point of a liquid is raised by the addition to it of any soluble non-volatile substance. This fact of reduction of pressure can only be explained upon the hypothesis that there is no free water present at all; that is, that there is no water present which is not more or less under the influence of the dissolved substance.

What becomes of water of crystallisation forms a part of the same question as to the relation of solvent to solvent. Observed facts lead us to conclude that white copper sulphate, blue anhydrous cobalt chloride—and, by analogy, other salts which are colourless—retain their hold upon water of crystallisation when they are dissolved in water. A very important observation has been made by Dr. Nicol which bears directly upon this question. In his study of the molecular volumes of salt solutions he finds that, when a salt containing water of crystallisation is dissolved, this water is indistinguishable by its volume from the rest of the water of the solution. In the report presented to the British Association last year, the following passage occurs: "These results point to the presence in solution of what may be termed the anhydrous salt in contradistinction to the view that a hydrate, definite or indefinite results from solution; or in other words, no part of the water in a solution is in a position relatively to the salt different from the remainder."

These two statements, however, are not strictly consequent upon each other. The view seems preferable that (save, perhaps, in excessively dilute solutions) the dissolved substance is attached in some mysterious way—it matters not whether it be supposed to be chemical or physical—to the *whole* of the water. We cannot otherwise get over the difficulty presented by the hydrated salts, which give coloured solutions, by the control of the vapour-pressure of the dissolved salt, and by the altered specific heat. With regard to water of crystallisation, E. Wiedemann has shown that hydrated salts in general expand enormously at the melting-point; and the observations of Thorpe and Watts on the specific volume of water of crystallisation in the sulphates of the so-called magnesium group show that, whilst the constitutional water occupies less space than the remaining molecules, each successive additional molecule occupies a gradually increasing volume. So that when a salt, with its water of crystallisation, passes into the liquid state (either by melting or by solution in water), it requires a very slight relaxation of the bonds which hold the water to the salt for it to acquire the full volume of liquid water, whilst the water of constitution is not so easily released. And this conclusion accords with Nicol's observations on the molecular volumes of the salts when in solution.

Now comes the question as to what determines the solubility of a substance. Why, for example, is magnesium sulphate very soluble in water, whilst barium sulphate is almost totally insoluble? With regard to salts the following propositions seem to be true:—(1) Nearly all salts which contain water of crystallisation are soluble in water, and for the most part are easily soluble; (2) insoluble salts are almost always destitute of water of crystallisation and rarely contain the elements of water; (3) in a series of salts containing nearly allied metals the solubility, and capacity for uniting with water of crystallisation generally, diminish as the atomic weight increases.

The fusibility of a substance has also much to do with its solubility. Neither fusibility alone nor chemical constitution alone seems to be sufficient to determine whether a solid shall be soluble or not. But it may be taken as a rule to which there are no exceptions that when there is a close connection in chemical constitution between a liquid and a solid, and the solid is at the same time easily fusible, it will also be easily soluble in that liquid.

Salts containing water of crystallisation may be considered as closely resembling water itself, and these are for the most part both easily fusible and easily soluble in water. But space is wanting for the discussion of the details of these matters, as well as of the relation of molecular volume to fusibility of solids.

The fascinating character of the phenomena of supersaturation has attracted a host of experimenters, but no definite explanation has been generally accepted. In the opinion of the speaker supersaturation is identical with superfusion. Supersaturated solution of, say, alum, thiosulphate of sodium melted in its water of crystallisation, and fused sulphur at 100°, exhibit phenomena of exactly the same kind.

Finally, we are led to the consideration of what is meant by chemical combination. From the phenomena under discussion, and others, the conclusion seems inevitable that chemical combination is not to be distinguished by any absolute criterion from mere physical or mechanical aggregation; and it will probably turn out ultimately that chemical combination differs from mechanical combination, called cohesion or adhesion, chiefly in the fact that the atoms or molecules of the bodies concerned come relatively closer together, and the consequent loss of energy is greater.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

CAMBRIDGE.—Of the students in Natural Science entered at Cambridge this term no fewer than 116 have already announced their intention of studying medicine.

DUBLIN.—The Senate of the Royal University has conferred the degree of Doctor of Science *honoris causâ* upon James Bell, Ph.D., F.R.S., Principal of the Somerset House Laboratory.

SCIENTIFIC SERIALS

Revue d'Anthropologie, troisième série, tome 1, Paris, 1886.—On the Simian characters of the Naulette jaw, by M. Topinard. This celebrated find, which was discovered at the bottom of an obscure cavern 25 m. below the present level of the Lesse, near Dinant, in Belgium, is chiefly remarkable for its excessive prognathism, which is due alike to the great thickness of the horizontal branch of the jaw when compared with its height, and to the special obliquity of the axis of the alveolus of the second molar. In its relative proportions the Naulette jaw must be characterised not only as non-human, but as plus-Simian. A careful comparison of the Naulette jaw with the maxillary processes of the anthropoids, and of several of the lowest extant human races, has led M. Topinard to the conclusion that in the age of the mammoth, tichorine rhinoceros, and cave-bear, there had already appeared numerous mixed human types, to one of the lowest of which it may be presumed that the Naulette jaw belonged.—On the population of Bambouk, on the Niger, by Dr. Colin. An interesting paper on an extensive, but very imperfectly-known, region of Western Soudan, exclusively inhabited by a branch of the great Manding race, known as the Mali-nkès. The Bambouk territories, more than 600 kilometres in length, and from 80 to 150 in width, are divided into numerous little States, most of which enjoy a complete autonomy. Their want of consolidation, and the indifference of the people to all forms of religion, have made the Mali-nkès objects of contempt to their Mussulman black neighbours, but according to the narrations of the Griotes, or itinerant bards, who are to be met with in every part of Western Africa, they had at one time extended their dominion over all the tribes on the right banks of the Niger, and were preparing to invade Saigon when the advance of the French forced them to fall back within their original limits. For a time they submitted to the restrictions of Mohammedanism, but now they appear to have absolutely no religion. They prepare an intoxicating drink from honey, called "dolo," in which women as well as men indulge to excess. The men are indolent, hunting only to avert starvation, and working their extensive gold-mines imperfectly, and chiefly by the help of the women, to whom falls the chief share of providing for the wants of the community, but who, after marriage, enjoy great freedom, although the young girls are kept under strict supervision.—On the human bones found in France in caverns belonging to the Quaternary age, by M. Cartailhac. Of such finds, none can be referred to the early period of the Saint Acheul, or Chelles deposits, the oldest belonging apparently to the Mousterian age, while the most abundant human remains are found in the comparatively recent beds of Solutré and La Madelaine. The former of these are remarkable for the enormous number of horse-bones accumulated about the stone hearths and in the kitchen-middens of this station. According to Dr. Cartailhac, 40,000 skeletons might be reconstructed from these equine remains, which seem to have been exposed to the action of fire, the greater number of the bones having been broken for the extraction of the marrow, whence he assumes that the horse must have reached its maximum development and served in the place of all other game at the period of the Solutré deposits. The writer compares together the human and other remains found in various Mediterranean and inland caves, with the special object of ascertaining how far the condition and mode of deposition of the skeletons can throw light on the vexed question whether the great preponderance of fractured over whole bones in these primæval graves indicates the practice of cannibalism, or whether it may not be dependent on the observance of special modes of burial, involving the burning or dismemberment of the body after death.—The facial angle proposed by Cuvier and Geoffroy Saint-Hilaire for comparative anatomical determinations and for measuring facial differences in the living subject, by Dr. Collignon. The writer, who considers at length the merits of the various angles proposed by Camper and others, concludes by showing the superiority, for practical purposes, of adopting Cuvier's facial angle, measured by Topinard's goniometer for determining the median angle.

SOCIETIES AND ACADEMIES

Paris

Academy of Sciences, October 26.—M. Jurien de la Gravière, President, in the chair.—On the unequal flow of gases, by M. Haton de la Goupillière. In continuation of his recent communication on this subject the author here deals with the reverse problem of a receptacle originally filled with compressed air discharging itself freely into the atmosphere.—On the intensity of the magnetic field in dynamo-electric machines, by Marcel Deprez. Assuming that the most important element of a dynamo-electric machine, whether employed as a generator or receiver, is the magnetic field, the author deals with the influence of the deviation of the magnetic pieces, and shows that, contrary to the opinion of certain electricians, the intensity of the field decreases far less rapidly than the distance of the magnetic pieces increases. The influence of the dimensions perpendicular to the lines of force is also considered.—Researches on the decomposition of the bicarbonate of ammonia by water, and on the diffusion of its components through the atmosphere, by MM. Berthelot and André. From the experiments here described, the authors are led to the conclusion that it is the diffusion of the carbonic acid that determines the decomposition by water of the bicarbonate of ammonia, and consequently the transport of the ammonia itself. These results are of the greatest importance even for the purely physical study of the circulation of gases between the ground, the waters, and atmospheric air, apart altogether from the phenomena of vegetation.—Note accompanying the presentation of his work entitled "An Introduction to the Study of the Human Races," by M. de Quatrefages. This is the first volume of the "Bibliothèque d'Ethnologie," edited jointly by the author and M. Hamy. It contains a summary of the views expounded in greater or less detail in his other writings, while dealing more fully with a number of other matters, which he had hitherto merely indicated, or else entirely neglected for lack of the fresh data and discoveries which now enable him to discuss them seriously. One of the most important is the question of prehistoric man, and he now shows that even in Quaternary times the human race had already spread over the whole earth to the remotest extremities of the Old and New World. This ubiquity of Quaternary man already suggested the existence of the species in the previous epoch, and direct proofs of this fact have recently been multiplied to such an extent that the presence of man in Europe during Tertiary times may now be regarded as placed beyond reasonable doubt, although his presence in America is not yet established. The results yielded by palæontology, geology, and even history point to the extreme north of Asia as the cradle of the human race and the centre of dispersion, which had already begun in Tertiary times. Here also were differentiated the three fundamental types, to which all races may still be reduced, as well as the three linguistic types diffused throughout the globe. It is further shown that hypsistenocephaly is the main feature distinguishing the American from the European primitive race, and that the man of Canstadt, hitherto regarded as the oldest Quaternary type, in reality dates back to the Tertiary epoch.—Note on the meteorite which fell on January 27, 1886, at Nammianthul, in the Presidency of Madras, by M. Daubrée. This meteorite, a specimen of which has been received from Mr. Medlicott, of the Indian Geological Survey, presents the ordinary characters of the group of small sporadic asters.—Experiments on the transmission of force by means of a series of dynamo-electric machines coupled together, by M. Hippolyte Fontaine. These important experiments (carried out with seven Gramme machines, under the inspection of the Commissioners, MM. Bertrand, Becquerel, Cornu, Maurice Lévy, Marcel Deprez, and Mascart) show that it is possible to transmit an effective force of fifty horse-power through a resistance of 100 ohms at a loss of less than 50 per cent.—On algebraic surfaces capable of a double infinity of birational transformations, by M. E. Picard. In supplement to his previous communication on algebraic surfaces, the author here shows that, for all surfaces capable of a double infinity of birational transformation, the co-ordinates of any given point are expressed by the uniform (Abelian) functions of two parameters.—On the transformation of surfaces in themselves, by M. H. Poincaré. It is shown in connection with M. Picard's theorem that, in certain cases, the Abelian functions may degenerate into triply periodical, elliptical, or even rational functions.—Extension of Riemann-Roch's theorem to algebraic surfaces, by MM. Noether.—On the recomposition of white light by means of the colours of the spectrum, by M. Stroumbo. A process is described by means of which the recomposition of white light is effected, taking as the starting-point the very colours of the spectrum, and utilising, as in Newton's experiment with the disk, the persistence of the images on the retina.—Note on the principal showers of shooting-stars and the aurora borealis, by M. Ch. V. Zenger. A careful study of M. Rubenson's great Catalogue of the Auroras from 1800 to 1877 has unexpectedly revealed the fact that August 10 and November 14 show a great frequency of these lights, thus coinciding with the periods of the shooting-stars and suggesting a connection between these two orders of phenomena.—Influence of the amplitude of the lunar oscillation in declination on the shiftings of the northern trade-winds, by M. A. Poincaré. A study of the tables for 1880 83 shows certain relations between these phenomena, which, however, differ greatly according to the seasons.—On the phenomena associated with the heating and cooling of molten steel, by M. Osmond. It is shown that, as the quantity of carbon is increased, the temperature of transformation of the iron is lowered, and that of recalescence raised, so that both coincide in the hard steel.—Saturation of normal arsenic acid by the water of baryta, by Ch. Blarez.—On the function of the semicircular canals of the inner ear, by M. Yves Delage. The chief function of this apparatus, as already recognised by Goltz, Flourens, and others, is shown to be distinct from that of the auditory sense, and connected rather with the rotatory movements of the head, either alone or with the body.—On Syndesmis, a new type of Turbellariæ described by W. A. Sillimann, by M. Ph. François. This organism is shown to be, not an ectoparasite of the large green nematoid, as supposed by Sillimann, but a true endoparasite of *Strg. lividus*.—On two Synascidians new to the French sea-board (*Diazona hebridica*, Forbes and Goodsir, and *Distaplia rosea*, Della Valle), by M. A. Giard.—Organisation of *Lepidomenia hystrix*, a new type of Solenogaster, by MM. Marion and Kowalevsky.—On the Gephyrians belonging to the family of the Priapulidæ collected by the Cape Horn Mission, by M. Jules de Guerne. The discovery of these organisms is a remarkable instance of the presence in the southern seas of forms almost identical with those of the Arctic Ocean.—The simple epidermis of plants considered as a reservoir of water, by M. J. Vesque.—Remarks on *Poroxylon stephanense*, by MM. C. Eg. Bertrand and R. Renault.—On the taxonomic importance of the petiole, by M. Louis Petit.—On the reproductive organs of vegetable hybrids, by M. Léon Guignard.—On the relations of geodesy and geology: a reply to the observations of M. Faye, by M. A. de Lapparent.

BERLIN

Meteorological Society, October 5.—Dr. Brix, in the name of the Telegraph Administration, handed over to the Society a paper containing the results of observations respecting earth-currents instituted through the medium of German telegraph lines, and giving a brief history of these investigations.—Dr. Assmann spoke of the thunderstorms of the summer of 1886.

Physical Society, October 22.—Prof. von Helmholtz in the chair.—Prof. Börnstein communicated the results of his investigations into the thunderstorms of July 1884. The days from July 13 to 17 were very prolific in thunderstorms, and respecting them the speaker had collected and elaborated observations from more than 200 stations in Germany. For twenty-four separate thunderstorms, drawings were made of the "isobronts," isobars, and isothermals, from which it appeared that a fall in the barometer always preceded the outburst of the storm; that with the occurrence of the sinking of the barometer the atmospheric pressure rose very steeply and then relapsed gradually to its former level; and that the temperature, which was very high before the storm, declined rapidly with the outbreak of the storm. Local observations had formerly led to the same result. The "isobronts," or the lines uniting the places where the first peal of thunder was simultaneously heard, had in general a north-south direction. The "isobronts" made the passage from west to east with an average swiftness of from 38 to 39 kilometres an hour. The "isobronts" were attracted by the mountains, so that the part in whose west-east direction a mountain was situated approached it sooner, and, after the passage of the "isobront," delayed there longer than did the remaining part. Rivers retarded the progress of thunderstorms, and small thunderstorms often terminated at large rivers without crossing them. This relation of thunderstorms to mountains and rivers might be explained on the assumption that the storms were caused by ascending air-currents. When such an ascending air-current approached a mountain, then the mountain hindered the horizontal air from flowing in at the anterior side of the ascending current. The air flowing in at the posterior side, on the other hand, thereby obtained the preponderance, and urged the phenomenon with all the greater force to the mountain. The reverse occurred after the thunderstorm had surmounted the mountain. The horizontal currents in front then obtained the preponderance, and delayed the progress of the storm. The influence of the rivers found its explanation in the fact that the air above the water was considerably cooler than the air above the land, whereby a descending air-current was continuously maintained, operating in opposition to the ascending current of the thunderstorm, to the possible degree even of annulling it. The speaker had been able artificially to produce an imitation of all these processes by causing, in accordance with the directions of Dr. Vettin, visible currents to ascend in a glass box filled with tobacco smoke, by means of local depressions of temperature, by setting these currents in constant motion, and making them strike against obstructions (corresponding with the mountains), as also on descending currents which were likewise artificially created. In the discussion which followed the above address, Dr. Vettin laid stress on the fact that precisely at the moment when the barometer mounted steeply from its lowest position, the thunder followed the lightning most rapidly, and discussed how, in accordance with his conception of the nature of thunderstorms, by the curving round of the ascending air-current, a whirling movement round a horizontal axis came into shape, whereby, as determined by its situation and its extent, were produced thunderstorms, sleet, and hail.—Prof. von Helmholtz described the formation of a thunderstorm observed by him in Rigi-Kaltbad. From a free point of prospect, allowing a survey of the plain as far as the Jura, he observed how the lower warm and moist layer of air was distinguished by a sharp horizontal boundary of somewhat long strips of cloud from the upper dry and cooler air. The cloud-masses resembling the stripe-shaped cirri diffused themselves and formed a coherent level boundary-layer between the two air-masses. He next noticed, at different spots, balls of cloud arise above the boundary-layer, evidently as the effects of ascending air-currents. The different cloud-heaps then rose higher and grew into larger cloud-masses within which different electric sparks leapt from one spot to another. It was only subsequently that he saw the lightning fly downward to the earth. At last a heavy rain rendered the lower air-mass, bounded by the horizontal cloud-basis occupying a position nearly at a level with the height of the stand-point, which had hitherto been clear, opaque. The phenomenon had developed itself under weather in which the wind was at rest, and could be followed very precisely into its details.—Prof. Schwalbe reported on an investigation of Herr Meissner, who, in the Strasburg Laboratory, had determined the warmth effect on the wetting of powdery bodies. In the way of powder were used amorphous silicic acid, glass, emery, carbon; as fluids, distilled water, benzol, and amyl alcohol. In all cases an increase of temperature was observed.

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